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Comment on essd-2021-131

Andrew Shepherd (Referee)

Referee comment on "Greenland ice sheet mass balance from 1840 through next week" by Kenneth D. Mankoff et al., Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2021-131-RC2, 2021

Greenland ice sheet mass balance from 1840 through next week, Mankoff et al

This paper describes an updated mass budget (input output method) for the Greenland Ice Sheet spanning the period 1840 to the present day. From 1986 onwards, the mass budget components are derived from 3 regional climate models, satellite-based estimates of ice discharge, and estimates of basal melting. Prior to 1986, all components are derived from a reconstruction of the surface mass balance and runoff. It is a valuable dataset and deserves to be published.

I have the following 6 main concerns.

- This study introduces a new term – marine mass balance, which is the sum of solid ice discharge and submarine ice melting. I am not convinced that the two terms should be added in this way as one is a lateral flux of ice and the other is a vertical flux of water, and they are in any case representative of different process. It seems to me that they should be kept separate.

- The submarine melting term is required because the flux gate is positioned in places over floating ice. I question why this is done, given that it adds complexity and uncertainty to the result, and it also means that the result is no longer pertaining to the

grounded ice sheet. My recollection is that this term was erroneously included in one mass budget assessment for the Antarctic ice sheet and has since been removed (Rignot et al., 2011). Why not position the flux gate so that no submarine melting is required? I would also like to see more discussion on this submarine melting term and some presentation of the data to illustrate its magnitude and spatial distribution.

- It is acknowledged that there are inconsistencies in the domains of the various input datasets at the ice sheet margin, and that this is a location where large signals are present. Some effort is made to align the dataset, but I could not follow the explanation in full. In any case, I would like to see some exploration of the sensitivity of the results to the location of the boundary to be sure that it is not erroneously positioned. This is somehow related to the need to include a submarine melting term; at the location between grounded and floating ice there are typically very large variations in surface and basal ice melting and I am not convinced they can be reliably separated. It would be interesting to see how the various mass balance terms vary as a function of domain size while for example eroding the domain by a pixel at a time.

- Prior to 1986, the ice sheet mass balance is derived form a scaled reconstruction of surface mass balance and an empirical model of the ice discharge. The SMB is scaled to fit the observations derived from regional climate models post 1986. The ice discharge is modelled as a linear function of the reconstructed runoff based on a fit to data recorded between 2000 and 2012. This interval is widely recognized to be a highly anomalous period in the Greenland Ice Sheet's history (see e.g. Boers & Rypdale, 2021), and I question the validity of extrapolating a relationship between runoff and discharge obtained from this period to other times. While I recognize that the method was introduced by other authors in 2013, this was prior to the anomaly was established and I believe the validity of the approach needs to be revisited and reestablished. What evidence is there that runoff and discharge were correlated prior to 2000? What happens if a different period is chosen?

- I have some concerns related to uncertainties as I could not find answers in the text. When the reconstructed SMB is scaled to the regional climate model data, is the dispersion added as an additional uncertainty to the scaled reconstruction? Similarly, when the ice discharge is modelled from the reconstructed runoff, is the dispersion added as an additional uncertainty? Finally, it is assumed that the basal and surface mass balance uncertainties do not have time-invariant components, but I find this difficult to believe. What evidence is there for this?

- I would like to see each of the terms produced in this paper plotted and tabulated, i.e. SMB components (at least runoff and snowfall), ice discharge, submarine melting, basal melting.

I have the following minor concerns:

L3: be clear about the period of data v models

L5: not sure you need to contrast performance to other mass balance estimates

L6: and in any case which elements update daily?

L9: define “general agreement”

L10: say a little more about which other products are referred to here

L14: im not sure this statement is correct as there have been a large number of studies reporting trends in the ice sheet mass loss. We do know where, when, how, and why the ice sheet has changed in mass. I think it would be a good idea to include a short summary here since it is absent elswehere in the introduction.

L21: im not sure "no information" is correct. Locating the imbalance in space is informative

L22: I don't think defintion is the correct word here, perhaps drop "definition of"

L24: integrated, not reduced

L24: suggest “typically” rather than “still”

L26: i think you need to be careful about the sampling afforded by the satellites as compared to datasets made publicly available

L26: this is not true; all methods give some information on processes its just integrated in the case of the raw VC and GMB methods but even in this case is informative because of the localisation. But in any case the methods are now routinely complemented also with RCM data to partition into dynamics and SMB as is done with IO. So I think the wording should be tightened up here. The IOM data are of clear value and its not neccessary to diminish the value of other methods

L32: Suggest “Inputs” not “IO inputs”

L33: Suggest “Outputs” not “IO outputs”

L34: is it really annual for each component? I think you are oversampling the discharge for example and undersampling the RCM data

L36: probably worth explaining to the reader what you mean here (presumably that the data will continue to be updated independent of this paper)

L37: suggest move the “Terminology” section to a glossary

L38: “Product description” seems more like text for the acknowledgements or a footnote of some kind. In any case it should presumably come after the methods and results are reported

L252: its only future relative to this dataset, its present relative to all the others

L280: suggest “start” instead of “top”

L294: suggest add e.g. “relative to recent data” after “slope” to clarify

L307: do you mean data products not aligned, or their domains? Also, aligned in space (not time)

L311: are you extrapolating data or interpolating it? I can't really tell. And are you resampling to a common grid?

L350: what does large uncertainty mean?

L413: not true, imbie data are equally weighting across all techniques present

References cited

Rignot, E., Velicogna, I., van den Broeke, M. R., Monaghan, A., and Lenaerts, J. T. M. (2011), Acceleration of the contribution of the Greenland and Antarctic ice sheets to sea level rise, Geophys. Res. Lett., 38, L05503, doi:10.1029/2011GL046583.

Niklas Boers, Martin Rypdal, Critical slowing down suggests that the western Greenland Ice Sheet is close to a tipping point, Proceedings of the National Academy of Sciences May 2021, 118 (21) e2024192118; DOI: 10.1073/pnas.2024192118