Author’s response to reviewer 1

We thank the reviewer for its very good points, which helped us make the manuscript clearer and strengthen the conclusions.

We noticed, however, that there has been an error in the supplementary figure which uses a logarithm of base 10 to draw the curve of drag coefficient against the cell thickness while it should be a natural logarithm. This is an important note as part of the comments were based on this figure. In our supplementary material, we draw the same curve(s) with a natural logarithm and for different z. As expected, the result is quite different.

Top-level comments

1) The main advance of the paper is in proposing the use of a formulation of the drag coefficient that is dependent upon the grid cell height (equation 3). However, this function is only very weakly dependent upon this height until the grid cell is below 2 metres or even 1 metre (see figure attached as PDF). This seems to be implying, for me, that for most purposes the assumption of a spatially constant drag coefficient should be OK. I feel that this key point should be mentioned. However, and more importantly for the paper, it leads me to wonder where the difference between the REF and LOG simulations comes from. At the moment I think that these differences are probably arising because REF uses the Losch layer of 30m to drive melting, while LOG uses the top wet cell. I propose that they are not caused by the change in the drag coefficient. Clearly, this distinction is central to the paper’s conclusions. I am reluctant to suggest any new simulations, but I think the only way to determine this is to run simulations with the melting parameterisation driven by the top wet cell, not the Losch layer, but using a constant drag coefficient (for both 75 and 121 levels). If these simulations look like REF then the paper’s conclusions stand – the change in the drag coefficient formulation is important. If these simulations look like LOG then the paper’s conclusions should be redirected towards studying the numerical stencil of the melting scheme, rather than the drag formulation. At the moment I think the latter point might be true.

This comment is partly based on the supplementary figure provided by the reviewer, which is not correct as mentioned above. But it is still a valid concern and we want to address it.

This is a very good and important point that is being raised here and this is true that the manuscript lacked a deeper reasoning around it. So we thank the reviewer to give us the opportunity to give more explanation on the use of a variable roughness length.

To start with, we don’t fully agree on the first assertion that the function is only weakly dependent upon the cell thickness. By looking closer at the curve (see supplementary Figure 1a), for z0=3e-4 m (which is the value used in the LOG experiments), Cd=0.0025 corresponds roughly to z = 1 m and Cd=0.0015 to z=15 m. These two Cd values are expected to lead to significantly different melt rates (e.g. Fig. 6 of Jourdain et al., 2017), and these range of *z* values is common due to the use of partial cells and to the fact that fields are usually calculated at the cell centers (i.e., *z* is half the cell thickness).

This brings us to the next, and most important, point stating that the differences between both experiments come from the computing of friction velocity in the Losch layer or in the first wet cell. We actually already had run a simulation with a constant Cd = 0.0025 and the friction velocity computed in the first wet cell for 121 layers (NOLOSCH121) so we have just made another simulation with 75 layers (NOLOSCH75) to have a complete picture. The results are actually different from what the reviewer was expecting, as now described in supplementary Table 1. With 121 layers, melt rates for NOLOSCH.L121 are closer to LOG.L121 than to REF.L121 for most ice shelves, particularly for Thwaites (0.6% vs. 55.6%) and Crosson (4.2% vs. 17.2%) ice shelves, but also for Pine Island (-5.4\% vs. -6.2\%) and Dotson (0.3% vs. -1.7%) ice shelves. Only Getz ice shelf is slightly above (15.4% vs. 14.4%). On the contrary, with 75 levels, the total melt difference for all ice shelves is 0.1\% between NOLOSCH.L75 and REF.L75 and 13\% between NOLOSCH.L75 and LOG.L75 with clear discrepancy for individual ice shelves. total melt for all ice shelves. So in summary, NOLOSCH is closer to LOG than to REF with 121 layers and much closer to REF than to LOG with 75 layers.

Moreover, and most importantly, the difference between NOLOSCH75 and NOLOCH121 (32.2% for all ice shelves) is much higher than between LOG75 and LOG121 (6%) and even REF75 and REF121 (22%). This means that it is absolutely not equivalent to have a constant or variable coefficient of drag in the first wet cell.

