General comments

The paper is presenting the results of a study dealing with 70 years of evolution of ice aprons (IAs), namely cold ice fields located in very steep slopes (>40°), in the Mont-Blanc massive. 200 IAs have been investigated. As a main highlight, the paper evidences the dramatic and ongoing decrease in area of most IAs mainly in response to rising air temperature, with however an impact which is reduced and even not perceptible at the highest elevations.

The novelty of the paper is high as there has been almost no publication dedicated to IAs so far.

The paper is well structured and refers adequately to existing literature. Data, method and results are almost clearly presented. I would, however, suggest to separate the discussion aspects in a distinct section. The figures are mostly adequate, need however some improvement (see specific and technical comments). The conclusions are well concise and supported by the results of the study.

Besides an additional slight reorganization of the Results section (see Specific comments), my main concern is about the evolution of the accumulation proxy as a precipitation-temperature dependent factor and its impact on the IAs area changes. The proxy – which I fully agree with – is presented in the methodological section, but not further in the results (sub-section 5.5).

I would consider the paper very worthy of being published, after minor improvements

having been undertaken.

REPLY:

Dear reviewer, thanks a lot for taking the time to meticulously review our paper and providing valuable feedback to improve the manuscript. Your encouraging response motivates us to improve the manuscript further, considering all your suggestions and feedback. As per your suggestion in this general comment regarding the separation of the results and discussion section, we completely agree with this. We assure you that the revised manuscript we submit will consider this specific comment from you. We will further work towards improving the quality of our images as per your suggestions. You will find our replies to the specific and technical comments you have on the manuscript below. Thanks in advance also for taking the time to read the revised version of the manuscript.

Specific comments:

As ice aprons are almost unknown in the literature and to facilitate the understanding, I would strongly suggest to insert an initial figure (picture) illustrating what is talking about. For sure, many very illustrative pictures should exist. The orthoimages presented in Fig. 5 are not sufficient for that purpose.

- Thanks a lot for this specific comment. We agree that an initial high-resolution figure would be helpful for a better understanding of ice aprons. We have many images and field photographs of ice aprons which we can insert in the initial sections of the manuscript. We will add some high-resolution photographs for this purpose in the revised manuscript.

There is only one year used for the longer-term analysis, namely 1952. The conditions during that year could be worth of being described. According to the GSB data, there was a severe heat wave of a few weeks in late June – early July. 1952 was also finishing a period of about 10 warmer years with some "hot" summers as 1947. A significant reduction of IAs took place during those years, before that the conditions became again more favorable for the next about three decades. This is attested for some alpine IAs outside of the MBM area (e.g. Mont-Blanc de Cheilon in the Valais Alps).

- This is a critical comment, and we fully agree with your suggestion. it is important to discuss the weather during this particular year to have a better idea of the evolution of ice aprons during this year. We will add a paragraph in section 3.3 to discuss this specifically in the revised manuscript.

(L. 425) I agree with the way of doing for estimating the accumulation on ice aprons being limited to precipitation by air temperature ranging between -5 and 0°C. It would be nice to provide an example for an annual period, at different elevation. It will show that such conditions are only (mostly) prevailing during the summer half-year and the winter precipitation are almost not entering into consideration (what is maybe however not the case on south slopes). Later in the result section, similarly to figure 11, it would be important to illustrate its evolution since 1952.

- Dear reviewer, thanks for raising this issue. We agree with your comment regarding the conditions being favourable for precipitation mostly in the summer months. We have added a

new plot for the year 2019 to show this as per your suggestion. The plot (also attached at the end of this document) clearly shows precipitation values are highest in the summer months. Some precipitation at lower elevations occurs in winter, but at higher elevations, most precipitation occurs only in the summer. This new plot can be added to the revised manuscript as per your suggestion.

