The authors are very grateful to the reviewers for their careful and meticulous reading of the paper. The reviews are detailed and helpful to finalize the manuscript. The authors would like to kindly acknowledge them. Here follow our comments to the major concerns and answers to specific points. Reviewers' comments are repeated below (in italic), they are discussed (normal type style), and, when needed, a suggested new text version is given in quotation marks.

General authors' comments to the three referees on the purpose of the investigation of the NAO's influence on Sarennes' mass balance components

It seems that our objective to study the relation of Sarennes seasonal mass balance terms with the NOA was not clear for the reviewers. Searching a relation between Alpine glacier mass balances and a synoptic or large scale driver as the North Atlantic Oscillation (NAO) index is an old, important but still partially unsolved question. It is justified as the annual variability of annual balance should be related to the different types (moisture, temperature) of westerly air mass transfer into Europe and therefore to atmospheric pressure fields reflected by the NAO. During winter months, the NAO is particularly responsible for much of the variability of weather conditions in the North Atlantic region. Therefore, a proper relation should exist between NAO and especially winter mass balances for Alpine and Scandinavian glaciers, as well as glaciers at the eastern boundary of the North Atlantic Ocean.

Such a relation is indeed reported by (Six et al., 2001) for Alpine and Scandinavian glaciers but for annual balances solely, and with rather weak correlations. Here, having the longest record of winter balance extracted form the variance analysis as well as underlying long term trends and breakpoints in the series made us thinking that we could bring new elements regarding this investigation. Indeed, long time series (few decades) of glacier winter balance are not widespread and winter accumulation has a finest and more tenuous influence on annual balances in the Alps. Thus, such a winter balance series has a proper time structure resulting from a climatic signal which may be hardly perceptible in an annual mass balance series. Moreover, previous studies have shown that signals from Alpine glaciers correlate poorly with NAO, while results for Scandinavian glaciers are slightly better. The higher significance of the relation for Scandinavian glaciers with regards to the Alpine's one has been mostly explained by the greater importance in winter precipitations on Scandinavian glaciers.

Choosing instead the Atlantic Multidecadal Oscillation (AMO) as proposed by the review is substantially very different as AMO is a temperature signal. It is well established that temperature signals correlate well at long distances (e.g. Böhm et al., 2001). As annual mass balances and summer balances are very well correlated with temperature (71 % and 89% of common variance at Sarennes), it is reasonable to think that a well marked relation between Sarennes summer and annual glacier mass balance and a large temperature index as the Atlantic Multidecadal Oscillation exist but that would not have brought especially new results with regards to those already published by Huss et al. (2010) for Switzerland.

Referee #1

1. Novelty of the study: *Although the analysis is comprehensive the authors might try to better work out what is new and "exiting" about their study. The last years have seen quite a number of publications about the Sarennes mass balance series (Thibert et al., 2008, JoG; Thibert and Vincent, 2009, AoG; Eckert et al., 2010, JC; Eckert et al., 2011, JoG). Therefore, it is important to clearly make the separation of these studies which are based on the same data set.*

- Thibert et al., JoG, 2008 is a comparison of geodetic and glaciological methods on Sarennes data in order to check and validate field data which can potentially be affected by systematic error over decades. As a result of the comparison, glaciological mass balance data are found to be consistent with geodetic ones. Hence, this paper is mostly dedicated to the error analysis and the metrological quality of Sarennes data set.

Following this first methodological paper, Thibert and Vincent, AnG, 2009, propose a method to combine geodetic and glaciological methods instead of using one series or the other separately and exclusively. The glaciological method provides annual time series at point locations at the glacier surface, while the geodetic one gives a spatially integrated balance over the glacier area and a cumulated balance over a multiannual period. The paper presents how their combination produces the best possible estimation of the mass balance, associating the annual resolution and the glacier-wide features of the series.

- Eckert et al., JC 2010 is a paper about temporal fluctuations of avalanche runout altitudes over the 1946-2006 period that uses similar methods and highlights a similar change point.

- Eckert et al., JoG, 2011, presents a method to extract refined temporal signals (annual and long term underlying trends) from the 6 decades of winter and summer mass balance records at Sarennes. Again, variance analysis is used to separate the spatial variability at the glacier surface from the overall temporal variability, but the method is implemented with advanced statistics (Bayesian estimation carried out through Monte Carlo simulations), so as to reconstruct missing values, assess the variables inter-correlation, detect possible change points in underlying trends and evaluate the related uncertainties. Among the few tested models, one with two separate change points in winter and summer series was found relevant for Sarennes.

In the present paper, we use the temporal signals in the winter and summer balances terms inferred from this data in Eckert et al., JOG 2011 to find how they are related to climatic signals of temperature, precipitation and the larger scale NAO anomalies. This main objective and its relation to previous papers will be more precisely stated in the introduction of the revised version of the paper.

2. Surface elevation change. *Relating measured mass balance at point locations to climatic drivers is straightforward as the dynamic response of glacier area can be neglected. I however assume that some of the measurement points experienced a quite significant decrease in surface elevation also having an impact on the relation of local mass balance to climatic forcing. The lowering of the surface and the potential effect on the results should be discussed, and if necessary, be corrected.*

It is true that altitude change can result in enhanced ablation at each measurement site. This then can produce a long term trend contribution in the mass balance. Altitude change at Sarennes can be calculated from photogrammetric survey in 1952 and 2003, which give a mean of -36 m over 51 years (Figure 1 of this document, Thibert et al., 2008, p.526). As plotted in Eckert et al., 2011 Figure 10 and 11, this altitude loss occurred mainly before 1976 for one third and after 1982 for two third, which results in mean annual rates of altitude loss of -0.51 and -0.92 m yr-1 over these 2 periods respectively. Regarding only the last period where the maximum of altitude change occurred, the forcing due to altitude loss can be calculated from the sensibility of the mass balance we find at Sarennes, which is +0.61 m w.e.yr^{-1}(100m)$^{-1}$. This results in a contribution of -0.56 cm w.e.yr^{-2} to the long term trend of the annual balance. The long term trend in the annual balance over the 1983-2007 period is -3.54 cm w.e.yr^{-2} (from Figure 2a of the paper). Therefore, 16 % of the long term trend in the annual balance can be explained by the forcing due to the altitude loss.

