(1) comments from Referees, (2) author's response, ***(3) author's changes in manuscript.***

Reviewer #2 : General comments

This paper presents a comprehensive assessment of long-term changes in a Pyrenean glacier. The authors have compiled an extensive basis consisting of various field data and geodetic surveys that allow reconstructing the evolution of Ossue Glacier since the end of the Little Ice Age. As glaciers in the Pyrenees are not well studied (e.g. in comparison to glaciers in the Alps) this article is a valuable addition to the literature. Furthermore, the processes determining the mass balance and the response of very small glaciers to climate change is not yet well known and new process understanding can be transferred to glaciers in other mountain ranges.

However, a considerable amount of work is required before this paper can be accepted. Most of my substantive comments refer to the presentation of the material. The paper is significantly too long at present with too many tables (see comments below). Furthermore, the description of some approaches needs to be clarified.

We thank the Reviewer for these encouraging comments and for the time he/she spent on our manuscript. As emphasized by the Reviewer we compiled many datasets, each with a different level of uncertainty and it is not easy to present them with all the necessary details in a concise manuscript. Based on the reviewer's suggestions we propose to improve the presentation of the datasets, methods and results in a revised manuscript as explained below.

Substantive Comments:

(1) Positioning of study: The actual aim of the study should be better specified. What do the authors want to find out with their data compilation ? Was is the main research question ?

The main goal of the study is to reconstruct the variations of a Pyrenean glacier since the end of the Little Ice Age. As mentioned by the Reviewer, the glaciers in the Pyrenees are not well studied. The large dataset collected on Ossoue glacier may contribute to understanding the evolution of the Pyrenean glaciers. To our knowledge there is no published mass balance reconstruction in the Pyrenees extending back to the early 20th century. The available reconstructions prior to the 80s are based on glacier length and/or area. Once this main objective is achieved, we also aimed at comparing the Ossoue glacier evolution with reconstructions in the Alps and other meteorological records to see if the glacier reflects the regional climate fluctuations. A final objective was to give a rough estimation of the glacier disappearance date based on the GPR-derived ice thickness map.

We will rework the introduction to better emphasize these objectives. As proposed below, we will also re-organize the Results section to respond to each of these objectives:

5 Results

5.1 Ossoue glacier metrics variations

    5.1.1 Length variations

    5.1.2 Area variations

5.1.3 Mass variations
5.2 Comparison with Pyrenean and Alps glaciers variation
5.3 Comparison with meteorological time series
5.4 Ice thickness maps

The argument of utilizing glaciers as a climate archive is somewhat delicate in my opinion as long-term meteorological measurements in the Pyrenees (also at high elevation, Pic du Midi) exist, and are used to interpret glacier data.

The reviewer is right that the temperature record at the Pic du Midi and the precipitation record at Tarbes extend back to 1882 and thus cover a large part of our reconstruction (1850-2013). However, the Tarbes station is located at 360 m a.s.l. and thus may not be representative of the precipitation regime in the high-elevation areas of the Pyrenees. The high Pyrenees are exposed to much higher precipitation rates and are the main contributor to the river discharge (López-Moreno et al., 2004). Before the 1990s, there is very little measurements of precipitation, especially above 1500 m (Soubeyroux et al., 2011). Precipitation data exists at the Pic du Midi from 1882 but the data are not reliable for climatic analysis (Dessens and Bucher, 1997).

Glacier are recognized as independent climate proxies (Oerlemans, 2005, Haeberli et al., 2007). The glacier evolution reflects a combination of various meteorological factors which are not limited to the air temperature and precipitation. Cia et al. 2005 and Chueca 2007 highlighted the value of the Pyrenean glaciers as proxies to monitor the recent climate change, as they respond with a short lag time to climatic fluctuations, due to their small sizes and latitudinal location.

The Ossoue glacier reconstruction provides another high-elevation climate proxy in the Pyrenees. Detecting common trends between independent proxies may make reconstructions more credible, whereas identifying differences may help for critical interpretation on meteorological drivers or even data quality.

However, I am sure that various other interesting questions could be defined and be addressed with the Ossue data set.

We agree that the use of Ossoue data as a climatic archive remains limited, especially due the low temporal resolution of the dataset (e.g. mass balance). We will reconsider the introduction section to diminish the emphasis put on the climatic interest.
We will remove:
p. 2434 l28: “The objective of this paper is to reconstruct the evolution of Ossoue Glacier based on these data to provide further information on the Pyrenean climate since the end of the Little Ice Age (LIA).”
and replace by:
“The objective of this paper is to reconstruct the evolution of Ossoue Glacier based on these data to provide further information i) on the Pyrenean **glaciers evolution since the end of the LIA ii) on the comparison between Pyrenean and Alps glaciers evolution iii) on the**

potential climate drivers of Ossoue glacier iv) on the likely evolution of the glacier in the near future".