Coming to the other suggestion for the accumulation proxy and its evolution since 1952, we initially thought of having this plot as part of our manuscript, but in contrast to the PDD plot (figure 11), this plot is haphazard and does not provide any clear information. We attach this figure also at the end of the manuscript for your reference. Instead, working on your other suggestion, we can provide a figure showing the rate of accumulation at all elevations between -5 and 0 ^{o}C for each time period (1952 to 2001, 2001 to 2011 and 2011 to 2019). As can be seen from the plot, accumulation rates are decreasing in general over the years. This can be a piece of relevant information, in our opinion. We attach the new figure also at the end of the manuscript for your reference.

A description of the spread of IAs over e.g. elevation and aspect is missing. There is the figure 1, but it does not help.

- We agree that this information is useful. For this inventory of 200 IAs, 77% of the IAs are located above the 3200 m a.s.l. (the regional ELA, according to Rabatel e.al, 2013), while a majority of them exist between 3200 and 3800 m a.s.l. (63 % of the total count). For the aspect, a majority of the IAs are located in the northern aspects i.e. N, NE and NW (55 % of the total), while the eastern aspects are the least dominant. We will add this to the revised manuscript along with a couple of graphs (attached at the end of this document) A big discussion on this topic is also a part of the paper which we have under review currently, while this information is also published for the entire database in Kaushik et.al 2021.

In the result section, beside a sub-section 5.3 is missing, I would suggest to invert the order of sub-sections 5.5 (Influence of changing climate…) and 5.4 (Influence of local topography…), as 5.5 appears to be more closely the follow-up of sub-section 5.2 (Total loss area… over seven decades).

- Thanks for pointing out the mistake with missing section 5.3. We also agree with the suggestion that section 5.5 can come before section 5.4 in the manuscript. We will correct this in the revised manuscript.

Minor/technical comments:

L.83 – Maybe replace "most" by "many" or "frequent", or does it apply to the MBM area only ?

- Suggestion noted and incorporated in the revised manuscript.

L.227 – What is the source of the GSB data? Is the homogenized time series used? Because there is quite a significant difference from the non-homogenized data.

- Dear reviewer, the GSB data comes from MeteoSwiss (https://www.meteoswiss.admin.ch/home/climate/swiss-climate-in-detail/homogeneous-data-series-since-1864.html?station=gsb).

Yes, the data we have used is homogenized, available from 1864 till today. We will also mention this important point in the manuscript.

L.243 – Using the GSB data for precipitation as a proxy for the MBM area is a bit tricky, as the GSB pass is located in the "shadow" of the MBM massive by "westerlies" and is largely influenced by precipitation coming from the south. But I know, this is difficult to do better.

- We agree there is a bit of uncertainty generated because we use the GSB data to calculate our precipitation proxy (especially from 1952 to 1959), where we lack the SAFRAN datasets. Although Col du Saint Bernard in Aosta valley represents almost similar conditions to the MBM, as you mentioned, certain site-specific differences exist which are hard to overcome and somewhat beyond our control. We are unfortunately also limited with data available from any other source for such a long term.

L.330 – Figure 3 is not depicting precipitation.

- Thanks for pointing this out. We have revised this in the manuscript and mentioned temperature here.

L.510 – There is some issue with the values of area loss and their correspondence to

figure 8 :

The area reduction is 2001 compared to 1952 is 25.4 % and must be rounded to -25%

31% is the relative area loss in 2019 compared to the 2001 area (this must be specified) or recalculated to the 1952 area. In addition, this value is wrong as it is obviously the addition of the area reduction in 2012 to 2001 and in 2019 to 2012. The area reduction in 2019 compared to 2001 is 28.8%

The "alarming rate" is not provided but left to the calculation by the reader. The values must be presented, for instance as an average annual rate compared to 1952, which appears to be 0.5%/year from 1952 to 2001, 1.1%/year from 2001 to 2012 and 1.2%/year from 2012 to 2019.

- Thanks a lot for pointing out this critical error in the text. We corrected this in our text in section 5.2. We have also mentioned the area loss rate, as you suggested in the revised manuscript.

