Dear Editor,

Firstly, we would like to thank the editor for their patience whilst we revised the manuscript in response to the reviewer comments. We received a mixed set of reviews in this iteration. One reviewer (R2) was happy to accept the manuscript as is, one (R3) provided a set of clear and constructive revisions that we have largely implemented, and another (R1) recommended rejection. It has taken us a long time to respond partly due to personal circumstances of the lead author and because of the seriousness of the comments from R1.

One of the main concerns raised by the reviewers was the disagreement between our dataset and one existing dataset (Rignot et al., 2019; R19). Indeed, Reviewer 1 stated that "comparison with other published work [was] almost avoided in this paper" – this statement is evidently incorrect because we had an entire section and figure devoted to comparing our estimates primarily with R19. These differences remain in the revised manuscript. In response to the reviewer comments and your own concerns, we have expanded this section, which now accounts for measurement uncertainty, incorporates other discharge estimates, accounts for errors and attempts to quantify the source of the differences between our estimate and that of R19. We think that there is now more than enough detail for the reader to understand the differences between discharge estimates and to evaluate the utility of our new dataset. While it should not be mandatory for all papers to perform formal intercomparisons with all previously published work (IMBIE, ISMIP6, GlaMBIE, etc have shown this is a substantial program of work in its own right), we think it is worth noting that R19 did no such comparison with the few other estimates available at the time R19 was published.

It is critical to have discharge estimates at the drainage basin scale, and as such reconciling differences at this scale is also important. We have attempted to calculate the contributions to the differences between our discharge estimates and those of R19 at the basin scale. The contributions are mostly from different choices of ice thickness and bed topography data and from using different ice velocity sources. Some details remain hard to quantify without access to the code or underlying data from R19 – but there are notable differences between their gap filling routine and ours, which will contribute to the discharge differences. We have also included a new appendix to describe and quantify the differences in ice velocity between data sources, which will also contribute to differences in discharge between this study and R19. There are several basins where R19 did not use ice thickness or velocity to estimate discharge – instead they used surface mass balance from one regional climate model under the assumption that the steady-state discharge must balance the basin-integrated SMB in order to maintain mass balance. In these basins, we use three regional climate models to estimate what the uncertainty in that flux is – in most of those basins, this uncertainty is larger than the difference between our discharge and that of R19.

Both Reviewer 1 and Reviewer 3 felt that our choice of bed datasets was not what they would have used. Reviewer 3 felt that BedMachine did not add much info beyond BM+HF14 – this is a fair point in most cases, so we have removed it from figures where these datasets were identical. We opted to retain it as an output, because we think it is useful to show the impact of including the Huss and Farinotti (2014) dataset on the Peninsula. Reviewer 1 felt than BedMap-2 should not be used at all because it was not designed for mass conservation. This is a surprising comment because R19 use BedMap-2 in their discharge dataset. Arguably, BedMap2 is now used less by the community than BedMachine, so could potentially be excluded, but we think the comparison between the two may be interesting to some readers and some members of the community may benefit from the option of BedMap2, depending on their research question. Ultimately, we feel that some of the value in our paper is that we have openly and transparently set out what the impact of using different input datasets is on the discharge estimate.

Reviewer 1 raised a number of other concerns, mostly shared privately. Some of these concerns were clear and constructive, which we have taken on board and revised our manuscript accordingly or have attempted to justify our choice. For example, the reviewer disagreed with our choice to use gridded bed elevation datasets extracted along arbitrary flux gates, rather than 'raw' Operation IceBridge data extracted along flight lines. Although it may be ideal to do the latter, we argued that the gridded datasets incorporate the 'raw' data sufficiently well, and account for distance to flight lines in their errors, that the ease-of-use of the gridded data outweighs the potentially small benefit conferred by using the flight line data directly.

Reviewer 1 was also strongly opposed to our use of a tuned bed topography dataset (previously FrankenBedAdj, now $BM+HF14_{Adj}$); specifically, they did not agree with the use of altimetry-derived mass change data to determine the tuning. Rates of mass change averaged over 30-years are similar between most altimetry and GRACE estimates, (they have been reconciled within errors in the latest IMBIE), except in mountainous areas like the Peninsula, so using another source of mass change data for the tuning would not greatly affect the tuning results. Reviewer 3 also felt that this tuning warranted more explanation, validation and discussion than was possible in this journal. Our preference is to retain this tuned dataset for the community to use, or not, as they see fit. In the revised manuscript, we have even more explicitly stated the assumptions and aims of the tuning, to avoid inadvertently misleading readers – it is not intended to represent a new bed product. It might prove to be a useful standalone discharge estimate for intercomparison purposes. As a minimum, it is a useful experiment to quantify how much the ice thickness would need to change in order to reproduce the observed rate of mass change. We argue that this is useful because it provides information that will help future researchers to reconcile mass balance estimates, when combined with information about SMB, firn compaction, elevation change and ice velocity uncertainties in each basin.