We will aso remove the first paragraph of the introduction:

p. 2433 l.20 to l.23: "Southern Europe is projected to be a hotspot of climate change over the 21st century, with increasing temperatures and decreasing precipitation (IPCC, 2013). Among other consequences, water resources, including snowmelt from mountain areas, could be affected while water demand should will likely increase (EEA, 2012; IPCC, 2013)".

(2) Length of the paper: More than half of the tables could be removed without a loss in clarity. Furthermore, also the text could be shortened and be made more concise which facilitates the reading. I provide specific suggestions below.

We propose to move Tables 1, 4, 5, 7 and 8 to the supplementary materials. We also propose to shorten the methods section by moving systematically the details on the errors estimation for each metric to the supplementary materials (in agreement with the suggestion of Reviewer #1).

(3) Glacier indicators: One subtopic of the paper are the so-called "glacier indicators". This part is weak and could be completely removed. First, it does not become clear what "glacier indicators" are (the term also seems to be inappropriate).

Thank you for this comment. We realized that the term "glacier indicators" was probably not the most appropriate to refer to the length, area or the mass balance. We propose to refer to these variables as "glacier metrics" in a revised version of the manuscript.

Second, the definition is highly qualitative.

To determine the sign of variations for each glacier metrics, we propose to remove the thresholds based on an absolute value, which defined the "marked" classes, whether positive or negative (see table 6, e.g. -10 m in a "marked" length variation, or +1 m w.e. in a "marked positive" mass balance variation). These thresholds were indeed subjective and thus questionable. However, we propose to keep the definition of a "no variation" class when the metric variation is within the estimated random error range. Therefore, we will consider three classes: negative, positive and no variations. We think that this approach is more rigorous and also will facilitate the interpretation of the results, in particular the comparison with the glaciers reconstruction in the Alps.

Third, it remains unclear what can actually be learned from this analysis. A more quantitative approach would be more beneficial. Lastly, the presentation of the glacier indicators (Results, Discussion) is very short and leaves the reader with more questions than answers.

We have worked on this issue and we propose to include two new figures to enhance the analysis of our multi-source reconstruction. This figure compares the evolution of various glacier variations in the Pyrenees and the Alps from the literature.

