Author reply to review comments on "Climatic interpretation of length fluctuations of Glaciar Frías"

Paul Leclercq, Pierre Pitte, Rianne Giesen, Mariano Masiokas, and Hans Oerlemans

1 General

We thank the reviewers for their useful comments. Their comments and suggestions helped us to reanalyse our results and main conclusions. Following the reviewers' suggestions, we propose several improvements to the manuscript. Here we will give an overview of the most important points by addressing the major comments shared by the revievers. More details as well as a point by point reply to all comments of each reviewer are given in the next sections.

Firstly, we have corrected the model code in *i)* the calculation of the precipitation such that the seasonality is kept at higher altitudes and *ii)* in the calculation of the SSC (were division by a factor 2 was missing). In addition, we have used an adjusted formulation of the atmospheric transmissivity τ. We used a constant value throughout the entire year, but based on measurements of the global radiation at Mocho Choshuenco (data kindly provided by M. Schaefer, CECS) we now introduce seasonal variability. In winter the atmospheric transmissivity appears to be lower than in summer, which is in line with the higher precipitation (more cloudy conditions) and the longer path through the atmosphere in winter. We superimposed a sinusoidal on the constant value: $\tau = 0.55 + 0.15 sin(t)$. As shown in Figure 1, this reproduces the monthly means of observed incoming radiation reasonably well. We have repeated all model runs with these improvements. The results are hardly different. Apparently the relative increase in winter precipitation compensates for the increased summer melt, that is a consequence of the higher atmospheric transmissivity in the summer months.

We extend the discussion of the climatic interpretation of the glacier length fluctuations, and the related uncertainties in particular. We include a more extensive decription of the glacier length record. We have performed additional model runs to investigate the uncertainty that results from the fact that the reconstructions of *Neukom et al.* [2010, 2011] do not give precipitation and temperature anomalies for the spring and autumn months. We are also more careful in our statements on the difference between climate deduced from glacier length and the proxy reconstructions of *Neukom et al.* [2010, 2011] and *Villalba et al.* [2003]. These 'very precise' numbers are meant as (quantitative!) indication of the difference, by giving an adjustment to the reconstructions that would result in agreement between modelled and observed glacier length.

To conclude, we have added a short overview of the fluctuations of other North Patagonian glaciers, for as far as they are known. There is indeed more information of glaciers in the region. In a broad sense this supports the idea that the glacier fluctuations represent a regional climatic signal. Especially the glacier length record of Glaciar Esperanza Norte by *Ruiz et al.* [2011] shows a striking similarity with the record of Glaciar Frías. This suggests that both glaciers express the same variations in climate despite the 100 km distance between them. Besides, this reconstruction of Esperanza Norte makes the length record of Glaciar Frías no longer the only long and detailed length record of the region.

Below a detailed reply to the comments of the reviewers is given. The replies are in italic.

2 Review 1, by R. Neukom

This manuscript represents the first attempt to combine a multi-centennial record of glacier fluctuations from the southern Andes with a mass-balance model. As the knowledge of past glacial fluctuations from South America as well as the dominant causes for these fluctuations is still very limited, this paper is a very welcome contribution to this field of research. I think the approach of comparing model-driven fluctuations with independent glacier length records is adequate and interesting. As I am not an expert in glacier reconstructions and modeling, I cannot comment on the methods used herein and have to assume that they were used correctly and were adequately adapted to the special situation of Glaciar Fras. My comments will be restricted on structure and content of the manuscript in general as well as the comparison with the reconstructions and climatological interpretations. From this perspective, I think this paper is worth to be published in CP after correction and adaptation the points listed below. In particular, I have one major concern regarding the interpretation of the model and resulting suggestion of errors in the reconstructions. This point concerns one of the main conclusions of the paper and needs to be carefully re-assessed before the manuscript can be accepted.

2.1 MAJOR POINT

P3675 line 10 – P3676 line 1
Is it really possible to make statements about the temperature amplitude of a warm phase between two dated advances such as around 1800? The dynamical calibration cannot reproduce any warming before ca. 1900 as the dated lengths only increase back in time. The authors claim that the reconstructed temperatures are ca. 0.7 °C too high around 1800 to reproduce the 1843 moraine. But for example a closer look at the black curves (Villalba et al. data) shows that this is not the only possible explanation. After the minimum around 1810, the black curve in Fig. 9a starts to increase rapidly. If the curve would continue to increase with the same slope until 1843, it would hit the dated glacier length of this year very closely. However, this does not happen, because there is another short warming phase in the Villalba et al. reconstruction around 1820 or so (Fig. 9c), causing the glacier length to increase more slowly. Without this short warming period, the modeled curve would probably have matched the 1843 record even with the warm phase around 1800. In short, it also possible that the maximum warming around 1800 was correct in the reconstructions but, for example, the subsequent cooling occurred to slowly. The modeled glacier length does not depend only on the amplitudes of climate maxima/minima but also on the length of the cold/warm periods and the velocity of the changes. Hence, the overestimation of temperature of 0.7 °C is not the only possible conclusion. The same criticism is also valid at other periods. The authors should re-evaluate, what kind of interpretations are possible with the model and dated glacier lengths available. From my understanding statements about the absolute error of extreme phases in the reconstructions can, if at all, only be made for cold extremes/glacial advances, at least in the period, where the glacier length record does not contain information about minima (actually the first dated minimum is 1970). From a climate reconstruction perspective it is more probable, that the reconstructions have issues with the variance back in time in general, not only in particular short periods (e.g. the warm years around 1800). Statements about the general under- or overestimation of variance back in time (for cold AND warm periods) would therefore be more plausible (i.e. would the match with the dated advances be better for reconstructions with more/less variability?). As the manuscript title focuses on climatic interpretation, this issue must be really clear and statements as on page 3675 and 3676 need to be re-assessed and explained in more detail, especially as they are included in the abstract (last sentence). Numbers such as the suggested 0.7 C overestimation of temperatures around 1800 should either be removed and instead be explained in a more qualitative way or they need to be verified by analysis such as alternative model runs. It could also be a stated a bit more clearly that the very large uncertainties in the model parameters or even the concept of the model may also be responsible for miss-matches between the modeled and documented advances.

We agree with the reviewer that the correction to the reconstructions we have suggested are not the only possible conclusions. The climatic conclusions that can be drawn from the glacier fluctuations are limited by the information we have of these fluctuations, i.e. basically we only know maximum stands. We do not have any information on the retreat between these maximum stands, and thus can not draw conclusions *on the magnitude of warm phases. However, if the warm phases in the climate reconstructions are too profound and/or too long, the next maximum glacier stand is not reproduced by the glacier model forced with this reconstruction. We think this is the case with the reconstructed warm period around 1800. We try to quantify the mismatch between the reconstructed climate and the observed glacier length. Therefore, we give a possible adjustment to the reconstructed climate such that the modelled glacier length would match the observed maximum stands. The mentioned 1 mwea*$^{-1}$ *for the period 1790–1820 would do that, but other adjustments (e.g. larger adjustment for a shorter period) could give a satisfactory result as well.*

Our study indeed suggests that the variance in the climate reconstructions is too large, because the modelled glacier lengths (when the model is forced with the reconstructions) give periods with too large as well as periods with too small glacier lengths.

The uncertainties in the glacier model, and especially in the modelled mass balance, are substantial. However, uncertainty in the model parameters can not account for too large and too small glacier lengths in the same reconstruction. In addition, the model adequately models a glacier length in the range of the observed glacier length in the period 1980-2009 from the 1980-2009 climate. It only needed tuning of the precipitation such that the winter accumulation agrees with measurements on Mocho-Choshuenco.

