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The Cryosphere
Discussions

Interactive comment on "Modelling the future evolution of glaciers in the European Alps under the EURO-CORDEX RCM ensemble" *by* Harry Zekollari et al.

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1 General comments

The manuscript by Zekollari and colleagues presents new estimations of projected glacier change in the European Alps. It is a well conducted study, the paper is well written and the results are interesting. The inclusion of ice dynamics, the use of CORDEX data instead of coarser GCMs and the large amount of calibration data are the main novel points in this study. It will become the new reference study for future glacier change in the European Alps and as such, it is likely to receive a larger interest from

the general public and the media.

I have two major concerns that need to be addressed before publication, as well a several specific questions / recommendations.

1.1 Validation and uncertainty

I acknowledge the efforts realized to use so many different observational datasets, an exercise only possible in the European Alps. However, I have several issues with the model validation in this study:

1. there is no information about how many glaciers (and how much ice area/volume) are simulated without any calibration data. For these glaciers, the geodetic MB is "interpolated" and the effect of this interpolation is not assessed
2. there is no indication as to the computation of the uncertainty ranges provided in the glacier changes (e.g. in the abstract). Does it originate from the forcing ensemble? The model RMSE? It is hard make further assessments without this information
3. the validation using observed traditional MB does not make sense to me, because the model has been calibrated on geodetic MB on the same glacier (both data are not exactly the same, but close - see also the comment of Ben Marzeion). If anything, you should use cross-validation here: when assessing the performance of the model on a given glacier, you remove the selected glacier from the calibration dataset, then use this data plus the traditional MB measurements to assess the model. This would also help to address point 1
4. it is problematic that the effect of the RCM forcing is not assessed at all. The plots all start in 2017, so any sceptic reader could say: "this is all extrapolated without

test in the past". I understand the problems behind the validation of RCM forcing because of internal variability, but: since you are bias correcting over a reference period, at least the MB model bias (not RMSE) could be assessed when driven by RCMs as well for glaciers with long observation time series. These data would provide a much better estimate of the true uncertainty of the model driven by RCM data for the future. I'm leaving it open to the authors if they want to implement this validation or not - I believe it would make their paper much stronger.

1.2 Glacier geometry

The Huss and Farinotti (2012) approach (HF2012), which is to "squeeze" glaciers into elevation bands is an interesting compromise parametrization, simpler than the multiple flowline algorithm followed by OGGM (Maussion et al., 2018) but still allowing for ice flow considerations. It has some advantages (I don't necessarily agree with the ones listed in the paper): it is programmatically more efficient, arguably more elegant (because simple), and it is probably less sensitive to uncertainties in glacier outlines or topography. It also has some disadvantages (mostly, the lost of geometrical information for more complex MB models, and the over simplification of the mass flow along multiple branches).

In an attempt to reproduce the method following the algorithm description by HF2012, I consistently obtain shorter glaciers than provided by the authors (e.g. as shown in Fig. 5). See https://nbviewer.jupyter.org/github/fmaussion/misc/blob/master/simplified_flowline_tests.ipynb for some code and graphics.

I wonder why I can't reproduce the authors' results, and I therefore have a few questions:

- what motivated the choice of 10m for the δz elevation bands? This is quite a narrow range and I get better results with larger bands (depending on the underlying

map resolution).

- what do you do when there is no glacier grid point in a 10m band? This happens quite often depending on the underlying map resolution (see graphs).
- do you do any kind of filtering for large slopes? The skewed slope distribution towards high slopes can affect the mean and, together with the missing bands, could explain why I get shorter glaciers.
- do you do apply any smoothing on the resulting band widths and areas? They appear quite noisy in my case (depending on the underlying map resolution).

I'd like to see these questions answered in this manuscript, unless I missed them from either HF2012 or Huss and Hock (2015), in which case I'm happy to be corrected and pointed to the location where the algorithm is described.

Similarly, there are some locations in the current manuscript where I find that the algorithm description is too vague to allow reproducibility (see specific comments below).

2 Specific comments

Abstract L10 : "which the latter" sounds strange. Rephrase?

Abstract L20 "RCM that is coupled to it" → the RCM is not "coupled" to the GCM (this suggests two-way nesting) ; maybe "nested in", or "driven by" the GCMs? Also revise other occurences in the text.

P2L13 "the evolution of the glacier" → "glacier evolution"

P2L21 "moderate" and "extreme" are subjective adjectives → be more precise, e.g. RCP or similar

Legend Fig 1 updated version OF Huss and ...

P3L23 "we aim at reducing the considerable uncertainties" → I'm yet to be convinced that increased complexity reduces uncertainty, and I'm not sure your study really deals with this topic or even actually shows that uncertainties are reduced. It's okay if you leave this sentence as is, but you don't need this paragraph to justify your study.