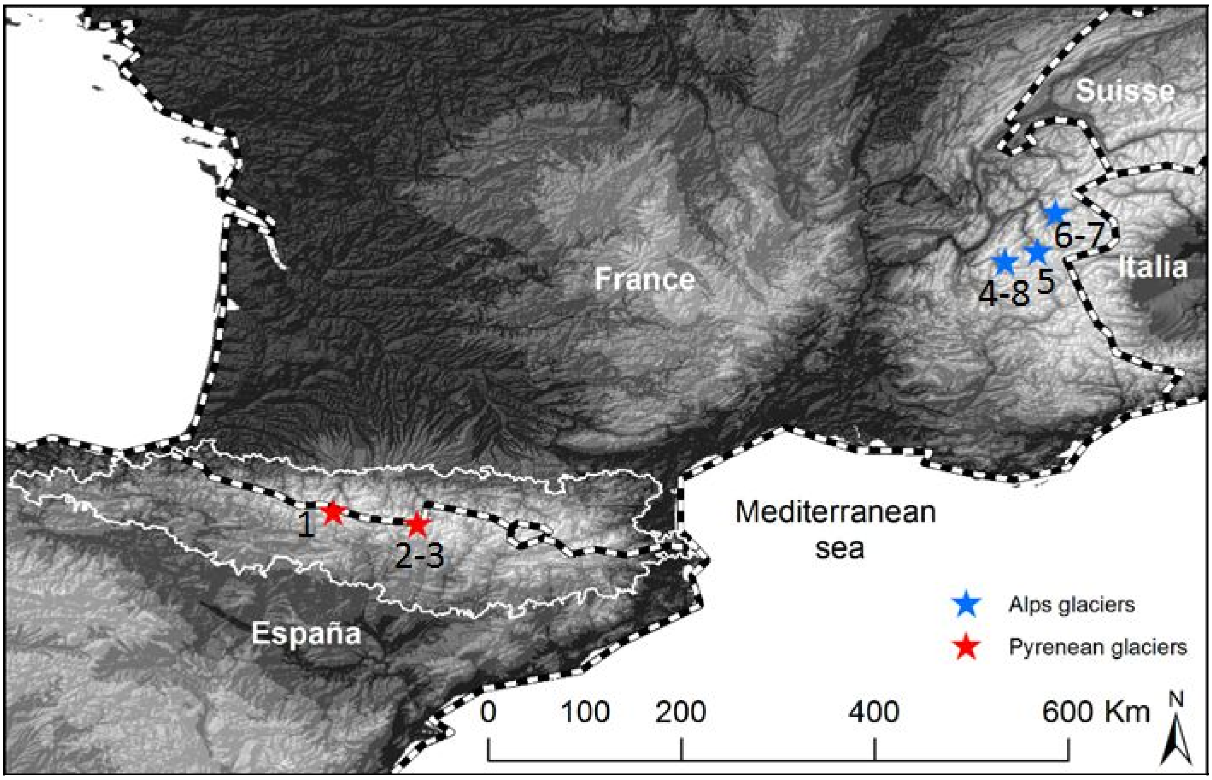

Fig. 1 Localisation of the Pyrenean and Alps glacier reconstructions selected for comparison.

Glacier name	Identification number on figure 1	Main metric	Distance from Ossoue	Publication
Taillon	1	Length variations	30 km	Gellatly et al. 1994
Maladeta	2	Area variations	80 km	Chueca et al. 2003
Coronas	3		80 km	Chueca et al. 2005
Saint Sorlin	4	Mass balances	550 km	Vincent et al. 2002
Gébroulaz	5		600 km	
Argentières	6		650 km	
Mer de Glace	7		650 km	
Sarennes	8		550 km	Thibert et al. 2013

Tab.2 Meta-data of glacier variations from the literature and used in this study for comparison between Ossoue and others Pyrenean or Alps glacier fluctuations.

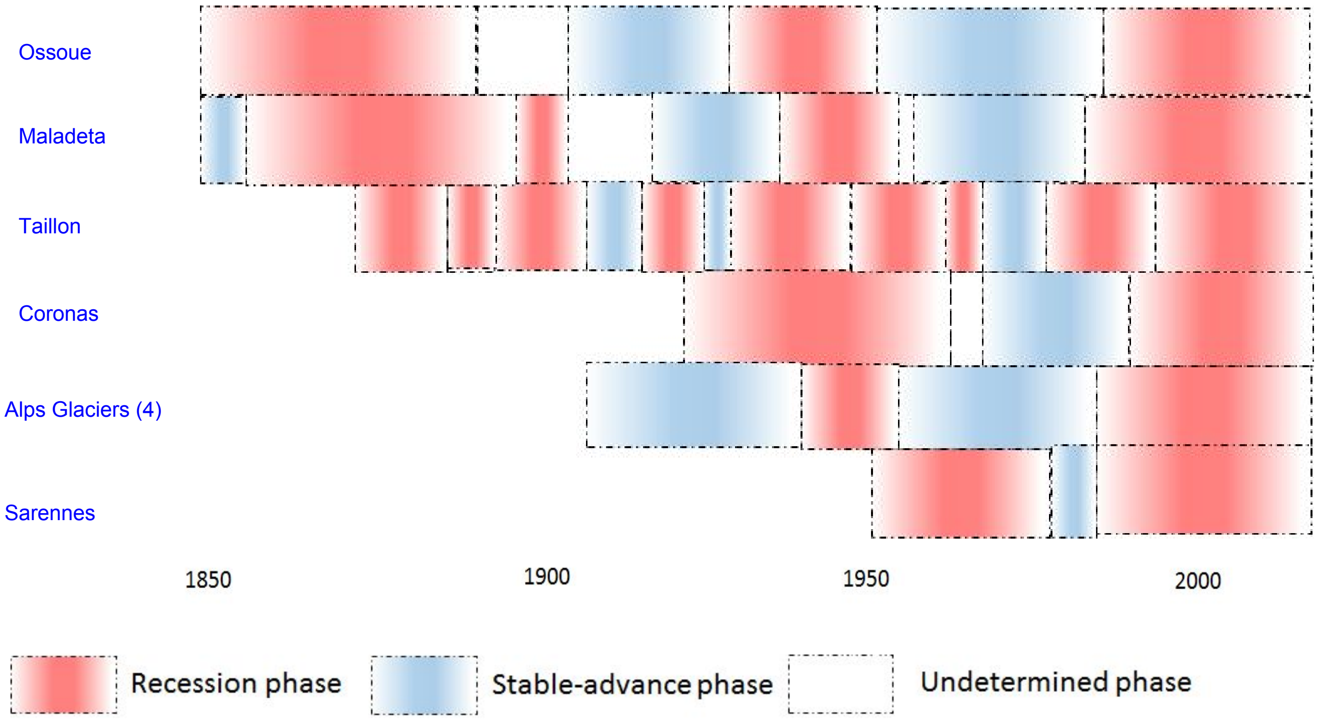

Fig.2 Comparison with Pyrenean and Alps glacier reconstructions.

