Review of Rahlves et al.

I am reading the manuscript for the second time. I found the updated version very much improved, and I am generally satisfied with the point-2-point answer. The newer version clarifies the methodical approach of absolute versus anomaly forcing. The manuscript reads well and is well structured and is generally easy to follow. But still, I am missing a clear story. Lines 64-68 of the Introduction formulate a clear research question, but the abstract doesn't reflect that. I have a few minor comments (see below) and two major comments:

Major comment(s)

(1) I still found the conclusion (abstract Lines 13-17) of the paper too much concentrated around the projections themselves, which is not a new result compared to Goelzer et al. (2020), Payne et al. (2021) and Rückamp et al. (2020). I think the abstract, discussion and conclusion should concentrate more on the initialization (i.e. Fig 9) and how this impacts the projections. It isn't clear what is new compared to the previous ISMIP6 simulations and what we can learn from your study. For me, questions remain like:

- In the historical period, you have models that match the observed/ERA5 slc better than others (Fig. 5). If you exclude the identified 'outliers' from the projected mean, is the spread in projected slc narrowed?
- What are the advantages/drawbacks of absolute vs. anomaly forcing for projections? Though these questions are somehow answered in the text, they are not explicitly given in the abstract and conclusions.

(2) The methodical approach to test the anomaly vs. absolute forcing is questionable. You say that the spread in slc between both forcings is small (text around Figs 7 and 8). But I don't find it surprising because you run your model to a steady state with the baseline/reference SMB in the initialization; the anomaly drives the subsequent projection, which is, in both cases, similar (as you said in Lines 388-401). I guess the inversion approach compensates the difference between absolute and anomaly forcing.

Minor comments

Line 81: Please add a citation to Weertman power law and give a value for the power. Based on Fig. 3b, I guess tau_b= C *u_b^(1/m). With m=3?

Lines 132 and 137: Please clarify that BedMachine and the present-day conditions are not 1960.

Line 179: typo: anomaly -> anomaly. Suggest separating the equation from the text.

Line 204: Please transfer the 'Gt'-values to slc to make them comparable to the following figures. Or just add the slc value.

Line 206: That's wrong, or Figure 4 is wrong. Please check. Fig. 4 Indicates a range of 2.675 to 2.71 in 1960 (-> end of initialization).

Line 210: initial ice thickness -> ice thickness at the end of the initialization. (In my understanding initial ice thickness is BedMachine). There are more instances where 'initial' might be misleading: Line 209 'initial mass'; Line 210 'initial SMB'. Line 232 'initial ice sheet'. Please check the manuscript.

Line 211: GCM-init -> ESM-init

Line 222: Maybe rewrite to 'from NorESM2-MM from the ESM-init ensemble'.

Line 224: ERA5 -> ERA5-init

Line 236: ERA5 spin-up -> ERA5-init (or?)

Line 244: NorESM-init -> NorESM of ESM-init ensemble (or similar)

Line 249. Please rephrase. You don't present mass loss. You present the time dependent ice mass.

Line 255: ESM-initialized: rewrite accordingly." Most simulations of the ESM-init ensemble..." Its still confusing, you use ESM spin-up, ESM-initialized, ESM-initialization and ESM-init. It should be ESM-init for all of them, or?

Line 253 to 255 and Line 260: I am not convinced. I found the statement about interannual and decadal climate variability a bit speculative. You don't show that. Maybe CES2-Leo represents the observed variability correctly, but temperature and precipitation are on the wrong level. In this case, an anomaly forcing or a bias adjustment of the ESM data could help to understand the behaviour.

Line 266. I need some clarification. In the description of the historical simulations, you say that the forcing for ESM-init is absolute (Line 167). So, how can you set the anomaly to zero? I guess you just force with the individual 1960-1990 reference climate. Well, the following sentence and figure caption clarify… Maybe rewrite

Line 340: I don't understand 'historic drift'. You make a large effort to reduce the numerical drift by the time-dependent inversion and additional relaxation. So, the drift should be small. If you refer to the SLC beyond 2005 in Fig. 6, this is just the response to the previous forcing, but not model drift. (Maybe it is committed to SLC with 1960-1989 forcing. But this scenario doesn't make sense when running projections …).

Line 341-345. Please drop the comparison to Goelzer et al. (2020). Just say, that your results are consistent with e.g. Payne et. al. (2021).

346: What do you mean by climate forcing? Only SMB? Or SMB with retreat forcing?

Line 368: Yes, you show that your volume/ice mass after initialization matches the observation (around Line 205). But I would be curious the see a map and/or RMS value. If RMS, please compute it similar as in Goelzer et al. (2020) (i.e. 5km grid) to make it better comparable to other models.

Lines 376-388. Drop this paragraph. I wouldn't discuss committed sea level rise. It is not a central point of your paper. I am just happy with the sentence in Line 27

Line 389: do you really mean ensemble projections? The approach is suitable for the intercomparison of individual models.

Line 390: Please rewrite. I guess 'full' equals to 'absolute'?

Lines 388-401: I don't find it surprising that your absolute vs. anomaly forcing are similar. Your ESM-init simulations are run until a steady state is reached. If you now add an anomaly (which in both cases ESM-init and ERA5-init are equal (your Lines 393)), the response should be similar.

Figure 1b: Please replace '**' in colorbar caption. Caption: ... "at the end of the initialization of ERA5-init".

Figure 2: Maybe I am a bit picky, but "initialization with NorESM2-MM" is in my opinion not true (following your descriptions in Sect. 2.3.1). Maybe 'modified ERA5-init with NorESM2-MM SMB forcing' or similar.

Figure 4: caption: drop 'absolute loss'. You show the ice mass itself, not the mass change.

Figure 5: caption ESM-initialized -> ESM init

Figure 7: I like the bars on the righthand side. Color-coding could be more optimal when compared to the previous figures. Suggest using equal colour coding for ESM and dashed/dotted/straight lines for scenarios... ah, I see. You also need a coding for ESM-init vs. ERA5-init. Maybe you can combine Fig 7 (just showing one init-scen) and Fig. 8.

Figure 8: Would rotate the figure by 90° so that the ESM names are easier to read. Maybe sort by SL contribution. Maybe give the dots the same colour as in the other Figure for easier identification, although for this figure is not necessary,

Figure 9: That's a very good representation; like that. You may add the range of the ISMIP6 simulations to this figure as similar bars. I think that would be a good basis for the discussion, as you did.

Figure 11: Would drop '-4km' on xlabels.

Typos etc.

Line 80 Eq. (2), Eq.(5) and Eq.(24) -> Eqs. 2, 5 and 24
Line 244. e. g. -> e.g.

Line 415: publishing the retreat forcing over the historical period would be valuable.