Summary

The manuscript of Rückamp et al. (2025) uses a full-stokes ice dynamics model together with an energy-balance model to simulate the glacier evolution from 1997 to 2100 for two well-known and well-studied glaciers in Central Europe, the Greater Aletsch Glacier in the Western Alps and the Hintereisferner in the Eastern Alps. By using observed ice velocities which were, in case of HEF, manually extracted just for this study, the basal friction parameter and rheology parameter were constrained. Observed MB gradients were used to tune the SEB albedo parameters. In general, they find that HEF is disappearing no matter which scenario is chosen while Aletsch glacier may survive for lower emission scenarios. These outcomes were expected, as they were equally found in other studies. However, the methodological approaches of combining ISSM together with the EB model of Evatt et al. (2015) by including the observed ice velocity and MB profiles to tune some free parameters are novel.

In summary, this manuscript has methodological novelty and directly compares two well-observed glaciers by using observations, model and calibration approaches that haven't been used before. Overall, the topic is interesting, the methods are understandable and the results are described in-depth. It is also great that all data and code will be made available (if I understood it correctly). However, there are a few major aspects that need to be revised, such as some description within the manuscript, an uncertainty analysis of your projections, the comparison to projections from other glacier models, the generalisability of the outcomes to all glaciers in Central Europe and the greater implications of the study. Therefore I believe that the study needs some major revisions before it is ready for publication. I don't expect that all of my comments are directly addressed, but I hope that some kind of uncertainty analysis can still be done together with clarifying some of the analysis and interpretation.

Thanks a lot for this nice manuscript. Please don't feel overwhelmed by my many comments, just try to consider my suggestions when somehow possible.

Major Comments

1. **Manuscript in general:**

- Overall the manuscript is quite long. Some "figure descriptions" can be condensed as these are already visible in the figures. For example, by always separating HEF from GAG, there is quite a lot of repetition in the manuscript, and it makes it more difficult to actually compare the behaviour of the two glaciers which is one of the actual motivations of the paper, or not? Would it be possible to reduce the amount of text by directly comparing the two glaciers? In addition it seems like sometimes the same things are described in different ways (for HEF, the "mostly gone" definition was introduced, while for GAG, you write again "... ice volumes drops below 10%" …
- I found a few statements that are without references or proofs. Please double-check these (see my line-by-line comments). Some passages within the discussion and conclusion could be a bit more concrete and related to your specific study.
- See more suggestions in the line-by-line comments.

2. **Added value of this study and comparability to other glacier model simulations**

- What was the goal of this study? Creating the "most robust" glacier projections of GAG and HEF by using a full-stokes model together with an EB model? Or was it to understand if it results in other estimates than existing glacier models? On line 75, you write that the aim is to "investigate the potential variability of glacier responses in the frame of physical process representation". I did not feel like this was the aim of your paper. You analysed two glaciers (see 'extrapolation comment' below) and did not look into the uncertainties within your approach (see 'uncertainty comment').
- In Sect. 7.2, you do a comparison to other glacier models which is great. Though, it doesn't seem like your study's goal was to directly compare results to other glacier models. You used other weather-station corrected ERA5 climate data, other climate models (with/without EURO-CORDEX), calibration data, initial volumes, RGI versions, (see line by line comments) which makes comparisons very difficult to interpret.
- Conclusion l. 528 onwards: I completely agree that it is difficult to compare your estimates to other model estimates. But this entire paragraph somehow also describes the limitation of your study, so I am wondering why you did not choose to do standardised tests in your study, e.g., by comparing directly SIA vs full-stokes. Of course, this is difficult to do, as SIA flowline-models are not really expected to match ice velocities. But, at least showing a range of different full-stokes modelled outcomes (e.g. in terms of equifinality and/or observation uncertainties) may help to understand how variable projections can be within the full-stokes approach.
- What you could for example do is to compare your glacier volume estimates at the RGI year to the ice thickness community estimate from Farinotti et al. (2019). As many large-scale glacier modelling studies use that estimate for their initialisation, this comparison would be nice to have. Another example is that you use RGI7, while most (all) existing modelling studies in Central Europe use RGI6. How different is RGI7 to RGI6 on these two glaciers? All these aspects, together with trying to compare projections from exactly the same climate models (maybe just use one climate model where data exists for all studies) and/or doing an internal uncertainty assessment of your model (see uncertainty comment below) would be necessary to understand a bit better from where the differences come from.
- If a comparison of the exact same climate models is not possible, then you may reduce the "interpretation" within the discussion Sect. 7.2, and just show the figure with the potential variability of outcomes. In the following are some important aspects for Sect. 7.2 to consider.
    - You write that under RCP 8.5, JH19 results in more mass loss, while under RCP 2.6 it results in less mass loss. Then, you later argue that this is because JH19 used a TI-model which overestimates melt. Isn't that contradictory? Why is JH19 not resulting in more melt as well for RCP 2.6? If you don't look into the full spectrum of possible MB parameters (equifinality), these statements are very difficult to validate. Specifically you mention in the next paragraph that "Under RCP 2.6, GloGEMow predicts a volume reduction of 57.6% for GAG, aligning somewhat more closely with our forecast than the JH19 study; likewise, for RCP 8.5, GloGEMow projects a 89.4% volume decline, which is close to our estimate than the JH19 study." Both JH19 and GloGEMflow (Z19) are temperature-index models, but are apparently more different compared to your study and Z19. For me, this shows that other

