Authors final response to Reviewer 3
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Note: Reviewers comments are given in blue font whereas the author response reads in black.

Recommendation: This manuscript, in anything like its current form, does not seem to contain a publishable idea. The most generous interpretation is that other researchers, over decades of analysis of temperature conduction in a solid rod, have failed to notice an intrinsic timescale which might relate to ice sheet binge-purge cycles. If that is so, something this reader thinks is not true, then the way the article is written must be completely redone. Critically, issues of incoherent definition ("potential periodicity" is here meaningless) and essentially-disregarded parameter dependence (the assumed initial basal temperature and geothermal rates are in fact dominant) must be somehow overcome. (It would be a different paper if so.) In any case, the many time scales potentially associated to full, physically-clear binge-purge mechanisms must be carefully considered if the claimed special time scale here is to be taken seriously.

It is clear from this review that our intended message has not come across clearly in the text, and this is something we will aim to improve in the revised manuscript. In short, we believe that our intended message is actually quite consistent with what the reviewer expects and the rather negative comments largely stem from misunderstanding of that message. We hope that addressing these comments will clarify any misconceptions and help us improve the manuscript. First, we would like to respond to the comments from this first paragraph briefly:

- *The apparent fact that the paper does not contain publishable ideas.* This statement is in disagreement with the other two referees. Referee 2 even considered the paper as "interesting" and "appreciated the simplicity of the approach and use of analytic techniques". We believe that this statement results from a thorough misinterpretation of our intended message. We will ensure that our message is much more clear in the revised text.

- *Incoherent definitions (e.g., "potential periodicity").* Our paper is framed within the binge-purge oscillator framework, even though it reaches far beyond it for its generality and simplicity. This term merely emphasises the fact that it is not a pure oscillation, but rather relates to the "binge" phase timescale that would yield a binge-purge oscillator, as defined in MacAyeal (1993a) model. It is clear that this phraseology gives the reader a false impression of our aims, and we will maintain the more accurate phrase "time to reach the pressure melting point" whenever possible.

- *Disregarded parameter dependency.* Parameter dependency is not disregarded - in fact, one of our main aims is to show precisely that there is no special timescale intrinsic to the system, but rather that it generally depends quite strongly on the boundary and initial conditions. The precise 6944-year value arises only when employing the exact parameter values as MacAyeal (1993) and we use this to show that our system behaves consistently when considering the same (over)simplification. Otherwise, as Fig. 4 shows, we have exposed a broad range of response timescales that depend very much on the choice of several parameter values.

Summary of the manuscript: The Introduction ties binge-purge (Heinrich event) cycles to ice temperature (which is fine) and concludes by asserting that 7ka periodicity is widely used in the literature. Section 2 sets up an initial-boundary value problem for a motionless ice column of finite length, with geothermal (Neumann) basal and Robin surface boundary conditions, and linear-in-height initial temperature. Sections 3 and 4 sketch, with details in the Appendices, a Fourier series solution of the problem, in which (generally) the eigenvalues solve a transcendental equation requiring numerical solution. Section 5 visualizes the temperature profiles and their time-dependence, with an emphasis on how they depend on the ice thickness L and on beta, an insulation coefficient in the surface Robin condition. Section 6 starts by defining a certain solution time as "potential periodicity"—there is no given justification for connecting *this* solution time to periodicity!—and then illustrates and discusses dependence of this time on parameters. Section 7 then focuses on the dependence of the time on L, as L becomes large, revealing a time 6944a in the limit. (This value, conveniently near 7ka, entirely depends on the assumed conditions at the base, namely the initial basal temperature θ_b and the geothermal rate G/k.) Finally the Conclusion again emphasizes the role of L. Appendices then give details of the standard Fourier series analysis.

The connection to periodicity is justified by the definition in MacAyeal (1993a) as the time required to thaw the base (Sections 4.2 and 4.3, MacAyeal, 1993a). For a binge-purge oscillator, as expected, the 6944-yr limit depends on the conditions at the base (since it is an initial boundary problem) and MacAyeal (1993a) identical values are employed in that section for a one-to-one comparison. Our results now show that for a finite domain there is an additional dependence on the ice sheet thickness (Fig. 5) that did not appear in MacAyeal's original papers since the heat solution assumed a semi-infinite domain. As mentioned above, we will generally use the phrase "time to reach the pressure melting point" to avoid confusion.

