Reply to anonymous referee #1

General comments: This is a well-written manuscript that describes a "milestone" in ice-stream research: the ability to explain the M_sf amplitude in the Rutford Ice Stream's horizontal flow. Prior to this study, the notion that an ice stream could have horizontal flow variations driven by tides at the M_sf frequency was understood; but was not well-reproduced in models, because of difficulty reaching the high-level of amplitude. This study finally adds the key ingredient to overcome this problem: the tidally driven fluctuations in the water pressure below the ice stream, driven by water pressure fluctuations at the grounding line caused by the ocean tide is what does it.

I have no substantial comments that would lead to clarifications, and believe the paper to be of great value and interest to the community of researchers studying ice stream phenomena.

Specific comments:

p 2398, line 10 - "tidal subglacial water pressure variations". . . It would seem to be better to say "subglacial water pressure variations with a tidal period" or "subglacial water pressure variations caused by the tide". . . as it is unclear what "tidal subglacial water pressure" refers to. . .

Elsewhere in the text we refer to "tidally induced subglacial pressure variations" and so this phrase has been used instead to make it clearer what is being referred to.

line 16 & 17 - ". . .show that the presence of tides. . ." Again, possibly being more specific might help. Tides in the ocean beyond the ice stream? Tidal variations in the subglacial water pressure?

Changed to: 'Coupled model results show that ocean tides downstream of the grounding line result in a ~12% increase in mean horizontal velocity of the adjoining ice-stream.'

p 2399, line 21: "provides a window into the mechanisms that. . ." I would prefer ending this phrase with ". . .mechanisms that influence basal sliding. Saying that "observing and modeling tidally-induced modulation . . . provides a window into the mechanisms . . . causing these effects" sounds sort of self-apparent, and I think the abstract, in the last sentence, has already pointed out that the value in studying this phenomena is to "see the bed". . ..

Done

p 2400, l 4: "tidal stresses" An oceanographer might get confused by what was meant here, e.g., are tidal stresses "frictions" from the ocean water flowing on the bottom of the ocean? I think that what is meant are the variations in stress caused by all the effects of the ocean tide seaward of the grounding line.

Changed to: 'tidally-induced stresses'.

p 2402, l 17 : Just a comment (based on my own ignorance): I think that in the present case, where the actual movement of the ice through a cycle of interest is relatively small (i.e., flow is only a few meters or tens of meters over a 14-day cycle) the upper convected time derivative may be replaced with the regular time derivative. (I have a very fuzzy idea about what the upper convected time derivative is relative to types of time derivatives; and wonder what a "lower convected" time derivative would be. I should probably learn this stuff.)

Although we agree in principle that the upper convected derivative is not essential in this case, since both strain and rotation are generally small in our model, this is the default implementation in the finite element solver that we use and can be applied to any finite strain so we choose to use it and avoid any ambiguity that might arise from using a regular time derivative instead.

p. 2403, l 13: Is there an estimate of how far the grounding line is expected to migrate (is that known from interferometry?) It is not an important detail, but the question occurred to me.

Rignot et al (2011) discuss grounding line migration observed through interferometry and present a dataset that includes upper limit of ice flexure. Across Antarctica migration distances of between 100m and 3km are reported but the RIS is not mentioned specifically.

l 21: add "the" after the word "between". . ..

Done

p. 2405, l 10-11: Why is a distinction being made between hydraulic head and (in the parentheses) water pressure. . .? Are they not the same?

Water pressure is different from hydraulic head, the two are related through Eq. 11. We choose to solve for hydraulic head because this lends itself to looking at perturbations in the subglacial pressure that drive the flow rather than making assumptions about the actual water pressure at the bed.

p. 2405, l 16-18: Is it being said that the basal slipperiness is being "linearized", i.e., that the tidally varying head is accommodated in an approximate way by accounting for its mean and the perturbation?