In terms of temperature forcing, the equivalence of the -0.56 cm w.e.yr^{-2} can be quantified using the sensibility of mass balance to temperature we have found at Sarennes from our work

(-0.62 m w.e.yr^{-1}°C^{-1}). This gives an equivalence of +9.10^{-3} °C.yr^{-1} over the last 25 years of record (+0.22 °C over 25 years). From the temperature signal of Figure 4 a, this is 13% of the long term trend which is about +0.0712 °C yr^{-1} (+1.78 °C over 25 years).

From these 2 calculations, we can conclude that, without being negligible, the altitude loss at Sarennes has a limited effect on the mass balance record, and its long term trend is not affected too much by the glacier topographic feedback. The authors propose to include some of the above details in the revised version of the paper.

3. Sensitivity to temperature and albedo change. *The authors find a constant sensitivity of mass balance on air temperature throughout the entire study period. The implications of this in terms of future climate change and glacier evolution could be discussed in more detail. On many alpine glaciers a decrease in surface albedo was observed in the last years (e.g. Oerlemans et al., 2009, JoG). I assume that this might also be the case for Sarennes. Could the authors comment on this, and investigate whether a potential decrease in surface albedo (or a change in the energy balance components in general!) might cause a change in the sensitivity of mass balance to temperature change. As this sensitivity is basically the degree-day factor that is used in many modelling approaches of snow and ice melt, a more in-depth assessment of this issue might be of major interest to many readers.*

Effectively, we find constant sensitivity for snow and ice ablation throughout the period of record. We don't have long term records of energy balance as provided by Oerlemans et al., JoG, 2009, to support firm conclusion regarding whether albedo has changed or not over the last years of the record. We however made some measurement of ice albedo (October 2011, using Kipp and Zonen CMA11) and found α=0.43±0.04 at 5 locations on the glacier in the ablation area.

This albedo is typical for clean ice (Patterson, 1994, p.59). Sarennes is effectively not covered by a lot of debris or dust (except very locally), and Figure 2 of this document gives an image of Sarennes at the end of the ablation season.

Ice albedo at Sarennes is high (0.43 in 2011) and it is rather difficult to consider that it was considerably higher in the past, as a maximum of 0.51 is known for very clean ice. Anyway, under the assumption of a decrease of this albedo from 0.51 at the beginning of the record to 0.43 as it was measured in 2011, it is possible to calculate what results from in the surface energy balance. Over a typical 39 days ice ablation period (22 August-30 September, Table 5 and Figure 9), at the latitude of Sarennes (N 45°07'), the mean global radiation at the top of the atmosphere is 310 Wm^{-2}, tacking into account shading and glacier slope. Using a total effective transmitivity of the atmosphere of 0.64 inferred from our weather station data (unpublished results), we consider a mean global radiation of 200 Wm^{-2} along the ice ablation period.

If the mean albedo over 1949-2007 is 0.47 (decreasing from 0.51 to 0.43), the mean short wave net flux is 106 Wm^{-2} (increasing from 98 to 114 Wm^{-2}, i.e. +16 Wm^{-2} between 1949 and 2007). All other components of the surface energy balance being maintained unchanged, this additional energy flux causes an ablation forcing of 0.16 m w.e. over the 39 days of the ablation season, using a latent heat of melting for ice of 335 $kJkg^{-1}$. This melting change is divided in -0.08 m w.e. and +0.08 m w.e. around the middle of the period of record, roughly 1980. As the mean ice ablation has changed of +0.73 m w.e. between the 2 periods identified in our paper (Table 5), this change in albedo could contribute to 11 % of the melt change around the mean value.

Considering that the mean of ice ablation measured over the overall period of record (1949-2007) is 1.01 cm $w.e.yr^{-1}$ for a 148 °C day amount (sensitivity 0.68 $cm.w.e.°C^{-1}.day^{-1}$), if the albedo change is not taken into account, the ablation change affects erroneously the sensitivity of ±8 cm w.e. over 148 °C.day = ±0.054 cm w.e. $.°C^{-1}.day^{-1}$. Consequently, and as shown in Figure 3 of this reply, the right sensitivity should be in the range 0.63-0.73 $cm.w.e.°C^{-1}.day^{-1}$.

The effect of the potential albedo change on the sensitivity is therefore limited. It is of the same order of magnitude than the year to year variability of the sensitivity as plotted in Figure 11 of the paper. This is due to the high albedo that we know for this glacier (0.43 in 2011) and the relatively short duration of the ice ablation period (39 days in mean) in relation to the high altitude of the glacier (2840-3050 m a.s.l.).

Identically, this unsuspected albedo change would be (wrongly) interpreted in an equivalent atmospheric temperature change of ±0.12 °C using PDD model and considering that the sensitivity is unchanged. This illustrates the limits of all PDD approaches. But this is anyway only 9 % of the temperature change measured by Lyon weather station since 1982.

All in all, an albedo change for ice is therefore possible at Sarennes since the beginning of the record, but it cannot be retrieved from our data, particularly the sensitivity of ice ablation to temperature along time. Furthermore, from the above calculation this possible change seams to have a small influence on our analysis and in our conclusions. We propose to add these precisions to the revised version of the paper.

4. Prolongation of the melting season. *I find the quantification of the relative importance of melting season length on the recent increase in melt rates intriguing, and the* *authors might want to highlight this result even more. Why do the higher temperatures only have such a small effect? Is the simple relation "higher temperature cause more melt" a misunderstanding, and should we direct more of our future attention to the prolongation of the melting season? Additional analysis of the drivers for the high relative importance of melting season prolongation would be most interesting and contribute to the quality of this paper.*

As pointed out by the reviewer, regarding the relative importance of the melting season prolongation (65%) and higher air temperatures (35%) on the mass balance change, the direct effect of temperature may appear small. But one should remind that we are focusing on a high elevation glacier (between 2900-3000 m a.s.l.) with surfaces close to the equilibrium line. As reported in many papers, the sensitivity of the mass balance decreases with altitude (Vallon et al., 1998, Braithwaite and Zhang, 2000; Gerbaux et al., 2005). From our data, the sensitivity is around -0.6 m w.e. $yr^{-1}°C^{-1}$ at this altitude while a sensitivity higher than 1 m w.e. $yr^{-1}°C^{-1}$ is expected around 2400 m a.s.l. (Vincent, 2002). Ablation at this altitude is not just depending on temperature but on the albedo. Close to the equilibrium line, the surface albedo is depending on the winter balance and on the relative ablation duration for snow and ice along the season. Snow at the surface is prevailing with 86 days while ice is just exposed to the atmosphere over 39 days. Long lasting conditions for melting at the end of the ablation season indeed depends on temperature but on the occurrence of the first important early winter snow fall that will definitively end the ablation season. This does not mean that surface mass balance is not a good *proxy* for studying climate changes in mountain area, but that the best location at a glacier surface is rather at lower altitudes where mass balance is a more direct and sensitive indicator.

