Review of, “Exploring the conditions conducive to convection within the Greenland Ice Sheet”, by Law et al.

Review by Michael Wolovick

Summary

In this paper, Law and coauthors use geodynamic modeling techniques to explore the possibility that local thermal convection may occur in the Greenland Ice Sheet. Thermal convection has long been hypothesized for continental ice sheets, mostly by Terry Hughes, yet it has never seen observational confirmation or enjoyed widespread acceptance in the glaciological community. However, over the past decade or so a growing literature has developed around the observation of large plume-like folds in northern Greenland. There is no consensus on the formation mechanism of these plumes, with proposed causal mechanisms including basal freeze-on, traveling slippery patches, rheological contrasts in the ice column, and cross-flow convergence. This paper adds to the literature on these enigmatic plumes by proposing that they may have formed through buoyant thermal convection, thus posthumously vindicating Terry Hughes’ long and lonely crusade to gain support among glaciologists for the existence of thermal convection in ice sheets.

The bulk of this paper is spent describing and analyzing the results of applying a geodynamic model to Greenland-like conditions. The authors’ model results indicate that thermal convection may in fact be possible under the conditions that prevail in north Greenland. They find that the development of convection is inhibited by higher surface accumulation rates and faster flow speeds, explaining why plumes are not observed in south Greenland. Their model requires ice viscosity significantly lower than standard rheological assumptions, which, if true, would have important implications for the modeling of ice sheet dynamics and for the results of basal traction inversions.

Major Comments

This paper is clearly appropriate for The Cryosphere. It represents both an important addition to the literature around a relatively new observational mystery (the large englacial folds) and a dramatic coda to a very old theoretical debate (thermal convection). The paper is well written and argued. Some of the figures need work in order to better display the authors’ results. In particular, they seem to have crammed their entire parameter space exploration into Figure 3, which is quite busy and difficult to understand. However, they only have four figures total in the main text, so they have plenty of room to split this information into an additional figure to aid comprehension. Their model has some issues, which I discuss next, that potentially undercut their conclusion that rheology must be softer than commonly assumed. However, their model also has some strengths relative to typical ice sheet models, and their overall conclusion that thermal convection can explain the observed plumes is supported by the model results presented. My concerns about the simplifications made in their model mainly fall under the heading of, “all models are wrong, but some models are useful”, and my concerns can be addressed through changes in the discussion or conclusions text, rather than a redo of the modeling itself. My recommendation is to publish with minor revisions. I now go on to discuss my main concerns with their model setup.

The authors use a geodynamic model rather than a classic ice flow model. This has some advantages that make it better for simulating convection, such as the inclusion of a buoyancy term and the use of the Full Stokes equations. However, it does mean that, because their model was not originally designed for glaciological applications, their implementation of boundary conditions and ice rheology are somewhat problematic.

For rheology, they used a linear Newtonian rheology for ice, derived from the non-Newtonian rheology by assuming a constant effective stress (τ_e=50 kPa) throughout the domain. They include the temperature dependence of rheology, but not the stress (or equivalently, strain rate) dependence. They justify this omission by claiming that the strain rates associated with convection are much lower than the background strain rates associated with horizontal ice flow, but the background strain rates should be highly concentrated near the bed, while they have used a constant 50 kPa value for τ_e rather than a vertically variable one. It is not necessarily clear to me that the strain rates associated with convection will be lower than the background strain rates in the mid-column, and the authors have not shown this comparison to justify their assumption. While I do not believe that the use of a linearized rheology undercuts their conclusion that thermal convection is possible for the conditions that prevail in north Greenland, it does undercut their conclusion that the enhancement factor must be much larger than typically used in ice flow models. The authors have concluded from their model results that E must be an order of magnitude larger than the typical value, but their results could just as easily be interpreted to mean that they used a value of τ_e that was too small. Additionally, it is not necessarily clear that the particular thresholds for convection that they found when varying E would hold up if they had used either a vertically variable τ_e or a fully nonlinear rheology.

