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Interactive comment on "The past, present, and future viscous heat dissipation available for Greenland subglacial conduit formation" *by* K. D. Mankoff and S. M. Tulaczyk

Anonymous Referee #2

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General comments

This work examines how specific climate change scenarios will increase the delivery of runoff-sourced gravitational potential energy to the bed of the Greenland Ice Sheet, which in turn will intensify formation of subglacial conduits there. The extent and size of conduits have (potentially profound) implications for ice velocity, which this manuscript touches upon but does not extensively discuss. Rather, the manuscript focuses on the assumptions and setup of the calculation. Its main conclusions are that gravitationally sourced heat dissipation exceeds geothermal flux and that conduit formation is expected to increase in future scenarios. These conclusions suggest the potential for slower ice velocity and enhanced sediment removal in land-terminating regions under

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future climate scenarios.

I found the idea behind this manuscript to be worthy and novel. However, the manuscript suffers from a lack of focus and is bottom-heavy, with a very involved description of assumptions (many of which are standard in subglacial hydrology) and the setup of the calculation, that builds to hesitant and limited larger arguments. These factors together limit the manuscript's publication readiness. I do think that the central idea is worthy and could be better highlighted, and its implications more thoroughly discussed, in future drafts.

The manuscript is quite light on the implications of its calculation. In particular, it would benefit from a more thorough discussion of the effects of increased runoff on ice velocity; as it is now, this treatment is brief (Introduction only; it should be moved entirely to the Discussion) and greatly oversimplified. The authors state that increased runoff "appears" (P2 L6) to lead to overall slowdown, but this is based on only two cited studies (Sundal et al., 2011 and Tedstone et al., 2015), both of which focus on beds under lower-elevation ice (s<∼1200 meters). The alternative (i.e., speedup) is not mentioned, even though this is an ongoing topic of discussion in the community. For instance, multiple studies (e.g., Bartholomew et al., 2011 and Shannon et al., 2013) contrast with the cited studies, especially farther inland (s>∼1700 meters) from the ice margin (e.g., Doyle et al., 2014), where subglacial flow currently does not channelize and may not reach the "runoff threshold" required for channelization (Mayaud et al., 2014; cited in this manuscript). Because this manuscript considers subglacial conduit formation at these areas of the bed (s<2000 meters) as well as those near the margin (where the cited work is more applicable), it is crucial to recognize these distinctions and the current uncertainty in the runoff / speedup literature.

With a more mature discussion related to ice velocity (or additional discussion of other implications hinted at; e.g., sediment evacuation), a reduction and better organization of the methods and assumptions sections, and attention to the points below (and those from AR1, which I largely agree with), I think this manuscript could make a good contribution.

General points

A good deal of the text in Section 3 (the details of each assumption) is unwarranted. These are standard assumptions that require a few citations, not multiple detailed paragraphs.

PTT and energy conservation. It took me some time to work out why the authors claimed that this _not_ zero-sum. My best understanding is that the PTT accounting begins at the bottom of the moulin (P5 L6), where the PTT is below zero. To cool to that temperature, the ice released sensible heat into the moulin (i.e., into the englacial, not subglacial, system). That sensible heat is, I think, part of the ∼35% of the gravitational potential energy that is lost (i.e., not transferred to basal ice). This needs to be made much clearer; perhaps with adaptations to Figure 1 (discussed later in this review).

By including the effects of the pressure-dependent melting point and using a relatively fine (5x5-km) grid, the authors obtain a detailed result of areas of expected basal freeze-on and of enhanced conduit formation. This has potential for application to local studies (and this appears to have been tentatively investigated, e.g. comparison to Bell et al. (2014), although without great success), or for regional assessments (Northeast, Southwest, . . .) as commonly done in Greenland. Yet the authors discuss only ice-sheet-wide totals; I am not convinced that more than the back of an envelope was really needed for this.

I was distracted by the authors' use of numeric examples with multiple intermediate steps to demonstrate simple arguments. It would be simpler, more compact, and more elegant if the authors were to state the expressions they wish to compare, and then plug in the physical constants. For instance, P6 L18-20 is easily expressed as PE / KE = 2 g h / vˆ2 = 200 for the stated h and v. Another example is with PTT on P5 L2-5.

Specific points

P1 L13-14 This sentence is difficult to parse

P2 L6-9 This sentence is also unnecessarily complex, with multiple negatives, and contains two thoughts (descriptions of two studies) spliced together. Fundamentally, the logical flow is backwards, with the conclusion presented first and the evidence trailing behind.