We would anyway recommend directing our attention to both melt intensity and duration. However this requires adopting the stratigraphic method for mass balance measurement rather than the fixed-date method (Cogley et al., 2011).

5. ***NAO effect.*** *The discussion of the effect of the NAO on Sarennes Glacier mass balance are not very conclusive and I asked myself if we know more after the analysis. However, I am not sure to provide any good advice how to strengthen this section. Furthermore, is the smoothing applied to get a significant correlation is statistically sound? When producing smoothed time series based on running means, the number of data points stays the same, but every annual data point is used more than once (running mean!) in the correlation. Maybe the authors might comment on that. I would rather propose to build* *pentadal / decadal means first, and then correlating these low-frequency components. In the very last sentence of the paper the authors take up the issue that Alpine mass balance could also be correlated with the Atlantic Multidecadal Oscillation (AMO). It is not clear why it was decided to use only the NAO for detecting potential large-scale drivers and climatic control on the mass balance of Glacier de Sarennes, and to neglect other possible indices. As it stands now the statement of the last sentence remains poorly supported by the analysis performed in this paper.*

For the choice of the NAO variable instead of other atmospheric indexes, the method used and the interest of the obtained results, see our general introduction and the detailed response to the main comment of referee 3.

Specific comments referee #1

1. *Rank citations consistently, i.e. after date of appearance.*

As far as we check, we follow the editorial recommendations in ranking citations with specifications for single, co-author and team papers

2. *page 2119, line 8: When discussing the Claridenfirn data (almost 100 years of seasonal mass balance measurements), also the equivalent series of Aletsch and Silvrettagletscher should be mentioned.*

According to the last available WGMS mass balance bulletin (n°. 11; 2008-2009) published in December 2011, longest and ongoing mass balance records in the Alps rank as:

- Sarennes since 1949 (France)
- Kesselwandferner and Hintereisferner since 1953 (Austria)
- Saint-Sorlin 1957 (France)
- Stubacher/Sonnblickees 1959 (Austria)
- Silvretta since 1960 (Switzerland)
- Gries since 1962 (Switzerland)

This list can be completed by:

- Claridenfirn since 1914
- Aletsch since 1923 (Aellen and Funk, 1990) which results from the hydrological method

We will add some information in the revised version of the paper.

3. *page 2121, line 25: How were the trends determined?*

Trends are quantified by parameters c_j and d_j in equation (2). Estimation is performed using simulation methods, typically Markov Chain Monte Carlo methods as detailed in Eckert et al. (2011).

4. *page 2127, line 10: Is there some explanation why the findings of this study are inconsistent with the results by Beniston and Jungo (2002)?*

No at present, at our knowledge it is still unknown why winter snow and weather patterns are better correlated to the NAO signal in Switzerland than in the French Alps, but the results we obtain concerning winter balance seem in good accordance with the findings of Durand et al. (2009a, b) indicating a very weak correlation. See also the detailed response to referee 3.

5. *page 2127, line 16: Clariden*

This will be corrected in the next version

6. *page 2128, line 13: Is it possible to compare these increases in melt season length with studies for other glaciers? Are the findings consistent for the Alps, or even beyond?*

Yes, it will be interesting. The ability to detect some expansion of the ablation duration depends on the method applied for mass balance measurement, i.e. fixed-date or stratigraphic methods (Cogley et al., 2011). With the fixed-date method, no change in duration can be detected. In the comparison paper between Claridenfirn and Sarennes glacier, Vincent et al., (2002) were forced to correct the dates to calculate a melt rate over the same period because measurement dates did not match, and no change in ablation duration vas investigated. There is still to do on the topic.

7. *page 2128, line 21: Here, and for other analyses in this paper, snow and ice ablation is separated. The authors state that this is done directly based on the field measurements. However, some more details on this procedure need to be given. According to the data section, mass balance readings are performed 6-7 times throughout the melting season. But is this sufficient to obtain a daily accuracy that is needed for the investigations carried out in this paper? Probably some temporal interpolation between the individual measurements is performed. This should be described, and the authors should address the potential uncertainties in their conclusions (e.g. melting season length and sensitivity to temperature change) due to their approach.*

Yes we used temporal interpolation between visits do get the date of the snow-to-ice surface change. The recurrence of visits is adapted along the ablation season, from one per month in the first April-May June, to one per 15-20 days in July August. Then again one in September, while the last ones are adapted to the early winter snow falls. We estimate inferring the snow-to-ice transition date within a few days. However, this uncertainty is random and does not affect the detection of the systematic shift in the snow-ice transition date. We will make this clearer in the revised version of the paper.

8. *page 2129, line 24: Omit ”Glacier” (only Claridenfirn)*
This will be corrected in the next version

9. ***page 2130, line 15:*** *How does this sensitivity compare to degree-day factors in temperature-index models, as e.g. compiled by Hock (2003, JH)?*
A variety of studies can be used for sensitivity comparison. Using the compilation of Hock (2003; Table 1, p. 106), sensitivities for snow range from 0.27 to 1.16 cm.w.e.°C^{-1}.day^{-1} with a mean of 25 values/sites equal to 0.48 cm.w.e.°C^{-1}.day^{-1}. Again for snow, Braithwaite and Zhang (2000; Table 4, p.9) provide very close values with a mean of 0.41 cm.w.e.°C^{-1}.day^{-1} among 13 values/site ranging between 0.28 and 0.57 cm.w.e.°C^{-1}.day^{-1}, excluding the former result reported for Sarennes (Vincent and Vallon, 1997). Our value of 0.41 cm.w.e.°C^{-1}.day^{-} is therefore in very good agreement.
Considering ice sensitivity, Hock (2003) reports a mean of 0.87 cm.w.e.°C^{-1}.day^{-1} for 33 values ranging from 0.55 to 2 cm.w.e.°C^{-1}.day^{-1}. Braithwaite and Zhang (2000) indicate a close value of 0.85 cm.w.e.°C^{-1}.day^{-1} (range = 0.55-2.2 cm.w.e.°C^{-1}.day^{-1}) for 26 values. In these 2 papers, data from Greenland are included where systematic high sensitivity are observed for ice (higher than 1 cm.w.e.°C^{-1}.day^{-1} for Thule Ramp, Camp IV-EGIG, GIMEX profile sites).
For alpine glaciers, most of the sensitivity values range between 0.50-0.94 cm.w.e.°C^{-1}.day^{-1} which is consistent with our value of 0.68 cm.w.e.°C^{-1}.day^{-1}. A paragraph summing up the above results from bibliography will be added in the revised version of the paper.

