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Interactive comment on "A deep learning reconstruction of mass balance series for all glaciers in the French Alps: 1967–2015" *by* Jordi Bolibar et al.

Anonymous Referee #1

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This paper present a reconstruction of the surface mass balance (SMB) of all French glaciers for the period 1967-2015 based on an deep learning (DL) approach. It strongly relates to the study on the methodology recently published by the same authors in The Cryosphere. The data set is comprehensive, interesting and certainly deserves publication. However, there are presently some weaknesses in the presentation of the findings, as well as in the validation of the approach.

Substantial comments:

- Structure: The paper needs to be restructured. A Data section is needed – this is presently merged into the Methods. I would also suggest that the study site character-

istics are presented in a separate section. At present this is contained in the Results section. The Methods section needs a clearer structure, separating between the actual approach and the uncertainty assessment.

- A major problem of the paper as it stands now is, in my opinion, the lack of validation with independent measurements: The training data set for 32 glaciers is based on "remote-sensing" (Rabatel et al., 2016). As this lies the foundation to the entire study, more effort should be invested to describe this data set (methods, uncertainties). The training data set also contains models and assumptions – the annual SMB of these glaciers has not been directly measured. This needs to be emphasized. Measured (!) information on SMB is available from two sources: (1) the direct glaciological surveys, (2) geodetic surveys.

Although (1) is probably included in the training data set, it should explicitly be shown (e.g. figure with cumulative SMBs) how well the DL approach reproduces the observed SMB series. This would give direct evidence on the performance of the approach, independent from the training data set by Rabatel et al. (2016) that also includes model assumptions.

I was surprised to see that the study does not show any direct validation with data on geodetic mass balance. With the annual resolution of the data set presented here, this would be straight-forward to achieve. For many glaciers in the French Alps, geodetic mass balances over varying time periods are available (see e.g. data base of the WGMS). I assume that some of them have already been used in the set up of the training data set, but probably not all of them. Geodetic mass balances would yield a fantastic way to validate the DL-based estimates of regional variability in mass balances. New remote sensing data sets (e.g. TanDEM-X in combination with SRTM, or ASTER) would also allow computing geodetic mass balances for each individual glacier. I see that deriving such a new geodetic data set is beyond the scope of this study, but it is indispensable to compile the available geodetic survey for French glaciers in this regional assessment in order to gain confidence in the results.

- Mass balance modelling: Deep learning and mass balance modelling are complimentary approaches. Whereas, the introduction gives relatively little details about the potential of mass balance modelling (and previous studies for the French glaciers) to reconstruct SMB (e.g. Vincent 2002), much detail is given towards the end of the text when comparing the present results to those by Marzeion et al. (2015). Also it was not clear to me why the authors decided to only rely on the results of this relatively coarse study (global scale, no specific calibration to the French Alps) and not more recent and more detailed model results on the past SMB of glaciers in the French Alps by Zekollari et al. (2019) (regional scale, high-resolution climate forcing data, glacier-specific calibration based on geodetic mass balances).

- Language: Although the paper is well-written in general, there are several instances where the writing could be improved (e.g. "allows to ...", p2, line 18 and other instances, is not correct English). Proof-reading by a native English speaker would certainly help.

Detailed comments:

- Page 1, line 8: cross-validation against which data set? Please clarify here.

- Page 1, line 16: I would not refer to "meltwater contributions" here: Annual values of glacier-wide SMB do not actually yield "meltwater" but just annual glacier mass loss. Meltwater has a strong seasonal component and also occurs in the case of SMB=0 or SMB >0! - Page 2, line 12: order references according to year

- Page 2, line 13: Digital Elevation Model, instead of "maps", is typically used

- Page 2, line 22: This is also true for various other global glacier models (see Hock et al 2019 for a compilation). All of these models provide annual SMB for French glaciers in the PAST (although with probably limited skill). See also comment above. The detailed regional modelling study including the French Alps (Zekollari et al 2019) should also be mentioned.

- Page 2, line 28: It seems strange to refer to the results of the present paper already within the paper itself. It is clear that the data set is already available, but it should not be referred to, as this is where it is actually described.

- Figure 1: Although it is stated in the caption that the figure is schematic, it leaves a wrong impression on the density of available information for training the DL approach: In fact, green lines should only make up for 5% of the glaciers, whereas the figure implies that it is more than a third. It should be revised accordingly.

- Page 3, line 10: Although this paper is closely related with TC paper of the same authors, the location of the training data sets needs to be presented here.

- Page 3, line 16: It is excellent that topographical information is available in repeated time steps throughout the study period. However, it remains unclear how this updated topographical data was included in the DL approach. This is quite relevant information as the feedback of retreating glaciers (shrinking area, changes in area-elevation distribution and terminus position) exerts an important effect on glacier-wide SMB. A detailed description of this is required.

- Page 4, line 1: Similar comment as above: I would just refer to the paper where the model is described and not have separate references to the model and the publication. You can state in the Data availability section where the code of the model is located.

- Page 4, line 15: It is a quite relevant aspect in my opinion (probably worth mentioning in the Intro) that the DL approach (e.g. in comparison to in-situ observations and mass balance modelling) only provides annual SMB, but no information on seasonal mass balance as well as mass balance gradients. If my understanding is wrong, please correct me. But these additional variables are crucial for various aspects of impact assessment and model development.

- Figure 2/5: Please add letters (A/B) to the panels

- Page 7, line 18: For a more recent reference on exactly this topic, please also see

Zekollari et al. (2020), Geophysical Research Letters

- Page 8, line 2: Please remove the reference to Huss&Hock, 2015. It does not fit here in my opinion.

- Page 8, line 31: see also substantive comment above: To me, this appears too long and too detailed. The comparison to a modelling study is a nice addition but it does not actually allow evaluation of the present results. More focus needs to be put on the validation using fully independent field observations (in-situ) and geodetic surveys.

- Page 10, line 3: Potentially also related to the way glacier retreat (updating of area-elevation distribution) is accounted for the in model by Marzeion et al 2015, and the present approach? Probably worth to discuss here as well.

- Page 11, line 6: Actually, all Supplementary Figures should be referenced from the main text. I found the analysis in the Supplementary interesting but not straightforward to understand. It might be beneficial to present this additional analysis more prominently in the main text.

- Page 12, line 12: Is there no possibility to go beyond the year 2015 by the way? In my understanding the trained DL approach should enable to predict mass balance also for the most recent years. This would be quite interesting as the last years were extraordinary in terms of their mass losses.

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