2.2 OTHER POINTS

1. Title: Maybe include an information about the temporal coverage of the paper e.g. by adding 1639-2009 at the end.
   We intend to chang the title to Climatic interpretation of the late Holocene fluctuations of Glaciar Frías, North Patagonia, Argentina

2. Abstract: The abstract does not contain much information about climatic interpretation as promised in the title. I suggest to either include more of the climatological findings into the abstract (e.g. the fact that the glacier seems to be more driven by temperature variability rather than precipitation) or adapt the title in a way that the focus is more on the modeling. To save some space, I suggest removing the naming of all the archives that were used in the reconstruction and just say . . .with independent tree-ring and multi-proxy reconstructions of. . . at lines 11-12.
   We have adjusted the abstract to include more of the climatic findings. In addition, we have been more prudent with the conclusions (cf. the major point).

3. General comment: Although Glaciar Frias is the best documented glacier from the area, there are dates of advances of other glaciers from the larger area as well. A short note on how well they compare to the variability of Glaciar Frias would be helpful to understand, whether the results can be interpreted as representative for a larger region or only for the location of the study area.
   We will include a comparison with fluctuations of other glaciers in the region. See the general comments (Section 1) for more details.

4. Page 3655, line 10 This statement could be a bit stronger e.g. . . .has not yet been addressed in a quantitative way instead of . . . fully exploited.
   done so

5. P3655 line 29 I dont think that the interaction between glaciers and climate is well understood, otherwise the influence of temperature and precipitation in the glacier model would not be a point of discussion later in the paper. I suggest rephrasing in a way saying that the general concept of climatic variables influencing glacier variability is well established, but relative importance of these variables is unknown in most cases.
   We do not agree with the reviewer on this point. The impact of changes in the climatic forcing (e.g. a 10% increase in precipitation) on the glacier surface mass balance can be calculated with a mass balance model. The climate – glacier relation is well understood in this sense. What is not known, and that is one of the discussion points in this article, is how large precipitation and temperature fluctuations in the past were. The precipitation signal is difficult to disentangle from the temperature signal if only glacier fluctuations are known, as it then becomes one problem with two unknowns.

6. P3656 line 10 Remove the southern part of, as the availability of historical evidence of glacier fluctuations is much less than in Europe in all Andean regions.
   done so

7. Section 2.3 I think it would be helpful to clarify somewhere that the gridded reconstructions available for South America were designed for analyses on sub-continental to continental scales and may therefore not be ideal for analyses on local to regional scales. This may help to explain some of the discrepancies with the glacier evidence.
   We include this remark in the section that describes the used proxy records (2.3)

8. P3670 line 9 Maybe provide the number for (1-e^{-1}) i.e. 0.63 so that someone who is not familiar with the variable response time can get an immediate idea of what it means. I think this is crucial as many readers may intuitively understand this measure as the time until the final changes is nearly or fully met, which according to Fig. 7 is much larger than 14 years.
   number added

9. P3671 line 26 These numbers are interesting and should be elaborated on a bit more. How does the change between the early 17th century (or the LIA maximum) and present look like in the reconstructions. Are the numbers for temperature (+1.16K) and rainfall (-34%) similar? If not, what may be the explanations? This would allow making some better estimates of LIA-present temperature amplitudes, which is of high interest regarding climate sensitivity issues etc. Is it possible to get some probability for the temperature and rainfall changes? For example further below it is concluded that fluctuation in precipitation are in general of minor importance for Glaciar Frias, so one would estimate that the true temperature amplitude is very close to 1.16K. Or maybe the amplitudes from the climate reconstructions can help refine the model parameters?
   These numbers will be elaborated a bit further in the revised version. In view of the results when forced with reconstructed climate, we think it is reasonable to ascribe the glacier retreat mostly to changes in temperature.

10. P3673 line 12-14 This sentence is in contradiction to P3672 lines 9-10 Rivera et al. (2002) attribute the retreat of glaciers in Southern Chile for a large part to a decrease in precipitation. So the authors should either change the argumentation on P3672 or provide an explanation for the difference of Glaciar Frias to the other glaciers.
    Stricly speaking both statements are not contradictionary. The period under consideration in Rivera et al. [2002] is different, it is much shorter. In addition, Rivera et al. [2002] have not modelled the mass balance of Chilean glaciers. They observed a retreat and pointed towards the decrease in precipitation as a possible explanation. Our model study suggests that precipitation variations are not likely to be a key factor in explaining the glacier retreat over the last century. At least not for Glaciar Frías.

11. P3674 lines 13-16 I think it would be good to show the dotted line (not including the reconstructed winter temperatures in the model) for the full period. If it performs better than the winter-forced model in general, I recommend using this version as the best guess. There are two good reasons to do so: first, summer temperatures are of less importance for the mass balance (especially JJA which are used in the reconstruction; Fig. 6). And second, the winter temperature reconstruction is less reliable than the summer temperature reconstruction (Neukom et al. 2011).
    The mass balance of Glaciar Frías is sensitive to temperature anomalies in each month of the year. It is therefore not very realistic to make a reconstruction by only varying temperature in summer months. This will simply give less variability. The dotted line was only intended to give an impression of the magnitude of uncertainty caused by the lack of winter data in the period 1600–1706. We include an reconstruction based on prescribing the summer temperature anomaly to all months of the year for the entire period of reconstruction (instead of keeping the winter months at the 1980-2009 average, as is done for the dotted blue line in Figure 9)

12. P3675 lines 15-19 and P3676 lines 1-5 difference in the early 20th century The large difference between the reconstructions in this period is indeed remarkable and as it falls into the calibration periods it

is most probably caused by the differences in the instrumental calibration data used. This should be mentioned. Whereas Nekom et al. (2011) used grid cells from the CRU TS3 cru grid; Villalba et al. (2003) used PCs of instrumental stations. This issue emphasizes the problem of getting adequate instrumental data for the region. As the ideal temperature during this time would be somewhere between the two reconstructions, this may help to identify the most representative meteorological data from the area. Does one of the stations Bariloche or Puerto Montt data show temperatures that are in between the two reconstructions during this period? If so, can a shorter version of the model be run using this station? This would provide helpful imformation about the climate at Glaciar Frias as well as the model dynamics.
We will mention this difference in calibration data set as a likely source of the difference between the two reconstructions. Although, we would think that the two data sets should not be that different, as the CRU grid is based on station measurements (from the same stations). Besides: running the glacier model with weather station data from Bariloche can not at the same time test the station data and the glacier model. One of the two is assumed to be valid/representative in a test of the other.
The station data of Puerto Montt are not usefull to resolve the issue of divergence between the reconstructions in the first half o the 20th century: the data we have (downloaded from the knmi climate explorer) start later (1951) and have several data gaps. Furthermore there is a discrepancy between the near surface trend and the temperature trend in the free atmosphere [Carrasco et al., 2008]. The temperature record of Bariloche starts earlier (1931) and is rather complete (except for the period 1990–1995). However, the record does not start early enough to get a grip on the divergence of the reconstructions in the first half of the 20th century. As shown in Figure 2, the initialization of the model in 1931 is crucial for the results in the entire first half on the 20th century. The observed glacier length in the second half of the 20th century are well reproduced, regardless the initialization.

13. P3677 lines12-14 Are there comparable projections from other continents to compare with? Is this projected retreat more than expected in the Alps, for example? Some more context would be very helpful here. A short discussion of the economic and ecological consequences would be interesting. Is the melt water of this glacier crucial for irrigation or hydropower generation?
Similar experiments have been performed for different glaciers around the world ([e.g. see Oerlemans et al., 1998, for a compilation]. In these model experiments significant losses in volume are found for several glaciers, comparable to the results for Glaciar Frías. The projected volume loss presented in this study is not exceptional.
The importance of the melt water of Glaciar Frías depends on the relative contribution of the glacier to the river discharge in this catchment. It is beyond the scope of this article to assess all relevant hydrological components of the river catchment. Seen the high levels of precipitation in this region (also in summer), our first guess would be that the melt water of Glaciar Frías is not crucial in the down-stream water supply.