(4) Calculation of geodetic mass balances: For the calculation of elevation changes, glacier is subdivided into two regions: (1) the area ice-covered in both DEMs and (2) the margin (only covered by one DEM). This approach is uncommon to studies of glacier volume change and geodetic mass balances and it makes interpretation of the results more difficult. For the calculation of the geodetic mass balance see e.g. Zemp et al. (2013). In response to this comment Table 10 and the text should be revised.

We agree with the Reviewer. Actually, the right method to calculate a geodetic mass balance depends on the purpose of the investigation (Cogley et al., 2011). As often in glacier study, our mass balance calculation is based on a mix of reference-surface and conventionnal mass balance approaches. The reference-surface balance is the glacier-wide mass balance that would have been observed if the glacier surface topography had not changed since a reference date. The time-invariant surface, called the "reference surface", is defined at some convenient time within a mass-balance programme, often at the start (Elsberg et al. 2001). For glacier-climate investigations, the reference-surface balance is recognized as a more relevant quantity, while the conventional mass balance approach, which require repeated mapping of glacier hypsometry at intervals appropriate to the rate of change of the surface geometry, is more relevant in the framework of hydrological applications (Cogley et al., 2011).
We will no longer split the glacier into two regions to calculate the geodetic mass balance for the revised version. We will only consider the maximal surface in the two survey dates (e.g. Carturan et al. 2013). Therefore, the equation (3) will be modified as follows:

$$B_{geod} = \rho \times \frac{\Delta V}{N \times A_{max.POR}} \quad (3)$$

where $A_{max.PoR}$ designates the largest area of the glacier in the two survey dates (PoR: period of record), and N is the number of years in the PoR.

The table 10 will be modified accordingly:

Period of Record (PoR)	1924-1948	1948-1983	1983-2013
ΔV	-0.0324 km^3	+0.0044 km^3	-0.0219 km^3
B_{geod}	-1.35 m w.e.a^{-1}	+0.14 m w.e.a^{-1}	-0.82 m w.e.a^{-1}
$B_{geod.PoR}$	-32.6 m w.e.	+4.9 m w.e.	-24.6 m w.e.
$\epsilon_{geod.total.a}$	0.11+$\frac{\epsilon_{1924}}{24}$ m w.e.a^{-1}	0.06 m w.e.a^{-1}	0.05 m w.e.a^{-1}
$\sigma_{geod.total.a}$	0.4 m w.e.a^{-1}	0.07 m w.e.a^{-1}	0.07 m w.e.a^{-1}

Tab 3. Ossoue Glacier ice volume variation (ΔV_{ice} in km^3) and associated mass change rates (in mw.e.a^{-1}) and mass change cumulated (in m w.e.). The last rows refer to the annualized systematic and random errors.

(5) Future of Ossue Glacier: The simple assessment of future glacier evolution should be revised / rethought. It is, for example, unclear why the authors define thickness classes that are "multiples of 1.5". The simplest approach would be to subtract the average distribution of surface mass balance 2011-2013 (in ice equivalent) from the measured thickness distribution in each year. This would be transparent, easy to describe and also account for the more negative mass balances in glacier center (page 2459, lines 10-20)

We agree that our thickness classes were a bit confusing. Therefore, we propose to follow the Reviewer's suggestion to generate ice thickness projection:
i) we interpolated on a 4m resolution grid the mean point mass balances measured at the stake locations over 2001-2013 on the current glacier outline (2011). The interpolation technique is the same that we used to generate the DEMs of the study, and is based on a discretized thin plate spline technique (Anudem 5.3, Wahba, 1990; Hutchinson, 2011). This interpolation is consistent with the glaciological method. The mean rate after interpolation is -1.5 m w.e.a^{-1}, while the mean glaciological mass balance rate is -1.45 m w.e.a^{-1}.
ii) we made the assumption that this spatialized mass balance rate will remain constant in the next decades.
iii) based on this mass loss rate we calculated the ice depth on the glacier plateau at decadal intervals from 2013.
We copy below a new figure based on this method that we could include in the new manuscript. As it is based on the superficial mass balance only, this very simple projection does not take into account the effect of the basal and internal mass balances nor the dynamics of the glacier.