To understand better why melt rates from NOLOSCH are closer to LOG when there is 121 levels and to REF with 75 levels, I plotted the histogram of drag coefficient (figure 1b) and the histogram of cell half thickness (figure 1c) for all ice shelves for 121 and 75 levels. For 121 levels, cell half thickness is always smaller than 10 m and the coefficient of drag is between 0.0014 and 0.0026 while for 75 levels the cell half thickness can be up to 30 m and the coefficient of drag down to 0.001. In other words, LOG121 drag coefficient is closer to 0.0025 than LOG75 and therefore NOLOSCH121 melt rates are closer to LOG121 although the difference is not that strong for all ice shelves. On the contrary, LOG75 coefficient of drag can be much lower than 0.0025 while the first wet cell thickness is closer to the Losch layer thickness used in REF75. For that reason, NOLOSCH75 is closer to REF75. It would have been interesting to run a NOLOSCH experiment with a drag coefficient close to the average of the LOG experiment. Nonetheless it shows that a variable drag coefficient, depending on the cell thickness, gives finer results and can be used the same way in any configuration, regardless the vertical resolution.

In a nutshell, because of the high range of cell thickness (from 1 m to 20 m for 121 levels and up to 60 m for 75 levels), having a constant or variable drag coefficient on the first wet cell is not equivalent and the conclusion of the paper still holds.

We have added this text in a new subsection (3.3. Law of the wall versus constant drag in the first wet cell) and the NOLOSCH experiment has been added in the experiments section. All figures from the supplementary have been added to the revised manuscript (figures and the table results are added to table 2).

2) There is a conceptual issue around this application of the law of the wall formulation (equation 3). This is designed for constant-stress boundary layers adjacent to an interface that is hydraulically smooth. It can cope with a variable roughness length, as used here, but only as

long as that topographic roughness is not larger than the viscous boundary layer depth, such that it would start to disturb the boundary layer with lee eddies, etc. This is apparent in the paper, because the roughness lengths used are tiny - of order 0.1 mm. However, the topographic features present beneath ice shelves and being invoked here are much larger than this – e.g. order 10m in the case of terraces, and order 100m in the case of rifts, ice blocks, and basal channels. This very large topography will completely disturb the boundary layer and so should not be expected to be well represented as a sub-millimetre scale perturbation. Extreme topography on this scale would not be expected to smoothly increase the stress transfer through the boundary layer. This kind of roughness could not even be simply characterised as 'form drag', let alone the viscous drag assumed here. The Hughes paper cited by the authors is a nice illustration of this. This is certainly the roughness that the authors are invoking, as it is only these very large scale/large amplitude features that will be present in the surface DEMs being used here. Given this uncertainty, I am not sure what to recommend. It is possible that a constant drag coefficient is even a better choice, in the face of the extreme uncertainty around this problem. I fully support the authors' goal of incorporating roughness into melting formulations, but the application of this law is not clear. I don't know how the authors might like to proceed. The best approach here probably depends on whether the drag law makes a difference (point 1 above).

The reviewer raises a very valid point, which has been a source of ongoing debates between co-authors. We tried to show this limit in section 3.5 but this could be better flagged following the reviewer's suggestion. One of the sentence in this section is: "The purpose of this study was mostly to use a proxy to consider the spatially variable roughness of the ice shelf base." and a better law could be further developed and probably should.

In any case, as shown in the answer above and given the conclusion of the paper, a constant coefficient of drag is not a better choice.

We have added this sentence in the section limits of the study: "This is designed for constant-stress boundary layers adjacent to an interface that is hydraulically smooth. It can cope with a variable roughness length, as used here, but only as long as that topographic roughness is not larger than the viscous boundary layer depth, such that it would start to disturb the boundary layer with lee eddies, etc."

And we have added this sentence in the conclusion: "This study shows the importance of better representing spatially variable ice-ocean drag for large-scale melt patterns, and it is an incentive to put more effort in the development of more realistic laws for turbulent transfer at the ocean-ice interface."