Major concerns:

Understanding the consequences of conservation of energy in ice sheets is a nontrivial matter, thus it is included as a 3D partial differential equation into most modern ice sheet modeling efforts, and it is important because internal energy (e.g. temperature) is tied to the long time-scales at which ice sheets change. Because ice sheets are thin, variations in the vertical are generally larger than in the horizontal, but nonetheless the problem is advection-dominated. In ice columns near the divide the strongest direction of ice advection is typically vertical, but over large areas of ice sheets this direction is horizontal so that column-wise temperature distributions are commonly far from what any isolated vertical-column model might generate. Furthermore the bases of ice sheets are usually near or at the pressure-melting point. The thermo-mechanical condition of near-basal ice can dominate overall ice sheet dynamics because the presence of pressurized liquid water facilitates ice deformation and basal sliding. The near-basal thermal regime is dominated by geothermal flux, dissipation heat from sliding, and at times the transport of liquid water from elsewhere (e.g. ice surface or through subglacial hydrology). Because of the strong role of liquid water, it follows that conservation of energy is a two-phase problem, thus not one which can be well-modeled by temperature alone.

We agree with the conservation of energy reasoning. And it is clear that a more realistic and sophisticated description of the thermomechanical processes at the base of an ice sheet is possible and is employed by 3D ice sheet models. However, conceptual studies also have great value in helping to understand the importance of different processes, and mathematical simplicity allows for analytical solutions that facilitate the analysis. The main aim of our paper is to reevaluate an important foundational piece of literature in the binge-purge hypothesis (MacAyeal, 1993a, b) and advance our understanding of how the thickness of an ice sheet influences its thermal evolution. The context of the problem here relates to a region where the ice is initially frozen to the bed. Thus horizontal advection can be expected to be low, and there should be no liquid water at the base. The question addressed is how long would it take such a column of ice to reach the pressure melting point. The subsequent evolution of the ice sheet would indeed be more complex, but is outside the scope of this simple scenario. It can be argued, as indeed MacAyeal (1993a, b) did, that this initial time to reach the pressure melting point is related to the binge timescale of the binge-purge mechanism. We also note that the solutions calculated here are furthermore not restricted to a particular problem and can be used in any physical system that satisfies the initial boundary problem.

The current manuscript considers none of these realities, nor does it provide this reader any insights about ice sheet thermodynamics. Instead it examines a conduction-only isolated column model. Within this narrow, unpromising model it proceeds to ignore the dominant parameter dependencies and instead extract a special 7ka time scale, a time scale for temperature change at the base, by surreptitiously fixing some dominant, but unexamined, values. Then it confusingly discusses dependence on less-dominant parameters, especially ice thickness L and surface conduction beta, simultaneously arguing that L is important and irrelevant.

As mentioned above, the aim of the paper is not to provide a full description of all processes concerning the energy conservation within an ice sheet. It is well known that there exists no analytical solution to describe such a system. Nonetheless, parameter dependencies are not ignored in our description. They are considered in Fig. 4, where a broad range of values are employed to compute the time required for the base to thaw (i.e., periodicity, as defined by MacAyeal, 1993a). Moreover, ice thickness is never simultaneously argued as important and irrelevant. A careful look at the paper reveals the subtleties of such degrees of freedom (even in this idealised system).

Thus the manuscript first fails to consider the actual thermodynamics of ice sheets, and then it makes unreasonable claims for the relevance of its very-simplified model. An extremely well-trod mathematical analysis, namely Fourier series applied to conduction in an interval, a problem already addressed by Fourier and Kelvin, is offered as new and insightful, which it is not. The modeled time evolution of a column's basal temperature profile simply does depend strongly on the column thickness L, despite the "strongly dependent" claim in the abstract (line 5). The particular 7ka time scale revealed herein, and unconvincingly tied to binge-purge oscillations and Heinrich events, actually does have strong dependence on particular basal parameters in the model, namely the assumed geothermal flux rate and initial basal temperature. However, this special time scale would in any case be destroyed by any (here missing) advection mechanism including sliding, critical to any serious discussion of binge-purge.

Fourier analysis is not presented as new, but rather as a standard approach (appendices were only included for clarity with readers not familiar with it and will be deleted considering the comments of the other two referees). Yet, to the authors' knowledge, this method has not been applied by the glaciological community to address the current problem. In particular, MacAyeal (1993a) did not use it but instead resorted to considering an infinite domain to simplify calculations, arguing this could be justified. We here show that considering a finite domain leads to a dependency on ice thickness that is not present in MacAyeal's solution. In addition, we demonstrate that the time scale also depends on the initial and boundary condition of this problem as expected (but ignored in the original work). As posed by MacAyeal (1993a), no horizontal advection is considered in this problem, though vertical advection is neglected by estimating the e-fold decay of a sinusoidal signal at the surface with a constant vertical velocity comparable to the accumulation rate at the summit of the GIS (e.g., Alley et al., 1993).

A key sentence (lines 138-140) is that "We further calculate the time required for the column base to reach the melting point ..., hereinafter referred as potential periodicity". There is no offered justification for why this solution time is a "periodicity" for anything! Indeed binge-purge is a periodic mechanism, one of great interest and importance, but there is not even an attempt to explain why this time is related to the desired periodicity.