Reviewer 1 also disagreed with our use of multiple data sources. They are not clear exactly what data sets they are referring to. There are indeed large differences between each of the velocity datasets in some locations, and it would be worth rigorously evaluating, intercomparing and understanding these differences in future studies. In the revised manuscript we have improved our approach to aligning the velocity datasets, which has reduced our total grounding line discharge compared to the previous submission. We have also added a new appendix to illustrate the velocity differences and show our alignment approach. In the same vein, Reviewer 1 strongly disagreed with our use of multiple regional climate models (RCMs) everywhere, arguing that an arbitrary combination of RACMO2.3p1 and p2 (split spatially between some basins) is preferable. Firstly, for the discharge estimate, the RCMs are only used to estimate the gate-to-grounding line surface mass change. This is a relatively small correction that is not always used (Mankoff et al., 2019, 2020; Rignot et al., 2019) and is only very briefly described elsewhere (Mouginot et al., 2014). For mass balance calculations, the discussions presented in Mottram et al. (2021) show that the choice of RCM is more important. Mottram et al. (2021) demonstrated large differences between RCMs but no significant differences in performance compared to in-situ SMB measurements and therefore suggested that an average of all RCMs may be the best approach for approximating the true SMB. We think it is worth noting that the ice sheet total and basin scale SMB estimates have changed changed, sometimes substantially, every time there is a new version of each RCM, so it is unreasonable for any reviewer to insist that one particular version of a single RCM is used. We know from communications with the RCM community that Reviewer 1's preferred SMB model will soon be superseded by a new version (with ~200 Gt yr^{-1} greater Antarctic SMB), which is in the process of being finalised and published.

Reviewer 1 argues that "information is plentiful", when referring to the number of grounding line discharge estimates for Antarctica. Reviewer 1 is the provider of the only discharge time-series for Antarctica, which is now seven years out of date. Only three other Antarctic-wide discharge estimates are available to our knowledge: one (Depoorter et al., 2013) provides a single number for the ice sheet, based on scaling the discharge from ice shelves; another (Gardner et al., 2018) provides two estimates (2008 and 2015) each for Antarctica, East Antarctica, West Antarctica and the Peninsula; the third (Miles et al., 2022) provides only discharge change and the data are not tabulated. Therefore, for one of the most important metrics of ice mass flow globally, the scientific community only has one excel spreadsheet containing annual measurements up to 2017 and two tables in manuscripts containing a total of 9 measurements. Given that these measurements disagree everywhere except West Antarctica, there is clearly a need for more discharge datasets in the community and open discussion on how the measurements should be calculated. We think it is worth noting that the altimetry and gravimetry mass balance communities are substantially larger, with 7 and 15 mass balance estimates submitted to the community intercomparison exercise (IMBIE, 2018) from these techniques respectively. It is unfortunately necessary to question the motivation, and fair, impartial and objective judgement, of the lead author of the only available Antarctic discharge time-series when they appear to be seeking to limit the publication of any new discharge estimates.

Reviewer 1 generally seems to oppose our approach of providing multiple discharge estimates, stating that instead we should aim to provide the best possible estimate. This point is well taken and is certainly a desirable goal, but we do not think that there is sufficient validation data to determine what the best dataset is. There are lots of options, of which we have transparently explored many, but there is often not a convincing argument for using one combination of options as opposed to another. In the revised manuscript, we more clearly present BM+HF14 as our favoured bed/discharge dataset, but we don't see the value in removing the other datasets. The comparisons alone provide information that is not readily available elsewhere, and we feel the scientific community will value this information.

In summary, grounding line discharge is an important metric that can be used as part of investigations that can improve our understanding of recent drivers of ice flow variability and that can be used to estimate ice sheet mass change. Despite its importance, few estimates of grounding line discharge exist; those that do exist are now several years out of date and are, at best, annually resolved and do not resolve individual glaciers on the Peninsula. In addition, those estimates appear to disagree by an amount that can affect our conclusions regarding the direction of ice sheet mass change. In the revised manuscript, we have demonstrated that our discharge dataset performs at least as well as other datasets whilst providing additional benefits of being up-to-date and capturing more and smaller basins, especially on the Peninsula. It is provided in accessible formats and has clearly defined errors – admittedly, these should not be defining features, but that reflects the standard of existing Antarctic discharge datasets. There is clearly a desire in the scientific community for this dataset – our Zenodo repository has been downloaded over 1000 times – and we feel confident that our detailed revisions now mean that the dataset and manuscript are up to the high standards required by the community and ESSD.

Yours Sincerely,

Benjamin Davison

On behalf of the authors:

Benjamin J. Davison, Anna E. Hogg, Thomas Slater, Richard Rigby and Nicolaj Hansen