Referee #2

Specific comments

1. *Homogenized times series. It is not clear from the paper, whether you used homogenised time series of the stations Lyon-Bron and Besse. Please give the source of the data source and state that the data are homogenised. If you did not use homogenised data, please do so, because otherwise you cannot be sure, that the detected change points are real climate signals rather than inhomogeneities in the data series.*
Temperature and precipitation data come from Méteo-France as mentioned on line 7 of p. 2122 and p. 2123 line 2. Météo-France only provides homogenized data (it is almost impossible to get not homogenized data from Météo-France). This will be specified and in the next version of the paper.

2. *NAO and mass balance. Concerning the part of the paper which links mass balance to NAO: Please consider motivating more clearly, why you carry out that analysis or/and shorten that part of the paper. It reads as if the aim was obtaining optimal correlation between NAO and mass balance. What is the gain in knowledge of correlating the NAO index with glacier mass balance?*
Again, regarding the choice of the NAO variable instead of other atmospheric indexes, the method used and the interest of the obtained results, see our comment in the introduction together with detailed response to the main comment of referee 3.

3. *"Synoptic" term. The NAO is an index for larger-scale atmospheric circulation, but a weak predictor for the synoptic situation. If your goal is to show the linkage of glacier mass balance with synoptic patterns, I think you should rather use an index of weather-type classifications. I don0t agree with your use of the word "synoptic". Synoptic systems have typical time scales of some days. An averaged NAO over several months is even lesser an index to describe "synoptic" conditions. Depending on what you want to show, maybe "larger scale atmospheric pattern" is a more adequate term (as you use it in p 2118 line 23), rather than "synoptic".*
There is in fact no well establish consensus about whether the word "synoptic" has a temporal sense or not, it depends of the community, meteorologists or climatologist. To avoid

ambiguities, we will nevertheless follow the suggestion and replace “synoptic” by “large scale atmospheric index” in the next version of the paper.

4. *Correlation between of smoothed series I think it is problematic to calculate correlations of smoothed time series. You discuss that (p. 2125, line 8), but then you do it anyway and justify it by citing other studies. I don0t understand or cannot find a plausible physical reason, why you have to smooth the NAO before comparing it to local data. Just to increase correlation?*
For the general problem of correlating smoothed series and the method used and its results for the NAO index, see our detailed response to the main comment of referee 3, especially the comment on “data processing method”

5. *p. 2123 line 235. You justify smoothing NAO by "..seasonal synoptic patterns can influence local climate for longer than they actually last (van Loon and Williams, 1976)“. That is not convincing, because related to your work, it means, that the mean NAO index of one year (or season) has an influence on the following years (or seasons) of glacier mass balance. I think if you argue like that you should propose a physical mechanism how this should work.*
We are not climatologists and our goal is not to propose, in a first step, a new mechanism, just to report and describe facts (see our detailed response to the main comment of referee 3 about correlation between smoothed data and link between underlying trends in temperature, precipitation, mass balance series and NAO). Nevertheless having some persistence/memory is the atmospheric system seems not counter-intuitive for us. Anyway, our formulation which may be seen as abrupt will be smoothed in the next version of the paper.

6. *p. 2135 line 14-19 : you write "The physical explanation proposed by Beniston and Jungo (2002) for the positive correlation between NAO winter anomalies and high altitude summer temperatures...“ I don’t find any statements concerning winter NAO and summer temperatures there and I don’t understand why that should be connected.*
OK, since we do not have a physical explanation for this (see our response to the general comment of referee 3), we will just present our result as a surprising fact, possibly an artefact, but which warrants further investigations.

6. *p. 2135 line* 14-19 : *".. inducing vertical atmospheric circulation, decreasing cloudiness, and thus persistent warming“. Consider writing "inducing downward atmospheric motion..“ or "subsidence“ instead to make clear that it is not ascending motion that is induced.*

This will be changed in the next version of the paper.

7. *p. 2138, line 1-4: p. 2138, lines 1-4: "...the NAO index... corresponds to the influence of blocking events in winter or spring on summer balance.“ Isn0t that too speculative to have that in the conclusion section? As far as I know, this has been observed in the last 2 decades only, so it might not be representative for the whole mass balance period of Sarennes?*
OK, we will make a distinction of this in the next version of the paper.

8. *You show that temperature sensitivity of stake 4 has no trend over 6 decades. That is interesting as it is contrary to results of other studies. Is that likewise true for the other stakes? I think the paper would benefit being more focussed on results on the relationship of mass balance data to local meteorological data, because here you can come up with a lot of quantitative results.*
We have analyzed that point in section 3. **Sensitivity to temperature and albedo change** in the **Referee #1** section. We have also analyzed the detailed record of stake n° 2 and went to the same observation that no trend is detectable among the interannual variability of the sensitivity. Sensitivity is a little bit higher at the downhill stake n°2 with 0.71 cm.w.e.°C^{-1}.day^{-1} while Vincent and Vallon found 0.62 cm.w.e.°C^{-1}.day^{-1} at stake n°3 over a shorter analyzed period (1949-1994). Again considering variability in the times series as well as the spatial variability that is expected from albedo and roughness at he glacier surface, disparity of these values can be considered as reasonable.

9. *Surface elevation changes*. Did you correct the stake elevation for surface lowing during the 6 decades? Could you comment on that?
This question has been pointed out by referee #1 as well, so please refer to the point 2. **Surface elevation change** in the **Referee #1** section

10. *Title: I think the title might be somehow misleading. Reading only the title I would expect an analysis which covers all possible "drivers“ of glacier mass balance to determine their specific importance at Sarennes glacier, but your analysis is based only on precipitation and temperature. In p 2122 line 1 you write "To infer potential drivers, we relate...“ This also leads to the expectation that you have included at least radiation in your analysis, because radiation is the main "driver“ of ablation in a physical sense, whereas temperature is just a* *good "predictor“ of the summer mass balance, because it is a good proxy for all related processes.*

We don’t think that the title may misguide the reader. The authors agree that estimation of radiation fluxes, as well as a complete surface energy balance model would provide a finest and real physical analysis of the variations and trends identified in the mass balance series. However, long time series of radiation fluxes (short and long wave) are not available to cover the 6 decades of Sarennes record and a parameterization using meteorological data is necessary to reconstruct downward radiation series in the past. This should be the purpose of a further paper, but we think that it requires first an additional period of record over which direct measurements of radiation should be performed to validate the parameters to be used in such reconstructions.