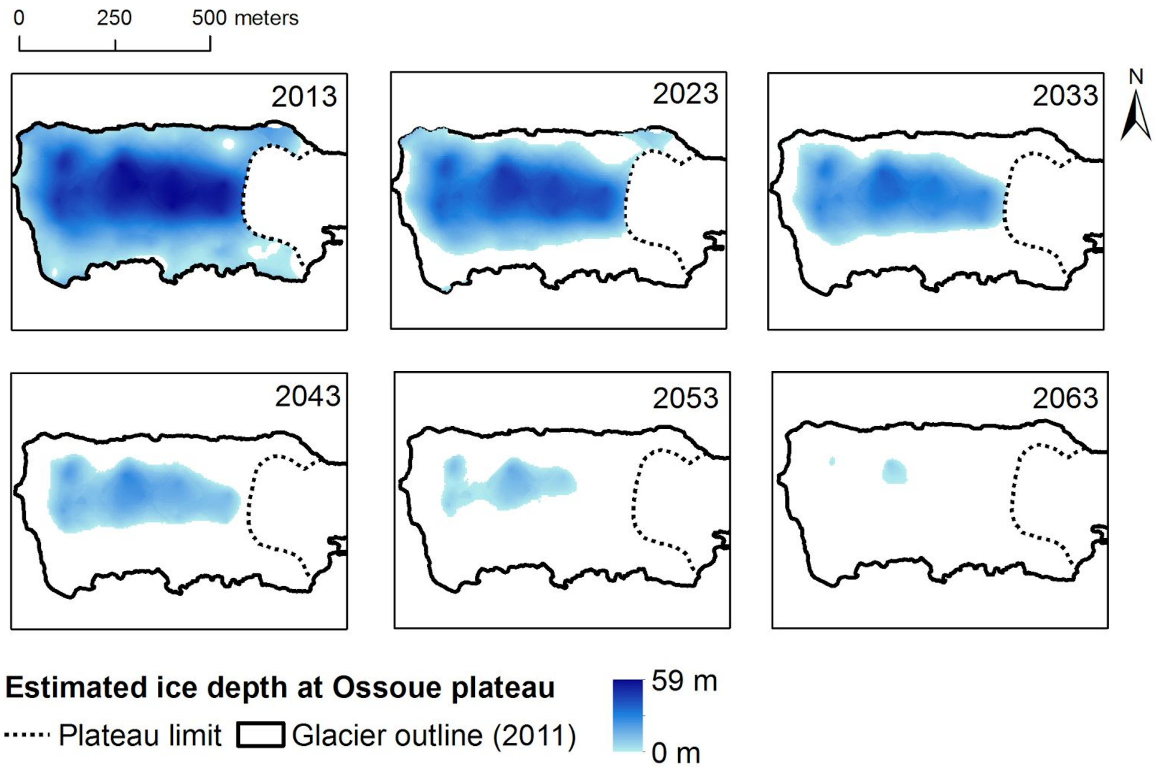

Fig. 3: Evolution of estimated ice depth in the plateau of the Ossoue glacier based on the spatialized mean stakes measurements over 2001-2013 (pixel size: 4 m).

(6) Comparison to glaciers in the Alps: One of the most promising potential aspects of this study would be a more detailed comparison of the long-term changes in Ossue Glacier in comparison to glaciers in the Alps. Such a comparison (in terms of annual mass balance / long-term volume change) would not be difficult to achieve but very interesting regarding differences in the most important driving factors between the Pyrenees and the Alps.

Thank you for this suggestion, we will include a comparison with glaciers in the Alps (see above).

(7) English language: Throughout the paper the language could be improved (native English speaker).

The manuscript was edited by American Journal Expert (the certificate can be checked online at https://secure.aje.com/certificate/verify using the following code : 7D86-4248-2DE6-554A-A32F). However, we will do our best to improve the readability in a new version of the manuscript. If the Editor requires it, we can ask another editing.

Details Comments:

- Page 2433, line 4: What does this elevation mean? It does not seem to be related to the study site. Should be removed.

This elevation refers to the highest peak of the Pyrenees, the Aneto Peak at 3404 m a.s.l. As it appears unclear in the manuscript, we will remove it in the new version.

- Page 2436: Although this historical overview is interesting, it would benefit from some shortening

We would like to keep this section as it is because we think it provides all the necessary elements to understand the origin of the historical datasets used in the following sections.

- Page 2436, line 8: Time periods for area changes in European Alps should be given; should be consistent with that of the Pyrenees.

We assume that the reviewer actually refers to page 243**7**, line 8:
"In comparison, the area of glaciers in the European Alps decreased by 35% (Hoelzle et al., 2007)".
We will replace (Hoelzle et al., 2007) by (Zemp et al., 2006) and indicate the time periods:
p.2437 line 8: "Alpine glaciers lost 35% of their total area from 1850 until the 1970s, and almost 50% by 2000 (Zemp et al. ,2006) ".
This area changes for all Alps glaciers are based on the date of the available complete inventories.