P2 L13 data _are_

P2 L18 Mayaud et al. (2014) studied Paakitsoq, not Russell Glacier.

Regarding Mayaud et al. (2014): That study is a nice introduction to this manuscript and should also appear in the Discussion, not just the Introduction. That paper makes an explicit link between increased conduit formation and ice velocity that is only summarily discussed in this study; the authors could (and should) leverage this to make a more convincing argument about future ice velocity.

P2 L25-26 Pollard and DeConto (2012) – Similarly, this citation and future application belongs in the Discussion or Conclusion, not the Introduction.

P2 L28 extra word “a”

P3 L3 capitalize Glaciers

P3 L4 This section could use a short introduction that explains that all terms of the Bernoulli equation and all stages along the water’s path will be examined to make the best approximation. The authors do part of this in P3 L14-17, which should be moved earlier in the section and emphasized.

P3 L11-13 I found these two sentences to be a bit pedantic. At the very least, they are unnecessary because these terms are never again used in the manuscript.

P3 L22 Figure 2 shows energy flux, not energy.

P3 L24 Need to specify “in situ” for this statement to be correct.

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P3-4 L28-3 The elevation z_s is set to a maximum of 2000 meters, even if the melt forms above there. However, in RCP8.5, runoff forms as high as 2800 meters (Figure 2d; South Greenland summit), which would contribute an additional 800 meters (+30%) of head. I would expect that when weighted with the small runoff volumes at high elevations, this addition would be small – but the effects of this approximation could be mentioned.

P4 L8-9 This seems ancillary. I would encourage the authors to drop it or weave it in better.

P4 L13 This reference is for alpine glaciers, which is fine, but this, and its applicability to Greenland, should be pointed out.

P4 L18-19 The second clause is redundant with the preceding sentence.

P4 L20 dissipates

P4 L21 I would like to see some justification for why this heat is "lost", presumably to the atmosphere (as suggested in the legend of Figure 1). Does significant wind pumping really occur at 50-100 meters depth inside a moulin? Alternatively, if this heat is "lost" to melting the moulin walls, this should be stated.

P5 L2 This is not an equation but an expression.

P5 L7 This sentence suggests that this is not done for marine glaciers, although I believe it does need to be done (in an integrated-displacement sense, the water parcel _does_ decompress to atmospheric pressure).

P5 L18 The link to the Supplemental Material is broken.

P5 L24-30 The logical ordering here is flawed. The explanation for the 1/3, 2/3 values needs to be explained up front before the results are presented. As it is, the 1/3, 2/3 numbers appear mysteriously and add confusion until they are explained a few sentences later.

P6 L 7-8 Choose either "downstream" or "down-stream" (I believe the former is more standard)

P6 L13 Strictly speaking and if you are willing to be arbitrarily precise, this is true of _all_ water.

P6 L25-27 Neither is this sentence true (due to hydrostatic pressure energy; see comment above for P5 L7) nor does it belong in this specific subsection.

P7 L1-14 is way overblown. Furthermore, it reads like a response to a review, not like a journal article.

P7 L18 Really do not need these details.

P7 L19-20 It is unclear why this is relevant.

P7 L27 "by the second law of thermodynamics" is completely unnecessary

P8 L2 A "conjecture" is not a very convincing statement.

P8 L5 "heating" here could refer to the water heating the ice, whereas I believe the authors are referring to sensible heating (PTT) of the water itself.

P8 L12-13 The figure does not show different units; both color bars show total annual energy flux. The only difference is /yr versus /sec. The "energy" unit stated in this sentence is incorrect.

P8 L30-31 It is unclear what error the instantaneous heat transfer assumption is introducing. And "because due to" is a typo.

P9 L4-5 capitalize Russell and Leverett Glaciers; is estimated

P9 L8-9 The location of GHF = VHD appears to be coincident with the s=2000 meters location, the elevation where the authors first allow runoff to penetrate to the bed. This may be a coincidence; however, it does seem that the penetration contour would be a strong control on the amount of VHD at inland locations. How sensitive is this result to

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using s=1500 meters penetration contour, or a gradual ramp-up (i.e., 100% of available melt penetrating at s=1500 meters, 50% at s=1750 meters with the other 50% being routed downhill, and 0% penetrating / 100% routed downhill at s >= 2000 meters)? If the relative contribution of GHF versus VHD continues to be a primary conclusion of the study, this is a worthwhile exercise to include.