11. *p. 2124, line 10. "However, ...NAO series cannot be statistically analysed in the same way as the Sarennes data and local covariates.“ Why not? As I understand, you do the same analysis, but just the outcome is different in the sense, that you get two change points instead of only one.*

This sentence means that variance decomposition is not useful as the time signal coming from local and larger scale covariates has not to be separated from a spatial variability. The time signals are analyzed in the same way.

12. *Table 2 and 3. Tables 2 and 3: Lyon temperature means May-October daily means temperatures as in Fig. 4? And Besse precipitations means Besse winter precipitation as in Fig.4?*

It is right: temperatures are Lyon May-to-October daily means (as plotted in Figure 4a) and precipitations are Besse winter precipitations cumulated between Sarennes field visits days (Figure 4b) and Figure 6 as well)

13. *p. 2116 line 1-2: That has been done in the previous paper by Eckert et al. and I think that should be made clear.*

Effectively, the 2 first sentences of the Abstract should be rather written as:

“Refined temporal signals extracted from a glacier winter and summer mass balance series recorded at Glacier de Sarennes (French Alps) using variance decomposition are related to local and synoptic meteorological data in terms of interannual variability and structured trends.”

…without considering here our specific comment on the use of the word “synoptic” discussed above.

14. *p 2119, line 19: Is there a reason why you didn0t update the dataset to 2011?*
Indeed, the period has been maintained unchanged for consistency why the former analysis of Eckert et al. (2011) in which the data treatment was proposed for Sarennes. Expanding the period to the last years of the record would result in slight changes in inferred trends. It is largely feasible but may be confusing for the readership. The next re-analysis of the series will be conducted with the following photogrammetry in 2012 or 2013.

Technical corrections

p 2120, eq (2): please specify the meaning of N
Letter “N” denotes the normal distribution (Gaussian distribution law) whose mean is c_j+d_jt, and whose variance is $(\sigma_j)^2$. This will be added in the revised version of the paper.

p 2122, line 10: here you cite (Böhm et al., 2001), but it is not included in the references
The reference is:
Böhm R, Auer I, Brunetti M, Maugeri M, Nanni T, Schöner W, Regional temperature variability in the European Alps 1760-1998 from homogenized instrumental time series, International Journal of Climatology, 21, 1779-1801, 2001.

p 2134, line 2: replace "snow“ with "ice“ before "sensitivity“
Effectively, it is ice sensitivity.

Referee #3

General comments

…However, I feel that the synoptic dimension of this research is weakly developed and needs to be addressed. The choice of investigating the NAO’s influence on the mass balance components seems to be a choice of convenience, rather than of scientific merit and the initial justification for its inclusion is weak. Indeed, the absence of any substantive relationship found after the analysis, leaves me with the impression that little is gained from this part of

the study. In summary, I think the paper should be published, but first needs to be improved by either i) omitting the synoptic aspect of this research and expanding some of the other issues raised in this paper (see the remarks in 'specific comments'); or ii) more clearly defining the aims of the synoptic part of this study, and then adopting a means of analysis to suit. An analysis of weather types (e.g. Grosswetterlagen), or 'local' pressure gradients (as alluded to in the conclusion), would probably be more appropriate for extracting information about synoptic controls on mass balance (see e.g. the classic study by Hoinkes, 1968).

The main objective of the paper is to relate refined temporal patterns and underlying trends in seasonal mass balance terms with local and large scale covariates that define or reflect the "state of the atmosphere". Regarding the problem of choosing the best large scale variable, the choice of the NAO seems relevant to us since it has been the most largely considered one, but with results that remain still debated.

Our objective was to test the previously obtained results with the very long glaciological data at of Sarennes. In this analysis, a particular interest is the introduction of the winter balance series. This seasonal term has much weaker contribution to the annual balance of Alpine glaciers (28% of common variance at Sarennes) than for Scandinavian glacier (particularly Norwegian maritime glaciers) whose annual balances have been reported to be well related to NAO (Pohjola and Roger, 1977, Nesje and Dahl, 2000, Fealy and Sweeney, 2005). For Alpine glacier, the relation seems to be less marked (Six, et al., 2001). Winter precipitations have been reported to explain both the positive cumulative balances of Scandinavian maritime glaciers observed since the 70's, and the contrast with Alpine glacier.

We therefore expected a significant link with NAO, introducing the winter balance as a separate series. Results obtained were not as conclusive as could be expected, even with optimized data processing techniques (see the « method" comment below). However, this brings somewhat an added value with regards to previous studies, and consistence could be found with results regarding the winter climate – NAO relationship: Indeed, in the Alpine area, air temperature and moisture have been reported to be very sensitive to NAO indexes) in certain countries like Switzerland (Beniston and Jungo, 2002), but much less in the French Alps, as reported by Durand et al. (2009a, b). Particularly, according to Benniston and Jungo, during strong positive anomaly phases, winters have encountered much more warm and convective weather patterns in Switzerland, whereas there is a very weak correlation between smoothed (see below) NAO series and refined snow cover variables as detailed in Durand et al. (2009a, b).

Having the summer series, we also tested its correlation with winter NAO and, surprisingly enough, a much stronger correlation was found. We don’t really have a physical explanation for this result which may, as said in the paper, be just a spurious correlation linked to the data processing method used.

Finally as said in conclusion, there is still much space for understanding our results, the discrepancies with other studies (i.e. why the climate/mass balance data in Switzerland are better correlated with NAO than in France), and/or to search for synoptic indexes better correlated with high altitude local series. Our results however provide something new that warrants for us publication, so as to draw attention of the glaciological and climatologic communities on the need of additional investigations regarding the link between NAO, alpine climate and the different mass balance components.

On data processing

It is true that the first noticeable result is that, at the annual scale, the correlations with the NAO signal are nearly zero. This is also found in other studies that then correlate the variable of interest (e.g. a mass balance series) with smoothed NAO series, speaking of “removing the interannual noise”, and then highlighting some correlations with the variable of interest.

This can be seen as awkward and we dislike it for several reasons:

- first since the fluctuations are for us not noise but signal as well (at least high frequency one),
- second because it seems inconsistent to correlate a smoothed signal with a non-smoothed one.

