Response to C. Mayer (Referee)

(Original comments are in bold, our replies are in standard font.)

General This manuscript presents glaciological/geophysical observations from two glaciers in the Nepal Himalaya. Given the number of continuous glacier monitoring studies in this region, this presentation provides an important insight into the mass balance conditions for one of the main glacier regions of the world. Even though Pokalde glacier is tiny, the comparison between these two glaciers shows interesting details about the influence of monsoon strength, humidity and glacier setting on the specific mass balance. This makes the manuscript a valuable contribution to our knowledge about the mass balance conditions in the monsoon influenced region of the eastern Himalaya. There are, however, several points which should be improved or need clarification in order to increase the quality of the manuscript. Besides some inaccuracies in the description of the experimental setup and the mesurements, especially the "Discussion and Conclusion" section is not well organised and contains information which should be moved to the "Results" section. The conclusions is very short and not very satisfying. Also the estimation of errors is not convincing in several sections of themansucript and should be reconsidered

Thank you for your comments. Your comments have been taken into consideration. See below for specific replies.

Specific comments: P 3338, L 7: The annual precipitation can fall as rain or snow. This is a crucial point which should be considered much more in this manuscript. There is only one short comment in the discussion about the lower limit of snow fall during monsoon. But this is probably one of the determining parameters for the balance conditions: how is the evolution of the boundary between solid and liquid precipitation duringthe summer?

We fully agree that the snow/rain limit in summer is probably a key parameter for the balance conditions, as well as the onset of the monsoon season (Mölg et al., 2013). But unfortunately, in one hand, our mass balance series is still too short to establish a significant statistical relationship between meteorological variables (i.e. amounts of snow and rain at ELA during summer or June precipitation, for instance) and annual mass balance data, and in the other hand, our reconstructed meteorological dataset at hourly time-step (especially for precipitation) was not accurate enough to provide a clear understanding of the annual variability of the mass balances since 2007. Figure B gives monthly amounts of snow and rain reconstructed at Pyramid (5035 m a.s.l.) using data from other stations when missing and extrapolated at 5500 m a.s.l. considering a temperature gradient of -5.75 °C km^{-1} and a threshold temperature of 1°C to discriminate snow and rain at hourly time-step (see section 6.4 for details). Over the 4-year period where a qualitative comparison is possible (no data in 2008-2009 for precipitation), there is no clear relationship between the annual mass balance and either the amount of rain or snow received at glacier elevations, or the quantity of precipitation in June. The discussion regarding these points in section 6.4 has been completed, but we do not think that Figure B could bring significantly more information than that of Table 2. Thus it has not been added in the revised manuscript.