P9 L20-22 As far as I can reason, I believe this conclusion is backwards. If the authors' model included conduits, this would increase VHD near conduits, but at the expense of VHD at locations without conduits. Since conduits are more likely to form near the terminus than the interior, this inclusion would draw VHD _away_ from the interior.

P9 L24 What specific components of flow routing and VHD are the authors referring to? Obviously, flow routing (a path) and VHD (a quantity) are not the same.

P9 L28-29 Probably the authors mean that the DEM is not _accurately_ representing the topography.

P9 L31 How far, on average, are the Bell et al. (2014) basal freeze-on packages from where these results predict?

Regarding areas of basal freeze-on: This water will no longer follow subglacial hydropotential gradients, but instead surface gradients because it is attached to the ice. These are not always the same directions. This may or may not be important (likely it is not), depending on what fraction of water gets frozen on. This would be worth addressing briefly.

P10 L4 “conduits systems” typo; “non-conduit region” typo

P10 L7-10 These sentences are awkward. Is it always the same grid cell? What are “such locations”, probably marine-terminating glaciers with large catchments?

P10 L11 These assumptions have already been stated in the Methods and Assumptions section, which is a more appropriate place for them than the Discussion.

P10 L13 This should be phrased something like “the basally sourced meltwater carries away the initial gravitational potential energy of the runoff, in the form of latent heat, as it exits the subglacial system.” The water itself is not heat.

P10 L15-16 This sentence is interesting and important, and should be more prominent and/or pointed.

P10 L17-18 I do not think that any evidence for this statement has been presented. Also, “percentages” is vague – percentages of what?

P10 L21-22 According to Dow et al. (2014) (cited elsewhere by the authors), the subglacial conduit network is _unlikely_ to expand significantly inland.

P11 L2 Should specify “over land-terminating ice”.

P11 L7 Should specify “total GHF integrated over the runoff area”. As it reads now, it sounds like the solid earth will be warming in RCP 8.5.

P11 L10-13 This is a smorgasbord of facts. Needs better organization, logical flow, and build-up to the main idea.

P11 L19 are

P11 L21 cause

P11 L24-25 This idea is interesting, but the evidence the authors present suggests millennial timescales, not < 1 century.

P11 L27 missing “a”

P12 L2 missing “orders of [magnitude]”

P12 L6 Not just the ablation zone: the area with s<2000 m includes parts of the wet snow zone

P12 L8 Not “will become”, as the authors have shown that it is already the dominant basal heat source (both P and H scenarios)

P12 L8 “swamp” seems a bit informal

P12 L28 its

Figures

Figure 1: The info is very small relative to the rest of the space, and the short definitions given to each colored box are inscrutable without reading the manuscript carefully. It is unclear where the water travels and why the bar can sometimes exceed 1. I had a very difficult time with it, and there are still components that I do not understand (what do the authors mean by “latent heat”? presumably that refers to VHD? why not call it VHD?). As a first figure, it is so inscrutable that it will drive away all but the most invested readers.

Figure 1 presents results, whereas it is customary for the first figure to illustrate the setup. I think this figure would benefit from a major overhaul, with the results removed and a focused inclusion of methods components, such as the path of the water, the transfer of VHD into / out of the ice sheet along an undulating bed, input / output / conversion points for the various terms (gravitational PE, PTT), etc. I do not think anything would be lost from the manuscript if the colored bars were removed entirely.

Figure 2: A negative sign is missing from the color bar label (W mˆ-2)

Figure 4: The units here are a bit confused. Delta-phi should be in Pa, as is phi. If the authors have divided by the 5-km distance between grid cells, that is Grad-phi and has the units shown on the color bar (Pa / m).

Figure 6: I agree with AR1 that the numerical gradient issues must be addressed.

Panel a: see units (Pa versus Pa / m) comments on Figure 4

Panel b: Some of the lines show nonzero subglacial flow inland of the s=2000 meter contour (x $\sim$ 140 km), which the authors defined as the upper limit for melt to reach the bed. What is the reason for this?

Caption: should specify that b) is the flow rate _of subglacial water_

Figure 7 caption: use parallel definitions for the three cases; i.e., either all three are "the increase from A to B" or all three are "the difference between B and A". Also "Joules".

Panel c: The symbol that the legend refers to is unclear.

References

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