L.617-618 - The evolution of the accumulation rate must be provided as well

- Dear reviewer, as per the previous comment, we can add a new graph in the manuscript to show the evolution of the accumulation rates and mention this also in the text.

L.636 – 4 IAs are increasing is size. Is this significant for all ? Where are these 4 IAs located ? Maybe worth of providing a picture of each ?

- Dear reviewer, thanks for pointing this out. It will be relevant to give information about these IAs. This is not significant as other IAs do not show the same trend. But nevertheless, for future analysis these four IAs could be of interest. The 4 IAs in the question here are: 2 IAs on the N and NW face of Rochers Rouges Inferieurs (~4350 m a.s.l. and 4050 m a.s.l.) near the Grand Plateau, 1 IA on the NE face of Col de la Brenva (~4160 m a.s.l.) and 1 IA on the S face of Col du Bionnassay (~4050 m a.s.l.). As observed, all these IAs are located at elevations higher than 4000 m a.s.l. It can be expected that a few IAs could show an increase in surface. However this increase in surface area is not dramatic (~10 % increase in surface area).
  The pictures of all these IAs can be provided, however since the number of images in our manuscript is already very high, maybe we can provide them as supplementary material instead of adding them in the manuscript.

L.678 – The comment on L. 510 must be considered and the sentence adapted in Accordance

L.680 – The "climate forcing parameters" must be specified

L.683 – 685 – This bullet can be omitted at it would not say anything else than the next one, but keeping vague ("some topographic factors… , while other factors…")

- All the following comments were noted and incorporated into the revised manuscript.

Figures

As a general comment for the figures: the layout must be improved for many of them. The legibility must be checked, the character size must be homogenized and made large enough, the use of caption and brackets in the axis legends must be homogenized, all unnecessary surrounding boxes (e.g. fig. 9 – 10) should be removed.

- Dear reviewer, thanks for this general remark. We will consider all your suggestions and try to homogenize all the images in the revised manuscript.

Figure 3 – The figure is not legible. If it is meant to show an annual cycle at different elevations, it must provide just one year (which could be the mean 1952-2019). If it is meant to show the overall trend, only a running annual (or multiannual) mean should be represented.

Why 8/1/1952 in the time axis ?

What does the box "Interpolated data from GSB temperature" mean ? Better to insert an arrow to the 1952-1958 box.

There are two issues with the blue (2400 m) curve. First, it is mostly shifted in comparison to the other (e.g. for the last years, the peak temperature is appearing in winter). Second, there is a peak temperature apparently in 1985, which has never occurred. There is a mistake somewhere. July 1983 was extremely hot, but nothing occurred in 1985.

- Dear reviewer, thank you for pointing out this error with the graph. We agree with your suggestion completely as the previous graph did not provide much information. We have

made a new figure to show the multiannual variation of mean temperatures from 1952 to 2019. The new graph shows mean annual temperature values for every year from 1952 to 2019. The figure is attached at the end of the document for your reference.

Figure 4 – Again, there is an outlier at about +14°C (in SAFRAN), which is doubtful. This is probably the 1985 peak mentioned above… but why not at the same temperature (+15°C in Fig. 3) ?

- We cross checked this value again from the SAFRAN data. The value (13.7 ^{o}C) actually comes from August 2020. We also believe this is an outlier value not truly representative of the actual temperature during this month. But, SAFRAN data shows this value. We can remove this data point as 2020 is not part of the observed study period and give a new figure in the revised manuscript.

Figure 5 - Yellow on white is not adequate. Maybe orange ?

- We will redraw these images again, taking into consideration your comment.

Figure 10 – The layout (legend, axis label, dot size, etc) must be homogenized and made legible on all figures.

- We will redraw all these images taking into consideration your comments.