aspects like equifinality or downscaling choices may explain these differences and not specifically the choice of TI-model vs EB model.
    - What I find interesting is for example that OGGM projects regrowth for GAG under SSP1-2.6, but your study did not. However, as you are likely not comparing the exact same climate models, it is very difficult to interpret if this comes from the SIA vs full-stokes choice, the different mass-balance model, climate downscaling choice or actually from the different climate model ensemble where the one shows some regional cooling while the other does not. Is it possible to look into this further?
    - From where do you have the glacier model evolution data of GAG for the RCP 8.5 / RCP 2.6 scenarios of OGGM. Are these really available from Schuster et al. (2023). I couldn't find them there. Or did you use the OGGM standard projections? I assume you show the ensemble median over the different five climate models and over the various calibration approaches and TI model choices? If you can compare the exact same climate models: Is the variability of ensemble members from Schuster et al. (2023) similarly large as the differences between your study and Schuster et al. (2023)?

3. **Missing uncertainty estimates:**

- In the abstract, you claim that you give a well-constrained estimate of HEF and GAG projections that complements large-scale modelling efforts. Although you do use more complex and physical models (full-stokes ice dynamics, EB SMB modelling), I am wondering about the uncertainties in your input data and the equifinality of the additionally introduced free parameters. In my opinion, analysing whether different parameter combinations within the "input uncertainty and equifinality" space affect the projections would be essential to understand how well-constrained your projections are. You mention some of these uncertainties qualitatively in the discussion, but you haven't done any quantitative uncertainty assessment yourself. Doing this is difficult, but at least a rough analysis with multiple model estimates is necessary in my opinion.
    - For example, aren't there any other parameter combinations that result in a similar performance as the chosen one for the albedo calibration? If yes, how does that equifinality influence the validation and projection results?
    - By using an EB instead of a temperature-index model, you also have an increased equifinality due to the large amount of introduced new free parameters that need to be downscaled (SW, LW radiation, temperature, surface wind speed, humidity, precipitation). You mention the downscaling uncertainties in the discussion (l.427 to l438) but do not really do anything about it. I am wondering if the quality of the data is sufficient to apply an EB model without accounting for its uncertainties. For example, on l.173, I was thinking: How certain are the radiation gradients? And, isn't it strange to use a precipitation gradient for HEF that was only used before for GAG?
    - The data assimilation does not at all account for the uncertainties in the ice velocity. How strongly can you trust your ice velocity observations? Can the velocity extraction approach similarly be applied to other glaciers? How different is this approach to e.g. Millan et al. 2022?

- Of course you can't check all of these aspects. I am just wondering if the glacier projections are sensitive to these assumptions? Are these choices less important than the choice of using SIA vs. full-Stokes or a TI-model instead of an EB model?

East-West comparison (Sect. 7.3)

- Isn't the longer lifetime of GAG also the higher elevation-area distribution (see Fig. 14)? Not just the larger elevation range? It seems like you partly mention that aspect in l. 509 onwards, but it would make sense to condense these things all together. Specifically, GAG has most of its area around 3500 meters, while HEF has it more around 3000 meters.
- These area-elevation distributions may also explain why the MB gradient is difficult to match at the upper part of HEF (Fig. 6): there is actually not so much area at the highest elevation-bands, which is quite different to GAG. Does this specific shape of the area-elevation distribution also explain parts of the response differences of the two glaciers.
- What is the influence of the glacier slope? Is the current HEF steeper than GAG, and how will that evolve in the future? The different glacier geometries could e.g. be mentioned in l. 504 - 508.