2.3 TECHNICAL COMMENT

Please be consistent with temperature units, use either °C or Kelvin in all instances.
we have expressed temperatures in °C

3 Review 2

3.1 General comments

This is a very interesting contribution comparing the detailed glacier length reconstruction of Glaciar Frías with the newly developed climate reconstructions by Neukom et al. and Villalba et al. As to my knowledge, this is the first approach using a surface energy and flow-line models to a glacier in South America. The detailed comparison of southern hemispheric glacier length changes and climatic information is novel, too. The knowledge of past glacier behaviour in South America is still sparse as compared to the northern hemisphere and it is therefore welcome to see this detailed climatic interpretation of best-documented Glaciar Frías.

However, given the quality and uncertainties of the input data, the authors should be more careful in their climatic interpretation of the results and the statements made, also in view of the scope of the journal (it is CP and not TC, with focus on the climatic interpretation and not on the technical description of the glacier model etc.).

First, the uncertainties (regarding input data and model calibration) should be quantified (as good as possible). For instance, I miss a detailed description of the glacier length record and related uncertainties. The glacier reconstruction consists of only 6 dots before AD 1900. Is there any information from other glaciers in the area, e.g. for the gaps around 1700 and 1800? This lack of information stands in contrast to the precise statements made in the paper (e.g., dT of 0.7 °C for the early 19th century, mentioned in the abstract; discrepancies to the independent climate reconstructions). In addition, climate reconstructions by Neukom et al. for comparison with the glacier behaviour are only available for DJF and JJA, but spring and autumn conditions might have substantial influence on the glacier, too. This point must at least be addressed.

We have revised the decription of the glacier length record, including more details concerning the methods and related uncertainties (c.f. spec. com. 6). Furthermore, we have performed additional experiments in order to assess the uncertainty in the reconstructed length record that results from the mssing information in the climate reconstructions of Neukom et al. [2010, 2011] (c.f. spec. com. 16).

Second, the reconstructions starts with the LIA maximum position in 1639. The major glacier advance is therefore not covered by the modelled time period, and I am wondering what this might have for implications for the calibration of the model.

The model is only calibrated on the steady state run with the 1980-2009 mean climate by increasing the annual precipitation such that the accumulation (winter mass balance) at 2000 m altitude is comparable to the measured accumulation at Mocho Choshuenco. The LIA maximum extent has no effect on the model calibration.

In the climate reconstruction with dynamical calibration, the maximum extent is modelled with an equilibrium state. The real temperature might have been lower in this period as it is very unlikely that the glacier was in equilibrium state. In a fluctuating climate, the period in which the maximum extent was reached needed to be cooler than the temperature needed to match the maximum length in an equilibrium state. This would lead to a larger temperature increase over the period 1640-2010 than we have reconstructed. The magnitude of this effect is unknown.

More details are given in the specific comments below. They need to be addressed before publication in CP. Apart from that, the paper is very well written and solid.

3.2 Specific comments

1. P3653: The title should be more concise, including the time frame (LIA? Holocene?). And, as the title place emphasis on the length fluctuations, the glacier reconstruction should be described more in detail in the paper.
   We intend to change the title to Climatic interpretation of the late holocene fluctuations of Glaciar Frías, North Patagonia, Argentina

2. P3654: Line 4: write AD 1639 Line 13: tree rings are mentioned twice, and indicate what kind of sediments are used (or delete this sentence) Line 17/18: write observed glacier length changes. Line 20: write and to make reliable Line 22/23: This is not only the case of southern South America, but applies to all regions. Maybe remark at this point the importance of South American climate and glacier studies/reconstructions (best information for the southern hemisphere available, inter-hemispheric comparisons possible).
   We have changed the text according the suggestions of the reviewer.

3. P3655: Line 2: Which are the other climate proxies? Line 4: write Neukom et al. (2010, 2011)
   We meant the proxies that were mentioned in the reconstruction of Neukom et al. [2011]. However, we have have altered the text, such that this sentence is no longer present.

4. P3656: Line 5: delete thus Line 10/11: delete the southern part of (because the scarcity of historical information applies to the whole Andes)
   done so
   Lines 16-23: This section could be rewritten and made more clear (explain better). In addition, there is a rather abrupt change from the individual case (Glaciar Frías) to this more general section.
   We have revised this paragraph to connect it better to the rest of the text and to make it more clear. The goal of the paragraph is to explain why we need a more sophisticated model for the study of this particular glacier than earlier studies have used to reconstruct temperature from a large sample of glacier length records.

5. P3657: Line 1: write historical (note the difference to historic) Line 1/2: This is not a full sentence. Line 3: Give reference of these records. Line 5: To the accuracy of the existing proxies: it is not clear to me whether you also refer to the accuracy of the glacier record. Line 10: better: a peak 3448 m high on the Line 11: this might be misunderstood: one might read that the glacier itself is temperate and not cold. Line 22: write thinned by
   text changed accordingly

6. P3658: Line 4: What do you mean with field evidence? Line 5: What do you exactly mean with historical position? (Was there only one?). This applies also to line 8 on the following page. Line 6: to the dendrochronological dating: with trees in-situ or not? Line 7: Please give more information on these historical sources, and related uncertainties (beyond Table 1). Line 9: You state that the glacier record has been revised: why has it been revised, and what is new? Is it more accurate now? Is there new evidence? Lines 14-16: This is rather interpretation. Line 19: what kind of satellite images? Line 21: field measurements: do you mean length measurements? Line 25: Give more information on this etching. Line 26: The moraines, how were they dated?
   We use the reconstructions of Villalba et al. (1990) and have added data from (recent) satellite images. We have revised the original record by identifying the moraines, dated by Villalba et al. (1990), on the satellite images. In addition, we have connected the field measurements of S. Rubulis to the record by matching the length measured from a Corona image (1979) with the field measurement of that year. At present all length changes are measured along the same flowline (Fig 1) projected on the same 2009 SPOT image and 2000 SRTM DEM. Following the reviewer's comments we revised this part of the paper to give a more detailed description of the glacier length record.

7. P3659: Line 20: delete a-1 (annual precipitation is always per year) Line 26: write out ELA here (first appearance of the term)
   done so

8. P3660: Line 19: delete also Line 25: reference?
   done so and added reference

9. P3662: Lines 14-16: What are the implications of this simplification for the model results? Line 25: additional question: Why have these records not been used by Neukom et al.?
   We have estimated the implications with some additional runs of the model, see 16.
   Neukom et al. have selected only those records that match well with the CRU TS3 gridded data set. Apparently the tree ring records used in Villalba et al (2003) do not reproduce this data set very well. Villalba et al. use the principal components of local weather stations to derive their transfer function. The difference in data set used for calibration can be an explanation of the difference between the two reconstructions. A discussion of this is included in Section 4.2.2 of the revised paper.

10. P3664: Lines 23-25: Please better explain the choice of those parameters, and why you choose a comparable climate as in maritime Norway. Line 25: The reference to Table 2 should be placed in the beginning of the Section.
    The net long-wave and turbulent fluxes depend on, besides temperature that is a variable included in the parameterisation, the moisture content of the air (including presence of clouds) and wind speed. Thus the ψ function is different for a wet maritime climate than for a dry continental climate. The difference is made by choosing different parameter settings. In the ideal case the parameters

are derived from in-situ measurements. Such measurements do not exist for the Tronador region. Therefore, we have to derive representative parameter values from measurements made in a region with comparable continentality, which brings us to Norway.

11. P3666: It would be helpful to add a list of the variables of Section 3.2, too. Or shorten the Section and give a more qualitative description of the model, including how the glacier length is addressed. *done so*

12. P3669: Line 9: delete a-1 Line 14: Here, a remark on the response time of the glacier could be given. Line 27: better write highly maritime Line 28: But there are large differences between summer and winter. Line 29: Correct the English.
we have corrected the textual remarks and added a comment on the differences between winter and summer sensitivity.