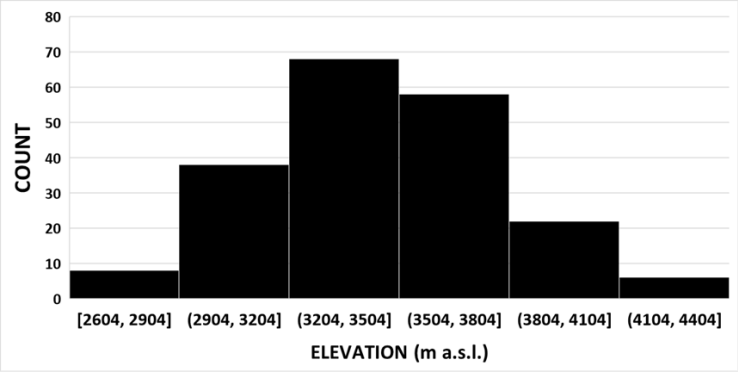
Figure 11 – What are the represented values ? What is for instance a PDD ranging from +14 to +30°C at 2400 m ? I don't understand.

- Dear reviewer, the values represent annual PDD values in ^{o}C. This is to show the evolution of the temperature proxy since 1952. We realized the axis label on the y-axis should be PDD (^{O}C) to avoid confusion. We changed this in the revised manuscript. PDD ranging from 14 to 30^{o}C is the annual PDD value for each year (the sum of all positive mean monthly temperatures in one year).

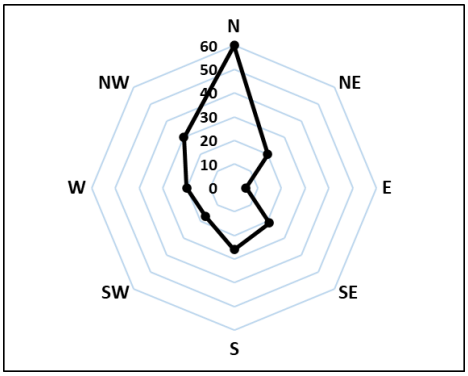
Figure 12 – Legend … "The colour and size of the ticks represent the mean elevation of the IA". I guess the colour one is representing the elevation, the dot size being representative

of the IA size (in this case, the legend must be provided)

- Dear reviewer, in our case, both the size and colour represent the mean elevation of the IA. We realize it is probably unnecessary to represent the same information in two different ways. It is just for better visual interpretation. We can change all ticks to the same size if it creates unnecessary confusion.



(a)



(b)