4. **Extrapolation of HEF and GAG results to all over Central Europe**:

- I find it difficult to extrapolate results from HEF and GAG to the entire Alps. You could check how representative HEF and GAG are by analysing their characteristics compared to all other glaciers in the Alps, and/or compare per-glacier model estimates from regional-scale glacier models of HEF/GAG to regional-scale estimates. Maybe GAG is "representative" in some sense for the entire glacier mass evolution as it represents a relatively large fraction of the entire glacier mass in Central Europe, but this is not mentioned/analysed here. In addition, the "representativeness" also changes over time.
- l. 502: "In the worst scenarios, most glaciers also disappear at the end of the 21st century". To my knowledge, almost all (or even all) published studies over Central Europe write about glacier mass evolution, not number of glaciers. From which source do you have this estimate?
- Conclusion on l. 524: "Our findings suggest that glaciers in the eastern European Alps are likely to diminish by the mid-21st century, and only larger glaciers with higher elevation ranges in the western European Alps will remain until the end of the century.": Can your study really prove that sufficiently? Please consider removing or rephrasing.
- I am missing a little bit the discussion that your approach is only possible for these two glaciers because of the available data (good-quality remotely-sensed ice velocities and MB profile). Specifically because you do not account for the uncertainties. Maybe add a paragraph on the following aspects: Could your specific model combination approach (using ISSM and a SEB model) also be used for follow-up studies on the two glaciers or on other glaciers? How can your study be used to improve/validate regional-scale glacier modelling/projections?

5. **Novelty of the data assimilation approach** (l. 78):

   I am not an expert in data assimilation, but isn't e.g. IGM (Jouvet et al. (2023) or

Cook et al. (2023)), Jouvet et al. (2019), also using observed ice velocities for the inversion? Or is it the specific approach with ISSM that is novel? Please specify.

6. **Climate model choice and scenario descriptions**:
   You compare RCP scenarios to SSPs. You show that RCP2.6 & RCP 8.5 result in less warming over Europe than SSP1-2.6 & SSP5-8.5, and with that in less glacier volume loss. Is the reason for these differences the EURO-CORDEX downscaling of the RCP scenarios, or do you see the same effect globally, i.e. does the chosen climate model ensemble result in less global warming for RCP2.6/RCP8.5 vs. the SSP scenarios? That means, please mention the median global warming from the different scenarios for your chosen climate model ensemble (with that you may also rephrase l.421 to l.426 and other related line-by-line comments).

7. **EB model calibration approach with calibration vs validation data**
   (Table 1 & Fig. 7, Sect. 5.2 & 5.3):

- Do I understand correctly that you use the average observed SMB gradient from each year (i.e., n=13, 8 gradient observations). That means you do not use the entire MB profiles, but the timeseries of MB gradients and not just the average MB gradient over the time period (i.e., one observation), correctly? I think it would be important to clarify a bit more this calibration procedure within Sect. 5.2.
- I am specifically asking because I was first astonished to see how well the cumulative MB is matched. If you used the entire SMB profile timeseries, the cumulative MB over that same time period would not be anymore a true "validation" as it is somehow an integrated value of the MB profile? But, as you wrote in l. 340 to l. 342 that you used independent model fields, I expect, you only used the gradients (such as e.g. 0.007 m w.e. m^{-1}) without an information of e.g. the intercept. These MB gradients, are, I guess, sufficiently “independent” enough from the cumulative MB. But, are they also completely independent from the elevation change observations?
- Related to that (specifically on l. 322): How much does it make sense to match the gradient over the entire elevation-area distribution, if there is at the upper part only little glacier area (in case of HEF)?

Minor Comments

- maybe replace always ice volume/ice area to glacier volume/area (all over the text). At the moment it is a mix of both.
- In all maps (Fig. 5, 7, 8; always a,b), it would be quite interesting to see the differences between modelled estimates and observations. I guess this is not always possible, as the observations are partly only available on point scales, but at least where possible, this would be maybe great to at least have a look into instead of the modelled values. The scatterplots are relatively difficult to interpret due to the many overlapping dots.