13. P3670: Line 4: Please indicate what concept of response time you are referring to.
e-folding response time. added

14. P3672: Line 11: give reference Line 28: error/uncertainties of the dating of the moraines?
added reference. The timing of two of the advances (max in 1639 and 1916) is not within the uncertainty of the dating of the moraines. revised the text to make this point more clear.

15. P3673: Line 1: write 1970s
done so

16. P3673/3674: To the explanation of the differences: How are the results influenced by spring and/or autumn conditions? What is the uncertainty of the glacier reconstruction? Might there also be an effect of changing ice dynamics during the Little Ice Age (model calibration)? These are crucial questions to be carefully addressed. Note also that the modelling results show discrepancies to both the Neukom et al. and Villalba et al. reconstructions.
We do not have temperature and precipitation anomalies for the autumn and spring months. As the glacier mass-balance model needs input in for every month of the year, we have assigned the summer anomaly to the summer half of the year (November–April) as well as the winter anomaly to the wiinter half of the year (May–October). We have tested whether our results are sensitive to our choice in the division of the year with two additional experiments. We have assigned the summer temperature anomaly to spring months MarAprMay (and winter temperature to autumn months) and the summer temperature anomaly to autumn months SepOctNov (winter anom. to spring). For both experiments, the calculated historical glacier length record is hardly different from the original reconstruction (Figure 3). Hence, the results are not influenced by the exact division of the months of the year into a summer half and a winter half.

    This last experiment does not give the complete uncertainty in our results due the missing autumn/spring information. In our method it is implicitly assumed that the autumn/spring anomalies are similar to the summer/winter anomalies of the same year. We think it is reasonable to assume some coherence over the seasons in warmer and colder periods. For example, we would not expect the 20th century warming (as well as the 21st century warming) to be limited to particular seasons. However, seasonal anomalies in a particular year are generally not identical. It is difficult to assess the influence of different autumn/spring anomalies. We will test the results by imposing random autumn/spring anomalies on the proxy record instead of applying the summer/winter anomalies on spring/autumn months. The winter temperature anomalies prior to 1706 are also random. The anomalies are randomly drawn from a Gaussian distribution with the same mean and standard distribution as the reconstructed winter and summer anomalies. To limit the time of computation, we have not calculated the mass balance for each of the perturbations, but we have used the SSC for the spring and autumn months to calculate anomalies w.r.t. the mass balance record with only winter and summer anomalies. As the SSC is in principle only valid for a specific geometry, this introduces an error which is indicated by the reconstruction of the original result (summer/winter anom. applied to spring/autumn months) using the SSC (black line in Figure 3). This test assumes no correlation between the seasons of a year, i.e. there are no generally colder or warmer periods. The resulting *modelled glacier lengths are shown in Figure 3. The decoupling between summer/winter and autumn/spring anomalies has a leveling effect on the modelled glacier fluctuations. The uncertainty in the reconstructed glacier length is mainly determined by the question to what extent the reconstructed anomalies for winter and summer are also valid for the months not included in the reconstructions. The spread within the ensemble is rather small.*

As discussed in the reply to the comments of Reviewer 3, we do not think it very likely that the ice-dynamics (i.e. the flow parameters) have changed to such an extent that the length reconstruction is much influenced. Generally (apart from surge events), variations in mass balance rather than variations in ice-dynamics are most important in the explanation of the historical glacier fluctuations.

17. P3675: Line 16: explanation for this striking difference?
The most likely explanation, as suggested by R. Neukom, is that the differences in the two reconstructions come from the different data sets used for the calibration of the reconstructions.

18. P3676: Lines 6-19: This paragraph needs to be shortened and constrained to the facts. E.g., the first sentences can be omitted. Line 3676: write IPCC A1B scenario
Paragraph is revised, added IPCC

19. P3677: Line 8: dramatic is not an appropriate word here: do you mean that the retreat is unprecedented (e.g. during the Holocene)?
We wanted to express that a 80% loss of glacier volume is large. It is unlikely that it is precedented in the last four centuries for which we have the length record. We cannot put this in perspective of the entire Holocene, as we do not have any data on glacier fluctuations of Glaciar Frías prior to the LIA maximum extent. We have deleted the phrase in the revised version.

20. P3677ff In general: the Conclusions should be more precise (shorter and more concise).

21. P3678: Line 3: write sensitive to temperature Line 8/9: This is vague. Line 22: The information on stable North Patagonian climate comes very abrupt here (Reference?). Address this earlier in the paper. Line 28: Do you mean climate proxies?
Corrected.
We meant that the modelled mass balance is in line with the regional ELA reconstruction and is in broad agreement with the mass balance measurements on Mocho-Moshuenco, 130 km to the north, during two hydrological years 2005–2007.
We refer to the relatively little change during this period in the climate reconstruction that is based on the dynamic calibration. This reconstruction shows that little change in climate is necessary to explain the difference in glacier length between the maximum extent in 1639 and the mid 19th century. However, this reconstruction is only based on the best match of the maximum extents in this period and thus not fully represents the climate variability. Therefore, this formulation is confusing and we have changed the text on this point in the revised manuscript.
Yes, added climate in line 28.

22. P3679: Line 1: better write represent, otherwise reconstructions reconstruct. . . Line 4: Which reconstructions do you mean here? Line 12: Why are there no mass balance measurements? This might be beyond the frame of the presented paper, but I still want to raise the question.
revised text based on textual remarks (the climate reconstructions of Neukom and Villalba are meant). The reasons for the lack of mass-balance measurements are not strictly scientific and therefore indeed beyond the scope of the article. The glacier was not selected for mass balance measurements, which might have several reasons, e.g. lack of funding, inaccessable because of the steep tongue, more interest in glaciers in semi-arid areas of Argentina (where glacier melt water contributes more to the water supply).

23. P3681: Line 24: Intended journal? Line 33: Storglaciären
The Cryosphere; corrected

24. P3682: Line 11: Briksdalsbreen
corrected

25. P3687: Fig. 1.: Name Monte Tronador in the map to the left.
We will include the name Tronador *in the Figure.*

26. P3688: Fig. 2.: elevation distribution from 2009?
no, the DEM is from 2000 (Section 2.1). Added that in the caption.

27. P3690: Fig. 4.: Give time period for which steady-state is calculated (1980-2009?).
steady-state is calculated for the forcing that represents the 1980-2009 average climate.

28. P3694: Fig. 8.: Measured glacier length. . .: Do you mean reconstructed? Maybe use two colour for glacier measurements (sensu stricto) and reconstructed frontal positions.
Given is the entire length record, based on reconstructed and measured glacier length changes. Changed the caption accordingly. For the origin of the data points we refer to Table 1, the uncertainties (in time and distance) which follow from the origin of the data points are given in the Figure 8.

3.3 Technical comments:

Use consistent spelling throughout the paper:

- ice-cores vs. ice cores
- long-term vs. long term
- high-resolution vs. high resolution
- Sect. vs. Section
- north-east vs. north east
- 1950s instead of 1950s etc.
- mass-balance vs. mass balance
- flowline vs. flow line vs. flow-line
- Glaciar Frías vs. Frías glacier

We have checked the paper for inconsistencies and we have opted for "ice cores", "long-term", "high-resolution", "Section", "north-east", "1950s", "flowline", and "Glaciar Frías". Furthermore, we have written "mass balance", but "mass-balance profile/measurements/model/. . ."

4 Review 3

4.1 General comments

The manuscript provides an important contribution for understanding the past climate of the North Patagonian Andes. By combining a surface energy-balance model and glacier model the authors compared the length fluctuations and corresponding glacier mass balance to independent climate reconstructions. As stated by the Anonymous Referee 2, these combination is also to my knowledge the first attempt for the Southern Andes. The approach is fair and clear, in particular, as they rely on the longest and most detailed glacier fluctuation record for the Southern Andes. The results provide interesting information about the dominating factors which causes length fluctuations.