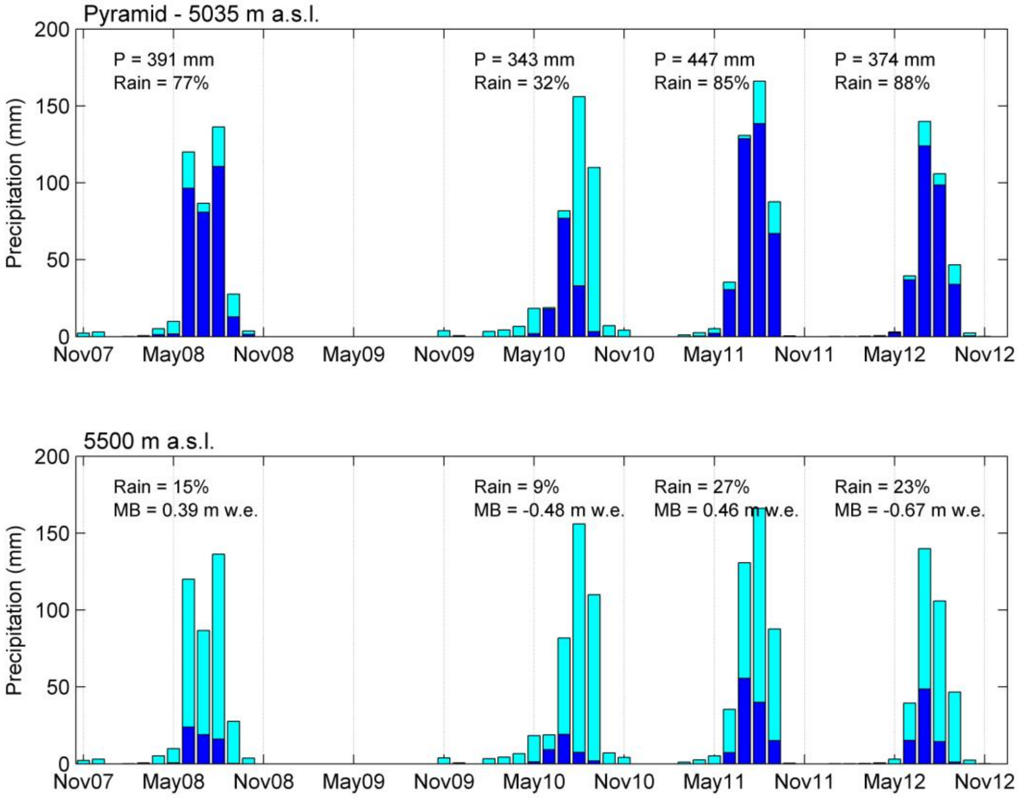

Figure B: Monthly amounts of rain (dark blue) and snow (light blue) from November 2007 to November 2012, reconstructed at Pyramid (upper panel) and estimated at 5500 m a.s.l. (lower panel) following the method described in section 6.4. 2008-2009 data are missing. Also shown are the total annual of precipitation (P), the percentage of annual precipitation falling as rain at both elevations (Rain) and the annual glacier-wide mass balance (MB).

P. 3340, L. 24: You provided numbers for the mass balance observations, please provide also number for the lowering of the Khumbu Glacier surface.

The sentence has been completed as follow:

"Kadota et al. (2000) followed by Nuimura et al. (2011) also measured surface lowering of Khumbu Glacier between 1978 and 1995 and then again in 2004. They observed no significant elevation change near the terminus over the whole studied period, a remarkable acceleration of surface lowering after 1995 in the middle part of the debris-covered area (present elevation change rate of ~2 m yr^{-1}) and a temporally consistent lowering rate in the uppermost part of this area (~0.6 m yr^{-1})."

P. 3341, L. 23/24: This is a strong statement. In my opinion these five years are a good start for understanding the glacier mass balance in this region. But you probably need another 15 years in order to really have a clue what is going on.

That's exactly our plan, to maintain a long-term measurement program including glaciological and meteorological monitoring. We hope that we will not have to wait 15 years more to have the first clues to understand the main climatic drivers for Nepalese glaciers!

P. 3342, L. 2: In fact the glacier is located between Hinku and Hongu Valley, because it drains into both valleys.

It is true that the glacier is draining into Hinku Valley (West) and Hunku Valley (East) as named in the Khumbu Himal Map, 1:50,000, Arbeitsgemeinschaft für vergleinchende Hochgebirgsforshung, 1965 and reedited in 1999. Hunku valley name has been included in the revised manuscript:

"Mera Glacier (27.7°N, 86.9°E, 5.1 km^2) is a debris-free glacier straddling Hinku valley and Hunku valley (Dudh Koshi basin, Everest region, central Himalaya), and has been monitored since 2007 (Figs. 1-2)."

P. 3342/3343, Section "Climatic setting": The description how you derived continuous data sets from the meteorological data is not really clear.

Some details have been added for clarification:

"Using the ten-year 2003-2012 dataset, the mean annual cycle of monthly air temperature (or precipitation) has been obtained by averaging all the monthly temperature available for each month of the year (i.e. all records from the January months between 2003 and 2012 were averaged together to give the monthly temperature of January in Fig. 4). When data were missing during more than 10 days in a month, the corresponding monthly record was not considered for the averaging. It is noteworthy to say that the resulting mean annual cycle of monthly temperature or precipitation is different from the real mean cycle for the 2003-2012 period because of numerous and random gaps in the dataset, but it allows us to discuss the local climate."

