Review of 'Numerical modeling of the dynamics of Mer de Glace glacier, French Alps: comparison with past observations and forecasting of near future evolution'

by Vincent Peyaud et al.
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In this manuscript, Peyaud and colleagues use the Elmer/Ice ice flow model to simulate the dynamics and the evolution of the Mer de Glace between 1979 and 2050. They pay elaborate attention to the evaluation of the model output by comparing their simulations to an elaborate dataset collected on the glacier between 1979 and 2015. Subsequently, they model the future evolution of the glacier under 26 different climate scenarios, for which they particularly focus on the modelled changes in glacier length and velocity.

This is a nice and detailed study, which has been well designed and is pleasant to read. The manuscript will be of interest to glacier modellers and fits very well in *The Cryosphere*. The level of detail of ground-truth data is very high and it is nice to see such an elaborate effort to compare these with modelled results. The list of comments and suggestions provided below may seem relatively long at first, but most of the comments should be relatively easy address and to incorporate.

General comments

- It is a pity to see that only the lower part of the Mer de Glace was modelled (as explained in section 3.2 and 4.2). While going through the manuscript, I was constantly thinking: why is this the case? The explanation, in which this is linked to the uncertainty in the bedrock, appears only towards the end of the manuscript (p.15, l.20-21). If you were to consider the upper part of the glacier also, you would indeed introduce additional uncertainty in your simulations; but you now also do so by imposing several conditions on the fluxes through the upper gates (Tacul and Leschaux): e.g. linking the flux at the gates with upstream integrated surface mass balance based on observations and imposing this for the future (while in reality the glacier response time will play a big role here). It would really be nice to see how much this influences your results by having some additional sensitivity tests in which you modify the imposed model settings. Even better would be to have some simulations in which you model the entire glacier (i.e. after inverting the ice thickness in the accumulation area) and see how they compare to your results. Would this be feasible? Such a test would require some additional work, but I honestly think that this would add a lot to your story and would also increase the impact of your story (as it could be used as a kind of reference for future studies that impose fluxes at gates and only model a part of the glacier – a method that may definitely gain in popularity for certain applications!)
- The level of detail of your analyses is very sophisticated and you consider several elements in your evaluation and for the projections that many other studies do not include.
    - However, it would also make sense to have insights in the more widely considered glacier characteristics, such as glacier volume and area:
        - For past: with this you can directly assess the performance of your model to reproduce e.g.:
            - Past volume changes (would in fact be a kind of test for your surface mass balance model in this case), which can be derived from DEM differencing (and which I imagine is maybe already directly available for this glacier?).
            - Past area changes.
        - For future: allows you to compare more easily to other studies in which the evolution of Mer de Glace is also modelled and comparison with other glaciers in the European Alps (e.g. is Mer de Glace more/less retreating than other glaciers,...; list of studies is provided further on)
    - In general, you refer to your study as an elaborate evaluation, which it definitely is, and which I think is very impressive. However, some of the agreement may also results from several choices you made, which are not always explained (see comment below on this). It is therefore difficult to disentangle which part of the agreement results from a kind of calibration ('tuning') and is therefore not a real kind of validation/evaluation (as you want the calibration and the evaluation data to be – ideally entirely – independent).
- There is a sort of discrepancy between the complexity of the model used for the ice dynamics and the relatively rough approach for surface mass balance and imposed boundary conditions at the gates.

Given that the glacier is so well studied, why did the authors not consider more complex approaches for this (e.g. thinking of e.g. debris cover; constant mass balance gradient)? A few additional sentences and motivation would be nice.

- Some assumptions are made, and it is not always clear how these affect your results. Would be good to have some additional insights in the sensitivity of your findings to your various assumptions. This includes assumptions related to:
    - Constant mass balance gradient
    - Imposed ratio between sliding and surface velocities at the Tacul gate
    - Assumption that relationship between ice flux at Tacul gate and integrated surface mass balance for upstream area remains the same
    - Minimal thickness and velocities at the gates
    - Linear decrease ice thickness at Leschaux gate over time,
    - ....

  For a full list and more details, refer to the specific comments below.
- Most of the figures could be improved relatively easily to enhance their readability: see suggestions below.

Specific comments and suggestions

Abstract:

- p.1, l.2: 'All alpine glaciers are shrinking and retreating at an accelerating rate...': technically this is not entirely true. It is the case for most glaciers, but there are some exceptions (e.g. glaciers that are almost gone or those that disappeared; i.e. where the retreat does not accelarate). Suggest changing this to: 'Alpine glaciers are shrinking and rapidly lose mass in a warming climate'
- p.1, l.8-9: 'To our knowledge a comparison to data at this detail is unprecedented': indeed a very detailed comparison to data is present, which is very nice. But not sure you can claim that it is unprecedented, as comparisons to other studies are not straightforward (in some studies other types of data have been considered). Probably best to remove from abstract and mention in this in the main text, where there is room for more nuance. Check studies on individual glaciers with elaborate evaluation and/or calibration with ground-truth data (e.g. Adalgeirsdóttir et al., 2011; Zekollari et al., 2014; Hannesdóttir et al., 2015).
- p.1, l.9-10: You mention the velocities and the elevation changes for the model evaluation. What about the mass balances and the length variation, which you mention a few sentences before (in l. 6): how do these perform? This becomes clear in the text, but for consistency would be good if you could already mention them here.