I am not an expert in surface energy-balance modeling as well as in climatic interpretation, therefore it was sometimes hard to follow. As the other referees (R. Neukom and an Anonymous Referee 2) have already reviewed the paper and made many suggestions (that I have read, and I agree with most of them), in particular on the climatological interpretation, I will try to avoid repeating suggestions, except in a couple of cases in which I wish to emphasize that the change should be done or I wish to provide some further comment on the same subject.

As I am more familiar with glacier dynamics (in contradiction to R. Neukom), I focused on that subject.

From this perspective the authors should take care about the usage of the shallow ice approximation (SIA). SIA is not capable for numerical modeling the flow dynamics of valley glaciers (or small glaciers) like Glaciar Frias. However, I figured out that you are using the approximation which is adequately for modeling the dynamics of a valley glaciers (Both equations are looking almost similar; otherwise I would not trust the modeled ice dynamics). Additionally, the authors should address some more details and/or discussion about the long-term ice dynamics. I dont think that the glacier dynamic only depends on the glacier geometry in the entire investigated time period. As its a manuscript for CP and you dont want to overload the paper with glacier-dynamical issues it would be at least helpful to bring some statements about your assumed simplifications (more details in my comments below). These simplifications and related uncertainties have to be carefully discussed and probably you can refer to them in a short outlook.

In general, the paper is well written and well structured. However, the manuscript lacked in consistency (namings, spellings, symbol usage etc). After adaption and correction of my comments and the comments of the other reviewers, the paper is worth to be published in CP.

I will state my comments/suggestions by order of appearance (not in order of importance), and will include at the end a list of technical comments.

4.2 Specific comments

1. P3653 title include the temporal coverage
   included, see also 25

2. P3658 line 1 and line 10 You give a reference to Fig.8 before Figures 2-7 are referenced, in particular Fig. 8 showing results which are explained later in the text. However, you cannot always account for this in a paper. Especially in this case you can reference Fig. 1b (and Table 1).
   Following the reviewer's advise, we have made reference to the length changes as indicated in Figure 1b, instead of the length record in Figure 8.

3. P3658 line 9 see comment of the second Anonymous Referee 2.
   we have changed this paragraph considerably, following the suggestions of reviewer 2.

4. P3659 line 1 line 9 Please clarify the height reference you use. On P3665 line 14 you provide a height reference as a orthometric height (m a.s.l.). All other heights in the manuscript are just given in m. Are they ellipsoidal heights?
   all heights are relative to sea level.

5. P3659 line 26 write ELA out (first appearance of ELA)
   done so

6. P3660 line 3 I dont found the Volcano Villarica on Fig. 1. Either you drop the Figure reference or include the location of the Volcano.
   We have dropped the Figure reference, as it is difficult to include the name Villarica in the inset of Figure 1a such that it remains readable.

7. P3664 line 22 The parameter T_a is not introduced. The parameter c appears in Table 2 as c_1.
   Both are corrected in the revised version.

8. P3663 line 12 Here B is introduced as annual surface mass balance at a certain point. On P3666 line 18/20 B is named as specific mass balance. Also, you should be especially careful about this, since the Glossary of Glacier Mass Balance has been announced http://unesdoc.unesco.org/images/0019/001925/192525E.pdf The authors provide a convention for lower-case and upper-case usage (Sect 3.2.3): lower-case symbols refer to quantities at a point on the glacier surface or to the column beneath such a point, and upper-case symbols refer to glacier-wide quantities.
   We have added the subscript "ann" to the annual mass balance in equation (1). Furthermore, we intent to follow the UNESCO guidelines in the revised paper, as suggested by the reviewer. We have used lower-case for the specific mass balances. However, we have not used lower-case for the glacier

thickness H (which would be according to the guidelines), as lower-case h refers to the altitude of the glacier surface in this paper.

9. P3663 line 18 You introduce the altitude dependent lapse rate p and make a reference to Table 2. However, in the table the parameter is named precipitation vertical gradient. I suggest to use consistent namings. Check the whole manuscript if the namings in the table are the same as in the text or vice versa.
We have made the teminology in Table 2 in accordance with the text.

10. P3666 line 2/4 If you introduced the reference profile B$_ref$(z) you can also use the Symbol in the following text (for instance on P3668 line 9, P3668 line 21, P3669 line 14/15). Compare my comment for P3669 line 14 line 17.
We have followed the advise of the reviewer by using the symbol b_{ref}, instead of the (sometimes different) formulations that are confusing (see also to this reviewer's comment 19).

11. P3666 line 22 w0 is not introduced
Corrected

12. P3667 line 1 Is H the thickness at the flowline?
Yes, as we assume the cross section has a trapezoidal shape, H represents the ice thickness over the entire width of the glacier w_0 accross the glacier bed (perpendicular to the flowline).

13. P3667 line 6 I am rather sure that you are not using SIA. SIA is valid for the big ice sheets, where the aspect ratio (typical thickness/typical length) is small and ice flow is only determined from τ_{xz} and τ_{yz}. The approximation you use, is the so called cross-sectionflow where τ_{xy} and τ_{xz} remains (cf. Budd and Jenssen 1975, Paterson 1994). May you take a quick look in Greve and Blatter (2009) and compare the SIA and valley glacier equations. For instance, the authors state on page 153 (sect. 7.3): For these reasons, the shallow ice approximation is no longer applicable for small glaciers. However, the equations are looking almost similar, but I would strongly recommend to delete SIA in the text to avoid any misunderstandings.
We do use the shallow ice appoximation: in the derivation of the expression for the vertical mean ice velocity as given in Equation (11) we **i)** *ignore longitudinal stresses and assume that the vertical gradient in the shear stress τ_{xz} accounts for the ice deformation, and* **ii)** *assume isotropic viscosity (the entire glacier is temperate).*
We do not agree with the reviewer that SIA does not work for mountain glaciers. Probably, the SIA will not give satisfactory results on small spatial and time scales, but the focus of this paper is on the long-term (centennial) glacier fluctuations. Leysinger Vieli and Gudmundsson [2004] have compared the difference between a full-Stokes ice model and a SIA ice model for the geometries typical for mountain glaciers. They conclude that the SIA adequately describes the glacier advances and retreats due to changes in the ELA. To describe glacier fluctuations, the main attention should focus on an accurate description of the mass balance.

14. P3667 line 7 I dont found Budd et al. (1979) in the References.
Reference is indeed missing, which is strange. The reference should be: Budd WF, PL Keage, NA Blundy, Empirical studies of ice sliding, Journal of Glaciology 23(89) 157-170 (1979).

15. P3667 line 10 Please explain the values f_d and f_s (similar as in Stroeven et al., 1989). I was surprised about the value of f_d, which looks similar to the commonly used Arrhenius factor/flow parameter in ice modeling (cf. Paterson 1994), which indicates a very cold glacier (Paterson 1994, Hooke, 1981). In this regard you have to explain the thermal state of the glacier (I dont found a statement in the manuscript). I am rather sure you treat the glacier as entirely temperate due to the prevailing maritime climate etc. Than you can also say that basal sliding (second term on the right hand side of Eq. 11) is allowed/expected everywhere.
We divide the ice flux into two components: deformation and sliding, following the suggestion made by Budd et al. [1979]. We describe the deformation with the SIA and the sliding with a Weertman-type sliding law. The value for the sliding parameter f_s follows from the experiments of Budd et al. [1979]. Several values for the deformation velocity parameter f_d for temperate glaciers exist in the literature

(e.g. 1.9 x 10^{-24} [Budd et al., 1979; Oerlemans, 1997a,b]; 0.76 x 10^{-24} [Giesen and Oerlemans, 2010]; 0.6 x 10^{-24} [Stroeven et al., 1989]; 0.92 x 10^{-24} [Flowers et al., 2005]; 2 x 10^{-24} [Le Meur and Vincent, 2003]). These values were based on different considerations: Stroeven et al. [1989], Le Meur and Vincent [2003], and Giesen and Oerlemans [2010] optimized the flow parameters using the (measured) ice thickness, Flowers et al. [2005] used the Arrhenius equation, and Budd et al. [1979] (and therefore also Oerlemans [1997a,b]) are based on Budd and Jenssen [1975]. In this study we take f_d to be 1.9 x 10^{-24}, as we have no independent measurements of the ice thickness to optimize the used values of f_s and f_d.