P. 3343, L. 21: I am not sure if "inner in the mountain range" is correct. Please check.

Changed into "closer to the Tibetan Plateau". Inner also changed in "interior" P3342/ L17.

P. 3344, L. 4/5: I do understand why the measurements are made in November from a practical point. But there is no reason given why this timing should give reasonable results for seasonal mass balances. In any case there is a discussion lacking about the meaning of the classic alpine approach of seasonal mass balances for summer accumulation glaciers.

This point is now discussed in the dedicated section 4.1:

"For practical reasons, measurements are performed each year in November on Mera Glacier or between the end of October and the beginning of December on Pokalde Glacier. It would have been more appropriate to systematically carry out field trips at the end of the melt season (i.e. beginning of October), but considering that neither ablation nor accumulation are significant between mid-October and mid-December on Nepalese glaciers (see section 5.2), selecting this period for field measurements does not have significant impact on the determination of annual and seasonal mass balances."

P. 3344/3345 and further on: In my opinion the description of the measurements is a bit confusing. Comparing the number of stakes in the figures with the text, they do not match. Also there are stakes in the figures which are outside of the Mera Glacier. Are they also used in the study. It

seems from the further description of measurements that there are also some stakes at the snow pit locations, but this is also not really clear. As well, the highest snow pit is in the drainage basin of the Naulekh part, but is used for determining the balance gradient of the Mera part. A critical review of consistency is required about these details.

> This methodology section was indeed confusing and was lacking some important information. All points raised by the reviewer have been addressed and the entire section has been completed. The entire section is now:

"Since 2007 and 2009, annual mass balance measurements have been carried out on Mera and Pokalde glaciers respectively. For practical reasons, measurements are performed each year in November on Mera Glacier or between the end of October and the beginning of December on Pokalde Glacier. It would have been more appropriate to systematically carry out field trips at the end of the melt season (i.e. beginning of October), but considering that neither ablation nor accumulation are significant between mid-October and mid-December on Nepalese glaciers (see section 5.2), selecting this period for field measurements does not have significant impact on the determination of annual and seasonal mass balances. Extra stake readings have been performed in April 2009 and April 2013 on Mera Glacier, and every 2 to 5 months on Pokalde Glacier. The direct glaciological method is used for these measurements (Cuffey and Paterson, 2010).

In the ablation area, annual mass balance is determined from bamboo stakes inserted up to 10 m deep in the ice. In the mass balance calculations, ice density is assumed to be 900 kg m^{-3}, and in the presence of snow, its density is measured in the field. Snow densities are not very spatially or temporally variable with average values of 370 kg m^{-3} (standard deviation of 30 kg m^{-3}) below 5600 m a.s.l. The number of ablation stakes has progressively increased on Mera Glacier from 28 in 2007 to 45 stakes in 2012, and all stakes are located between 5000 and 5550 m a.s.l. (Fig. 2). Five stakes have been inserted on Pokalde Glacier in 2009 between 5500 and 5600 m a.s.l. (Fig. 3).

In the accumulation area above 5600 m a.s.l., 6 cores (5 on Mera Glacier, 1 on Pokalde Glacier) are drilled to measure the annual net accumulation from snow layering (stratigraphy) and density measurements. Snow densities ranged from a mean of 380 kg m^{-3} (standard deviation of 30 kg m^{-3}) at 5700-5800 m a.s.l. to 450 kg m^{-3} (standard deviation of 10 kg m^{-3}) at ~6330 m a.s.l. Recco avalanche reflectors systematically tied to 3-m long bamboo accumulation stakes and blue-coloured chalk powder spread out over a 2 m^2 surface mark an annual horizon. This horizon is then located easily the following year thanks to the Recco detector, and the previous annual surface is identified while drilling. During years of high accumulation, ablation stakes at ~5500 m a.s.l. also serve as accumulation stakes and conversely, when ablation is strong, accumulation stakes are used to measure ablation.