- Equation 3: I suggest following Zemp et al (2013) here (as elsewhere in the paper). Normally geodetic mass balance is expressed in m w.e. a-1. The time components is missing in the present formulation

According to Cogley et al., 2011: "Whether to present the balance as a rate or not will depend on the context of the investigation". We agree that the mass balance expressed as a rate facilitates the comparison between the different periods of records and with the glaciers from other massif or mountain range. We will systematically refer to the geodetic mass balance first as a rate, and indicate the cumulative mass balance into parenthesis. As stated above, equation (3) has been modified accordingly:

$$B_{geod} = \rho \times \frac{\Delta V}{N \times A_{max.POR}} \quad (3)$$

where $A_{max.PoR}$ designates the largest area of the glacier in the two survey dates (PoR: period of record), and N is the number of years in the PoR.

- Page 2443, line 20: I do not completely agree with that. This assumption would only hold if the entire firn coverage has been completely (!) removed already by 1983. This was most likely not the case. In contrary, I expect firn thickness to be at a maximum (after some glacier mass gains) and has almost disappeared until now resulting in a volume loss with a density <900 kg m-3.

We agree with the reviewer that a snow-firn pack was most probably present in 1983, after the stable-positive mass balance period of 1948-1983. To take into account the difference of density between firn and ice, we will used a mean density of 850 kg m^{-3} with an uncertainty range of 50 kg m^{-3} (Huss, 2013) to calculate the geodetic mass balance over 1983-2013. The manuscript and the table 10 will be modified accordingly.

- Equation 4/5: If the systematic error (bias) is known / can be quantified, the DEM should be corrected accordingly. In that case, the error would not appear in the uncertainty assessment. Obviously, the authors were able to quantify their systematic errors. They could thus simplify this section.

We corrected the 1948 and 1983 DEMs after a vertical coregistration on the 2013 DGPS elevations. However, we could not identify a systematic error (vertical bias) on the stable bedrock between the 1924 DEM and the 2013 DGPS elevations as explained p 2443 l.8-10: "For the 1924 DEM, the elevation differences did not follow a normal distribution and it was not possible to determine the elevation bias (noted as bias $\epsilon_{bias.1924}$)".
For that reason, we introduced this notation in the text ($\epsilon_{bias.1924}$), and we mentioned this error term in the meta-data of Ossoue Glacier volumetric measurements errors (table 3). Note that we will move the detailed description of the errors estimations to the supplementary material (see above), which will simplify this section as suggested.

We also considered an additional systematic error term ϵ_t due to the time lag between the raw data acquisition date and the first day of the next hydrological year, fixed to 1 October (p. 2443 l.25). Pending a better assessment of that error term (e.g. degree-day model-based correction), we preferred to keep this term as an error rather than correcting the mass balance value, using a floating-date system (Cogley et al., 2011).

- Equation 7/8: The subscript "bias" for a random error seems to be inappropriate. Bias normally means a systematic error.

Thank you for that remark. This term will be corrected accordingly in the text and in the equation (8).
p.2443 l.11-13: "The SD of the elevation difference values on stable areas was considered as to be a representative value of the vertical random error and was noted as $\boldsymbol{\sigma}_{\mathbf{coreg}}$ ".

- Page 2447, line 4: I have the impression that this approach is not feasible: When measuring surface elevation change using a dGPS both the contribution from melting and ice dynamics (flow) are measured. These results can, thus, not be directly compared to local surface mass balance and would result in a too high error estimate. Of course, the impact of glacier flow will not be very large on a small glacier but the aim is to quantify uncertainties in the range of decimetres.

We agree with the reviewer that we neglected several terms in calculating this estimation of the annualized systematic error associated to the glaciological mass balance (noted $\varepsilon_{glac.total.a}$ in the study). The terms neglected are: the internal and basal mass mass balances, and the ice mass variations due to the ice flow.

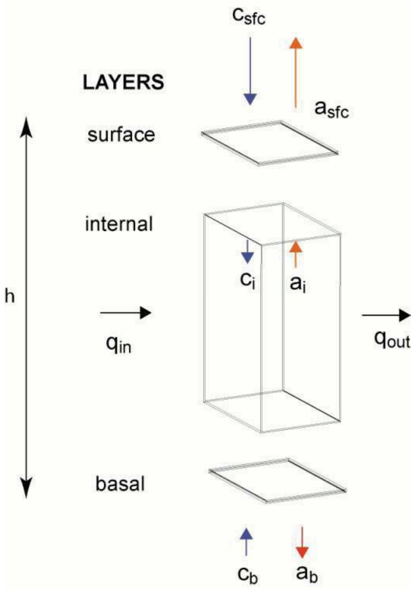

Fig. 4. The mass balance of a column of glacier ice, firn and snow (from Cogley et al. 2011).

