

The Cryosphere Discuss., referee comment RC1
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Comment on tc-2022-23

Johannes Sutter (Referee)

Referee comment on "Predicting the steady-state isochronal stratigraphy of ice shelves using observations and modeling" by Vjeran Višnjević et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2022-23-RC1>, 2022

Višnjevic et al. present a novel method to efficiently model the age structure of synthetic and Antarctic ice shelves and compare the latter to observed IRH's to deduce information on the strength and weaknesses of model assumptions and climate forcing with respect to mismatches against the observed internal stratigraphy. They further find that the Roi Baudouin ice shelf consists of two distinct regions which stability is either dominated by ocean or atmospheric conditions with potentially important implications for its future stability. The findings and methods presented in the manuscript are timely, interesting, and fit very well with the scope of The Cryosphere. The authors introduce novel approaches to glean insights from observed ice shelf stratigraphy and mention opportunities for formal inversion in the future. The manuscript is generally well written and the results intriguing. I would consider this manuscript for publication in The Cryosphere. However, to strengthen the main findings and to make for an easier read, some improvements should be considered for the structure and discussion of the results. Furthermore, the analysis could benefit from two additional experiments investigating the effect of changing climate forcing which I suggest further below (not strictly necessary but would strengthen the analysis and the scientific value of this study).

General comments

It is sometimes difficult to identify what the primary focus of this manuscript is supposed to be. While reading, to me it seemed that you mainly focus on the CMI/LMI transition and its role for future ice shelf stability. However, in the abstract, the foremost motivation seems to be model calibration -> "potential to assist model calibration". Relatively late in the manuscript you also touch on the impacts of the steady state assumption, boundary conditions and importantly the climate forcing. But these points are only very quickly

summarized. I think they would merit a more in-depth analysis. The subsection titles are a bit general, and it is sometimes difficult to pick up the main thread of the story. I assume your main motivation is to improve our understanding of ice flow, ice shelf stability and climate conditions by targeting the isochronal structure of ice shelves. However, throughout the manuscript this motivation is at times buried underneath methodological discussions. I think it would help if you could clearly provide the main scientific motivation of the different sections and distinguish them from the mere methodological aspects (which is of course important in its own right) of this study. L143-146 are a case in point here. Also, in the abstract you mention that the presented method enables the investigation whether ice shelves are in steady-state. What method are you referring to? The identification of the CMI/LMI transition or the use of equation 2 for establishing the vertical flow profile (Drews et al., 2020)? With numerically efficient prediction you probably refer to eq. 2 right? It is not always straightforward for the reader to appreciate the scientific goals of this study throughout the manuscript.

Introduction:

In the introduction you mention that you focus on the CMI/LMI boundary but do not mention that you also look at the effects of (1) numerical diffusion, (2) the applied boundary conditions, (3) violation of the shallow-shelf approximation, (4) violation of the steady-state assumption, and (5) flawed surface accumulation or basal melt rate fields as outline in line 199-200. This should be motivated here as well as one of the premises of using isochronal geometry in models is to identify issues with model setups, the computed velocity field, and uncertainties in the climate forcing.

Methods:

From my point of view the methods section is a little confusing. I think a more intuitive approach would be to start by introducing the model setup, i.e. the ice shelf configuration of which you model the isochronal/age structure (1st idealized MISIP ice shelf, 2nd RBIS) and why and then discuss how you derive the velocity field and compute the age (equations 1 and 2). Following that you can elaborate how you validate eq. 2 with Elmer/Ice in the MISIP3D experiment and how you quantify numerical diffusion in the synthetic case.

Results:

It is sometimes unclear whether you discuss the modelled isochrones based on eq. 2 or based on Elmer/Ice (also in the figures) and when in the synthetic test case. Please make this more explicit throughout. I suggest coming up with abbreviations e.g. RBOI, Stdn, Synth. It adds confusion that you call both the MISIP3d setup and the small 2D ice shelf you use to estimate the effect of diffusion *synthetic*.

Discussions/Conclusions:

Here I am missing a statement on future steps/plans to e.g. include transient climate conditions, non-equilibrium ice flow, coupling to the grounded parts of the ice sheet and how your methodology can be applied beyond the differentiation between CMI and LMI etc.

Specific comments:

L1 maybe "moderate" is more fitting than "decelerate"?

L3 I wouldn't necessarily call surface accumulation and basal melting parameters in this context, maybe "these components of ice shelf mass balance"

L3 more concise -> ... introduce uncertainties in projections of ice-sheet evolution.

L16 ... holds a sea level equivalent ice volume of ca. 58 m

L18: ... with maximum estimates of up to ...

L19 ... play a major role in these future projections due to their buttressing effect on glacier flow (e.g. Fürst et al., 2016).