Abstract:

l.2: maybe mention here also the used surface mass balance model complexity?

l.8: "comprehensive glaciological observations" → maybe clarify that you use in-situ MB gradients for calibration e.g. l. 11-12: maybe mention the median global warming in 2100 from these RCP/SSPs, otherwise it is unclear to understand. The choice of climate models strongly influences the actual warming within one RCP/SSP scenario ...

l. 15-16: "...; however, a rough model-intercomparison study reveals a large spread of volume projections with the different glacier models.": would be great to compare this to a model-internal projection spread if you can do this additional analysis (see major comments)

Introduction:
l.20-26 : references missing , a bit "vague" …

l.28-32 vs l.32-35: in the first sentence you mention different applications (not the models "per se"). Therefore, I would recommend to write "regional-scale/global-scale projections". You also present examples, therefore, it would be good to add "e.g." ... maybe also mention e.g. an IGM study (e.g., Cook et al., 2023) as that one emulates full-stokes.

l.32: Maybe remove "due to computational constraints", because there are other reasons such as the only poor velocity data available for the inversion on a regional to global scale compared to HEF and GAG which are one of the best monitored glaciers world-wide

l.35: "In most of these models": well, actually only in two of the four mentioned models. Please adapt and differentiate between retreat parameterizations (GloGEM, PyGEM) versus SIA (GloGEMFlow, OGGM).

l. 37: "Despite potential shortcomings ... ": when doing regional projections, there are always shortcomings, but these are often due to missing glacier observations, thus consider reformulating.

l. 38-39: RCP8.5/SSP5-8.5 is by today's standards a **very** high-emission scenario resulting in more than 4.0°C global warming in 2100. It could even be 4.5°C depending on the choice of climate models. I would suggest adding "very" to high-emission scenario.

l. 41: "or the low-emission scenarios RCP 2.6 and SSP1-2.6, which are basically in line with the global warming target of 1.5°C ... (UNFCCC, 2015)": Please double-check the resulting global warming of your ensemble of chosen climate models, I am pretty sure it is higher than 1.5°C.

l.44-47: some references?

l.58-62: references are missing (daily vs. monthly available in Schuster et al., 2023, bias-correction in Weathers et al. (2025)). What about the influence of equifinality (Rounce et al., 2020; Schuster et al., 2023)? These may also be very important sources of uncertainties.

l.75 & l.81: Related to one of the 'Major Comments': "To investigate the potential variability of glacier responses in the frame of physical process representation" --> you compare two different glaciers, how largely do they represent the variability of glacier responses within Central Europe? Maybe the "aim" should be reduced a little bit? Similarly you write later: "In order to capture and analyse regional differences of ice volume loss, the model is applied to two valley glaciers in the Alps". By just choosing two glaciers, you can't capture the regional differences. Please adapt.

l.77: SMB is not yet defined, I think.

Fig. 1: for GAG another time period is chosen than for HEF, isn't there a common time period? Also mention in the caption that these are the outlines from 1850 until 2015 (I guess they are not all coming from RGI7?). From which study do these outlines come from? At the dz legend: --> is the unit "m w.e. yr-1"?

Study sites

l. 90: "outlines from the last decades": maybe replace by "outlines from 1850 until 2015"

l. 94: "15km³, 900m": at which year? Also in 1999, if yes, consider joining the sentence together with the last sentence to clarify this. Maybe also mention the percentage of volume relative to the entire Central Europe (I think around 10%)?

l. 97: What means "relatively dry", "very large amounts of precipitation"? Any quantitative numbers?

l. 107: HEF has less than 5% of the glacier volume of GAG. Maybe it is worth mentioning that?. Eventually consider comparing these estimates to Farinotti et al. (2019) as many glacier evolution models use this estimate. What is the relative amount of glacier volume loss in a relatively recent but common time period for the two different glaciers?

l. 111: Can you be more concrete? How many years are available for each of the glaciers? This is hard to see from Fig. 2.

Figure 2: What means "b"? Maybe rather use SMB as in other parts (similar issue in e.g. Fig.7c). Annual SMB should be in mm w.e. yr-1 ? ... “since 1961”: from which year to which year do you show the MB estimates? Could it be of interest to estimate with a scatterplot and a spearman rank correlation coefficient how much the two correlate? Consider increasing the size of the figure and eventually even adding a scatterplot to the left of it showing on the x-axis GAG annual MB and on the y-axis HEF annual MB.

l.125-127: How representative are these precipitation data? It is quite far away from the actual glacier, but I know that there is no better possibility.

l. 131: How do you justify a precipitation gradient?

l. 130: "almost minor mass balance changes" -->when you look at the cumulative MB, it is still relatively considerable for HEF at least.

l.132: typo in "regionla"

l.154: Maybe also mention over which period you compare ERA5 with the raw GCM/RCM outputs for the “bias adjustment”? Another comment: Could you call the temperature lapse

rate/precipitation gradient the "statistical downscaling"? And the correction of the GCMs/RCMs then the bias adjustment? Just to better coincide with how the different steps are called in other studies.
l.158: As you use ERA5 corrected by weather stations as a dataset for the calibration, isn't another reason why you do the additional bias-adjustment for the ISIMIP GCMs the usage of weather-station corrected ERA5 instead of W5E5? Or not?