3) I think the logic of the new drag formulation is this: When the top cell is thicker than the log layer it needs to be parameterised, but when the cell is thinner, it will start to resolve the log layer, and hence the parameterisation should be reduced. But, does the model resolve the log layer when the cells become thin? I am not sure we know this. I think it depends upon the vertical viscosity parameterisation in the model, and the implementation of the momentum boundary conditions. In real fluids the log layer profile arises because the eddy viscosity increases linearly with distance from the interface. I don't believe this eddy viscosity

parameterisation is usually used in ocean models. There is also an important corollary here: when the vertical resolution is varied, is the vertical viscosity varied as well? With a higher resolution it would be typical to reduce the viscosity. That would lead to a smaller boundary layer, which is harder for the model to resolve. So it is not clear that a finer grid resolution will better resolve the log layer, as assumed in this new parameterisation.

We do not claim that NEMO has the capability to resolve the log-layer. The top cell thickness is usually more than 1m, and even with a thin top partial cell, the thickness of the cells below is generally ~20 m so that there is no way to correctly resolve the log layer even if the viscosity parameterisation was suitable. Therefore, the log formulation is applied to all model cells beneath ice shelves, it is imposed exactly because NEMO does not produce such vertical log profiles.

We have added these sentences in the limits of the study section:

"In real fluids the log layer profile arises because the eddy viscosity increases linearly with distance from the interface. The third caveat is in the fact that NEMO, and our configuration doesn't resolve the log-layer. The top cell thickness is usually more than 1m, and even with a thin top partial cell, the thickness of the cells below is generally ~20 m so that there is no way to correctly resolve the log layer even if the viscosity parameterisation was suitable.) Therefore, the log formulation is applied to all model cells beneath ice shelves, it is imposed exactly because NEMO does not produce such vertical log profiles. "

4) The reduced sensitivity to vertical grid resolution changes is a key feature of the new approach. However, I wonder if the higher sensitivity of the old approach is due to the way in which the 'Losch' layer is implemented in NEMO. In the Losch (2008) paper, this approach was only used for temperature and salinity, not velocities. More importantly, the Losch layer thickness was equal to the top grid cell thickness, not the fixed 30 m that is used here. I wonder if the 22% sensitivity of the old approach here is caused because NEMO uses a fixed 30m Losch layer thickness? Clearly this is going to mean that as the cell thickness increases, fewer cells will be in the Losch layer. But with the original Losch approach, which I think is used in other models, there will always be 2 cells in the Losch layer. Given that the vertical viscosity may change as the grid resolution is changed (?), those 2 cells may contain a fixed fraction of the changing boundary layer thickness. Thus it could be speculated that the original Losch approach would be less grid dependent than the approach apparently used in NEMO. This implies that the new parameterisation may not be as beneficial in models that follow the original Losch approach. Gwyther et al (2020) document substantial variety in the way the models approach this. I think the only way to address this would be with additional simulations following the standard Losch approach, though again I am reluctant to suggest further simulations.

Losch (2008) indeed defined the layer as the thickness of a single full grid cell, so that even with partial steps, the same thickness was used for all cells. Losch used the original ISOMIP configuration with a uniform nominal resolution of 30 m for all levels. In contrast, most realistic NEMO configurations, like the one used in our manuscript, make use of a non-uniform vertical resolution. It means that the deepest parts of ice shelf cavities have a coarser vertical grid than the parts closer to the sea surface. Given that the Losch layer was invented to remove noise by ensuring a uniform layer thickness with partial steps, we consider that using a constant 30 m layer thickness for all vertical levels is totally in line with the spirit of Losch (2008). This explanation has been added to the introduction of the revised manuscript.

The reviewer is nonetheless right that there are several ways of making use of the Losch layer, with several examples in Gwyther et al. (2020). Losch (2008) and other studies only use averaged temperature and salinity in the Losch layer while we also use averaged velocities in the standard experiments. It is therefore possible that the added value of the new parameterisation is lower in models that do not average velocities in the Losch layer, although our response to the Reviewer's first comment indicates that a part of the added value is not related to the Losch layer itself. This point is now acknowledged in the limits of the study section in the revised manuscript.

