We would like to thank the three reviewers for reviewing our manuscript. One reviewer accepted the manuscript as is, one reviewer provided several constructive comments, the majority of which we have implemented in this revision, and one reviewer recommended rejection. In this revision, we have (1) included substantially more comparison with existing estimates of Antarctic ice sheet discharge; (2) renamed 'FrankenBed' to BM+HF14; (3) clarified a number of assumptions pointed out by reviewer 3; and (4) clarified the wording throughout the manuscript, especially where there was misunderstanding in the reviews. We have provided detailed responses to each of the reviewers' comments below.

Reviewer 1

1	In this study, the authors force the grounding line thickness to be such that the resulting discharge will produce a mass loss similar to that recorded by altimetry missions from ERS-1 to ICESAT (1994-2021), radar and laser mixed, effectively forcing the mass balance solution to agree with altimetry.	This was one relatively small component of our study, which we did to explore the contributions to uncertainty in mass balance.
2	Given that the ice thickness has been measured around a large share of Antarctica, especially for the largest glaciers most significant contributors to sea level rise, by Operation IceBridge and other airborne ventures, it seems to difficult to justify why these data should be migrated other than to force an agreement with altimetry at all cost. Since the migration is larger than the inherent uncertainty of these data, the adjustment is impossible to justify.	This point conflates some independent aspects of our processing. We migrated our initial flux to create 16 regularly spaced flux gates. This is similar to the approach that Mouginot et al. (2014) applied in the ASE. As far as we are aware, only one study (Gardner et al., 2018) uses a flux gate that is specifically placed to follow e.g. Operation IceBridge flight lines. In contrast, Rignot et al. (2019) appear to use the grounding line as the flux gate and Mankoff et al. (2020; Greenland) use flux gates some distance upstream of the grounding line. It's not clear that using OIB data directly would be better than using gridded products which all assimilate the OIB data. To be clear, the migration is not to force agreement with altimetry. As described above, we do include an experiment in which we query what the thickness would need to be in order to reproduce the long-term rate of mass change from altimetry, given the observed velocity and modelled SMB.
3	In addition, the authors employ 3 sets of SMB models: 1) RACMO, 2) MAR and 3) HIRHAM5. These models have various levels of accuracy and precision. Given that RACMO2.3p2 and p1 have a published record of being far superior to the others, it is not clear why the mixing of three SMB models would produce a better assessment of mass balance.	Our study focuses on producing grounding line discharge estimates, for which the SMB (between each flux gate and the grounding line) is a small contribution. The choice of SMB model does very strongly affect the resultant mass balance, which we include as an additional form of validation and for completeness. Contrary to the reviewer's claim, it does not seem that there is consensus in the RCM community as to which of the various RCMs is superior (see e.g. https://doi.org/10.5194/tc-15-3751-2021) and they are of course in constant development.

Reviewer 3

1	Renaming ”FrankenBed” – I think the abbreviation “BedMachine+HF14” would be a more appropriate way to denote the bed product is strongly similar to BedMachine (expect Antarctic Peninsula). At present, the FrankenBed name obscures/rebrands the BedMachine product.	Agreed and done.
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.	We have removed BedMachine only from all of the Antarctic-wide figures, but we think the comparison on the Peninsula is useful information. It illustrates the impact of our choice to use HF14.
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).	We agree that this is an assumption in our processing. We do already state this at the end of Section 2.3: “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.” But we now explicitly state this again in our description of equation 1: “where *V* is the **depth-averaged** gate-normal ice velocity **(assumed to be equal to the 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.	We agree that creating a **gridded** bed product in the style of “FrankenBedAdj” (now $BM+HF14_{Adj}$) would require more documentation, perhaps in the form of a separate publication. However, we only seek to scale BM+HF14 across each flux gate within each of the MEaSUREs regional basins. This is a much smaller undertaking than was BedMachine. It is not intended as a replacement for or improvement over BedMachine (as stated in Section 4.2 and Appendix A). We have added further clarification of the purpose of $BM+HF14_{Adj}$ in section 2.1 when it is first described: “We emphasise that $BM+HF14_{Adj}$ is calculated only across the flux gates (rather than gridded) and is not intended to be a new

		bed product. Instead, it merely provides an indication of what the thickness would need to be to reproduce observed rates of mass change, given the observed ice velocity and modelled surface mass balance in each basin. Differences between BM+HF14 and BM+$HF14_{Adj}$ are therefore indicative of bed elevation uncertainties, SMB uncertainties, mass change uncertainties and ice velocity uncertainties". We think this is a useful exercise that provides new insights into sources of mass change uncertainty that are not available only by using existing bed products.
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?	We agree that 917 kg m^{-3} is an upper limit for ice crossing the grounding line. However, to the best of our knowledge, several other major grounding line discharge studies for use an ice density of 917 kg m^{-3} (Rignot et al., 2019; Mankoff et al., 2020; Mouginot et al., 2019). To maintain comparability with these datasets, we think it would be best to retain the upper limit ice density. In the revised manuscript, we have stated that the use of 917 kg m^{-3} is an upper bound that neglects the effect of crevasses, and that discharge would scale with ice density.
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.	Agreed. We now ensure comparisons are done as the reviewer suggests throughout the manuscript (in places this entails taking an annual average of the monthly discharge estimates in the latter part of the timeseries).
7	Rignot Comparison – In Figure 13i, I see many large basins with differences of up to +/- 50%. It seems that the apparent agreement at ice-sheet scale is underlain by substantial spatially compensating differences. I think the reader would benefit from a more thorough discussion of these differences, at least identifying the main cause of difference between the opposing West and East Antarctic biases, and possibly also at the scale of larger glacier catchments/systems.	We agree that the differences between our discharge estimates and those of Rignot et al. (2019; henceforth R19) were emphasised but not sufficiently explored. We have made substantial changes to the text of section 4.1 exploring these differences and have added another figure to illustrate the contributions to the differences in each basin where they can be calculated. Although there are differences between our central discharge estimates and that of R19, our respective estimates generally often within error or disagree by

		a similar amount as do other studies. This is true at the ice sheet scale as well as for the majority of basins. There are 29 basins in which our discharge estimates do not agree within error with R19. R19 used SMB to estimate discharge for 15 of those – for 13 of those 15, the spread in SMB estimates between RACMO, HIRHAM and MAR is greater than the difference between our discharge and R19. For the remaining 14 where R19 actually used thickness and velocity to calculate discharge, we present the contributions from thickness and velocity – velocity is generally the dominant contributor and stems from (1) our use of multiple velocity datasets; (2) different filling approaches (linear in time here, vs linear in space or nearest in time, depending on the size of the gap, in R19), and; (3) R19s use of a scaled reference flux. The total difference between our estimates and R19 in those 14 basins that used thickness and velocity is 69 Gt/yr, the majority of which (59 Gt/yr) stems from 6 basins – Whillans, MacAyeal, Foundation, Evans, Crosson and Bindschadler ice stream. For those basins, we show in Appendix 3 that there is a large spread in velocity estimates from different velocity sources and that the coverage is low.
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.	We fully agree with this statement and have modified our wording accordingly.
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.	We agree that this could be an interesting analysis, and it would be possible with the datasets we hope to release. Indeed, we provide that information already, though in a less succinct form, in our summary figures. However, we think that this type of analysis goes beyond the type of dataset description suitable for ESSD. We think it is sufficient to summarise the effect of ice thickness changes on grounding line discharge as presented in Section 3.3 and Figure 11.