The second point is why we don’t do that: we try to correlate only the smoothed mass balance signals extracted using ANOVA with smoothed NAO series, so as to highlight possible convergent low frequency patterns as it is done with temperature and precipitation trends, keeping in mind the limitation of the method which is clearly highlighted in text: a too high level of smoothing may nearly always produce a non zero correlation. And, in fact, correlation between smoothed glaciological data and underlying trends in temperature, precipitation, mass balance and NAO which are less/not visible at the annual level are thus highlighted. Maybe this is less chocking for temperature and precipitation than for NAO, since the control mechanism between these variable and glaciological variables is clear. Nevertheless the principle is similar, even if the correlations then obtained may be fortuitous and only related to the smoothing. We tried to say that clearly in text, for instance with regards to previously published studies which use such kind of approaches without care, and also to use smoothed

NAO series, “as little smoothed as possible”, which was the aim of the analysis detailed in Figure 5.

Finally, regarding the significance of the correlations for smoothed series, it is true in general that the autocorrelation structure has to be taken into account while computing confidence intervals and making significance tests. However, in practice, this autocorrelation structure is generally not known and empirical estimates for the correlation coefficient are nevertheless computed and evaluated using the “usual” formula. We do the same for correlations between trends (and not between a trend and an annual value!), even if it is true that the autocorrelation between smoothed series is by definition partially known and stronger than between the raw annual values. Hence, significance levels provided in Table 3 are truly overoptimistic and must be interpreted taking into account this limitation.

Removing this assumption and taking into account the smoothing in the correlation significance testing is possible, but much more difficult. For two running mean series there is an analytical solution by computing an equivalent data size taking into account the loss of freedom levels due to the moving averages. It is also possible with a modeling approach, and it is done under the Bayesian paradigm for evaluating credibility for correlations between trends in the different mass balance components in Eckert et al. (2011). Making a similar quantification for the link between the trends in mass balance and the smoothed covariates could hence be done by including directly the covariates into the modeling, as time dependent control variables with pluriannual autoregressive dependence, but this is clearly beyond the scope of the paper, and would not change much the “mean estimates” provided in Table 3, just decrease the number of significant values at a given significance level.

The solution suggested by referee one, considering decadal or pentadal means, was not considered because of a drastic reduction of the number of data points to evaluate the correlation which does not reflect the quantity of information available (even with a k year running mean, the number of freedom levels is much higher than N/k, with k the sample size).

These points will be made clearer in the next version of the paper.

Specific comments

1. *Considering the emphasis that is placed on the ANOVA employed in this study, I think that the section where this is introduced could be a little more comprehensive. For example, how do you calculate the trends? I find this particularly interesting because the authors subsequently correlate trends and NAO anomalies; trends are affected by multiple values, and* *each value is therefore not independent. Should some mention be made about this with regards to the significance of the correlations? The same point applies to correlations with smoothed NAO anomalies.*

Regarding how the trends are extracted, a sophisticated method fully detailed in Eckert et al., 2011 is used for the seasonal mass balance component. This is largely mentioned in text that only remembers the general principle to make the story understandable, and we think that it is enough. We will however make an effort to improve the readability of this section in the revised version of the paper.

Regarding trends in NAO series, the method is quite standard and does not need to be more detailed. Regarding its choice, see our general comment about NAO and mass balance component and the next point.

2. *Why can't the NAO series be analysed in the same way as the Sarennes data and the local meteorological data (p 2124; lines 10-15)? Surely it's the results that differ (multiple change points). Indeed, perhaps more should be made of these change points - there has been considerable research into change points of the NAO - how do these results compare (see Fealy and Sweeney, 2005 and references therein).*

NAO series are not analyzed in the same way as Sarennes data, first because there is no need for variance decomposition between space and time, and second, as you suggest, because of the presence of two marked change points on the low frequency trend instead of only one over the period of study. It is true that these change points (and additional ones) have been widely studied in the literature, but it is largely beyond the scope of our paper to redo this analysis and to try to understand its results. This will be indicated in the next version of the paper and the suggested reference will be added.

3. *The large ratio between winter mass balance and Besse winter precipitation is interesting, particularly so, because the authors note that this value is similar to that obtained at Saint Sorlin glacier. However, if 'drifts from surrounding nonglacial slopes and avalanches' are responsible for the steep gradient, then wouldn't this indicate a strong dependence on local topography (i.e. snow blow area, exposure etc)? If so, then perhaps such similar values between these glaciers would be surprising. Maybe the authors could comment on this. In any case, to draw such a general conclusion (p2127, lines 31-32) is, I think, unfounded.*

The sentences you mention on p. 2126 lines 31-32 have been certainly awkwardly worded. We first comment the high ratio between the valley and the high altitude locations of the glacier, meaning that the altitudinal gradient of precipitation is responsible for a part of the ratio but that specific processes (snow spatial redistribution from wind drift, avalanches) which effectively depend on the local topography are also involved. We observe a consistent ratio for Saint-Sorlin. However, we didn't expect finding such a consistent ratio with Sarennes. This consistency is at the same time reasonable since the 2 glaciers are very close, in the same range and altitude conditions but also fortuitous considering the local topographic differences, aspect, etc… of the 2 glaciers. This will be made clear in the next version of the paper.

4. *The analysis of melt factors is valuable, and I think it would be beneficial if this could be expanded upon. The authors report that the value of the melt factor has been stable throughout the period of study (for both snow and ice). However, it is also reported that the length of the ablation season has lengthened; this might be anticipated to affect the seasonally averaged energy balance (e.g. a lower average albedo if the ice is exposed for longer). Hence, the similarity of the melt factor before and after the change point might not be expected. Maybe this could be commented on.*

It is true that considering the overall ablation season, ice ablation being lengthened, the mean albedo tends to decrease which affects the mean surface energy balance as well. That is why a separate analysis is to be conducted for snow and ice ablation periods. Separating snow and ice melting periods allows quantifying the sensitivity independently of the ablation duration changes occurring for snow or ice. Identically, a surface energy balance analysis should be conducted in the same way, i.e. separating snow and ice melting periods.

5. *Related to the above, I think Figure 11 should be appropriately labelled- do the lines denote means or trends?*

Horizontal lines denote mean values over the period of record. The labels will be changed to make it clear.