For the boundary conditions on the upper surface, they used Dirichlet conditions to impose both horizontal flow and surface accumulation, rather than having a stress-free free surface like the real ice sheet. This could have been more problematic but did not end up being a huge issue because they included the temperature dependence of rheology, so shear was concentrated near the bed anyway. A bigger issue is the lower surface: they did not state what boundary conditions they used for temperature, but I was able to infer from other parts of their model setup and results that they used a Dirichlet condition with basal temperature set to -2°C, which is a reasonable approximation of the pressure melting point under 2 km of ice. It is likely that they did this because the classic convection problem in fluid mechanics or geodynamics involves a fixed ΔT across a specific layer thickness. However, in ice sheets, the basal boundary condition is typically Neumann (gradient determined by geothermal heat flow) up until the point that the basal temperature reaches the melting point, when it switches to Dirichlet. Thus, the authors' use of a Dirichlet condition for temperature at the ice base implies that the ice base is wet, but this contradicts statements in the paper that the authors believe that the bed is frozen where the plumes are observed, and also contradicts the authors' use of a no-slip condition at the ice base.

The presence of sliding at the ice base does not necessarily undercut the authors' conclusion that thermal convection is possible; if anything, since they found that vertical shear suppresses the development of convection, allowing basal slip may actually broaden the parameter range over which convection is possible. However, the presence of sliding may undercut their rheological conclusions, where they argue that sliding is less extensive than existing inversions have found. Also, if the basal temperature is tied to the melting point, then the temporal and spatial variations in conductive heat flow that they found (Figures 4 and S7) should be associated with changes in the melting or freezing rate, which should impact the availability of basal water and the basal traction. Those changes in the basal boundary condition should, in turn, influence the ice flow field. Thus, we would expect thermal convection to interact with both the basal freeze-on and traveling slippery patches mechanisms. In order to truly model convection alone, the authors would need to use Neumann conditions for temperature at the ice base, and choose a parameter range where they were sure that the base would never warm to the melting point. A model of thermal convection in ice sheets with a warm bed but no sliding and no variations in basal melting or freezing is necessarily omitting some pretty important feedbacks.

To be clear, I do not think that there is anything wrong with writing a paper focused on convection alone, especially considering convection's contentious history in glaciology, and considering the fact that the original papers on basal freeze-on and traveling slippery patches (which I was involved in as either first author or coauthor) completely ignored thermal buoyancy. However, when discussing results and drawing conclusions, it is very important to make note of what processes were omitted from the model, and to think about how those processes might affect the results. In reality all processes are coupled together, and we cannot think of convection as being truly independent of basal freeze-on or traveling slippery patches (or, for that matter, from anisotropy and cross-flow convergence). In the real world, the basal boundary condition is not fixed and immobile; rather, the variations in the englacial temperature field seen in the authors' model will produce spatial and temporal changes in the conductive heat flux at the ice base, which produces changes in the melting or freezing rate, changing the availability of basal water and basal traction, which finally feeds back on the original englacial flow field produced by convection in the first place. Even if the background temperature field is initially cold-based, the uplift underneath a rising buoyant plume is likely to have a lower conductive gradient than its surroundings, warming the bed locally and potentially producing a local patch of basal melt. Do these feedbacks act in a way that amplifies convection, or suppresses it? Will they broaden the parameter range over which convection is possible, narrow it, or merely shift it?

As I said previously, I think that my criticisms of the authors' model setup mostly fall under the old saying, "all models are wrong, but some models are useful". The authors used a model that is well-designed for simulating convection and poorly designed for other things. In most geodynamics problems there is no need to simulate a dynamic basal boundary that switches modes in response to changing conditions within the model domain; thus, the authors' model can't include subglacial hydrology. This doesn't mean that the authors' results have no value. On the contrary, they have showed, at long last, that thermal convection is possible in ice sheets, and they have connected this model result with an enigmatic set of observations that could plausibly be caused by convection. This is an important result and worthy of publication. My main concern is that the discussion and conclusions sections need to be more forthright about the limitations of using a geodynamics model for glaciological problems, and these sections should also discuss potential feedbacks between thermal convection and other mechanisms that might contribute to the observed folds, especially mechanisms that are sensitive to the thermal state of the ice base. Finally, I think that the authors' conclusion about the likely value of the enhancement factor E should be given more caveats. Given the simplifications that they made to ice rheology in their model setup, and given that they omitted feedbacks that could potentially change the parameter range over which convection is possible, I am not convinced that we can necessarily use the observed plumes to infer that ice is softer than commonly believed. The authors are free to keep arguing that, of course, but I think some caveats are necessary for that particular conclusion.