5) A key conclusion is that increasing the roughness increases the melting, which could mean there is a feedback between increased ice shelf damage and increased ocean melting. I like this result, but isn't the top-level result already obvious? If we increase the drag coefficient, melting will increase, as shown by previous studies, and so this conclusion should be stated in relation to this existing knowledge. Where this study could offer a new finding is in the precise sensitivity of melting to changes in roughness – i.e. the curve of melting vs z0. The authors could consider plotting a curve of drag coefficient versus z0 to show how melting might change as the roughness changes? I assume it is highly nonlinear in some way, which would be an interesting finding. However, there is of course the high uncertainty in whether this is the right drag law to use (see above), so perhaps the authors may feel this is not useful. There are also lots of other considerations in terms of how melting might change, such as how the velocity changes with roughness.

It is true that higher melt for higher roughness can be expected from the three equations combined with the law of the wall, but (1) as far as we know the effect of increasing damage on melt rates has not received a lot of attention so far, and (2) there are feedbacks between melt rates and ocean velocity and temperature that may give unexpected melt sensitivity to roughness. We nonetheless agree that providing a quantitative value would be more useful than this simple statement.

In supplementary figure 2, the coefficient of drag is plotted against $\ln(z_0)$ for different cell half thickness z. This is only giving a partial picture of what the melt rates would look like because of its dependency on velocity, temperature, salinity. Nonetheless, one may see it as a coarse way to speculate on the roughness sensitivity.

We add the figure to figure 9 and the text in section Increased damage (LOGx5 vs. LOG).

Smaller points

General: With partial cells, do the authors apply a minimum cell thickness? I believe that is standard, to avoid numerical instability as the cells get very thin. If, for example, a cell cannot go below 1m thickness, this will reduce the influence of the new drag law even further (see above)

In NEMO, the partial step thickness has a minimum. In our configuration, it is set to be the minimum between 25 m and 20% of the level thickness. For both 121 and 75 layers, the minimum cell half thickness is around 0.5 m so a cell thickness of 1 m. As discussed above, the influence of the law is still important.

Line 95: 'upper and lower' is confusing

Instead we can use the x-ward and y-ward velocity components. This is corrected in the text.

Line 100: why the subscript 3 ?

This is the way it is called in NEMO. The subscript 3 refers to the vertical component while 1 and 2 refer to the horizontal component. It is now mentioned in the text.

Figure 2a: I'm afraid I found this really confusing because the cells are all in different places. I think this is because the z* coordinate means that all of the different levels are at different heights and that is randomly applied here, though I am not sure. Could it possibly be redrawn for z coordinates, since that expresses the key difference here (Losch vs 1st wet cell), and then just note that actually z* is used? Also, the cells next to the ice only have 1 open horizontal face (right), with their left boundary being ice. I think it is far more common that they would have two open horizontal faces, with ice only on the top, so maybe better to draw that? This emphasises the need to average over the 4 u and v values on the cell edges.

The purpose of the figure was double: understand what a partial cell of the z*-coordinate system was and show the difference between the Losch layer and the first wet cell. As it seems to be too much info for one picture we split it into two schemes, each showing one of the concepts.

Concerning the ice boundary, the purpose was to show the need to average different levels of u and v but the comment is relevant and we now have a flat ice boundary.

Figure 3a: Similarly, I found this really hard to interpret. Maybe select the depth range of ice shelves and plot cells on a linear vertical axis? Maybe that is not possible. Give a table with cell heights at chosen depths (10m, 100m, 500m, 10000m)? Also it looks like the 121 levels were deliberately chosen to represent ice shelf cavities better – is that true? i.e. they are not a uniform increase in resolution, relative to 75 cells.

Yes it is true, the 121 vertical resolution has a fairly constant nominal vertical resolution between 100 and 1000 m depth (ie. level thickness between 20 and 30m). This is now clarified in text: "Ice shelf drafts in the ASE are typically spanning between 100 to 1000 m depth, and we use 121 vertical levels defined as in Mathiot and Jourdain (2023), with a fairly constant vertical nominal resolution (before partial stepping) between 100 and 1000 m (level thickness around 20-30 m). This means that level thickness is around 20-30 m in most ice shelf cavities."