16. P3668 line 6 line 15 To reproduce the bedrock topography from the inverse modeling is a solid approach. However, I think you have to address the statement The bed profile that reproduces the present-day (referred to 2009?) surface best . . . with some error values. I am wondering why your provided bed-profile is the best: The resulting glacier length is 6025m vs. an observed length of 5550m. Therefore, you would receive very high mismatches for x¿5550, because there is no ice observed. Are you able to keep the ice thickness equal to zero at x=0m and x=5550m during the inverse modeling? I would expect (just a guess) that a glacier geometry with a modeled length closer to the 2009 length will give a better result with respect to a defined error indicator, in particular as your surface altitudes are from 2000-2009. I suggest to include a difference plot of the modeled and observed surface vs. distance in Fig. 2a.
The difference between the DEM derived surface altitude and the reconstructed from the 'inverse modelling' is relatively small, within 10 m (as shown in Figure 4 for $2500 < x < 5800$). This is within reasonable limits, and therefore satisfactory to us. In the revised version we will replace "best" by " within 10 m".
The surface altitude at the first grid point is equal to the second, which is a boundary condition of the ice-dynamical model (a surface gradient between the first and second grid points would induce an ice flux from the top of the mountain). Therefore, we have no problems with the derivation of the bed altitude at the first grid point. At the lower part of the glacier the ice thickness is not limited. The bed for $x > 5570$ is reset to the DEM altitude after each iteration. The ice of the reference run is thicker close to the position of the tongue in the observations ($x = 5570$) than the ice was when the DEM was derived, which leads to an underestimate of the bed altitude for the grid points just before x = 5570 and a jump in the derived bed at x = 5570 m. To resolve this, the derived bed for the 4 grid points (100 m) before x = 5570 is replaced by spline interpolation between bed$_{x=5470}$ and bed$_{x=5570}$.
We will include a difference plot in the revised Figure 2a.

17. P3668 line 8 You have to define an equilibrium criterion.
We assume equilibrium is reached when the length and volume have not changed for a time of 50 year and meanwhile the glacier mass balance has approached zero.

18. Sect. 4.1 Steady state I suggest to include a figure which shows basal velocity, deformational velocity etc. (similar as Fig. 8 in Oerlemans (1997a)) to show that your modeled ice velocities are in a reasonable range. Obviously, ice velocities out of range would highly affect your thickness evolution (Eq. 13). I tried to calculate some by estimating the slope and the thickness from Fig. 2a, some make sense to me some not.
We have included such a plot here (Figure 5). It shows that ice velocities are mostly in the range 50–100 ma^{-1}, occasionally reaching allmost 150 ma^{-1} in the steep parts. Moreover, sliding is dominant with the used values for parameters f_s, f_d. These values for the total ice velocity seem realistic. They have to be, as in steady state the ice flux is completely determined by the prescribed mass balance b_{ref}.

19. P3669 line 14 line 17 It is not surprising, that your modeled glacier length is in good agreement with the observed glacier length and the calculated mass balance is fairly accurate. To summarize your procedure:
    (a) inverse modeling (Sect. 3.2): You are using B$_{ref}$ (Fig.4) together with the glacier model to generate iteratively the bedrock topography until the equilibrium state match the observed width and surface.

Table 1: Equilibrium lengths (m) with different bed topography (rows) and forcing (columns). The subscript in the first column indicates the mass-balance profile that was used to derive the bed (i.e. bed$_{-0.5}$ is the bed derived by using $b_{ref} - 0.5$ in the derivation of the bed that makes the steady state surface altitude (with $b_{ref} - 0.5$ forcing) fit well with the observed surface altitude).

	$b_{ref} - 0.5$	b_{ref}	$b_{ref} + 0.5$
bed$_{-0.5}$	5000	6075	6950
bed$_{ref}$	4875	**5725**	6825
bed$_{+0.5}$	4800	5425	6800

(b) Definition of steady-state model runs (inferred from P3668 line 20): Running the glacier model with time-independent boundary conditions (B$_{ref}$) until an equilibrium state is reached. Again, you have define the equilibrium criterion! You dont dropping the time-dependent terms in Eq. 13?

(c) Steady state (Sect. 4.1): Now, you are using the generated geometry from 1) as input , running the glacier model with B$_{ref}$ (Fig. 4) as forcing until an equilibrium state is reached.

So, you have in advance adjusted the bedrock topography using B$_{ref}$ that it fits roughly to the observed glacier geometry. I dont expect then any sensitivity of the glacier model forced by B$_{ref}$. Your inverse modeling procedure is responsible for your good accuracy to the observed glacier length and calculated mass balance. It is just an idea, a sensitivity analysis would be:
1) to estimate upper and lower bounds of the calculated mass balance profile and run the model with these three scenarios.
2) to vary the bedrock topography. The time period from 1980 to 2009 is characterized with a rapid retreat of Glaciar Frias. Therefore, you can generate three different bedrock topographies with respect to three different glacier lengths (1980, 2009, mean; the latter corresponds roughly to the scenario you have shown). (If you are able to keep the length of the glacier constant in the inverse modeling runs.)
Otherwise, if I got it all wrong (steps 1,2, and 3), I got lost, for instance in your multiple definitions: climatological mass balance (Fig. 4) on P3668 line 9; present-day climatological mass balance profile (Fig. 4) on P3668 line 21 calculated climatological mass balance of the period 1980-2009 on P3669 line 14/15
All mentioned multiple definitions refer to the same b_{ref}: the mass-balance profile from the 1980–2009 climatic mean. In the revised version we will try to avoid confusion by being more consistent.
The reviewer raises the question if the modelled length with the b_{ref} forcing is by definition in agreement with the observed length, because we have used the same mass balance forcing to adjust the bed. The reviewer suggests a sensitivity experiment. We have performed such experiments by using a more positive ($b_{ref} + 0.5$ mwea^{-1}) and a more negative mass-balance ($b_{ref} - 0.5$ mwea^{-1}) profile to derive the glacier bed topography. The differences between the derived bed profiles are limited (Figure 4 and inset), but have an impact on the calculated equilibrium length through the altitude–mass balance feedback (Table 1).
The difference in equilibrium length for the same forcing but with different bed topographies (in the order of 100 m) is much smaller than the differences between the equilibrium lengths for the different forcings with the same bed (in the order of 1000 m). Moreover, the equilibrium length is not the same for any mass-balance profile if this profile is also used to derive the bed topography (cf. the diagonal in Table 1). This justifies our conclusion that the fact that the agreement between modelled equilibrium length and observed glacier length indicates that the mass-balance model gives a reasonable result.

20. P3670 line 9 see comment of R. Neukom regarding the response time.
added number in revised manuscript

21. P3670 line 5 Please reference an Eq. where you added the temperature perturbation.
The perturbation is added to the forcing of the mass balance model, i.e. to the 1980-2009 mean of every month of the year. The climatic forcing is not expressed in an equation. Therefore, we do

not add a reference to an equation, but the temperature perturbation affects the mass balance (and therefore the glacier extent) through the parameterization of $\psi(T)$ (equations 2 and 3).

22. P3671 line 12 I think the unit should be m w.e. a-1 instead of m w.e. a-2
m w.e. a^{-2} is correct. The mass balance is given in m w.e. a^{-1}, the trend in mass balance (change per year) is therefore given in m w.e. a^{-2}.