Figure: Distribution of the IAs with: a.) elevation and b.) Aspect

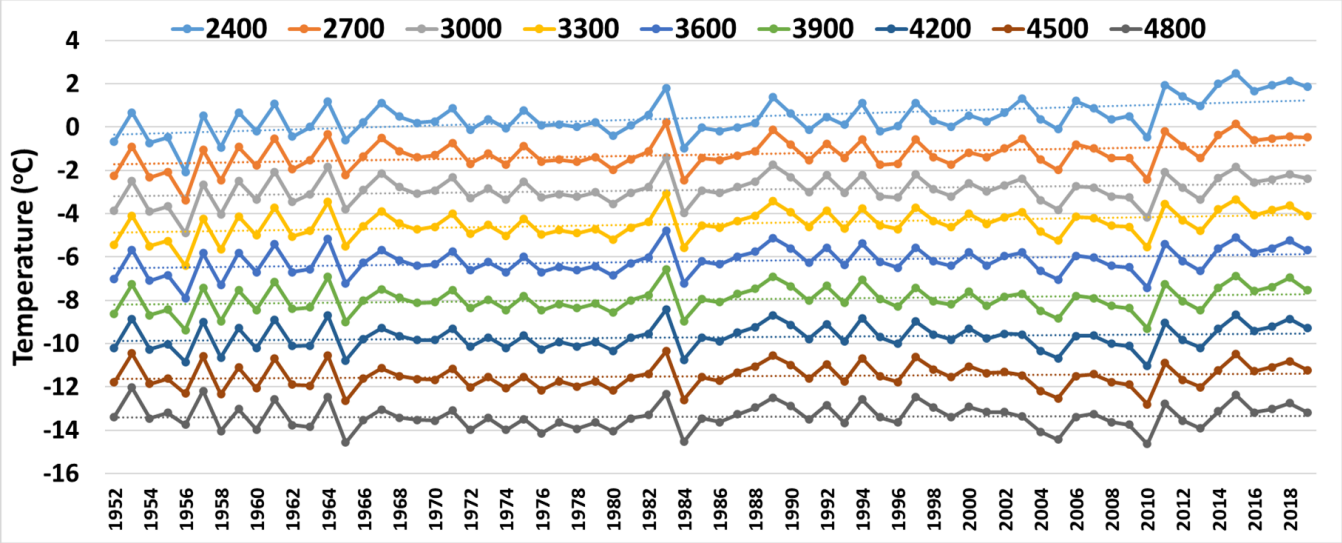


Figure: Variation of mean annual temperatures from 1952 to 2019 at different elevations.

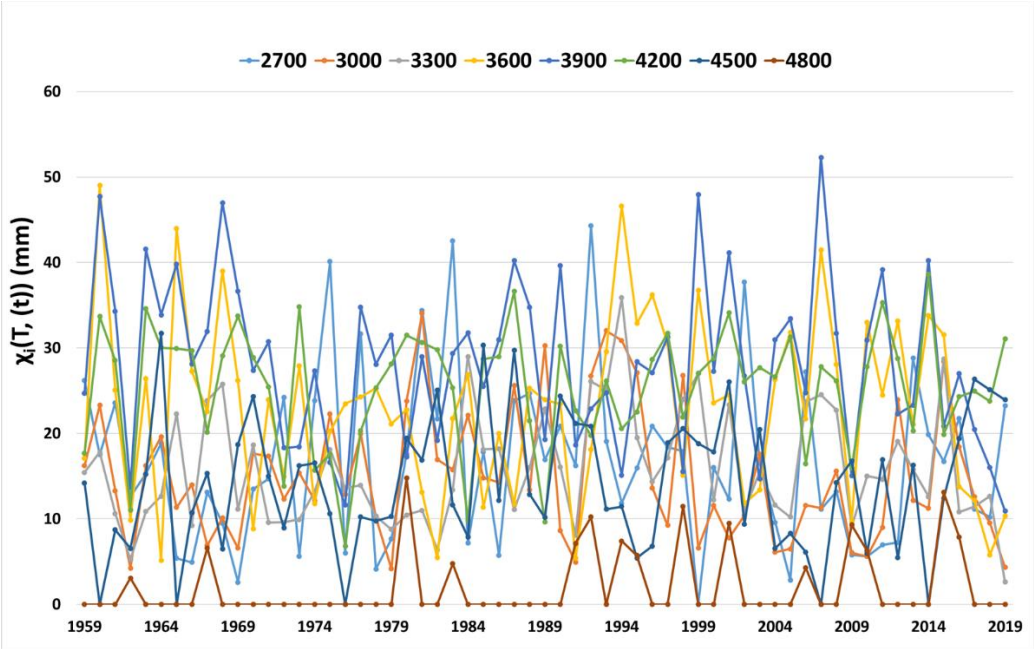


Figure: Variation of precipitation occurring between -5 and 0°C at different elevations from 1952 to 2019.

