Response to reviews of " Antarctic Ice Sheet grounding line discharge from 1996 to 2024"

We thank the reviewers and the editor for their time and effort in reviewing our paper, "Antarctic Ice Sheet grounding line discharge from 1996 to 2024", submitted for publication in ESSD. We welcome the positive feedback and insightful comments which we have endeavoured to fully address in this revision, and we hope you agree this improves the manuscript. We have incorporated the majority of the suggestions made by the reviewers (indicated by 'Done' at the start of our response). In the few cases where we have not implemented the reviewer's suggestion (indicated by 'comment' at the start of our response), we have provided a detailed description of the justification for each decision. The changes are highlighted in the manuscript through the track changes function. Please see below a point-by-point response to the reviewers' comments, where all line numbers refer to the revised manuscript file with the tracked changes.

1. Renaming FrankenBed – done in previous iteration

2. Removing BedMachine-only – I don't think the BedMachine lines contribute much beyond the BedMachine+HF14 lines in the plots. Therefore BedMachine lines could be removed, as they are 90% the same as BedMachine+HF14 line, and only vary for good reason on the Peninsula. [still in Figure 9].

Done. We have removed BedMachine only from the revised manuscript.

3. Vertical velocity profile – Equation 1 should contain some assumption about the ratio of depth-averaged velocity to surface velocity. For perfectly deformational ice flow, the depth-average velocity is 0.8 of surface velocity. At present, the implicit assumption is 1, or plug flow, which means that basal sliding velocity is assumed to be equal to surface velocity along the entire flux gate perimeter. This is likely to be the case. See how https://doi.org/10.1029/2001JD900033 estimate this ratio on a gate by gate basis. The authors need to explicitly say they assume plug flow at all their gates, or make gate by gate assumptions, for example informed by convex/concave surface elevation profiles indicative of basal sliding (https://doi.org/10.3189/172756505781829430). [the revised version just makes the assumption explicit but does not explore associated uncertainty (or propagate it)]

Done. Assuming surface velocities are equal to depth-averaged velocities to calculate ice flux is well established in the literature (Gardner et al., 2018; Rignot et al., 2019; Mankoff et al., 2020; Mouginot et al., 2014) and is supported by modelling studies e.g. "*fast sliding is not common just to fast-flowing features but is widespread on the continent"* (Morlighem et al. 2013).

In the previous version of the manuscript, we made our assumption regarding the use of surface ice velocity clear: "*As in previous studies (Mankoff et al., 2019; Mouginot et al., 2014; Mankoff et al., 2020), we assume the depth-averaged velocity is the same as the measured surface velocity"*; we thought this would address the reviewer's concern that "the authors need to explicitly say that they assume plug flow", which "is likely to be the case", but we apologise if this was not clear enough. In the revised manuscript, we have further clarified this assumption by stating that "*As in previous studies (Mankoff et al., 2019; Mouginot et al., 2014; Mankoff et al., 2020), **we assume plug flow** i.e. the depth-averaged velocity is the same as the measured surface velocity".*

4. Removing "FrankenBedAdj" – I applaud the authors for trying to also take the opportunity to improve existing bed products, but I feel that their further adjustment to BedMachine+HF14 would be an article in itself. In brief, they seek to use mass continuity to solve ice thickness as the residual of velocity, surface mass balance anomaly, and transient ice thickness change. This is more complex than the approach of BedMachine, which applies mass continuity to just velocity and surface mass balance (not SMB anomaly, and not transient dH/dt). It is promising, but presently appears underdeveloped and documented, especially in the absence of any description of how vertical velocity profile impacts balance velocity in each basin. [*No, this is declined. This is my biggest reservation, and likely a deal-breaker. I don't think maintaining the FrankenBedAdj residual without a comprehensive uncertainty assessment is a valid option.]*

Done. We acknowledge the reviewers' concern here and to address this we have removed "FrankenBedAdj" from the revised manuscript and dataset.