There is clear justification in Sections 4.1, 4.2 and 4.3 (MacAyeal, 1993a): "once the basal temperature reaches the melting point, the ice sheet begins to move". This is in fact the end of the growth phase (binge) of the cycle. We had assumed that the ideas presented in such a paper are known and fully understood by the reader, but we will explicitly address this to make our link to that work more clear. The 6944-yr periodicity is then elaborated in Section 5.0 of the same paper.

This "potential periodicity" time is completely dependent on a parameter which is completely arbitrary, namely $\theta_b = -10°$ C as the starting point at time 0. It also depends strongly on the geothermal flux rate, which is known to vary substantially over a continent. (Geothermal flux rates are available for modern North America and thus could be used to explore this parameter dependence.) As shown in Figure 4(d), stably across a broad range of ice thicknesses L, variation of θ_b from -15C to -5C implies "potential periodicity" which ranges from about 4ka to about 20ka. Lines 161-162 actually mention this but the rest of the manuscript drops it: "the potential periodicity appears to be rather sensitive to the initial basal temperature, rapidly saturating to values above 25 kyr for $\theta_b < -11$ C". Attempting to interpret time scales as depending on L seems to deliberately ignore that they depend much more strongly on an uninspected parameters θ_b and G/k. Possibly θ_b should be regarded here as a proxy for the coldness of the cold part of the atmospheric-driver temperature cycles, but (as far as I can tell) even this is not argued.

Indeed, the timescale to reach the pressure melting point depends on θ_b (already noted in

MacAyeal, 1993a). Nowhere in the paper is the contrary stated and, the fact there exist additional dependencies (e.g., L, β, G, $k\ldots$), does not say otherwise. We first explore a broad range of θ_b values (Fig. 4d), so we do not fully understand why the referee stated that this parameter had been ignored. Then, to perform a one-to-one comparison with MacAyeal's 6944-yr estimation, we employed an identical value of $\theta_b = -10°$C as we did in our Section 7. Additionally, geothermal heat flux values span those available for North America, thus exploring a realistic range. Likewise, θ_L is not ignored and presented in Fig. 4c.

Finally I want to describe two important figures, so as to illustrate the inappropriateness of the manuscript's analysis. Figure 4: What the parts of this Figure actually show, though this is ignored, is that the strongest dependence of the "potential periodicity" time is on the geothermal flux rate and the initial basal temperature. The discussion of dependence on air temperature and ice thickness is mostly a distraction.

As expected, there exists a dependence on both the geothermal heat flux and the initial basal temperature. Nevertheless, Fig 4b is enough to understand that the air temperature and ice thickness are of paramount importance. It is quite interesting to notice the sharp dependency with thickness for air temperatures below -20^{o}C. Even more, all panels in Fig. 4 share the x-axis where the strong L-dependency is clearly shown. These dependencies will be further elaborated in the text for completeness.

Figure 5: Here is my attempt to say what is shown in this Figure; note that Figure 2b in particular supports my interpretation. A geothermal rate and ice conductivity are fixed, giving a fixed value G/k. An initial basal temperature (θ_b) is fixed, most likely as -10C consistently with Figures 2a and 3, though its value is unstated. Then the time for the base to warm to 0C (the mis-named "potential periodicity") is shown as a function of ice thickness L. Different surface boundary condition treatments give several curves, but for $L > 2.5$ km they all coincide at a time about 7ka. I observe that the explanation for this value of 7ka is actually quite clear! Namely, as long as the top of the ice is far away, the chosen values of the initial basal temperature and the geothermal flux rate will determine the time taken for the base of the ice to warm up to 0C; this is a balance of upward conduction with the delivered heat in the time interval. Thus the special value 7ka is actually (and strongly, and entirely as L goes to infinity) a function of θ_b, G, and k, which were all fixed at certain values for no stated reason. This dependence should be examined, but instead the paper looks elsewhere, at L and beta, and then it spins the results as related to Heinrich events.

In the manuscript, our intention was essentially to give the conclusion of the reviewer here. The point is that the widely cited value of 7ka is not special at all. Indeed, as L goes to infinity the time required to reach melting is a function of θ_b, G and k. In this section, the values used here were chosen to replicate those of MacAyeal (1993) and in that way show that our solution converges to his in such a limit. Meanwhile, the dependence of the timescale on various parameters has already been examined in our more realistic analysis with a finite L: different values of geothermal heat flux and the initial temperature profile of the column are tested (that is precisely the message of Fig. 4).

In the revised manuscript, we have made an extra effort to clarify the new results we present, namely that more realistic treatment of the problem demonstrates a strong relationship of timescales to boundary and initial conditions, and therefore no special timescale of 7ka should be expected to exist. We maintain the section demonstrating how the 7ka timescale can be obtained under the assumptions made by MacAyeal (1993) and emphasise that there is no reason a priori to expect those assumptions to hold universally. We hope that with these changes, it will be clear to the reviewer that the message of our paper is quite consistent with their expectations.