The depth range of ice shelves is in plain color while other depths are more transparent. We add a table as suggested so it is clearer.

Line 127: I'm afraid I didn't understand how this roughness measure was calculated, and this is important. I assume that in the NEMO cell there are many DEM cells. How are all of these cells compared to arrive at R?

Line 127 states:

"To calculate the "topographic roughness", R, at the base of the ice shelf, we use a simple definition: the largest inter-cell difference between a central pixel and its surrounding cell (Wilson et al., 2007)."

It means that for each resolution and for each grid cell we compare the difference between the depth of this cell and the ones of the surrounding cells and take the largest difference. Afterwards, to get a value in the Nemo grid, we use a conservative regridding.

We change the sentence to: "To calculate the ``topographic roughness'', R, at the base of the ice shelf, we use a simple definition: the largest inter-cell difference between a pixel value and its adjacent cells . We chose this method for its simplicity and its correlation with the maximum slope. Other methods are discussed in section 3.6. Thereafter, we use a conservative regridding method."

Line 134: The mention of 2 m resolution here is confusing.

This is the highest resolution but could be confusing with respect to the following part of the paragraph. So we remove it as it is not necessary.

Line 170/Table 1: I was confused about this roughness length, because it gives a drag coefficient much higher than 0.0025 for all grid cells (see figure above) and yet it gives the same melting overall. I think this confirms that it is not the changed drag coefficient formulation that is important, it is the switch to using the top cell velocity.

This comment is based on the supplementary figure provided by the reviewer, which we flagged to be incorrect.

Line 221: this explanation does not seem to work for Thwaites?

Yes this is true in the Eastern sector of Thwaites ice shelf close to the grounding line. This is now mentioned.

Paragraph 1 on page 14: I'm sorry, I was not able to follow the 3 points of explanation offered in this paragraph.

We rephrase it like this:

"This is due to the fact that, in the current version of NEMO, using the Losch layer approach, the mean horizontal current speed, used in the calculation of the friction velocity, is the magnitude of the components averages and not the average of the magnitudes"

Figures 9 and 10 and sections 3.1 and 3.2: I could not find a physical explanation of why the melt rates were different in these cases. There are interesting patterns of difference, but these were not explained in terms of the underlying velocities in the different regions, and how well they were represented by the different drag laws, and the different vertical resolutions.

The value of each cell depends on the velocity profile and it is not straight-forward to interpret the pattern as such. We believe that, for the REF experiment, the Losch layer can be larger than 30 m (as the first wet cell can be larger) and the coefficient of drag is then not suitable anymore.

That is what is explained in this sentence:

"Essentially, the cell thickness is variable but the drag coefficient is constant. If the 1 st wet cell thickness is thinner than the Losch layer, with a coarse resolution, we use the weighted velocity of the next cell below (as explained in Appendix B1), which could have a thickness much thicker than the Losch layer. In this context, depending on the velocity profile, the Losch layer speed could be under- or overestimated."

As we can't explain the figure, we can remove it since the total melt for each ice shelf is more important.

Equation (B4): bottom equation has an extra term C, which I don't follow. Is this a constant background velocity?

It was a mistake, this is removed.

Author’s response to reviewer 2

We would like to thank reviewer R D Larter for his valuable comments and we reply to them hereafter (reviewer’s comments in bold and reply in plain).

I consider this to be an interesting study addressing a fundamental problem in cryospheric science, the parameterisation of ice shelf basal melting. Having said that, I am not a numerical modeller or mathematician and think it is essential that the manuscript is also reviewed by someone with expertise in these fields (I have not yet looked at the other review I was notified about because I want to provide completely independent comments).

Recent studies have revealed that ice shelf basal melting is more spatially and temporally variable than previously appreciated, so new ways of modelling the melting, particularly for areas where ice shelf basal topography is complex, need to be explored. Therefore, I am pleased to see this account of such a study.