Stakes are sometimes lost, buried under snow or broken by wind or climbers but our observation network allowed for a minimum of 17 point mass balance measurements in 2011-2012 (reading of 14 ablation stakes and 3 accumulation measurements performed) to a maximum of 31 in 2008-2009 (reading of 27 ablation stakes and 4 accumulation measurements performed). Ablation measured at stakes located on Naulek Glacier outside the Naulek branch of Mera Glacier are consistent with ablation measured at the Naulek branch stakes (inserted on similar slopes with same aspect, Fig. 2). Those stakes have thus been included in the mass balance calculations to increase the number of ablation measurements especially during years where this number was low (i.e. 2009-2010 and 2010-2011)."

> And in the mass balance calculation paragraph:

> "We consider that the single measurement performed at ~6330 m a.s.l. is representative of the uppermost net accumulation in the drainage basins of both Mera and Naulek branches."

P. 3346, L. 4: In order to assess the quality of the DGPS measurements, some more details are required. Which type of instrument (single/dual frequency), length of base lines, occupation times at the stakes etc. It is surprising the the accuracy is the same for the horizontal and the vertical component.

> Topcon devices, dual frequency, 1-sec acquisition frequency, ~30-sec acquisition time, now specified in the revised manuscript. A new sentence dealing with accuracy has been added:
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> "The accuracy of DGPS measurements depends on the number of operating satellites, their geometrical configuration in the sky, the distance to the DGPS base station and the acquisition frequency and duration; maximum uncertainty is ±0.1 m for horizontal and vertical components, the horizontal uncertainty being usually lower."

P. 3346, L. 21: I am surprised that the positioning error during radar measurements is better than for the stake measurements. Can you provide a reason?

> The accuracy is less good for stake measurements because the positioning error for stake measurements takes into account not only the DGPS error but also the uncertainty related to the size of the hole commonly surrounding the stakes at the snow/ice surface. This point is now clarified with the new sentence added concerning DGPS accuracy (see previous comment).

P. 3346, L. 23ff: For me this description of obtaining the bedrock geometry is rather unclear. The reclection you obtain is dependend on the bedrock geometry. If the bedrock is very steep you will never get a reflection from the point perpendicular beneath the instrument.

> We agree that reflections come from the nearest points beneath the instrument and thus not systematically from the perpendicular point below. It was not clear in the original MS and has been clarified as follow:
>
> "The surface of the bedrock was constructed as an envelope of all ellipse functions, which give all the possible reflection positions between sending and receiving antennae. Reflections are not considered to come from the point perpendicular beneath the antennae but from the bedrock point nearest to the instrument. Estimates of bedrock depths were then migrated and interpolated to reconstruct the glacier/bedrock interface in two dimensions. In this way, we account for the bed slope. See Azam at al. (2012) for details of the methodology and an example of a radargram acquired on Chhota Shigri Glacier (India) using the same device."

P. 3347, Section 4.4: It would be good to have an idea about the distribution of DGPS points used for validating the elevation model.

> The DGPS tracks used to correct the planimetric shift of the SPOT5 image and DEM have been added in Figure 1. Figure C below shows also the DGPS points used for validating the Pleiades DEM of Mera Glacier.