6. *Where does the 10% value for the underestimation of ELA sensitivity come from in the discussion regarding ELA sensitivity to a change in temperature?*

p. 2132, lines 1-2. In his analysis based on degree-day factors, Vincent (2002) made a sensitivity analysis by increasing the temperature by 1°C over the whole ablation season, the ablation duration was considered as constant. As can be seen in Figure 12 of our study, increasing the temperature curve also increases the number of days with positive temperature as the red curve is wider than the blue one. As the PDD amount is actually the surface under these curves (counted in degree-day unit), increasing the temperature results in 2 contributions in the PDD sum as plotted in Figure 4 of this document. Using the Figure 5 of Vincent (2002), the expansion of the ablation season can contribute to 10% of the whole PDD amount, depending on the shape of the temperature curve along the season. This effect was not accounted for in Vincent (2002).

Furthermore, p2132, lines11-12: I think more could be said regarding "feedback due to precipitation, cloud longwave radiation" (this sentence also needs re-wording) –how would changes of these forcing affect ELA sensitivity?

Here, we mean that quantifying the ELA sensitivity to temperature from empirical regression of ablation to degree-days integrates the effect on ablation not only of the actual temperature rise, but of all other meteorological parameters that control ablation and which may change along with temperature (summer and winter precipitations, downward longwave radiation, cloud cover moisture and global radiation). These parameters can have positive and negative feedback, which means raise or lower the altitude of the equilibrium line. On the contrary, quantifying the ELA sensitivity from surface energy balance models enables to study the sensitivity to various meteorological parameters separately, and combined them as needed.

For example, the winter balance may vary with an increased atmospheric temperature. If we expect milder and moister winters, we would expect larger winter balance that would delay melting in summer and thus lower the ELA (negative feedback). The same question is relevant for summer precipitations. More precipitations as snow at high altitude would reduce the summer ablation through the albedo feedback. The question of precipitation is important at the equilibrium line as the mass balance at this altitude is very sensitive to precipitation through the albedo retroaction (Gerbaux et al., 2005). Long wave radiation emitted from le low troposphere is also expected to increase under warmer temperatures, but this depends how cloud cover and air moisture may change along with temperature. When accounting for the long wave irradiance rise associated to +1°C of warming (holding on cloud cover and moisture), the sensitivity of the ELA to is found to increase of 28% (Gerbaux et al., 2005) which is a non negligible positive feedback. Another example is that a cloud cover rise under warmed conditions is not only expected to increase long wave radiation but to decrease solar radiation, thus reducing melt and lower the ELA.

As based on PDD regression, our ELA sensitivity is quantified here with a mean sensitivity of snow melt to temperature that integrates all the variations listed above that may have occurred along with temperature variations over 6 decades. All the variability in melt is explained through a linear dependence to temperature fluctuations, the sensitivity representing a mean factor whose variability (i.e. inaccuracy) accounts for all other processes of melt varying not linearly with temperature. Quantifying the ELA change from PDD factor therefore necessarily remains an unrefined estimation. It is also difficult to conclude comparing different ELA sensibilities obtained from so different approaches ranging from an empirical to surface energy balance models. A careful attention should be taken when assessing the future evolution of glacier equilibrium line altitudes to temperature atmospheric changes using "sensitivities" which differ so much in their physical content.

We propose to include the above comments on the revised version of the paper.

Technical corrections

p2125 line 3: change "points up" to "points out"

Done

p2125 line 22: reword "Varying by 1-2_C, the rain snow divide does not significantly change results" to (suggestion): "Varying the rain snow divide by 1-2_C does not significantly change results"

Done as suggested

p2127 lines 12-17: reword, this sounds very awkward.

These lines will be reworded as

"This states that the temporal pattern of in Sarennes winter balance and high altitude precipitations (strong breakpoint in 1976) has almost a regional significance."

*p2131 line 23: "of 138_C" – shouldn't this be "**by** 138_C"?*

Yes, it should be written "by 138°C day"

p2135 line 3: change "form" to "from"

Done

p2135 lines 20 – 21: DJF NAO anomalies are ***negatively*** *correlated with summer balance (Table 3), hence need to change: “ : : :this* ***positive*** *correlation between summer balance and DJF NAO anomalies..”*

Done

p2135 line 27: “Nesje”, not “Nasje”

Done

References

Aellen, M. and Funk, M.: Bilan hydrologique du basin versant de la Massa et bilan de masse des glaciers d’Aletsch (Alpes Bernoises, Suisse), IHAS Publ., 193, 89-98,1990.

Böhm R, Auer I, Brunetti M, Maugeri M, Nanni T, Schöner W, Regional temperature variability in the European Alps 1760-1998 from homogenized instrumental time series, International Journal of Climatology, 21, 1779-1801, 2001.

Braithwaite, R., and Zhang, Y., Sensitivity of mass balance of five Swiss glaciers to temperature changes assessed by tuning a degree-day model, J. Glaciol., 46, 7-14, 2000.

Cogley, J.G., Hock, R., Rasmussen, L.A., Arendt, A.A., Bauder, A., Braithwaite, R.J., Jansson, P., Kaser, G., Möller, M., Nicholson, L., and Zemp, M.: Glossary of Glacier Mass Balance and Related Terms, IHP-VII Technical Documents in Hydrology No. 86, IACS Contribution No. 2, UNESCO-IHP, Paris, 2010.

Durand, Y., Laternser, M., Giraud, G., Etchevers, P., Lesaffre, B., and Mérindol, L.: Reanalysis of 44 year of climate in the French Alps (1958–2002): methodology, model validation, climatology, and trends for air temperature and precipitation, Journal of Applied Meteorology and Climatology, 429–449, 2009a.

Durand, Y., Laternser, M., Giraud, G., Etchevers, P., Mérindol, L., and Lesaffre, B.: Reanalysis of 47 Years of Climate in the French Alps (1958–2005): Climatology and Trends for Snow Cover, Journal of Applied Meteorology and Climatology, 48, 2487–2512, 2009b.

Fealy, R., and Sweeney, J.: detection of a possible change point in atmospheric variability in the North Atlantic and its effect on Scandinavian glacier mass balance, Int. J. Climatol., 25, 1819–1833, 2005.

Gerbaux, M., Genthon, C., Etchevers, P., Vincent, C., and Dedieu, J.P.: Surface mass balance of glaciers in the French Alps: distributed modelling and sensitivity to climate change, J. Glaciol., 51, 561–572, 2005.

Hock, R. Temperature index melt modelling in mountain areas, J. Hydrol., 282, 104-115, 2003.

Huss, M., Hock, R., Bauder, A., and Funk, M.: 100-year mass changes in the Swiss Alps linked to the Atlantic Multidecadal Oscillation, Geophys. Res. Lett., 37, L10501, doi:10.1029/2010GL042616, 2010.

