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Interactive comment on "ENSO influence on surface energy and mass balance at Shallap Glacier, Cordillera Blanca, Peru" *by* F. Maussion et al.

Anonymous Referee #2

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General comments

Congratulation, good paper! This paper analyses the relationship between mass balance of Shallap Glacier and ENSO phases (mostly sea surface temperatures of Pacific Ocean), based on a downscaling model dealing with mass and energy balance. Although the ENSO – MB relationship has been previously established and findings are not new, this paper brings a substantially new approach that can be used for other glaciers and eventually other regions (and in open access). Using this method, the findings regarding the sensitivity of glaciers to ENSO phases ("Niño –> dry and warm –> negative MB; Niña –> wet and cold –> positive MB") have been confirmed and extended back in time. This methodology is very promising and according to me, is worth

being published in The Cryosphere. In its present state, the paper is well-written, well-organised, figures are good and the reference list is exhaustive. An analysis of the limitations of the methodology as well as the uncertainties is presented. I recommend publication of this paper, with minor revisions. Find below some comments that should be addressed before publication.

1. I am wondering how dependent the results are on the choice of the ENSO classification. In this paper, the authors are considering the Niño3.4 region SSTA with a threshold of 0.5K or -0.5K following Trenberth (1997) to separate Niño, Niña or neutral hydrological years. Then part of the analysis (Fig. 6, 7 and text) is done based on this classification. But would the results have been changed using a different threshold (sometimes the used threshold is 0.4K / -0.4K), or the Niño1.2 region (like in Francou et al., (2003) - see specific comment #9)? It is also surprising to make this classification on an annual basis although the analysis is conducted at monthly scale. Using an annual scale explains why the classification applied in this study differs from the classification obtained with the multivariable ENSO index. I suggest to change the ENSO classification and to follow the classification obtained with MEI. If the authors decide to keep the original classification, some justifications are needed and a comparison of the results using different Niño/Niña periods could be interesting.

2. I believe that the analysis of the performance of the model especially regarding its ability to reproduce the glacier-wide MB (p3016 mainly) should be done more carefully (see my specific comment #11). I suggest to add a figure with the modelled gradient of MB as a function of altitude (VMBG), and to perform a detailed comparison with available observations: comparison between modelled and VMBG measured on some glaciers in the Cordillera Blanca and comparison between the mean modelled MB over the period 1980-2013 and observations (see Rabatel el al, 2013).

3. This analysis spans over a 33-year period, but some reanalysis products used in this study are available before 1980 (section 5.3). It would have been interesting to extend back in time this analysis to check if the relationship between SST and MB is

still valid before 1980.

Specific comments

1. P3005, line 1: how can periods overlap when the AWS on the moraine stopped in 2009, and the AWS on glacier started in July 2010?

2. P3005 line 24: it is equivocal to speak about “conductive heat flux from the ground”. The energy flux inside snow/ice or into the glacier body is a better formulation

3. P3006 line 15: why assuming that F=QM although in the calculations, surface temperature is probably available? This might bring a substantial bias especially for the elevation slices in the vicinity of the ELA (Fig A1) and also but to a lesser extent in the accumulation area (explaining why the BSS score is low at high elevation as stated p3015, line 21). Moreover, this assumption will limit the transferability of the model to other regions than the tropics i.e. mid-latitude or polar glaciers.

4. P3010, lines 16-17: this statement depends on your classification (see general comment #1). Actually, it is not true that Niños are often immediately followed by Niña (3 times over 7 years on Fig 2) and due to the classification applied here, Niño are always one-year long but it is usually not true: 91-93 is considered as a multi-year Niño, and some Niños are shorter than 1 year.

5. P3011 line 15: how can the model better catch the precipitation inter-annual variability than the reference climatology?

6. Fig 3: the agreement between full model and reference is good because the whole period serves as calibration. What are the results if only a two-year period is used as a calibration period, and the 2 remaining years as a validation period? And there are also some data from the AWS on the glacier (July 2010-Sept 2012) not used so far. I think it would be interesting to compare modelled data with observations during this period 2010-12 for validation.

7. P3012 line 5: how can modelled precipitation be negative? What does that mean?

8. Fig 4: it would have been interesting to see QS versus air temperature and QL versus wind speed as well.

9. P3014, line 21. Francou et al (2004) based their analysis on Antizana Glacier (inner tropics) and obtained the best correlation between monthly MB and SSTA in Nino3.4 region applying a 3-month time lag. But for the outer tropics (Chacaltaya Glacier), Francou et al (2003) obtained the highest correlation between MB and SSTA in Nino1.2 region with a 2-month time lag (revised to 4-month lag by Rabatel et al (2013)). Since Shallap glacier is located in the outer tropics, I would have intuitively expected a better relationship between MB and SSTA in Nino1.2 region with a 2 (or 4)-month time lag instead of SSTA in Nino3.4 with a 3-month time lag. Could you comment on that? And what are the results with SSTA (Nino1.2; 2-month lag)? and with SSTA (Nino1.2; 4-month lag)?

10. P3015 line 23-24 : What is the reason for negative MB at 5450m? Over-estimated melt (see specific comment #3 above) or under-estimated solid precipitation, or both?

11. P3016 line 1-13: it is a true problem while considering tropical glaciers where this transient snow cover in the vicinity of the ELA is responsible for a large variability of albedo, and consequently of the MB, especially during the wet season (which is the main melting and accumulation season and in turn the important season for the MB). According to me, this should be less a problem for mid-latitude glaciers, because in summer, the surface state will be less variable in time. May be I mis-understood here, but I do not understand why the authors say that “DownGlacier will perform poorly on mid-latitude glaciers” (line 11). I believe that their conclusion i.e. to complement this approach with physical albedo models (line 13) is more justified for tropical glaciers. Since the model in unable to reproduce abrupt changes of MB from one month to the other, in the vicinity of the ELA, due to surface conditions, which is a key process over tropical glaciers controlling the MB, I do not understand why finally it performs rather well (line 15). I am wondering if there is any error compensation here, with for instance an over-estimation of modelled accumulation in the upper part of the glacier

compensated by a too high melting in its lower part? It might be instructive to show the gradient of MB as a function of elevation. P3016, line 22: MB (1980-2013) = 0.04 $\pm$0.4 m w.e./yr. It looks high (Rabatel et al (2013) give values closer to -0.6 m w.e./yr over this period see their fig 8) and difficult to explain only by the fact that glacier geometry is assumed unchanged. Any explanation for this?

Technical corrections

1. P3000; P3001, line 7 remove “the” right at the beginning of this line
2. P3003, line 14 and everywhere in the text and reference list : Francou et al., 2003
3. P3010, line 5 : the base period is 1980-2010 (caption fig 2) or 1981-2010 (line 5)?
4. Tab2: what is MSub? I believe it is MSubs i.e. subsurface melt (eq. 2)
5. Fig 13: y axis : add SSTA at the end of the legend “Correlation with Nino3.4 SSTA”

Interactive comment on The Cryosphere Discuss., 9, 2999, 2015.