Figure 3: Over which area is this? Averaged over the two gridpoints of HEF and GAG? I guess it is over the Alps, as described in line 160. Please clarify the exact region over which you average. As precipitation is shown as a ratio, the unit should be removed. Consider writing "Precipitation ratio" or "Precipitation relative anomaly". Consider also adding the amount of ensemble members to the legend. I guess this is n=10, n=10, n=65, n=22.

l.163: It is great that you mention the regional temperature increases, but I think comparing the SSP1-2.6 regional warming to the global warming target of 1.5°C is a bit strange. As mentioned in one of the ‘Major comments’, can you also compute the global warming of this ensemble of GCMs/RCMs and compare that to the regional one?

l.167: I would suggest adding this information directly into the statistical downscaling paragraph of l. 130. When I first read l. 130, I thought that you did not correct the radiation terms, for example.

l. 212: I am not sure what the consequences are of setting the minimum ice thickness to 5m. Does that mean that an elevation where the glacier has an ice thickness below 5 m is considered as ice-free?

l.230 onwards: over which time step is the albedo updated? The SMB profiles are only available on an annual scale

l. 257: What does 0.05 and 0.25a mean, does 0.25a mean a geometry update four times a year?

Table 1: HEF SMB gradient: shouldn't the reference be WGMS (2024) as the elevation-band mass-balance estimates are published there, or not?

Fig. 5: (a) and (b): For GAG, it seems like low observed velocities are overestimated in the model. It would be nice to see the velocity differences directly on the map. Make sure to use a consistent description of the units (double-check with "The Cryosphere" regulations). At the moment, it is mixed ("m a-1" and "m/a"). "c" and "d": What do you represent with "vmod -vobs"? Is this the bias, and with that the mean over the differences? If yes, add an "average bar" on top of the equation. RMS or RMSE? Aren't you showing the root mean square error (RMSE) or do you really just show the magnitude of the observed velocity (RMS)? How many data points are used, please add the number. Do you believe that using an average over eight years for the initialisation in the year 2011 is problematic for a quickly melting glacier? Why did you choose a different velocity dataset than for Hintereisferner?

l. 322: “RMS” shouldn't that be RMSE (similar on other lines before, and in Fig. 7)

Fig. 6: Consider using the same color/line style scheme as in e.g. Fig. 7 for modelled vs observed estimates. That means maybe use always dashed lines for observations and solid lines for modelled estimates. At the moment, the green line in Fig. 6 is very thin and one has to compare that to the black line? What does the black line mean? Is that the average over the entire period? This is not very clear from the legend (I think the two "observed" labels have to be switched). You could also just have one legend outside of the figures with those labels that are the same and make sure to write out the accronyms. In (c) and (d), the lines seem to be even thinner than in (a) and (b), consider making them thicker. Also, the y-labels are different between (a) and (c). Can you maybe clarify in the figure or caption which period is used for "calibration" and which one for "validation"? Interestingly, I guess by coincidence, the "validation" period is matched better than the calibration period for both glaciers (in terms of RMSE and MSD).

l. 348: Could these localised modelled spots come from e.g. “shading” which is not accounted for in the gridded radiation data?

l. 366: The “mostly gone” definition and likely range used here seem to be very similar to what is used in the https://goodbye-glaciers.info/ project. Is that a coincidence? Although the “mostly gone” numbers for GAG and HEF are available from that project (based on three large-scale glacier models, https://goodbye-glaciers.info/glaciers/RGI60-11.01450.html, https://goodbye-glaciers.info/glaciers/RGI60-11.00897.html), it does probably not really make sense to compare the numbers directly as other climate models are used, SSP126 is between 1.5 and 2.7°C and you would need to recompute your values to be relative to 2020 instead of relative to 1997.

Table 2: please use consistent descriptions. In the text you always had median [q17 to q83], In the table it is a mix of round brackets with sometimes “-” and sometimes “->”. Why do you mention here the initial volume of the glaciers for two different time steps? The “gone” and “mostly gone” definitions are in both cases relative to 1997, or not?