1 Introduction:

- p.1, l.19: 'sea-level rise': could be worth referring to the new GlacierMIP studies, in which the future sea level contribution from glaciers are obtained from a community-wide intercomparison effort (Hock et al., 2019; Marzeion et al., 2020)
- p.1, l.21-22: 'first studies': you are not very specific here. Given that you model a single glacier and have not mentioned the 'large-scale' glacier modelling aspect yet, one would assume that these are the first studies for the evolution of individual glaciers in the European Alps. I suggest being more specific here (mentioning the regional aspect) and/or to refer to pioneering studies in which ice dynamics are included for individual glaciers (e.g. Huybrechts et al., 1989; Letréguilly & Reynaud, 1989; Stroeven et al., 1989; Greuell, 1992). Would somehow be strange to spend your introduction focusing on large-scale glacier modelling, while your work in fact focuses on very detailed glacier modelling.
- p.2, l.6: better also update with the new numbers from the second GlacierMIP effort (Marzeion et al., 2020)
- p.2, l.10-12: list of references for 'model describing the complex three-dimensional geometry of a whole glacier' is a bit odd:
    - Some studies do not take into account the glacier evolution over time
    - Others are in fact based on the SIA , which makes them rather 2D (as described in the title of Le Meur & Vincent, 2003) and more in line with what you describe earlier (p.2, l.4-5) as 'Process-based model ... to take into account simple dynamics' (Clarke et al., 2015)

- The ITMIX experiment, which focuses on ice thickness reconstruction (Farinotti et al., 2017), is also a bit odd to mention here
  - Why not simply focus on what you also do here: 3-D time-evolving simulation of a single glacier? (e.g. Schneeberger et al., 2001; Le Meur et al., 2004; Jouvet et al., 2009, 2011; Zekollari et al., 2014; Ziemen et al., 2016; Jouvet & Huss, 2019; Gilbert et al., 2020; Schmidt et al., 2020). Would also be interesting for the discussion to compare your modelled future evolution of Mer de Glace with the modelled evolution of other glaciers in the European Alps (see also general comment on this).
- p.2, l.21: 'This dynamics' → 'These dynamics'

3 Ice flow model:

- p.3, l.29: Value for the rheological parameter for ice: how was this value chosen? Quite often this is used as a calibration parameter as it has a large influence on the ice thickness (/glacier volume). By just taking a value from literature: difficult to assume that this will work well immediately for your glacier of interest. See studies in which this was analysed / where this rheological parameter was tuned (e.g. Schmeits & Oerlemans, 1997; Albrecht et al., 2000; Vincent et al., 2000; Giesen & Oerlemans, 2010; Adalgeirsdóttir et al., 2011). From my understanding, in your study the calibration occurs through the basal sliding, in which you try to match observed velocities: but what is effect of this approach on modelled ice thickness evolution? i.e. How are you sure that the modelled evolution is related to physical forcing and not to some kind of model drift? Would be good if you could explain this a bit in the manuscript.
- p.4, Figure 1: would have been nice to have surface elevation information in this figure (vs. visual imagery). Through this, would be easier to orient for someone who's not very familiar with the glacier.
- p.5, l.16: model domain does not cover the entire glacier. Why? (I saw later that this is explained towards the end of the manuscript) Should really clarify this choice. Pity to not have the entire glacier in / or additional experiments in which this is the case to compare to,
- p.5, l.28-29: 'Bedrock elevation...interpolation (Fan et al., 2005) of all available observations': does this mean that the bedrock is simply obtained from a kind of kriging? Is it not justified to rely on a more sophisticated approach, especially given the fact that you then use a very complex 3-D model to solve for dynamics and temporal evolution? Would also be good to have an idea where the ice thickness (/bedrock elevation) was measured (unpublished data is mentioned later; but maybe you can add the profiles in a figure somewhere?).