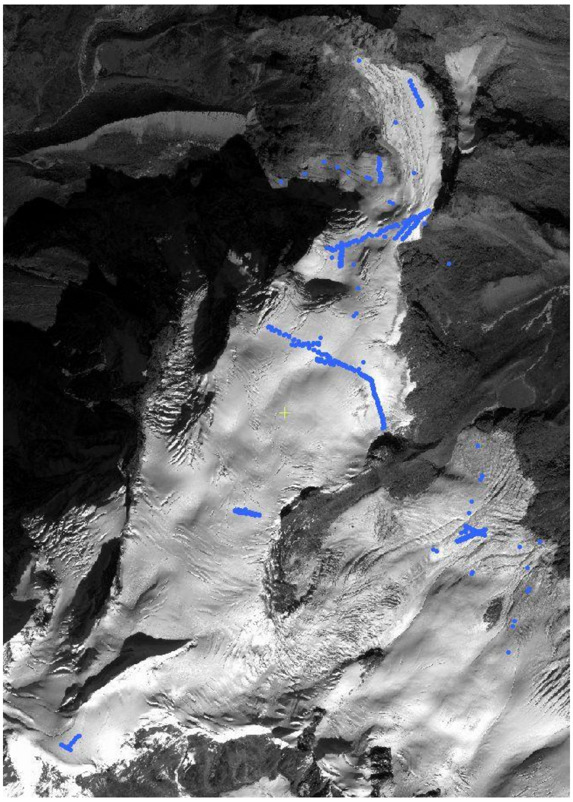

Figure C: Map of Mera Glacier showing the ground control points measured with DGPS in the field in November 2012 used to validate the Pleiades DEM of this glacier

P. 3348, section 5.1: It is rather unclear how you produced the balance gradients. Later in the text you mention that gradients are derived seperately for the ablation and accumulation parts. But Fig. 3 shows a rather different picture. For the first four periods also the first few points in the accumulation area are used for detemining the balance gradient of the ablation area. However, they are neglected for the gradient in the accumulation area. For the period 2011-12 even two stake measurements of the ablation area are used for determining the gradient in the accumulation area. This is not very convincing. You can produce any gradient you wish in this way. There should be a clear description on how you derived your balance gradients, otherwise the entire discussion is useless. As adressed earlier, the highest snow pit is located on the Naulekh part, but is used for the Mera part. In my opinion this is not problem, because accumulation conditions probably do not vary much close to the summit, but it should be made clear in the text.

We agree that the way of separating gradients in the ablation and accumulation parts was rather inconsistent as already raised by M. Pelto. Actually, originally, we used point mass-balance data above and below 5600 m a.s.l. to derive gradients in the accumulation and ablation zones respectively, but this was not really consistent in 2011-12 where point mass balance data measured at 5670 and 5790 m a.s.l. were negative. Consequently, exceptionally for this specific year, both point mass balance data have been used to derive the linear regression line over the ablation area, the upper regression line being chosen equal to the mean vertical mass balance gradient obtained over the four previous years in the accumulation area, i.e. 0.06 m w.e. (100 m)$^{-1}$. The resulting 2011-2012 glacier-wide mass balance is now -0.67 m w.e. (instead of -0.77 m w.e.), the new ELA is 5800 m a.s.l. (instead of 6055 m a.s.l.) with AAR = 0.29 (instead of 0.10). The whole manuscript has been corrected accordingly and details of the method are now provided in the caption of Fig. 3 (Fig. 5 of the revised MS) where two sentences have been added:

"Also shown are the linear regression lines used to derive the annual glacier-wide mass balance B_a (point mass-balance measurements above (below) 5600 m a.s.l. are used to derive the mass balance gradient over the accumulation (ablation) area except in 2011-2012). [...] In 2011-2012, point mass balance data measured at 5790 and 5670 m a.s.l. were exceptionally negative and thus used to derive the linear regression line over the ablation area, the upper regression line being chosen equal to the mean vertical mass balance gradient obtained over the four previous years in the accumulation area, i.e. 0.06 m w.e. $(100\ \text{m})^{-1}$."

The correlation coefficient r^2 between annual glacier-wide mass balance, and ELA is now 0.97 (n = 5 years), instead of 0.90.

