This paper introduces LADDIE, a 2D model that implements the depth-averaged navier stokes equations for ocean physics over a mixed layer thickness. The equations have been implemented before (e.g. Holland and Feltham 2006), but there are new modifications (for instance avoidig a hard constraint on minimum thickness), and moreover it is written in the form of an open source python code intended for wide use (though I have not tried it, and am not clear on how easy it is to port to another domain!) The model results are carefully compared against available observations for select ice shelves with high quality modelling and satellite observations of ice ocean interactions.

I have very few issues with this paper. The model itself is a step forward, and the reasons for it being a step forward are explained thoroughly, legibly, and carefully within the introduction, results and methods sections. The paper does make a very good point that there is a limit to how useful 3D ocean models can be due to cost and resolution required -- but is very clear on what LADDIE is *not* able to do ie model deep cavity dynamics -- and i found the discussion of its limitations (and possible extensions) to be very thougtful. As a scientific paper i feel adequate attention is given to the extensive literature on modelling and observing ice ocean interactions. I would recommend publication following the address of a few minor comments, which i feel will be quite easy.

two general but still minor comments are on the Dotson/Crosson results section:

a) there have been other studies with 3D ocean models run at higher resolution e.g. 1km, I wonder why you did not want to compare to these?

We have chosen the comparison to these ~3km resolution simulations based on their availability and their relatively long simulation times, making it possible to assess a quasi-steady state. We will phrase this more explicitly in the methods section.

b) Given the emphasis on the channelised melt, I think it is worth mentioning that a recent coupled modelling study (Goldberg and Holland 2022) saw this channel melt completely through within 50 years (in line with the extrapolation of Gourmelen 2017), and the ice-dynamic impact was minimal, somewhat downgrading its importance. The same is not true for the internal shear margin and grounding line of course!

Thank you for pointing out this relevant paper. We will refer to this to place the relevance of channelised melt into context.

line 34: "presumed stagnant" -- this is an assumption of LADDIE, not the physics of entrainment into the actual ML, which is how this reads.

Agreed, we will remove this classification

line 44: at-->over

Agreed, we will correct this

eqs 1-5: it would be nice to state whether these differ from the PDEs solved in Gladish et al 2012, and how if so. Also, this is for the author to decide, but an appendix showing how to do the integration that arises in the pressure terms (1st 2 terms on the LHS of (2) and (3)) would actually be quite helpful -- because im not sure i've ever seen clearly how these come about, or how to do the layer integration and with which boundary conditions. For your consideration.

We appreciate this suggestion. However, these equations have been published in a considerable number of studies already. The primary addition of LADDIE to this research field is the numerical implementation of these equations, the development of an open-source model, and the application to realistic geometries. We therefore do not believe an additional derivation is required for this study. The pressure terms referred to are derived for a dense bottom boundary layer by Killworth and Edwards (1999), so we will add a reference to that derivation.

eqs 1, 8-10, and 14. Can you state that m_dot>0 indicates melt (if this is true). I don't think you do. (1) indicates it is, and i can reason this is consistent with (8) without referring to other papers. But i have not seen (14) before -- my simple understanding of it is that entrainment is enabled by positive TKE production, and (where there is freezing) by negative buoyancy flux. All seems consistent but it would be nice to be sure.

This is indeed true, and we will state it explicitly.

L192: i don't understand what a weighted average between free and no slip is. are you solving the model twice at each time step with different boundary conditions? would showing an equation help?

We have adopted this formulation of partial slip from the NEMO numerics and will expand its explanation.

L213: "one can interpret"... i think this is only true if the 3D model is isopycnal.

Agreed, we will mention this explicitly

L215: i like this rather than a hard constraint.

This is good to hear

L224-5: "to ensure continuity" -- by you, or the satellite analysts?

By the satellite analysts. We will rephrase this to clarify

L339: just to point out that these values for Ah are not huge but bigger, for instance, than that suggested by the MISOMIP protocol. What happens when you have Ah=5, do things change then? or is LADDIE unstable?

We agree that these values are still reasonable. Indeed, lower values of Ah lead to numerical instability which can either be resolved by a shorter time step or a smoother topography. Hence, LADDIE can perform the ISOMIP experiments with Ah=6 and Kh = 1. We will clarify this.

L400: "near-zero due to the lack of simulated barotropic flow" -- you don't show any evidence of this, or of it being the cause of low melt rates. My recollection is that the column here is quite a bit bigger than at the Smith and Pope grounding line. On the other hand the

Naughten model has pretty coarse vertical resolution at this depth and so the resolution of near-ice variation is particularly poor.. could this be a another potential reason?

Yes, we agree that we have not assessed this in detail, and the vertical resolution can certainly be a dominant factor. We will nuance this discussion and mention MITgcm's vertical resolution in this region as a possible explanation for the low melt rates.

Figure 6: could you show profiles from the 3D model as well?

Based on comments from reviewer 1, we have moved the discussion of 3D forcing to the appendix to avoid confusion. We therefore minimise the emphasis on this forcing and will not include these profiles.

line 444: what do you mean by the remote sensing not showing conclusive evidence? of channelised melt? or of specific features mentioned above? Im also not sure what you highlight in 3a -- there is very little detail here.

This notation referred to the separate meltwater pathways. We will rephrase this to avoid confusion.

L 458: it is possible that channelisation can lead to enhanced stresses and damage, but a reference would be nice here.

Agreed, we will add a reference

L495 -- where does this warm water come from? surface-warmed or other?

Indeed it is most likely that this is a surface-warmed water mass. We will mention this explicitly.

L534: propose-->suggest

Agreed and will implement

L608: a good point about subgl outflow. Is it not in fact trivial to add this?

Yes, this is indeed trivial to include if data is available. We will mention this

L620-623 -- a really good point about thin columns. Should note though this assumes detailed knowledge of bathymetry, which i think can only be this good if there is drilling, no?

Yes, we agree. We will clarify that this requires detailed knowledge of bathymetry