In general, the manuscript appears to me to clearly articulate results of a thorough exploration of different approaches to parameterising basal melt. I have no major comments, but I have annotated a copy of the manuscript with many minor ones. I have also included a few of the more important of these comments below.

We agree with all comments in the annotated file and have corrected the manuscript accordingly.

A general comment is that annotations on several of the figures are in a font that is too small if they are going to be published at the same size as in the review copy. Colour scale bars in several figures are annotated with values expressed with exponents, whereas I think converting these to more simple numbers would make them easier to understand for a greater number of readers.

We have followed the reviewer’s suggestion..

Labels need to be added to identify the different parts of figures 2, 3 and 4 that are referred to in the captions.

True. These have been added.

After studying Fig. 2a and the related text it is still not completely clear to me where on the boundaries of cells various parameters are being computed. I infer that the blue box on the left of the figure is depicting a plan view of a cell since it is labelled with x and y axes, but does the position of the T-point that is depicted mean that is on the upper or lower face of the cell, or both, or in the middle of the cell? The V-point is described in the text as being “at the center of the upper and lower grid face”, but if the blue box labelled with x and y axes is indeed a plan view, then the V-point appears to be on one of the lateral faces. I think it would be helpful if the authors could try to further clarify their explanation of where these different points lie by modifying the text or the figure, or both.

The other reviewer also had a comment on this figure and we have therefore created a new version to address these comments.

We now show a 3D version of the cell. The T point is actually in the middle of the cell. For u, we can use the east and west side of the cell and for v, we can use the north and south side of the cell to be clearer.

We also changed the explanatory text:
"For instance, temperature and salinity are computed at the centre of the cell (T-point or coordinate indices i,j) and the x-ward and y-ward velocity components, u and v, are computed at the cell boundaries (U-point of coordinate indices i+1/2,j for u, and V-point of coordinate indices i,j+1/2 for v)."

Fig. 2b is the first place in the paper that maps of the different ice shelves appear. It would be useful to include a regional map in the figure, or in an earlier figure, showing where the areas represented by these local maps lie. A similar map to the one in Figure 4b could be used for this purpose. It would also be helpful to add distance scale bars to each of the local maps to make clear that they are at different scales.

This is a good suggestion. Our previous Figure 4 has been moved before any other figure showing the ice shelves and we have added a scale bar close to ice shelves.

The detail I am most puzzled about is the apparent near random distribution of first wet cell heights shown in Fig. 2b. Fig. 2a and Fig. 3a indicate that first wet cell height increases with depth of the ice shelf base. Therefore, I would expect to see a trend towards more purple and black cells, indicating greater cell height, nearer the grounding lines. Unless an error has been made in the production of the figure, I think a further explanation of the apparent near random distribution of first wet cell heights is needed.

It was indeed unclear. This is because we use the z*-coordinate system with partial steps. We have made paragraph 2.1.1 clearer: "We use a curvilinear z*-coordinate system with partial steps to adjust the thickness of grid cells adjacent to the sea floor (index k_b) or ice shelf draft (index k_t) in order to simulate a realistic water column thickness. Therefore, the cell thickness near the sea floor and ice-shelf draft depend not only on the k vertical grid index, but also on the horizontal i,j indices." This is what we tried to show in the previous figure 2a. This is now better represented in figure 2b.

We can add an explanation in the figure caption: "First wet cell half thickness below ice shelves in the ASE configuration with 121 layers: Pine Island, Thwaites, Dotson-Crosson and Getz from left to right. The cell thickness is adjusted to the ice shelf draft through partial step.

I am also puzzled about the difference between the values of the constants given in the "roughness length" column in Table 1 and the values of the constant α given in the caption to Figure 5. Perhaps I have misunderstood something, but it seems to me that these constant values for a particular experiment should be the same in both places.

This is a mistake from our side. These were values from an older version. This has been corrected in the revised manuscript.

There are a couple of places where variable terminology is used to describe the same thing, e.g. "forcing" and "driving", and "hydrographic roughness" and "hydrographic roughness length". More consistent terminology would make the paper more accessible to non-specialists.

Good point. We use hydrographic roughness length everywhere.