As mentioned earlier, we assume that the point mass-balance measured at 6330 m a.s.l. over the Naulek branch basin is representative of both drainage basins, Mera and Naulek:

"We consider that the single measurement performed at ~6330 m a.s.l. is representative of the uppermost net accumulation in the drainage basins of both Mera and Naulek branches." Added in section 4.1

P. 3349, L. 19ff: How do you define summer and winter balance, when you do your measurements in November? Given the characteristic precipitation distribution, the main accumulation occurs probably during July and August, depending on the thermal situation. In some years rainfall reaches high elevations, in some years the snow line is very low in the summer. But during September and October, precipitation is very low and there are still enough clear days with strong melting conditions. During this period of the year, however, this region is rather often coverd by low clouds (from my own experience). Therefore the total melt during this period of the year depends very much on the local situation. This cannot be considered in this mansucript. But it might be a good idea to try and obtain observations already during the autumn months, in orer to evaluate the detailed discrimination between accumulation and ablation conditions.

In this specific section dealing with seasonal mass balance (section 5.2), winter and summer mass balances are defined according to the measurement dates i.e. 6 Nov. 2008 – 24 Apr. 2009 (winter 2008-09); 24 Apr. – 5 Nov. 2009 (summer 2009); 23 Nov. 2012 – 18 April 2013 (winter 2012-13) ±4 days. It was clearly specified for 2008-09 but not for 2012-13, which is now the case. We also added the exact months in Fig 6 (old Fig 4) caption, and the exact dates in Fig 7 (old Fig 5) legend, to avoid any confusion. We fully agree with your comment regarding the accumulation and ablation distribution over the monsoon months even though we guess that precipitation is lower in June and September than in July and August but still significant (Fig. 4 (= old Fig. 2)). More observations are required to have a full understanding of accumulation and ablation regimes over this key period.

P. 3349, L. 23/24: It is probably worthwhile to mention that ablation during winter mainly affects the snow which is deposited in the winter. Summer snow usually is protected by a melt crust and is hardly removed by the wind.

We agree and added the following sentence:

"Snow accumulated in summer is likely to be less remobilized because it is often protected by a melt crust."

P. 3351, L. 10/11 What is the effect of the different size of the accumulation basin?

It is hard to say. Above 5800 m a.s.l., both Mera and Naulek branches have a similar accumulation area (Fig 9 (= old Fig. 7)), and consequently we believe that the snout position depends mainly on the mass balance gradient. Differences in topography must also play a significant role.

P. 3351, L. 28: The sruface velocities are measured, not estimeted

Yes, the word "estimated" changed into "measured".

P. 3353, L. 10ff: The error estimate of the GPR derived ice thickness is not convincing. Even the accuracy depending of the wave length is about 10m for a frequency of 4 MHz. But then picking errors and uncertainties about wave speed (ice temperature) are not included.

The error estimate of the GPR derived ice thickness is assumed to be ±15 m like in Azam et al. (2012) where we applied exactly the same device and methodology. At 5520 and 5350 m a.s.l., there is only ice except for the first 1 or 2 meters (ablation zone) and thus the uncertainty due to the wave speed (167 m μs^{-1} for ice) is likely to be low. The uncertainty related to the variation of wave speed as a function of ice temperature cannot be accounted for because this temperature is not known. Radar wave velocity in ice is mainly a function of permittivity and to a lesser extent, electrical conductivity and radar wave frequency. The permittivity of ice is sensitive to material properties, including crystal orientation and water content. The uncertainty of the propagation velocity of radar waves in natural ice can be assessed to ±10% (Hubbard and Glasser, 2005). At 6350 m a.s.l. (accumulation area), the error is probably higher explaining why actually we consider a large error range (= approximately ±30%). We have not modified the text but the reader is invited to look at Azam et al. (2012).

P. 3354, L. 10ff: It might be worthwhile to show the different drainage parts of Mera Glacier based on a proper drainage basin calculation in the figure. This description in the text is not very instructive.

The description in the text has been removed and the areas feeding CS_5520 and CS_5350 are now shown on Fig. 9 of the revised MS.

P. 3355, L. 14: It is not shown that sublimation is the main process. This is just a guess. Therefore "most likely" is not appropriate.