23. P3673 line 17 line 26 How do you vary the sliding and deformation constant f_s and f_d?
1) Do you vary them in time? In any way related to climate, so that a warmer climate intuitively induces higher deformational velocities and probably changing sliding velocities (Taking some response time into account until the warmer temperatures are transferred to the glacier. I dont have an citation on hand regarding this issue, but I would assume that the glacier reacts more or less instantaneous)? However, if you bring a statement such as the glacier is assumed as to be temperate over entire time-period the motivation for a time-independent deformational constant is roughly justified. To make a statement about the thermal state of the glacier would be also interesting, as you conclude that the length fluctuations of Glaciar Frias are temperature-driven (P3678 line 27). Changing sliding velocities are more difficult to discuss as its regarded as one of the key problems of glacier flow (dependent on various parameters). To pick up the argument of the Anonymous Referee 2, surface ice velocities could also show inter-annual variability probably due to varying sliding velocities which are coupled to the available surface meltwater (drained through crevasses to the base) .
2) Do you just vary them for each individual model run? Please clarify and add a (short) discussion about changing ice dynamics over the observed time period.

1) We have assumed that Glaciar Frías is temperate. This assumption is supported by the calculated mass balance of the glacier. There is summer melt on the entire altitude range of the glacier according to the model (e.g. Fig. 5 in the discussions article). The (modelled) melt water will penetrate and heat the snowpack such that the resulting glacier ice will be temperate for the entire glacier. We also keep the flow parameters constant throughout the year, despite the fact that ice velocities of glaciers are observed to vary during the seasons with an increase in ice velocities at the beginning of the melt season [Iken and Bindschadler, 1986]. Fo the long-term response of glaciers to climate that is studied here, the seasonal variations in ice velocity are of minor importance. We assume that the calculated velocity represents the annual mean ice velocity.

2) In the test of the sensitivity of the model result to the chosen parameters f_s and f_d are different for the different runs. We have not included time-dependent variations of the flow parameters. We think the glacier is temperate throughout the entire period under consideration. It might be that interannual variations in melt water production lead to interannual variations in sliding velocities, but any prescribed relation would be speculative for Glaciar Frías and we did not include it in this study. Furthermore, we think that possible variations in the ice dynamics are less important than the variations in the mass balance.

Additionally, see comment of the Anonymous Referee 2 for P3673/3674.
For the reply on this comment we would like to refer to the reply on comment 16 of Reviewer 2.

24. P3674 line 4 write SE out (first appearance of SE)
done so

25. P3686 Table 2 Some parameters of the ice-dynamical model are missing (density, the acceleration due to gravity).
ice density is given in the Table, gravity constant is added.

26. P3687 Fig. 1 In the legend the accumulation area is colored in black. Probably a problem with my printout. Take care of this within the proof-read stage. Additionally, contour lines a hard to identify. May you can improve it by making them thicker and/or choose another color. I also missing the coordinate axes.
Figure 1 will be revised and checked in the proof reading

27. P3688 Fig. 2 Please rephrase the figure caption. I think the first sentence is misleading. The surface altitude is taken from the DEM. The bedrock topography is derived from your inverse modeling.
caption is reformulated

28. P3689 Fig. 3 temperature label: unit is C
corrected label

29. P3690 Fig. 4 As I understood this is the B_{ref}(z) profile? ! use B_{ref}(z) in the caption. What means the abbreviation NB?
That is correct, it is the b_{ref} profile, we have included that in the caption. NB stands for nota bene *(= take special note).*

30. P3693 Fig. 7 Are the model runs with T=+1K around 150a in equilibrium? In the caption and the manuscript please use T= value. The value t=25a is not explained in the text.
The T= +1 °C has just reached equilibrium at t=150 year (i.e. 125 year after the perturbation). This is not evident from the graph. We chose for a limitation of the displayed time to keep some detail in the displayed retreat of the other 3 scenarios.
There is no specific meaning in the year t=25. As written in the text the perturbations are performed on a equilibrium state. The equilibrium state is displayed in the time t<25. This time is arbitrary. We will in the revised version set the perturbation at t=0 and show from t=-25 to t=125. Maybe that improves clarity.

4.3 Technical comments

- Either avoid present-day or define it at first appearance on P3659 line 8.
- Use equal namings/spellings for parameters both for multiple appearance in text and appearance in text and tables.
- ! see Technical comment of Anonymous Referee 2
- Although it is clear in most cases which model do you run, but please avoid to use only model in the text (for instance P3668 line 8). Choose a consistent naming for both models in the whole manuscript.

We avoid present-day by replacing it with 1980-2009 or XX_{ref}, and we check the manuscript for consistency.

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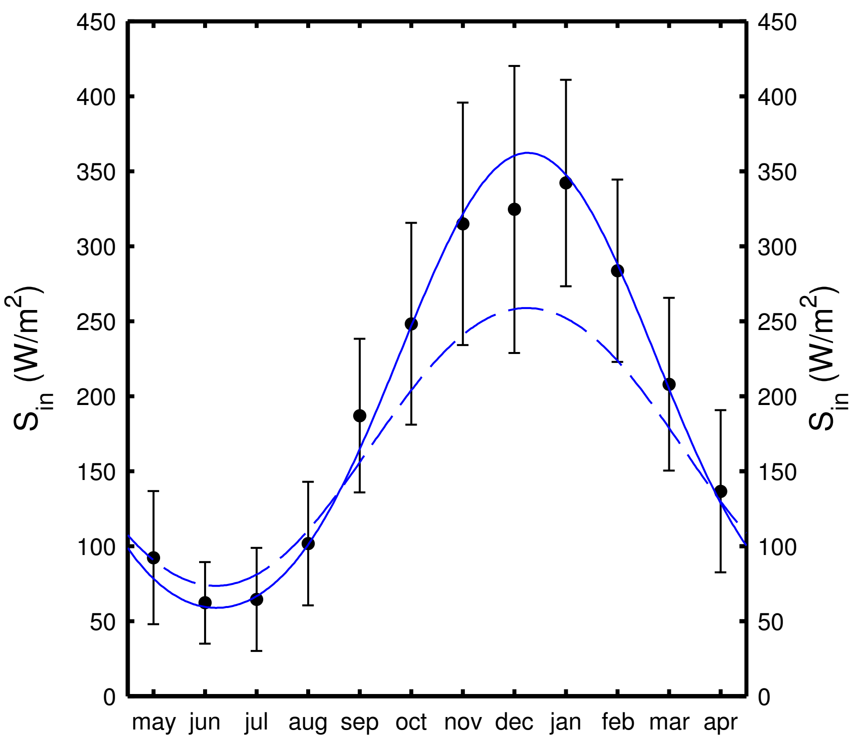

Figure 1: Measured daily average incoming solar radiation at Volcan Mocho Choshuenco (black), data kindly provided by M. Schaefer, CECS. Two years of measurements are binned in monthly intervals. The dots represent the mean of measured daily averages, bars give 1 standard deviation. The blue line gives the modelled daily average incoming radiation at this latitude with a seasonally varying τ. A transmissivity given by $\tau = 0.55 + 0.15 sin(\frac{2\pi t}{365} + \delta t)$ (t in days and δt a phase shift of $\frac{6}{5}\pi$) gives a good match between model results and observations. The modelled incoming radiation with a constant $\tau = 0.5$ is given in dashed blue for comparison.