P4L3 what is the "local surface slope"? According to HF2012 it is the bin average for each elevation band. Be more precise in the formulation here (see also general comment about that).

P4L7 "little-known connections" → I don't understand what you mean. Connections are maybe more complex in a dynamical sense but so are other locations on the glacier as well. Furthermore, "ignoring" these connections is not making them less complex, it's just avoiding them. So, I suggest to remove this sentence (see also general comment)

P4L9 trapezoidal sections: how does this go together with the ice thickness inversion? What cross-sections are used in HF2012? If rectangular (I assume), by using a trapeze you are either reducing the sections volume or increasing the thickness h_0, i.e. you are not physically consistent between the inversion and the forward model.

P4L17 "close representation of past temperature and precipitation and certain events" → Reanalysis datasets also represent weather events well thanks to data assimilation. It's okay to use ENSEMBLES, but you should argue otherwise, maybe because of uncertainties in quantitative precipitation estimates or the coarse resolution of reanalysis data, for example.

P4L24 I think this whole justification paragraph is more confusing than helping. I think

it's okay to use an observational dataset for calibration and validation instead of reanalysis, consider shortening this paragraph.

P4L28 is "chains" the commonly used word for this? I thought that "realisation" or "simulations" would be more appropriate. See also other occurrences in text.

P5 Eq. (2) I have several questions here. First, you don't say over which observational period you compute the averages for the monthly bias correction. Is it 1961-1990? The entire observation period? I assume that σ_{obs} and σ_{obs} is computed for the same reference period as the bias. Then, why choosing a 25-yr period, and not a period of the same length as the reference period? Please also add a sentence as to why you don't apply such a correction for precipitation. I understand that the arithmetics are not so easy for multiplicative bias corrections, but in theory some kind of correction would also be possible (and might be needed by looking at Fig. 02).

P5L26 "based on a combined criterion weighting both horizontal distance and the difference in area." Can you be more specific here? (reproducible science versus "black box"). How many glaciers have Geodetic and traditional MB observations? Which area does it represent? How many glaciers needed this kind of interpolation?

P7L25 what kind of numerical solver are you using? It's not an harmless choice, as shown by Jarosch et al 2013.

P8L10 "Notice that through this approach, the glacier is not assumed to be in steady state at any point in time, but that an artificially modelled steady state is obtained by imposing a MB offset." → I don't understand what you want to say here. I'm also quite confused at the statement "A determines volumes, SMB bias determines the length". Is this based on you own experience, or is there a physical explanation? Finally (and most importantly), why is length used as convergence criterion instead of area, which is the only variable which is almost perfectly known at the inventory date?

Model initialisation needless to say, the iterative initialisation procedure is... unconventional. I'm not asking to change it, because it serves one purpose: find a transient glacier which is consistent with the forward model at a reference date. This is necessary because the ice-thickness inversion model and the forward model in GloGEMFlow are probably not consistent between each other (different MB profiles, different A, different bed shapes). However, I would like to add that I don't really think that this iterative method has much to do with finding an "appropriate" A for each glacier. Let's take the first step as an example: since you drive your model with an SMB such that the present day geometry is in equilibrium, modifying A so that your glacier has to grow will always tend towards lower values of A in order to create a thicker, longer equilibrium glacier in 1990.

P8L23 this cannot be considered an "independent" validation (see general comment)

P8L25 "rather than the coupled SMB – ice flow model" → this is a bit of a missed opportunity, because there are chances that the varying geometry actually improves the SMB validation, by taking geometry changes into account which are present in observations but not in the static model.

Fig 4 Legend r2 is the "coefficient of determination"

P8L29 elevation bands and correlation → I agree with Ben Marzeion

Fig 5 intuitively, I would swap the glacier flow direction so that the distance on model grid (x-axis) is starting from zero at the glacier top. This would also allow to read the length of the glacier directly on the x-axis

P9L21 how did you compute the surface velocity out of the depth-integrated velocity given by the shallow-ice approximation?

P10L12 note that other length records are also available for the non-swiss glaciers (WGMS or Leclerq database)

P10L20 remove "highly significant" and the p-value to read "the correlation is r2 = 0.37 ($p < 1e-3$)"

P11L4 unit km2 yr-1

P12L6 "highest correlation with the maximum glacier elevation" → is this sentence correct?

Fig 9 Legend remove the "two" in "two present day"?

Fig. S3 Consider adding Fig. S3 to the main manuscript.

P14L10 what do you mean with "ice is more pronounced"?

P15L12 when the variable IS considered? I'm not sure I fully understood this section.

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