**Summary**

This manuscript of Rückamp et al. (2025) presents projections of the future evolution of two Alpine glaciers (Great Aletsch Glacier and Hintereisferner) using a full-Stokes ice flow model coupled with a surface mass balance (SMB) scheme driven by the surface energy balance method. The model is initialized using observed velocity fields, and future projections are forced with bias-corrected climate data under different scenario's. Their projections suggest that Hintereisferner will vanish mid-century even under the low-emission scenario, while Aletsch will significantly shrink, possibly with a near-complete disappearance under high-emission scenarios by 2100 AD. The paper addresses an important scientific and societally relevant topic, as glacier-scale projections using full-Stokes models remain relatively rare, and the work provides valuable insight into the dynamic response of two iconic glaciers. The manuscript is generally well written (also some typo's remain and some things can be formulated better) and the results are of interest to both glaciological and the broader climate-impact communities.

The manuscript is scientifically valuable and should ultimately be suitable for publication in The Cryosphere. However, several scientific and some small linguistic issues here and there need to be addressed before publication. I recommend major revisions to (i) strengthen the treatment of the model forcing and bias-corrected climate data, and (ii) elaborate a bit more on model uncertainty related to the future projections. I suggest publication once the comments below have been addressed.

**Major comments**

1) Model forcing and bias-corrected climate data: The model forcing is described in Section 3, but I would like to have a more detailed explanation of some things that have not become very clear to me from the text.

   - Applied bias correction: In L124, you mention a "simple correction" has been applied to ERA5-Land data using observational temperature and precipitation data, and in L132 you say that you "shifted and scaled" the temperature and precipitation for the 1961-2023 period. Can you be more specific about what you did? Did you just scale the data so the mean of the overlapping period matches? Did you also adjust the variability (standard deviation)? Did you scale temperature and precipitation differently (i.e. additive for temperature, multiplicative for precipitation)? Did you apply the bias correction with reference data using a daily temporal resolution? Moreover, you mention other data as well for the SEB model (wind speed, radiation, humidity) but some explanation for a bias correction procedure for those are not mentioned for the “observational” (1961-2023) period. Did you scale these too for 1961-2023? I think the whole procedure can be further elaborated step by step into more detail to make it clearer for the reader. Moreover, applying a bias correction to the future climate data is a necessary step but it may result also in a change in the long-term trend of the data contained in the original output. Have you checked the preservation of the trends after bias correction?

- Compatibility of ERA5-Land for glacier-specific meteorology: You explained well that you use ERA5-Land for the bias correction of the meteorological forcing. Have you checked how well ERA5-Land and your observational data (from the meteo stations) agree during the overlapping periods, using some statistics? It is, for example, well-known that reanalysis data like ERA5-Land may not fully represent small-scale processes like glacier winds (which should be however the dominant wind regime over the glacier), but rather synoptic-scale wind patterns. Is this also applicable to your data? You briefly mention this in the text (in Section 7.1) in a qualitative way but I think that this warrants some further investigation or at least a statistical quantification of the level of agreement during the overlapping period.
- Model selection for future projections: It would be beneficial to have a list of used GCM/RCMs (for example as an Appendix) that are used for the future projections. I say this because a subset of the CMIP models are found to be ‘too hot’ and may lead to an overestimation of glacier mass loss. Hausfather et al. (2022) has suggested that models with a TCR that lies outside the ’likely’ range of 1.4-2.2 °C should be left out to avoid overestimation. You may look into this or at least mention it in the text.

2) Uncertainty of future model output: Some more incorporation and/or discussion of model output uncertainty by the variation of some key variables (for example related to the SEB/SMB) is warranted in my opinion. Also, the evolution of supraglacial debris is not included in the model, which I can agree on given the current minor debris extent on both glaciers, but its potential future effects should be more clearly acknowledged in the text.

- Model output uncertainty: In my opinion the discussion of the model uncertainty can be improved. I understand that most quantified model uncertainty in the Figs. 9 and 11 and in the uncertainty intervals comes from the different GCMs/RCMs and future climate scenario’s, but what about the uncertainty of the SMB profile that results from these climate data? This is only very briefly mentioned in section 7.1 but still important because the future evolution of the glaciers is in the end SMB-driven. For example, from my understanding, the gradients used in this study (L169-172) are taken over from other studies and not tested for validity for this study. How do they affect the SMB profile? In other words, how robust is the SMB model to internal parameter choices? This can be briefly discussed. I understand that it is computationally expensive to also include the sensitivity of various parameters to the SMB model in all calculations, but I think the manuscript would benefit from at least a sensitivity analysis of the SMB model (for example, with a Monte Carlo approach or a figure or table summarizing the sensitivity to major parameter uncertainties).

- Mentioning of supraglacial debris cover effects: Currently, debris cover effects are minor but certainly already present. A clear inversion of the SMB gradient in the lower parts of Aletsch is seen in your figure 6b, and a clear dampening of the surface lowering on the southeastern part of the Hintereisferner is apparent from your figure 1b. This coincides with an area of debris cover on the snout, which is clearly seen on satellite imagery. The effects of debris are indeed highly glacier-specific (depending on debris thickness/area, debris properties, and climatic conditions) and future trends are difficult to establish. However, given that the effects of debris are already present to some degree and generally expected to increase in the future (e.g. due to enhanced melt-out, increased bedrock exposure and slope instability, decreased flow velocities/debris discharge off-glacier) and given that it can potentially have an impact in the future (which was already modelled on the specific Aletsch glacier by Jouvet et al. (2011)), I think it warrants at least some further explanation of why you didn't include it and/or discussion on its potential effects in the paper.