P2 L25/26 I would disagree, that the isochronal structure of Greenland or Antarctica has been used numerously in conjunction with ice flow modelling. I'd say, that the internal stratigraphy of the grounded ice sheet is very much underexplored as well. To my knowledge, so far there is no isochrone constrained model reconstruction or projection of ice sheet/glacier dynamics except for first efforts in that direction (e.g. Juvet et al., 2020 (mountain glacier), Born et al 2021 (Greenland), Sutter et al., 2021 (Antarctica)). The references listed are either aimed at determining ice age (e.g. Nereson and Waddington, 2002) or reconstructing surface accumulation (e.g. Leysinger Vieli et al., 2011). I suggest providing a clearer differentiation with respect to the focus of the cited papers. Otherwise, the reader might surmise that for the grounded ice, using englacial layers is already a standard procedure in models which is probably not what you intend to convey here.

L35 ... to approximate an equilibrium 3D velocity field ...

L35 is this approach new or has it been used in previous studies (e.g. the ones you mention in the previous sentence)? If it is new, I would explicitly state this here. If not I would mention how you do things differently.

L36 I assume you can only predict the ice shelf stratigraphy which is not formed by the inflow from the grounded part of the ice sheet? If that's the case, please reformulate the sentence. In fact, you differentiate between CMI and LMI in the next sentence. Part of this could be moved from the introduction into the methods, while only mentioning in the introduction that you try to predict the ice shelf stratigraphy. The caveats (CMI vs. LMI) would then be discussed in the Methods instead of the Introduction.

L38 when you write 'one outcome' do you mean 'major finding'? It seems that the CMI/LMI delineation is the main focus here.

L59 not sure whether "circumvent" is the right term (also, what is meant by *step*?). In a real-world application the ice shelf also depends on the tributary glacier and the transition across the grounding line, lateral stresses etc. all these aspects would include the challenges you mention for typical ice flow modelling. By focusing solely on the floating part of the ice sheet you must come up with boundary conditions for the ice age introducing uncertainties close to the grounding line (as you outline later). So maybe be more specific here with what you mean by *circumvent*. Also, your main reason to model the stratigraphy of ice shelves is not because you forego uncertainties associated with modelling grounded ice but rather because it is interesting in its own right. I suggest rephrasing this sentence.

L61 You illustrate the plug-flow character of ice shelves in the following sections? Maybe reword and motivate the Method sections more explicitly.

L63 I would start section 2.1 with an introductory sentence instead of jumping right into the coordinate system description. Something along the lines : "We derive the unknown vertical ice flow from observed surface velocities assuming steady state conditions (Drews et al., 2020) ..." and then introduce eq. 2 and the specifics (parameters) and how you arrive at this analytical expression.

Figure 1: I suggest using a different color scale for surface flow and accumulation rate (continuous and not divergent). The divergent red-blue cmap used now gives the impression of a transition.

Figure 1 caption: Basal melting rate - > basal melt rates (negative for freezing ...

Figure 2: scheme -> schematic/sketch/illustration. The many lines pointing to the age equation make this sketch a little busy. I would suggest dropping them. Font size can be smaller and not all in bold. Not necessary to write 'observations' every time. You can specify in the caption where the forcing comes from. Also, if I understand correctly in the idealized case (StdN, MISIP3D) forcing is uniform and not from observations. This is a bit misleading. I like the fish though.

L143 unclear what "limits of the approximated velocity field" mean, specify e.g. "explore the limitations of the approximated velocity field compared to a full stokes model"

L145 what do you mean by "draw out numerous characteristics". Please elaborate and specify.

l74-76. Maybe I am missing something here, but if I understand correctly, the initial age of the ice shelf is set to 0 while the upstream age (grounded ice) is set to the simulation's runtime? This means to the current model time (i.e. after 200 model years upstream age is 200 years) or the total runtime of the experiment? Maybe make this more verbose here. Also, I am missing a justification for this assumption. In reality, the age of the ice column at the grounding line depends on the depth of the ice (probably with a rather steep gradient) and the history of grounded ice flow. This is either unknown or highly uncertain if you use ice sheet model data from the grounded ice as an input. Wouldn't it make sense (as first approximation) to use the 3D velocity field of an ice sheet model (e.g. one with inversion of basal friction based on surface elevation/flow) to estimate the age input at the grounding line. It would be still quite wrong but less wrong than simply setting it to simulation runtime? How does the assumption of a uniform age throughout the ice column influence your results?

L80-82 If the real age structure of the ice shelf is unknown, how do you compare predicted and observed stratigraphy not knowing which layer corresponds to which age? Please elaborate.

L88 What do you mean by "cross-checked"? Please clarify? What was the results of this cross-check?

Figure 3 and section 2.2

L94 for those how do not know what the StdN experiment is (not mentioned in Pattyn et al., 2012 as it is part of the MISIP3D intercomparison experiments), a little more information is needed here.