Fig. 9&10: Why does ERA5 result in slightly less glacier volume loss than SSP1-2.6/SSP5-8.5 for both HEF and GAG? Is that because the bias-correction period is not the same as the ERA5 period? It seems like the RCP (EURO-CORDEX) scenarios match better to the ERA5 simulations for both glaciers.

Fig. 10&11: How do you define that a certain part of the glacier is gone? Is this the 5m threshold you mention somewhere in the methods section? Here it would be again great to have the global warming estimates for these two chosen scenarios.

l.409 : change “sime” to “some

l. 412: typo -> “methodological”

Fig. 14: It would be extremely helpful to use the same y-scales for the two glaciers. If it is really not possible, than mention this in the caption. The grey barplots are missing in the legend. Consider removing double legends, legends in (a) and (b) show the same, just show it in (a).

l. 511 to l. 515: You mention at the beginning and at the end the same statement, please reduce one of the two sentences.

l. 535: What do you mean with “GlacierMIP (2025)”? Maybe mention that the existing GlacierMIP studies (Hock 2019, Marzeion et al. 2020, Zekollari, Schuster et al. 2025) did not focus on that, but maybe this will actually happen in the next GlacierMIP round (i.e. GlacierMIP4).

l. 518: Did you check how representative HEF and GAG are in terms of valley glaciers in the European Alps? I would rather say, that these two glaciers are among the glaciers with a lot of observations which allows to apply the data assimilation approach using full-stokes and an EB model. But how similar are these glaciers to other glaciers in terms of their area-elevation distribution? Wouldn’t you need to check the RGI inventory and some statistics there to evaluate whether they are actually well representative? See related ‘Major Comment’.

l. 520: Here it would be great again to know the actual global warming of these scenarios.

References:

Farinotti, D. et al. A consensus estimate for the ice thickness distribution of all glaciers on Earth. *Nat. Geosci.* **12**, 168–173 (2019).

Weathers, M., Rounce, D. R., Fasullo, J., & Maussion, F. (2025). Evaluating the role of internal climate variability and bias adjustment methods on decadal glacier projections. *Earth's Future*, 13, e2024EF005624. https://doi.org/10.1029/2024EF005624

Rounce DR, Khurana T, Short MB, Hock R, Shean DE, Brinkerhoff DJ. Quantifying parameter uncertainty in a large-scale glacier evolution model using Bayesian inference: application to High Mountain Asia. *Journal of Glaciology*. 2020;66(256):175-187. doi:10.1017/jog.2019.91

Schuster L, Rounce DR, Maussion F. Glacier projections sensitivity to temperature-index model choices and calibration strategies. *Annals of Glaciology*. 2023;64(92):293-308. doi:10.1017/aog.2023.57

Jouvet G, Cordonnier G. Ice-flow model emulator based on physics-informed deep learning. *Journal of Glaciology*. 2023;69(278):1941-1955. doi:10.1017/jog.2023.73

Cook, S. J., Jouvet, G., Millan, R., Rabatel, A., Zekollari, H., & Dussaillant, I. (2023). Committed ice loss in the European Alps until 2050 using a deep-learning-aided 3D ice-flow model with data assimilation. *Geophysical Research Letters*, 50, e2023GL105029. https://doi.org/10.1029/2023GL105029

HOCK R, BLISS A, MARZEION B, et al. GlacierMIP – A model intercomparison of global-scale glacier mass-balance models and projections. *Journal of Glaciology*. 2019;65(251):453-467. doi:10.1017/jog.2019.22

Marzeion, B., Hock, R., Anderson, B., Bliss, A., Champollion, N., Fujita, K., et al. (2020). Partitioning the uncertainty of ensemble projections of global glacier mass change. *Earth's Future*. 8, e2019EF001470. https://doi.org/10.1029/2019EF001470

Zekollari, H., Schuster, L., Maussion, F., Hock, R., Marzeion, B., Rounce, D. R., ... & Sakai, A. (2025). Glacier preservation doubled by limiting warming to 1.5° C versus 2.7° C. *Science*, *388*(6750), 979-983.

Millan, R., Mouginot, J., Rabatel, A. *et al.* Ice velocity and thickness of the world’s glaciers. *Nat. Geosci.* **15**, 124–129 (2022). https://doi.org/10.1038/s41561-021-00885-z