We did our glaciological vs. geodetic method comparison in the upper area of the glacier (glaciological sectors 1 to 4). Therefore the term q_{in} is zero. We propose here an estimation of the term due to the ice flow (q_{out}) mentioned by the reviewer:

i) we make the assumption that the ice flow is 1 m a^{-1} and constant along a vertical profile. This hypothesis is based on the observed displacements of the stakes in the field, which were in general less that 1 m a^{-1}. We also consider that density ρ remains constant into the glacier, and is that of the ice.

ii) we calculate the volume of ice in this area (glaciological sectors 1 to 4) based on GPR-derived ice thickness.

iii) the glacier advanced by 1m over this period, hence we calculate the volume of ice V_{out} beneath a 1-m wide band at the lower periphery of the study area.

iv) we convert this volume in equivalent surface height by dividing by the area considered for the comparison.

v) we convert to m w.e. by multiplying by the ice density (assumed uniform to 0.9)

We find an equivalent surface mass loss due to ice flow of 0.03 m w.e. per year. The systematic error calculated by comparison between stake measurements and DGPS is 0.14 m w.e. As the reviewer warned, this error is overestimated due to the ice flow. However, we still consider that it is a good estimation since the effect of the ice flow remains small.

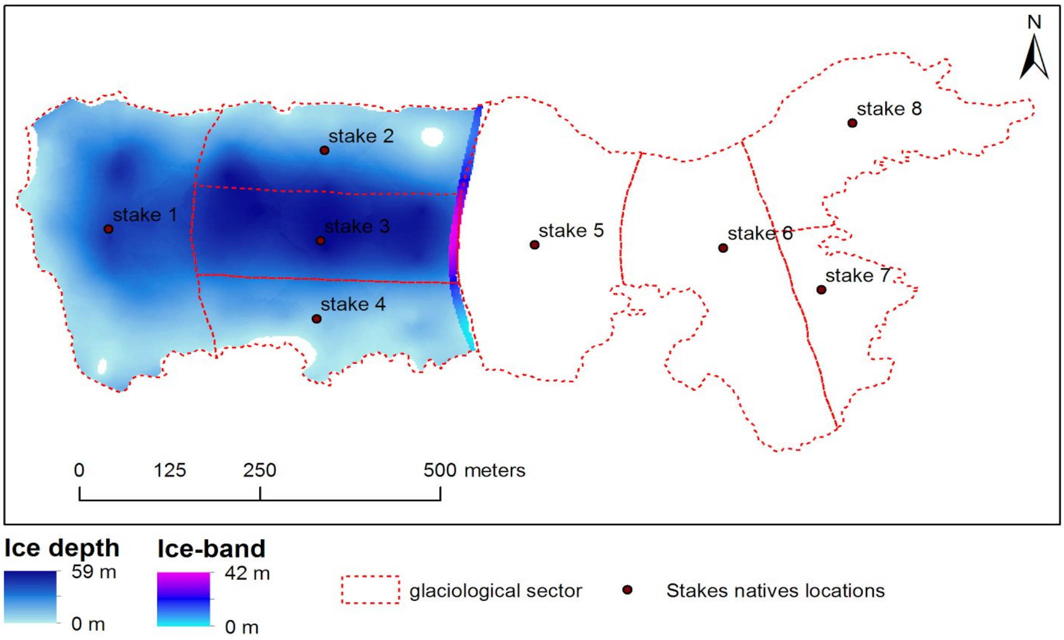

Fig. 5. Localisation of the sections of the glacier considered in the determination of the ice volume lost by the Ossoue plateau due to the ice flow.

- Page 2447, line 15: I have troubles to follow the authors' approaches here. Should be clarified.

We propose to clarify this in the text by adding the following sentence:

p.2447 l.15 **"To do so we used the DEMs made from the DGPS surveys performed in 2011 and 2012, as if each pixel was a (virtual) stake**. For every polygon 1 to 6 (only these polygons were surveyed), we calculated the variance of the differences between both DEMs."

- Page 2447, line 21: The unit should be m w.e. a-1

Thanks for that remarks, we will correct the units of the annual total random error accordingly, in the text and in the table 10. We will do this correction also for the annual total systematic error (p. 2447 l.4).

- Page 2449, line 10-23: These data should be presented in a data section

These lines are actually in the Data section (4. Data sets and methods). We think that this confusion stems from the fact that the data section was too long, but this will change in a revised manuscript as explained above.

- Page 2450, line 17: What is the rationale of using Spearman's rho instead of the more often used correlation coefficient r2? The latter would, in my opinion, by easier to interpret and understand.

The temporal trends in temperature and precipitation series, as well as the correlations with the glaciological mass balances terms were analyzed by a rank correlation method, using Spearman's ρ. This statistic is prefered because it is neither sensitive to the presence of outliers nor the existence of non-linear correlations (Borradaile, 2013, and in a glaciological context: Chueca et al. 2007). We consider the correlation coefficient ρ rather than the coefficient of determination ρ^2 to keep the direction of association between the two variables, given by the sign of ρ (e.g. Thibert et al. 2013).