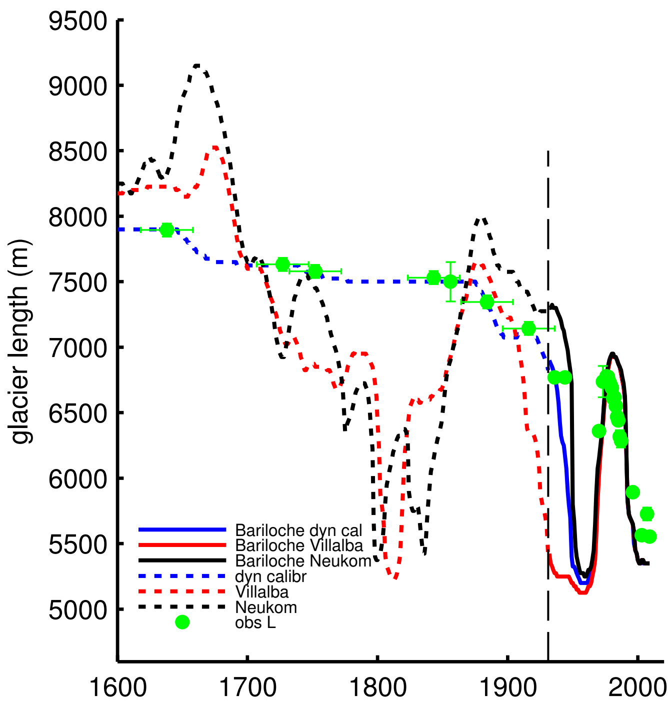

Figure 2: Modelled glacier length with the temperature anomalies of the Bariloche station as forcing in the period 1931–2008. The model is initialized three times with 1) the result of dynamical calibration; 2) the climate reconstruction of Villalba 2003; and 3) the climate reconstructions of Neukom 2010, 2011. The dashed vertical line indicates the start of the model run forced with Bariloche T_{anom} in 1931.

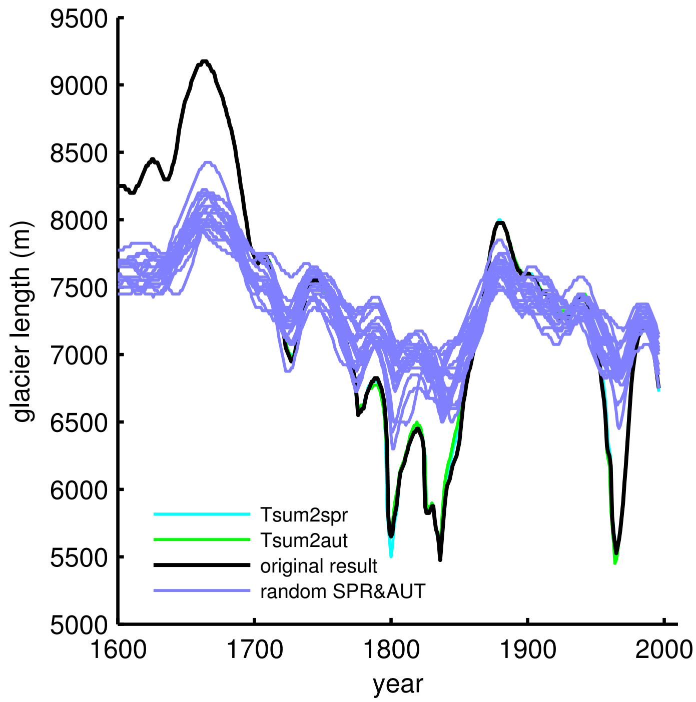

Figure 3: Sensitivity experiments spring and autumn temperture and precipitation. The reconstruction with summer temperture/precipitation anomaly applied to NOV–APR and winter anomalies to MAY–OCT (the original result) is given in *red*. The reconstructed glacier lengths when summer anomalies are applied to SEP– FEB (winter to MAR–AUG) and when summer anomalies are applied to DEC–MAY (winter to JUN–NOV) are shown in *cyan* and *green*, respectively. These are almost identical to the original result. The glacier length from the experiment in which the autumn and spring (MAR–MAY, SEP–NOV) temperatures and precipitation anomalies are random and independent from the winter and summer anomalies in the same year is given in *blue*. The ensemble consists of 20 odel runs. The resulting mass-balance anomaly is not calculated directly, but deduced from the SSC.

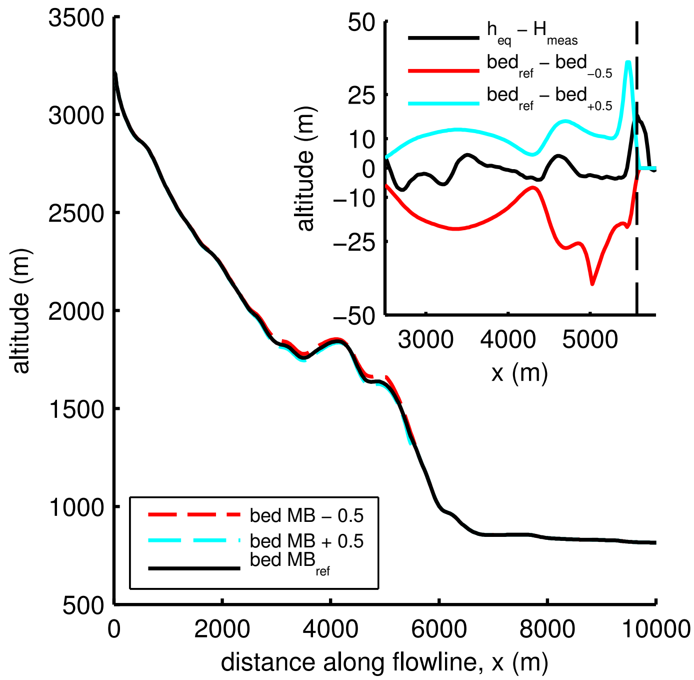

Figure 4: Sensitivity of the derived bed profile to the reference mass-balance profile used in the iterative procedure. The bed profile from b_{ref} is given in black. The bed is also derived with the mass-balance profiles $b_{ref} + 0.5$ mw.e.a^{-1} (cyan) and $b_{ref} - 0.5$ mw.e.a^{-1} (red). In the inset, the difference between the DEM surface altitude and the modelled surface altitude in steady state with MB $= b_{ref}$ (black), and the difference between the bed derived from the ice dynamical model driven with b_{ref} and the bed derived from $b_{ref} \pm 0.5$ mw.e.a^{-1} ($+$ cyan; – red) are shown. The vertical dashed line indicates the glacier length in 2009.

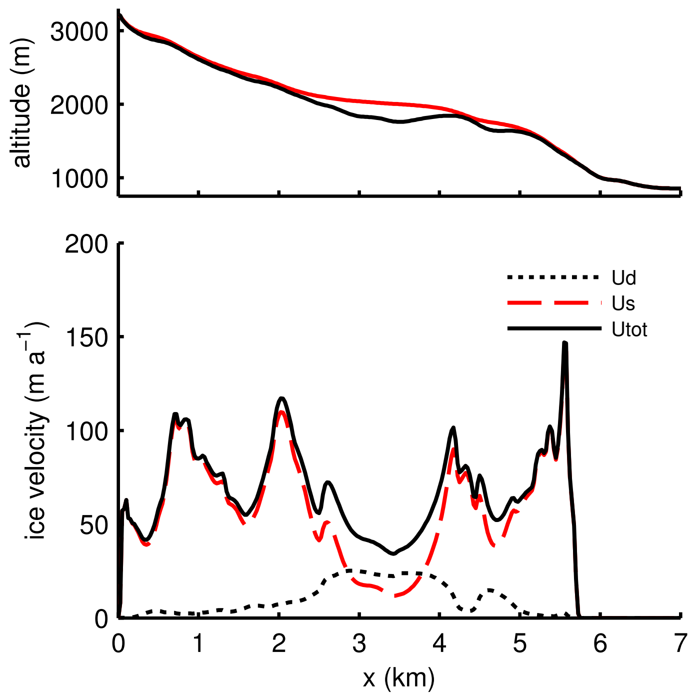

Figure 5: The upper panel shows the bed (as in Figure 4) and the ice surface height in equilibrium state when forced with b_{ref}. The lower panel shows the steady state vertical mean ice velocity (black) and its two components, vertical mean deformational velocity (U_d dashed black) and sliding velocity (U_s dashed red), along the flowline.