L95 here I am confused again. Do you do the cross-check for the analytical case on the same model domain (StdN MISIP3D) as for Elmer/Ice. If so it might be good to make this clear. Otherwise the uniform forcing of Elmer/Ice ($a=0.3 \text{ ma}^{-1}$, $b=0 \text{ ma}^{-1}$) are very different from the input data you use eq.2 (Figure 1). In that case, why don't you use the actual ice shelf geometry and boundary conditions in Elmer/Ice as well? I assume you do not use Elmer/Ice right away due to computational demands.

L147 Subection 3.1 is only two sentences long. Either be more expansive here or consider merging subsections 3.1 + 3.2.

Figure 3:

Wouldn't it be more interesting to show the difference in vertical velocity at two or more vertical profiles through the ice shelf (e.g. one 'borehole' at 10 km and one at 50 etc.). I assume the difference in the vertical velocity between Elmer/Ice and Eq.2 at the surface will be minimal by definition? This would also be more consistent with the data you show in 4b.

What significance has the pm 0.5 ma^{-1} delineation in Figure 3?

Figure 4:

Caption: It probably should read Fig 4b cross section on the vertical line in 4a (also check use of lower/upper case for consistency).

I do not see the white dots (calculated delineation). What are the white lines? I assume isochrones as in Figure 2?

Minorly is rarely used (I had to look it up actually)? Maybe change to slightly?

L177-179 difficult for me to follow, I suggest rephrasing this. Smeared?

25 m vertically I presume? Also depends on resolution, maybe provide a range for the resolutions tested here. E.g. 10-50 m or whatever would apply here.

Figure 5: What's the reason for the bulge of relatively old ice between 10-40 km downstream of the grounding line? Looking at Figure 1 there seems to be no salient anomalous melt/refreeze or velocity variations along this transect? Maybe worth discussing this? In Figure 6b it seems clear that the old ice pockets correlate to the areas with relatively fast ice flow. But what happens here?

L183-184 there is no reason to believe that the closest modelled isochrone should have the same age as the observed IRH, except if you are very confident about your velocity field. It is very probably that the modelled isochrone is either too young or too old, potentially by a lot. I am aware that it is unfortunately not possible to use ice core tie points or trace the nearest dated continental isochrone all the way to the ice shelf. But I think at this point there should be a discussion of the caveats of your comparison. If you discuss the elevation misfits of two isochrones of very different age it is not straightforward to assign these misfits to local mass balance/ice flow I guess. So, the question would be to what extent additional information can be extracted here except for the fact that cross-cutting is bad?

L186-188 I do not get this step. Do you mean by interpolating that you take the modelled isochrone above and below the IRH and then vertically interpolate the IRH's age? And then take the deviations between the nearest observed and modelled isochrone as a measure of systematic misfit? The temporal resolution of your isochrones is one year?

Subsection 4.1 title is a bit vague. Advantages and disadvantages with respect to what? I suggest being more explicit here.

L210 This or a similar sentence should also appear earlier when you discuss the methods.

Figure 7: I think you use jet-cmap or similar, for me it's fine, but I think it is not a good choice for color-blind readers.

L200 "In the following we discuss these effects separately"

L230-231 please elaborate and specify what kind of model developments are necessary to make this work in your opinion. I assume the main issue is that forward models would diverge to much from the observed real evolution of an ice shelf.

L233-236 please write out melt/freezing rate and accumulation.

If the maximum age of e.g. the RBIS is ca. 900 years old (according to your velocity fields) then past variations in ice shelf mass balance will probably be significant. You could test this by e.g. repeating your experiments with an mass balance forcing (melting/freezing and surface accumulation) which is e.g. half or twice (or maybe more realistically 10-20%) as large as the currently observed and have a look at how the age field changes. This would be a straightforward test to gauge the relative importance of time varying forcing.

Figure 9 maybe more illustrative to use percent changes to the interpolated IRH age. A difference of 10 years for an IRH of age 10 is more serious than for an 800 year old IRH.

Figure 10: again, please use a continuous cmap. The diverging cmap insinuates a transition which is randomly set to 50% LMI. Ignore this statement if this effect is intended (but then please explain why a 50 % LMI delineation is relevant.)

L283-285 again, you could test this by scaling your input fields and discussing the changes in isochrone geometry according to your local climate drivers.

L293-296 I would argue that this only holds for moderate changes in ice shelf mass balance. If surface and ocean warming are large enough than the current portion of CMI of an ice shelf probably does not matter much for its stability? If I recall correctly Kittel et al. show substantial increase in surface melt over ice shelves for the 21st century for high emission scenarios (e.g. for example if forced by CESM2). In some runs shown by Kittel also the grounded part of the ice sheet experiences widespread surface melt.

L310 reduces -> decreases