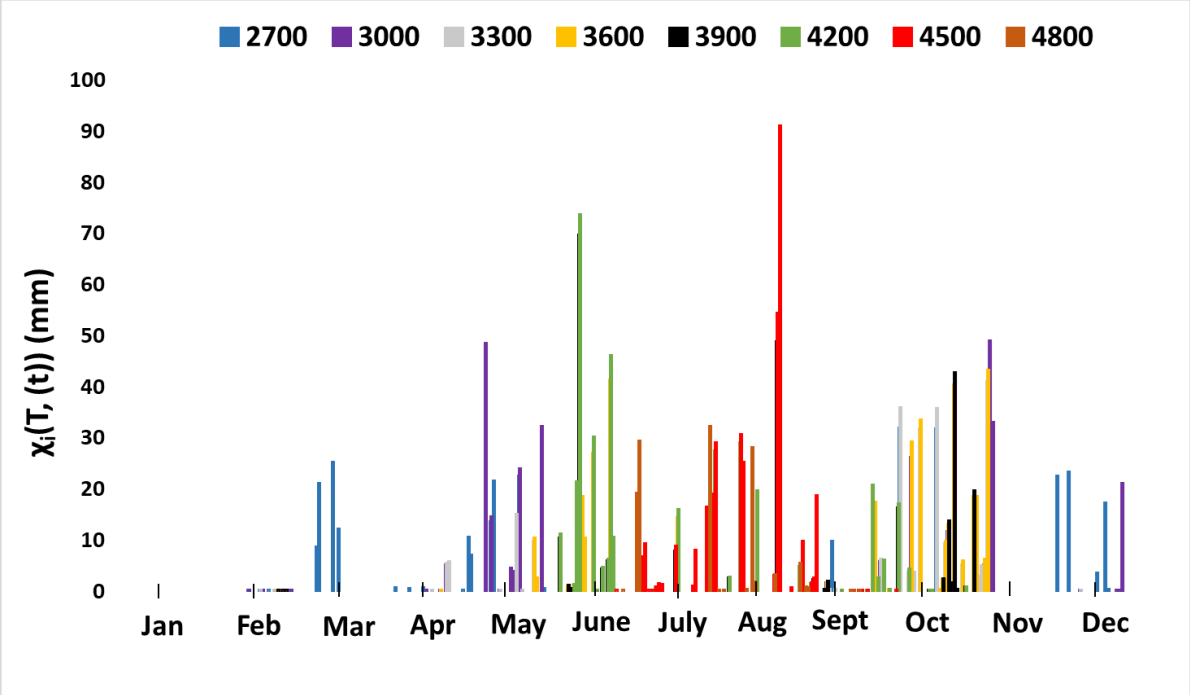


Figure: Variation in the total precipitation occurring between -5 and 0°C through 2019 at different elevations.

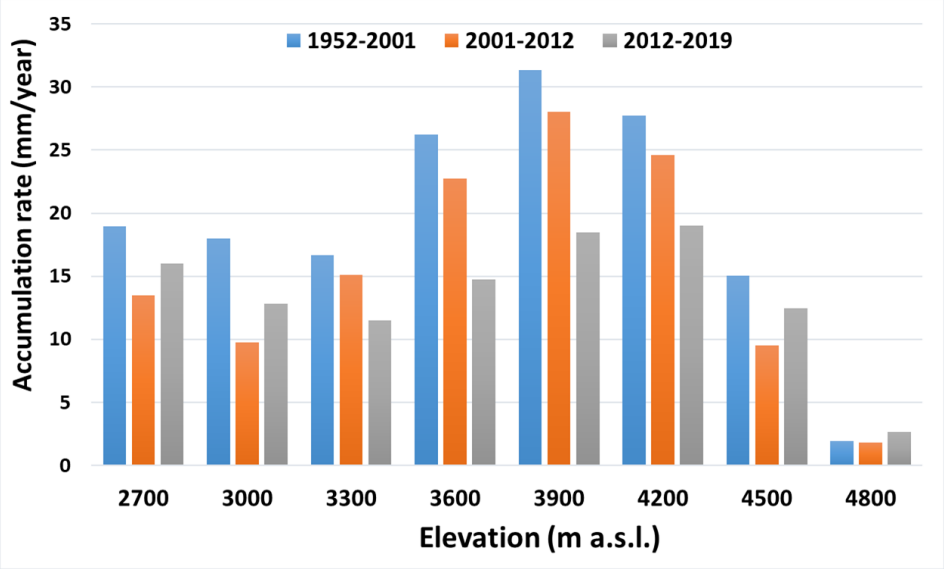


Figure: Variation in the accumulation rates at different elevations for each time period of observation