Minor Comments

Note that my copy of the manuscript does not have line numbers. I will try to place my comments by giving the section of the paper and a quote.

Introduction:

"At first glance, an entirely separate problem is the nature of the formation of large (>1/3 the local ice thickness) englacial plumes found by tracing reflections of equal age in radargrams (i.e., isochrones; Figs. 1A, 2, S1, CReSIS (2013))"

Add a reference to Bell et al. (2014) here. You have a reference to Bell elsewhere in the paper, but Bell et al. (2014) was also the first to describe the large plume-like reflectors in northern Greenland and they should be cited here, even if you don't agree with their interpretation of the reflectors as originating with freeze-on.

"Such plumes have previously been hypothesized to result from basal freeze on (Leysinger Vieli et al., 2018), or traveling basal slippery spots (Wolovick et al., 2014), which both require an at least temporarily thawed bed."

The end of this sentence is awkwardly phrased. It also misses the issue of spatial variability, which is important to both mechanisms in addition to temporal variability. Maybe try, "...which both require that the bed be at least locally or temporarily thawed".

"Both authors approach convection analytically only, by estimating a Rayleigh number Ra, the dimensionless ratio of heat transfer via upwards mass transport (i.e. convection) vs. thermal conduction (Rayleigh, 1916)...The formulation of thermal diffusion in Ra does not capture dynamical effects important in ice sheet flow, such as horizontal shearing; and the critical Ra is itself tied to the particular boundary conditions and the initial perturbation geometry"

Additionally, this analytic approach misses the fact that heat is *already* being advected by mass transport within the ice sheet- vertical thinning and subsidence associated with surface accumulation is pushing colder ice down. Any upwards convection needs to fight against this background state of subsidence, as you found in your model results later.

Materials and Methods:

"Surface mass balance is set to zero, i.e., no snowfall or surface melting (vz,s = 0 at the surface boundary condition … surface shearing velocity vx,s is uniform across the domain's top surface"

Does this mean that you apply Dirichlet conditions to velocity at the upper surface of the domain, rather than stress-free (Neumann) conditions? Does this also mean that your upper surface is a rigid lid, rather than a free surface? The correct boundary conditions are Neumann, and the correct way to induce horizontal flow is to have a slope in the free surface that drives ice flow downstream, and the consistent way to induce vertical flow is to have the gradient of this induced horizontal flow have an along-flow gradient that balances the accumulation rate. I suspect that you have done things this way because your geodynamic model was not built to study ice dynamics, and so the steady state that I just described is beyond its ability to compute.

I don't think that this necessarily invalidates your results, mostly because Figure S5 shows that horizontal velocity is mostly uniform in the upper column, with shear mostly concentrated in the lower column. If your model had produced too much shear in the upper column, I would suggest throwing the whole thing out. However, you need to explicitly state that you use Dirichlet conditions rather than Neumann conditions at the upper surface, and you need to have some text justifying this choice and explaining what impact (if any) it has on your results.

In addition, you do not state what boundary conditions you use for temperature on the lower surface, but because heat flow across the lower boundary varies in time (Figure S7), I infer that you also used a Dirichlet condition for temperature, with a value of -2°C (roughly accurate for the melting point underneath 2 km of ice). I discussed my issues with this at greater length in the Major Comments section above; for here, my main comment is that the basal boundary condition needs to be stated explicitly.