- Page 2452, line 9: Avoid separating "current" glacier area and margin and state geodetic balances. Consequently, most of this chapter should reformulated.

We will rewrite this subsection according to the reformulation of equation (3) and to the new approach defined to calculate the geodetic mass balance (as mentioned above).

- Page 2452, line 10: Here and throughout the paper. The stated periods are only defined by the dates of the available maps and are unrelated to actual climate variations! Interpretations should account for this.

We agree that the definition of the periods are based on the dates of the available datasets and do not necessarily coincide with climatic variations. However, the combination of metrics allowed us to partly overcome this issue. We will mention this limitation in the Discussion.

- Page 2454, line 8: What is "monthly" summer ablation? Has ablation been measured in monthly resolution?

Yes, in section 4.3, "Glaciological mass balances", we indicated that (p.2445 l.13): "Summer ablation measurements were repeated once a month until a date close to the beginning of the next hydrological year, according to the floating-date system (Cogley et al., 2011)".

- Page 2457, line 18: also here, the stated periods should discussed with care. 1999 did not mark the end of any period – recession continued until today.

We fully agree with the reviewer. We updated the recession period identified by Vincent et al. 2002 up to present in the new figure showing the Alps/Pyrenees comparison (Fig. 2 above).

- page 2459, lines 10-20: This a different topic (spatial distribution of surface mass balance) and should be separated from the discussion of expected future evolution.

We propose to move this discussion just after the discussion on climate drivers (p.2458 l.16).

Tables and Figures:

- Table 1: Could be removed. The important information (elevation range, length, location) are already given in the text. The rest is not necessary for understanding the paper.

Alternatively the authors could envisage publication of this table as a supplementary material.

We will put this table into supplementary material.

- Table 2: This Table is important and should remain in the main paper. However, it would benefit of some shortening by using less text (e.g. abbreviations for methods, remove source characteristics – already available from text)

We agree to remove the source characteristics column to use less text. However, we would rather not use abbreviations to facilitate the reading of the table.

- Table 4: remove or put into supplementary material

We will put this table into supplementary material.

- Table 5: remove or put into supplementary material

We will put this table into supplementary material.

- Table 6: These thresholds are highly qualitative and only valid for this single glacier. I would completely remove this topic (glacier indicators) from the paper.

As discussed above, we agree that the thresholds were subjective. Therefore, we propose to remove the thresholds that define the “marked” class, whether positive or negative. We will keep the definition of a “no variation class” within the estimated random errors range and thus we will consider only three class: negative, positive and no detected variation.

- Table 7: This information is directly shown in a figure (where it is easier to understand). Therefore, this Table is not necessary and should be moved into the supplementary material.

We agree, we will put this table into supplementary material so that anyone interested can more easily use the data.

- Tables 8 and 9: same comment as for Tab 7.

We will put these tables into supplementary material.

- Table 10: This table contains important data on the direct observation of mass balance, as well as seasonal components. I suggest swapping rows and columns to make it easier to read. The cumulative MB can be omitted.

We suppose that the reviewer also refer to the table 11 in that comment (table 10 is about geodetic mass balance and do not content seasonal components nor direct observations).

However, we will swap rows and columns in table 10 (see above) as suggested by the reviewer to make it more readable.

- Table 11: also here: swap rows and columns. The symbols (B_w etc) should be explained in the caption.

Thanks for that suggestion, we will swap rows and columns to make it more readable. As mentioned in the caption, End_w and End_s refer to the end of winter and the end of summer, respectively, in the floating date system (Cogley et al., 2011). According to the reviewer commentary, we will complete the description of the others variables in the caption:
B_w refers to the winter mass balance.
B_s refers to the summer mass balance.
$B_{glac.a}$ refers to the annual mass balance.
$B_{glac.c}$ refers to the cumulative mass balance.
In the manuscript, this variables are defined in p. 2446 l.12. We will omit the cumulative mass balance as it is also presented in Fig. 8.

- Figures 8 and 9: Provide label for y-axis!!

The Y-axis labels will be added in the new version of the manuscript.

- Figure 10: too small to read. Omit this topic completely (see comments above).

According to the commentary on table 6 and the reviewer recommendation, this figure will be removed, and replaced by the Fig. 2 presented above (comparison of the Ossoue glacier reconstruction to three others Pyrenean glacier reconstructions and the two selected studies in the French Alps).

- Figure 11: Provide label for y-axis, including units!! Furthermore, I would suggest to combine Figs 11 and 9 (at least for some variables) as the goal is to establish a link between variations in the glacier and climate (T, P, NAO, AMO).

We propose to indicate the timeline of Ossoue glacier variations (first line of Fig. 2 above) in the figure 11.

Again we sincerely thank the Reviewer#2 for this thorough evaluation of our manuscript.

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