5. Glacier density – I appreciate that 917 kg/m3 is the theoretical density of ice, but this is clearly an upper limit for ice crossing the grounding line. For example, at Columbia Glacier, the depth-averaged bulk glacier density downstream of ELA has been estimated to be as low at 750 kg/m3 (https://doi.org/10.1002/2015RG000504). I wonder if the authors should explicitly say they assume that bulk glacier density is not influenced by crevasses? Or, alternatively, at least use a conservative bulk density range like 900 +/ 15 kg/m3? [*I see no response on this point, although the revised methods suggest it can be a 2% effect, it does not appear to be propagated].*

Comment. Using an ice density of 917 kg/m3 to calculate ice flux is well established in the Antarctic Ice Sheet literature (Gardner et al., 2018; Rignot et al., 2018; Mankoff et al., 2020). The density value of 750 kg/m3 suggested by the reviewer is taken from a study of a highly crevassed surging glacier, so is likely to be a outlier in terms of bulk density and is a very different flow unit to those in Antarctica. In addition to not following methodological precedent for density assumptions in the Antarctic science literature, if we were to use a different density value, then it would make the results presented in this paper far less directly comparable with other published work, so we have retained our 917 kg/m3 bulk density assumption.

In the previous version of the manuscript, we followed the reviewer's previous recommendation to explicitly state our assumption of bulk density: *"This is an upper bound on bulk ice density and does not account for the effect of crevasses lowering ice density near the grounding line. The effect of ice density on discharge is linear, so reducing ice density to, for example, 900 kg m-3 would reduce our grounding line discharge estimate by approximately 2 %."*.

Although it is easy to implement various bulk density values, there is not an obvious way to determine an appropriate value for ice density around the perimeter of Antarctica. The reviewer is correct that the presence of crevasses would lower the bulk density compared to an idealistic case of an ice slab entirely enclosing the crevasses. However, we use a 200x200 surface elevation product derived from 2 m elevation measurements, which does not necessarily provide the elevation along the top of the crevasses. We do not think, therefore, that it would be appropriate to attempt to determine an alternative bulk density value, because all such values would be little better than guesses. Overall, we think that clarifying our assumption and providing an example of how much the discharge estimate would change when using a more conservative bulk density value is a comprehensive response to the reviewer's comment, whilst also keeping our method consistent with the established literature.

6. Temporal change statements – In multiple places, the authors state difference between July 1996 and January 2024, or simply 1996 and January 2024. But they also highlight an annual cycle in more recent data. It seems wise to limit temporal change statements to the same month, i.e. July-July or January-January, to avoid biasing multi-annual changes with a seasonal aliasing. [they say these statements still remain, even at abstract level].

Done. All statements relating to changes in discharge now compare like-for-like – e.g. comparisons between 1996 and 2024 now use the annually-averaged discharge in 2024. In a few places, we describe the temporal coverage of our dataset (1996 to November 2024).

7. Rignot Comparison – [I feel this could still be improved with tabulated basin-scale comparison plus difference map. A XY scatter plot is not the most informative presentation. I don't see how the argument that R19 didn't have to do this is relevant.]

Done. In the revised manuscript, we have provided a tabulated basin-scale comparison between our dataset and other studies, over equivalent time periods. We already provide difference maps in Figure 14, which address this part of the reviewer's comment.

8. Partitioning – L40 states that the input-output method can yield direct partitioning of mass changes between SMB and discharge. This is technically incorrect. If you only know the SMB and D today, you need to make an

assumption about the steady-state SMD and D, in order to partition mass loss today into both those terms. Their ideas around FrankenBedAdj show that the authors are close to estimating a long-term steady-state discharge, but not quite yet.

Done. Our manuscript already explicitly states the assumption that the reviewer requests on line 37: "when combined with an estimate of steady-state SMB and discharge". We have removed FrankenBedAdj ($BM+HF14_{adj}$) from the revised manuscript, so the second part of the reviewer's comment relating to that is not relevant to this new version of the manscript.

9. Changes through space/time – It would be help to see total 1996-2023/24 change in velocity plotted with change in thickness along the entire perimeter. Ideally, it would nice to identify where along the perimeter the competing effects of thinning and acceleration result in net discharge decrease versus increase.

Comment. On this point we stand by our original response, that this type of scientific analysis is out of scope for ESSD because ESSD's Aims & Scope (https://www.earth-system-science-data.net/about/aims_and_scope.html) states "any interpretation of data is outside the scope of regular articles". We do, however, provide summaries of the contribution of thickness changes to discharge changes (Figure 11 for the whole ice sheet, and our supplementary figures provide this information for all Antarctic drainage basins).