"A Newtonian rheology is appropriate here as strain rates due to convection are small compared to those from background ice flow (Fig. S5)"

Figure S5 does not show strain rates, it shows velocities. I would like to see a figure for strain rates to support this argument. In particular, I am curious about whether the strain rates due to convection are smaller than the background strain rates in the mid-column, not just near the bed. This is potentially an important caveat to your results and an important limitation of trying to use a Newtonian rheology to model a non-Newtonian material.

"Prescribing τ_e is also necessary as a full ice-sheet stress state can not be accurately replicated in a simplified along-flow slice"

Especially when you don't have a true free surface and instead produce horizontal flow by imposing a Dirichlet condition at the top of your domain.

"We set Fint = 0, thereby ignoring adiabatic heating and neglecting strain heating, to prevent simulations with greater vx,s and hence greater strain heating from evolving a different rheology along flow."

This is a decent first approximation for the slow-flowing areas of the ice sheet, but it is potentially an important limitation. At a velocity of 10 m/yr (appropriate for many of the observed plumes) and a driving stress of 100 kPa (a good ballpark number for ice sheet stresses in general), the integrated shear heating is 32 mW/m^2, or about half of the geothermal heat flow. This could potentially play an important role in warming and softening the basal ice, and also could contribute to convection by providing additional thermal energy in the lower ice column that needs to escape to the surface. At a velocity of 1 m/yr that would be reduced by an order of magnitude, but as I said, some of the plumes are observed where velocities are about 10 m/yr or even higher.

"We apply a transformation, T2 = ((T1+Ta)/Tb)(Tb +Ta) where T1 is the original temperature profile, Tb is the basal temperature and Ta is an adjustment term used to raise the basal temperature to −2o C. The temperature profiles are stretched and compressed when adapted to the range of ice thicknesses."

Two issues: 1) does this transformation leave the surface temperature unchanged, or are you also changing the surface temperature? 2) More importantly, this temperature profile is not going to be in steady state with the enforced accumulation rate and ice thickness in your model. Your Figure S7 shows a pretty dramatic initial adjustment of the domain-average basal conductive heat flow. That could be because your initial temperature is not in steady state with the downwards advection that is actually in your model. Why did you use this approach instead of computing a steady state vertical temperature field for your given accumulation rate and ice thickness? It is true that you don't necessarily know the shape function for vertical velocity until you actually run your model, but you can still get much closer to a steady state by applying a simple approximation (like a Nye model) rather than simply scaling the DYE-3 or NEEM profiles, which come from particular locations where the accumulation rate and ice thickness do not necessarily match the values used in your particular experiments.

Figures:

Figure 1.

Notes: 1) the descriptions for subplots b and c are swapped. 2) It would probably be good to reproduce the observed plume locations in every plot, not just a. 3) A shape factor of 0.8 is appropriate for n=3 rheology and constant temperature. With a more realistic temperature structure, shear should be more concentrated near the bed and the shape factor should be closer to 1. 4) Why evaluate effective stress at 5/14 depth? Where does this number come from? 5) Subplot e might be better on a log scale, since we are mostly interested in areas where the accumulation rate is quite low. Either that or just reduce the maximum of the color scale.

Figure 2.

These echograms are way too dark, at least on my screen. It is very hard to see anything. You should adjust the color limits to improve visibility. The oblique view is also pretty hard to make sense of. Why do you display the entire ice sheet, instead of just zooming in on the region of interest?

Figure 3.

There is a lot going on in this figure and it is hard to interpret. I would recommend splitting this figure into two figures. For one thing, the use of lines when you have timeseries data, but the lines don't actually represent progress over time, is very confusing. I would recommend that one figure be timeseries (ie, the x-axis should be time, with color representing some other parameter) so that the reader gets a sense of how the model evolves over time. Then another figure could focus on the parameter space exploration by showing 2D contour plots at various cross-sections through your 4D parameter space (the 4 dimensions being enhancement factor, surface speed, height, and snowfall). The second figure should not have any time dependence in it, you should just choose a single metric to quantify the strength of convection (based on the final paragraph of the methods section this would either be the max vz at 20 ka or the change in max vz between 4 ka and 20 ka). Thus, the first figure gives the reader a sense of model evolution for a handful of representative parameter values, while the second figure shows the reader how the strength of convection varies as a systematic function of parameter space. But as it is now, Figure 3 tries to show both an exploration of parameter space and evolution through time, and the result is that there is simply too much going on in one figure.