4 Methods:

- Name of the section ('Methods') is maybe not ideal, as in fact the previous section ('Ice flow model') is also really part of the methods. As you mainly describe the boundary conditions here (at the cross sections), you could consider renaming this section 'Boundary conditions' or something alike?
- p.6, Figure 2:
  - Quite difficult to decipher this figure: the grey line, which represents the 'average' is barely visible in the right panel.
  - Not ideal to combine green and red colours for lines in a single figure, given that a considerable amount of people cannot see the difference between these two colours (see e.g. https://en.wikipedia.org/wiki/Color_blindness#Red%E2%80%93green_color_blindness)
  - One needs to look up in the caption what the average stands for, maybe specify that this is the average of the RCPs? Same of Safran: maybe good to specify this, as not clear what this is at this point in the manuscript (i.e. Reanalysis 'SAFRAN')
  - Do not entirely get why you show RCP's for the past and how this should be interpreted. Makes sense that these are off if they have not been forced with reanalyses product (e.g. ERA5). With this, expect them to be much closer to SAFRAN reanalyses product also. Also, not entirely clear if what you show here is the SAFRAN original SMB, or the one that is corrected by scaling the precipitation with ca. +60-70% (as you describe towards the end of section 4.1.). I expect the latter, given the good agreement in SMB. If so, and if I understand it correctly, would it also make sense to have the 'modified' SMB (with precipitation correction) from the RCPs?
- p.6, l.5-7: 'For the forecast simulations from 2015 till 2050, results from climate simulations are used to evaluate the flux on the different boundary conditions of the glacier domain': I get the meaning of this sentence, but it is a bit strange / misleading to use 'evaluate' here, as this is what you use to

describe the evaluation of the hindcast also. Maybe change to: '...are used to simulate the future flux evolution at the boundary of the glacier domain'

- p.6, l.6: 'till' → 'until'
- p.7, l.4-5: 'Despite this strong variability from year to year (Fig. S1 in the Supplementary Material and Rabatel et al., 2005), a constant mass balance gradient of kb = 0.009 m^{-1} is adopted for hindcast and forecast simulations'. How does this affect your results / how would your result look like if you take into account the interannual variability and also not rely on a constant gradient?
- p.7, l.6-9: why did you consider these 26 future climate projections from the EURO-CORDEX ensemble, given that there's many more (>50) available? Any criterion used to choose only those? Moreover, are these simulations from the RCM at 0.11° resolution or at 0.44° resolution (or a mix?). Would be good if you could be a bit more specific on this.
- p.7, l.34 – p.8, l.1: 'we further assume a constant and uniform ratio between sliding and surface velocities of 1/3 at both gates': what is this assumption based upon? Given the lack of direct observations of basal velocities, the uncertainty on this statement is quite large. How does this influence your results?
- p. 8, l. 28-30: assumption about relationship flux at Tacul glacier and integrated surface mass balance higher area. You assume that this remains constant in the future: how much does this influence your results?
- p.9, l.7: imposed values for minimal ice thickness and surface velocities. Why are these values chosen? And what is influence on your simulations?
- p.9, l.14-15: linear decrease in ice thickness at Leschaux gate: again, sounds rather arbitrary. What is effect of this on your simulations? Can imagine that this could have quite an impact on projected future changes..
- p.9, Figure 3 + p.10, Figure 4:
    - Like for figure 2: would make sense to have information on what the different lines represent in the figure itself instead of in the caption. There is enough space, and would make the interpretation much easier and more intuitive (and an advantage if you plan on using this in a presentation later!)

Results:

- Although you do not really calibrate to this, it is not fully clear how the several choices you have made affected your simulations and can therefore be considered as an independent evaluation.
- p.11, l.1-2: 'In general, the lower the profile, the larger the delay between the start of decrease of the simulated compared to the observed surface elevation': this does not really come as a surprise, as you impose the fluxes at the Tacul gates to fit observations. As such the uncertainty in your results 'spreads' as you go away from these points (towards lower glacier elevations).
- p.12, Forecast simulation: part of the similarity in future evolution is driven by the fact that the SMB is quite similar, as the difference in the forcing (temperature and precipitation) increases with time and in fact only becomes really notable during the second part of the 21st century. However, a part of this is also simply related to the glacier response time, which is in the order of decades for this glacier. Would probably be worth placing this in context a bit (potentially in the discussion session) and making the link to response time studies on Alpine glaciers (e.g. Zekollari et al., 2020).
- Fig. 5-7: would again be good if could read the figure without having to refer to the legend (i.e. add information about the RCPs in figure directly). For RCP colours, it would be nice if you could use more conventional colours for the different RCPs (e.g. those used in the IPCC reports). Finally, mixing green and red in the same figure is still not a very good idea.

Discussion & conclusion:

- Nicely elaborated and very interesting in general!
- p.13, l.6-8: role of debris is mentioned. Given the complexity of your ice flow model and the level of detail of your analysis, would it not make sense to incorporate debris cover in your approach (and eventual evolution over time; see e.g. Jouvet et al., 2011)? Or maybe do some tests in which this is incorporated in a parameterized way to analyse whether this decreases the discrepancy between observations and modelling that you mention here.

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