"Most likely" changed in "potentially".

Also a new sentence with a new reference has been added here mentioning that blowing snow sublimation is also a key process in the dry Andes of Chili (Gascoin, S., Lhermitte, S., Kinnard, C., Bortels, K., and Liston, G. E.: Wind effects on snow cover in Pascua-Lama, Dry Andes of Chile, Adv. Water Res., 55, 25-39, 2013.)

"Blowing snow sublimation is also a key process controlling the spatial variability of the snow cover of the dry Andes of Chile, amounting to 18% of the total ablation at high elevations (2600-5630 m a.s.l.)."

P. 3355, L. 23: I do not agree with the winter-ablation type classification. Wind drift mostly removes the winter snow, apart from the very high regions. It all depends on the definition of winter and summer. Probably a large part of ablation occurs after the end of the monsoon, but before the start of winter. This needs further investigations.

As already requested by T. Nuimura, the assertion has been weakened saying that "further investigations are needed to confirm it".

P. 3357, L. 23ff: Given the comments above about the calculation of balance gradients and with including the error bounds (which also change considering more realistic errors for the ice thickness), the interpretation might need reconsideration.

We have revised the calculations of the balance fluxes on the basis of areas delineated now in Fig. 7, feeding both cross sections at 5520 and 5350 m a.s.l. and also considering the new mass balance gradients for 2011-12 (Table 3). This Table 3 gives also the kinetic fluxes of both sections, with their respective error ranges, calculated mainly from thickness errors (±15m). The interpretation has been rephrased:

"The kinematic fluxes through CS_5520 and CS_5350 are lower than the fluxes obtained with a mass balance of -0.08 ± 0.28 m w.e. yr^{-1} (= mean annual mass balance from 2007 to 2012) and higher than the fluxes obtained with a mass balance of -0.48 ± 0.28 m w.e. yr^{-1} (= 2009-2010 mass balance) (Table 3). Consequently, mean decadal mass-balance conditions of Mera Glacier are comprised between both above-mentioned values in agreement again with Gardelle et al. (2013).)."

P. 3358, L. 19ff: This part belongs to the results section not in the discussion.

Part of the entire section 6.4 has been slightly rephrased and moved to the result section under the title "5.7. Comparison of annual and seasonal mass balances with meteorological conditions".

P. 3359, L. 6ff: In my opinion this is a rather important finding which is worth to be discussed further.

Already replied above. Please refer to Fig. B and the corresponding discussion.

P. 3368, Table 3: There need to be errors attached to the ice fluxes derived from mass balance.

The error range associated to the ice fluxes derived from mass balance have been added in Table 3, considering a ±0.28 m w.e. error range for mass balance data distributed equally at every elevation on the glacier. Some clarifications have been added in the corresponding text also:

"The uncertainties on ice fluxes resulting from surface mass balance are directly derived from the mass-balance uncertainties (see section 4.1) assumed to be ±0.28 m w.e. at every elevation of the areas contributing ice to each cross section and also from the uncertainties in delineating accurately these areas."

P. 3372, Fig. 3: The red circles are difficult to see.

The two corresponding points have been changed in blue squares highlighted with a red contour to be more visible. The corresponding stake appears in red in Fig. 2 also.

P. 3377, Fig. 7: The velocity arrows should be a bit larger. The lower GPR (CS_5350) profile is not perpendicular to the flow from 500m to 800 m, therefore the flux calculations for this part are probably not realistic (but very small anyway). At the profile CS_5520 it is not clear if the interpolation, mentioned in the text for the left part is shown in this figure, or it is only the measured part.

The velocity arrows are unchanged in Fig. 9 but the lower panels have been moved to Fig. 10 and we will make sure that the figure is printed on a full page by the publisher (Copernicus) if our paper is accepted. The interpolated part of profile CS_5520 is now added in dotted line in Fig. 9. The corresponding caption has been completed accordingly.