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Interactive comment on "Seasonal and annual mass balances of Mera and Pokalde glaciers (Nepal Himalaya) since 2007" *by* P. Wagnon et al.

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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

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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 the mansucript and should be reconsidered.

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 during the summer?

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

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.

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

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

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

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.

TCD
7, C1580–C1584, 2013

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.

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.

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?

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.

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.

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

TCD
7, C1580–C1584, 2013

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.

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.

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.

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

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

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.

TCD
7, C1580–C1584, 2013

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.

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.

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.

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.

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

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

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

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

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.

Interactive comment on The Cryosphere Discuss., 7, 3337, 2013.