Figure 4. “a vertical enhancement factor of 2”

I think you mean vertical exaggeration? Vertical enhancement factor invited confusion with the rheological enhancement factor E. It might also be a good idea to adjust the color scale for the stratigraphy figures so that the layers have more contrast.

Discussion:

“(2) Total horizontal shear through the column must be less than around 1 m yr $^{-1}$”

Many of the plumes (especially upstream of Petermann Glacier) are in ice flowing faster than this.

“(4) the enhancement factor must exceed around 45-75.”

See my major comments about the need for caveats around your rheological conclusions.

“condition (2) is likely satisfied by low surface velocities throughout northern central Greenland”

I don’t know about that. The region with velocity less than 1 m/yr is actually quite small. The region below the 10 m/yr contour is bigger, but still doesn’t include many of the observed plumes. Later you suggest that the plumes may have formed further inland before being advected to their present locations, but it is worth emphasizing that the region within the 1 m/yr contour in Figure 1 is actually tiny, and it is associated with escape times of many tens of thousands of years. You also suggest that the plumes may have formed before the Holocene, when accumulation rates were lower, ice was thicker, and (presumably) flow was slower. However, any plume that is being passively advected by ice flow will also be shrunk substantially by vertical thinning: the characteristic vertical strain rate associated with surface accumulation is a/D, where a is surface accumulation and D is ice thickness, and for values of 10 cm/a and 3 km (being generous to plume survival with both parameters!), that is a strain rate of about $3x10^{-5}$ a^{-1}, or a characteristic thinning timescale of 30 ka. Thus we would expect a plume that initially formed near the ice divide where flow was around 1 m/yr and then took several tens of thousands of years to be advected to its observed position would have been reduced from its original size by a factor of 1/e. Since the observed plumes are already a third to a half of ice thickness, that is clearly impossible. Less generous assumptions about accumulation rate and ice thickness can easily lead to a thinning timescale shorter than the Holocene.

In addition, arguing that the plumes originally formed where ice flow is less than 1m/yr begs the question, “why don’t we observe any plumes within the 1 m/yr contour now?”. I suppose it is possible that the plumes formed during the LGM and then plume formation stopped during the Holocene, however, the Holocene stratigraphy above the plumes is deflected upward, which implies that they have been active during the Holocene. In my opinion, it is much more likely that the plumes formed close to their present-day locations (and that some of them are still forming!), where velocities are more likely to be around 10 m/yr rather than 1 m/yr. But in any event, you need to be explicit here that you are making a prediction: your argument implies that the plumes are currently being passively advected by ice flow, not actively forming. This prediction can be checked through the use of pRES data to measure vertical velocities within the ice column.

I do not think that all of this is necessarily fatal to convection as a formation mechanism, since (as I discussed in my major comments above) the presence of basal slip may reduce the vertical shear that tends to suppress convection, and also coupling between englacial convection and subglacial hydrology may modify the parameter range over which plumes form. But in any event, I think that the discussion needs to spend more time dealing with a few facts: 1) all of the plumes are observed where ice flow is faster than 1 m/yr, in some cases an order of magnitude or more faster, and none are observed where flow is slower than 1 m/yr; 2) escape times from the 1 m/yr contour are substantially larger than plume formation times; 3) passively advected plumes should shrink over time in response to surface accumulation and vertical thinning; and 4) Holocene layers above the plumes are deflected upwards. How do you square these facts with your model result indicating that convection requires velocities less than 1 m/yr, and your argument that the plumes initially formed during the LGM and are only being passively advected during the Holocene? Or, if you think that the plumes are being actively formed during the Holocene, then why don't we observe any near the divide, and why do we only observe them where ice is flowing faster than your model says should be possible?