Yes we assume that water pressure can be separated into a mean and tidal component and that there is an approximately linear relation, this will be made clearer in the text.

p. 2407, l 10: replace "slap" with "slab"

Done

p. 2408, subheading 1.5: "initialisation" should be "initialization"

Done

p. 2408: question of clarification. If only the largest 6 tidal constituents are used to force the model, is it really possible to study the M_sf response? Is what is referred to as the M_sf response simply the "harmonic beat" that is commonly referred to as the "spring to neap" tidal amplitude envelope?

The M_sf response on the ice stream is not a harmonic beat, this will be made clearer in the text. Modulation in flow at M_sf frequency arises because velocity variation over one tidal cycle is strongly asymmetric, and therefore during a spring tide the ice stream moves much further downstream than during a neap tide.

p. 2411, l 2: They are not really "tidal observations" that are being matched, they are the ice-stream velocity observations that show influence of the tide. . . tidal observations are different entities: they are observations of the vertical height of the effective sea surface with time. . .

This has been altered to 'no model has been presented that can reproduce the tidally-induced horizontal velocity variation observed on the RIS'.

l 3: change "Mechamism" to "mechanism"

Done

A further question of clarification: the CATs model is used to provide the tidal forcing, is the forcing expressed entirely by the change in effective sea surface elevation at the grounding line? or are other aspects of the CATs model (e.g., involving currents below the ice shelf out in front of the gl) involved?

The CATS model only provides changes in sea surface elevation beneath the entire floating portion of the model. The tidal forcing does not vary spatially, nor do we include tidal currents in the model. The effect of currents on basal drag is considered negligible (eg. Brunt 2008, Makinson et al 2012) and basal melt (which would be effected by currents) is not included in our model.

Finally, have the tides ever been observed at the grounding line, i.e., with an accurate GPS measuring vertical elevation changes with time? If so, how do they compare with CATs, and if not, why not?

GPS measurements of vertical elevation change on the RIS have been compared with the CATs model but this is not included in our paper. The model matches very closely with measurements but this is not surprising since GPS measurements from this area are used to constrain the CATS model. A previous comparison that also showed very close agreement was mentioned in Gudmundsson (2007).

p. 2414, l 5-6: GPS receivers that originally made the measurements. . . Is this a reference to measurements of the M2 amplitude (tide height) at the grounding line? or is it a reference to the ice stream's horizontal flow response at the frequency of M2? It would clarify (including some of the comments above) to have a paragraph early on that lays out what GPS measurements have been taken and how they are used (this may repeat what is in other papers, but is apparently worth it to clarify this one).

This refers to the horizontal component of ice-stream velocity at M2 frequency. We will make the description of GPS data clearer as suggested by the reviewer.

l 11: should "optimised" be "optimized"

Done

l 14: sometimes semi-diurnal is used and sometime semidiurnal is used. I think the latter is best.

All instances have been changed to semidiurnal

l 21: no hyphen in ice-stream.

Done

p. 2415, l 3: change re-run to rerun

Done

l 19: ice-stream, remove the hyphen

Done

p. 2418, l 10-11: I think that this is also suggested by Arbic et al. see: Arbic, B. K., J. X. Mitrovica, D. R. MacAyeal, and G. A. Milne (2008), On the factors behind large Labrador Sea tides during the last glacial cycle and the potential implications for Heinrich events, Paleoceanography, 23, PA3211, doi:10.1029/2007PA001573.

Yes this reference should have been included and has been added now.

Figure 1: y-axis label: it is not clear that horizontal displacements are plotted (surface displacement alone could be vertical).

Changed to 'de-trended horizontal surface displacements (m)'

Figure 2: I'm not sure that the term "clamp" is used in the text, so it would be useful to explain that this is a boundary condition (or condition needed to reduce dimensionality) in the caption.

This is now made clear in the caption

Figure 3: explain what the blue zone is and also the region of interest for the study in the caption, as a casual reader might be confused by seeing the other processes that are unrelated in the figure.

Ocean label added in figure and caption now mentions that effects of crevassing and tidal currents are not included in the model.

Figure 4: change "lenght" to length, also "interpolation" in the last sentence.

Done

Figure 6: ditto about the y-axis label as with Fig. 1

Changed to 'de-trended horizontal surface displacements (m)'

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.