**Minor comments:**

L20: The new papers of Dussaillant et al. (2025) and GlaMBIE (2025) may be a good reference here.

L56: you can elaborate maybe a bit more on the specific advantages of using full-Stokes over the SIA or higher-order approximation (e.g. what type of stresses are included). Does it really make that much of a difference and if yes, in which areas do you expect the most significant improvements? From my experience the SIA and HO models usually work really well for glaciers, so what are the main advantages of using the full Stokes when compared to other approximations? As you mention in the conclusion, a detailed quantitative comparison would be out of scope but I do think it can be briefly discussed.

L61: remove second )

L71: you can maybe mention here the dynamic calibration procedure that is usually performed (e.g. artificially adjusting the historic mass balance after a steady-state spin-up so that the observed and modelled lengths over the historical period agree until presentday). Was this dynamical calibration procedure not feasible for your model? Were you able to reconstruct and compare historic front variations?

L82: I think you can already briefly mention here why these two glaciers were chosen specifically and further elaborate on it in the section 2. They are for example not WGMS reference glaciers and it is not the first time that they are modelled, why are they specifically important and are they representative for the Alps in general?

L125: remove 'a'

L132: adjust 'regionla'

L170: The shortwave radiation is decreasing with elevation. Is this the incoming only or the total shortwave radiation (minus outgoing)? Usually, atmospheric transmissivity increases with elevation, enhancing the incoming shortwave radiation at higher elevations.

L220: can you show the equation used for the cosine interpolation? Or at least a graphical representation of it in the Appendix maybe.

L239: can you give an indication of the computational cost? How long does it take to run the model?

L234: I understand the albedo is used as a tuning mechanism, but do you have anything to compare their values to for checking its credibility? Data from an on-glacier AWS?

L280: do you have evidence that both glaciers are indeed isothermal such that a thermomechanical coupling is not necessary? Also adjust 'thermochemical'.

L306: ice velocities after initialization agree really well, nice! What about the ice thickness? Can you provide an RMSE for those as well?

L315: Add additional bracket )

Figure 6 caption: can you indicate here again where the SMB observations come from where you tune against? You show accumulation and melt separately, but do you have data to validate whether the distinction between these two is correct? Or do you only have observed data of the final SMB?

L350: missing point at end of sentence

L360: The term "disappearance" can be ambiguous (e.g. complete ice-free, negligible residual ice, disconnected patches, below a threshold volume or thickness?). You should clearly define here how you define glacier "disappearance".

L363: diverge -> diverges

L368: project -> projects

L371: Figure 10a and b displays -> Figures 10a and b display

Figure 10 caption: presents -> represent

Figure 9b and 10b: can you include the reference time period to which the volume loss (%) is compared in the y axis label? You may also want to include it in the captions of Fig. 10 and 12.

L388: leading -> lead

L389: project -> projects

L401: 2800 m, a.s.l. -> remove comma

L409: sime -> some

L412: the accuracy of ice velocity retrieval from satellites depends on the acquisition method. SAR interferomerty is usually very accurate for slower moving ice.

Section 7.3: I don't really like the title 'east-west comparison'. To me this section just reads like an attempt to generalize the behavior of the two glaciers over a certain region. To explain the different behavior of the two I think you can also elaborate more on (1) the climatic setting that may differ (can you give some quantitative climate data for both glacier environments to corroborate this?), but (2) also the climate sensitivity of the glaciers related to their geometry. Hans Oerlemans did a lot of research into this and you can maybe compare the glacier characteristics of both glaciers to explain their different future behavior/sensitivities (mass balance gradient, overall slope, glacier size, hypsometry (e.g. large accumulation area vs. narrow snout), etc.). This also adds to the difficulty of generalizing glacier behavior for a certain region.

**References:**

Dussaillant, I., Hugonnet, R., Huss, M., Berthier, E., Bannwart, J., Paul, F., and Zemp, M. (2025). Annual mass change of the world's glaciers from 1976 to 2024 by temporal downscaling of satellite data with in situ observations, Earth Syst. Sci. Data, 17, 1977–2006, https://doi.org/10.5194/essd-17-1977-2025.

Hausfather, Z., K. Marvel, G.A. Schmidt, J.W. Nielsen-Gammon, and M. Zelinka (2022). Climate simulations: Recognize the 'hot model' problem. Nature, 605, 26-29, https://doi.org/10.1038/d41586-022-01192-2.

The GlaMBIE Team (2025). Community estimate of global glacier mass changes from 2000 to 2023. Nature 639, 382–388 (2025). https://doi.org/10.1038/s41586-024-08545-z.

Jouvet, G., Huss, M., Funk, M., and Blatter, H. (2011). Modelling the retreat of grosser aletschgletscher, switzerland, in a changing climate. Journal of Glaciology, 57(206):1033–1045. https://doi.org/10.3189/002214311798843359