Müller, H., and Kappenberger, G.: Claridenfirn-Messungen 1914–1984, Zürcher Geogr. Schr., 40, 1991.

Nesje, A., Lie, Ø, Dahl, S.O.: Is the North Atlantic Oscillation reflected in Scandinavian glacier mass balance records? Journal of Quaternary Science, 15, 587–601, 2000.

Paterson, W.S.B.: The physics of glacier, third ed., Butterworth-Heinemann, Oxford, 1994.

Pohjola, V.A., and Rodgers, J.C.: Atmospheric circulation and variations in Scandinavian glacier mass balance, Quaternary Research, 47, 29–36, 1997.

Vincent, C.: Influence of climate change over the 20th century on four French glacier mass balances, J. Geophys. Res., 107(D19), 4375, doi:10.1029/2001JD000832, 2002.

Vincent, C., and Vallon, M.: Meteorological controls on a glacier mass balance: empirical relations suggested by measurements on glacier de Sarennes, France, J. Glaciol., 43, 131–137, 1997.

Vincent, C., Kappenberger, G., Valla, F., Bauder, A., Funk, M., and Le Meur, E.: Ice ablation as evidence of climate change in the Alps over the 20th century, J. Geophys. Res., 109, D10104, doi:10.1029/2003JD003857, 2004.

WGMS 2011. Glacier Mass Balance Bulletin No. 11 (2008–2009). Zemp, M., Nussbaumer, S. U., Gärtner-Roer, I., Hoelzle, M., Paul, F., and Haeberli, W. (eds.), ICSU(WDS)/IUGG(IACS)/UNEP/UNESCO/WMO, World Glacier Monitoring Service, Zurich, Switzerland, 102 pp.

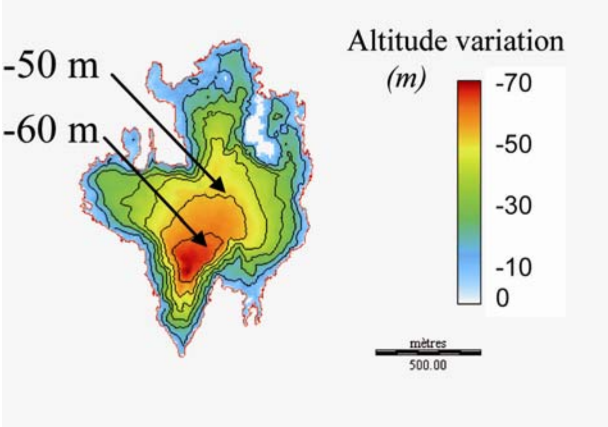

Fig. 1. Altitude variations at Sarennes between 1952 and 2003. Contour lines of altitude variations are at 10 m intervals (adapted from Thibert et al., 2008)

Fig. 2. Sarennes at the end of the ablation season in October 2007. Four years later in 2011, ice albedo around 0.43 was measured at the end of the ablation season.

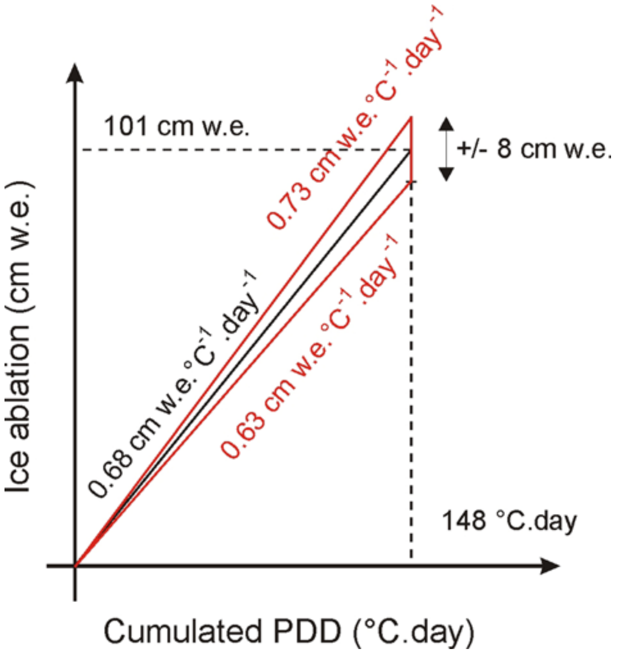

Fig. 3. Change in ice ablation sensitivity to temperature for an albedo change of +/- 0.04 which represents +/- 8.5% around a mean value of 0.47.

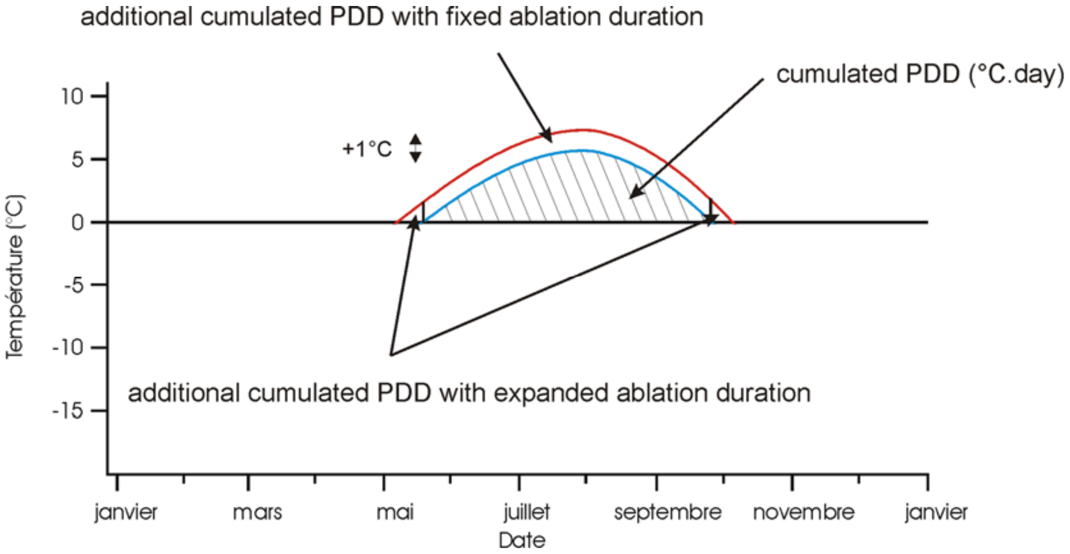

Fig. 4. The contribution of the expansion of the ablation season to the PDD amount.