"Traveling slippery spots (Wolovick et al., 2014) develop clearly in an controlled setup, but also require thawed bed areas in the same region…"

True, but I don't see why that's a problem. There's plenty of uncertainty in the basal temperature and its not unreasonable to postulate at least local areas of thawed bed in the interior of Greenland. Plus, your model setup has the basal temperature tied to the pressure melting point too.

"...and further do not appear to align with the observation of a highly deformed basal layer beneath the plumes (Fig. 2C), which may be more consistent with high rates of basal ice deformation than basal sliding (Y. Zhang et al., 2024)."

The traveling slippery spots mechanism does not require 100% basal slip in the slippery patches, just a decent contrast in slip percentage between the slippery and the sticky patches. Wolovick and Creyts (2016) goes into more detail about this, and also shows that the overturned parts of the folds are more likely to be found over the sticky parts of the traveling stick-slip trains anyway (DOI:10.1002/2015JF003698).

"we cannot rule out these two processes contributing to the onset of an initial perturbation."

That's a good start, but I want to see greater discussion of the ways that convection can interact with other proposed mechanisms. Not only traveling slippery spots and basal freeze-on, but the explanations based on rheological contrasts (DOI:10.1038/nature11789) and cross-flow convergence (DOI:10.1038/ncomms11427) too. I realize that you do talk about anisotropy later in the discussion, but mostly in the context of whether a softer rheology is reasonable, not in terms of formation mechanisms for the plumes.

"We emphasize, however, that rate-weakening in plumes is still anticipated to be small compared to the main coastward movement of the ice sheet which exerts a first order control over effective stress (Figs. 1D, S1, S3)."

First of all, I don't really see how Figs S1 and S3 address this question. Second of all, as I discussed in my major comments, I buy the argument that the effective stress is dominated by the background flow of the ice near the bed, but I am not convinced that this is the case in the mid-column. I would like to see a supplemental figure showing strain rates in the model, in addition to Fig S5 which shows the components of velocity. The plumes are a relatively short-wavelength feature with vertical flow rates quite a bit larger than the vertical flow rates due to accumulation rate, so I strongly suspect that they are the dominant component of the strain rate tensor in the mid-column.

Appendix A1:

“If we extend the basal viscosity (calculated at 5×10^4 Pa effective stress and −2°C) uniformly through a 2,500 m ice column”

This is a questionable assumption. It would be more accurate to use an effective viscosity appropriate for the mid-column, or at least the mid-lower column (for instance, one quarter or one third of the ice thickness).

Appendix A2:

“an initial approximation of ice rigidity is made based on ice temperature.”

Ice temperature from where?

Figure S2:

This figure needs some work to make it a better visualization. It is way too dark and I really get no information from it at all.

Figure S3:

As I mentioned in my major comments, using a single vertically constant value for effective stress is going to underestimate the vertical gradient in viscosity. Also, the caption should use “vertical exaggeration”, not “vertical enhancement”.

Figure S5:

As I mentioned elsewhere, I want to see an additional figure showing the strain rate components (and overall magnitude) in addition to the velocity components.

Figure S7:

This shows the average basal heat flux across the whole model domain, right? Your model has substantial local variation in the temperature structure, which should also produce local variability in the basal heat flux. It would be good if the caption explicitly stated that this is a spatial average of heat flux.

It is also worth noting two things here: 1) the initial steep decrease in basal heat flow is likely because your initial temperature field is not in equilibrium with your imposed boundary conditions. If you started with a steady 1D thermal profile (or at least, a reasonable approximation of a steady 1D profile), then this initial adjustment would be much smaller. 2) You have imposed a constant basal temperature because that is standard in convection modeling. However, an ice sheet bed can only be held at a constant temperature if there is water present. If the basal heat flux increases because of convection, and there is no water present, then the basal temperature will drop, thus weakening the convection. Alternately, if there is water present, then the increase in heat flow will lead to freeze-on. If sufficient water supply is available (an important limitation), then the extra heat flow from convection will be provided by the latent heat of freezing water.