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Interactive comment on "Heterogeneous glacier thinning patterns over the last 40 years in Langtang Himal" *by* S. Ragettli et al.

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General comments: In the paper Heterogeneous glacier thinning patterns over the last 40 years in Langtang Himal an interesting set of geodetic data from various sources is presented and used to infer the geodetic mass balance of the Langtang catchment in the Nepal part of the Himalayas focusing mostly on two periods (1974-2006 and 2006-2015). The authors undergo complex automatic classification of their data archive to sort out what they consider as reliable data for geodetic mass balance calculation as well as applying partly new approach to estimate the uncertainty. Unfortunately this work is not completed and is still some way from being scientifically sound. The main reasons for this are:

1) The logic behind the complex outlier removal is often difficult to understand, and

various steps in it are poorly justified. Despite this complex automatic outlier removal it seems to fail at many locations when looked at the difference maps of the 1974-2006 (Figure 6a). If the authors belief that the accumulation areas of glaciers thinned or thickened by 60-100 m at many locations as the this figure indicates they need to come up with some logical and justified explanation why (surges, enormous avalanches?) and they also need to explain the absence of this pattern of extreme thickening and thinning in the accumulation area of the glaciers for the period 2006-2015 (Figure 6b).

2) The explanation on how the uncertainty is calculated is not very clear and the procedure seems vague from statistical point of view. It is therefore hard to obtain any sense for its actual meaning. The authors do not even attempt to guess what confidence level it may represent. If more simple approaches such as using the e.g. standard deviation of off glacier DEM difference as proxy for the volume change uncertainty, it is at least known that such proxy is likely to result in very conservative uncertainty estimate compared to more advanced methods as shown by several studies.

3) The most critical weakness of this work is however that the authors seems neglect almost completely the uncertainty they actually obtain when discussing their results. A large proportion of the paper is spent on discussion on the temporal and spatial variation of the geodetic mass balance, while in most cases the variation they are discussing are not at all or barely significant if one believes the uncertainties obtained for the discussed values.

My main advices for the authors are the following:

a) Revise how you do your outlier removal, ideally make it more simple and if not make it such that the logic behind is understandable. It is also OK to use common sense when doing the outlier removal, instead of counting entirely on automatic outlier removal (this is presumably the difference between this work and the study of Pellicciotti et al. (2015) where part of the 1974 DEM of the accumulation area of Langshisha glacier was considered as erroneous data and therefore rejected).

b) Redo your uncertainty analysis. I would use approaches suggested by others unless you can better justify your approach and at least give the reader any evidence that the assumption you make when carrying out your uncertainty analysis is likely to result in an overestimate of your uncertainty rather than underestimate. You also need to be able to clarify what you mean by your uncertainty in terms of confidence level do give your uncertainty any meaning.

c) When the above has been done, carefully revise what your data actually tells you with any confidence. This could lead to a good concise paper if carried out in the above suggested manner.

Specific comments:

The list of the specific comments on the paper content here below should not be considered as complete, particularly regarding language, spelling, references etc., since in my opinion this manuscript and the work it describes needs almost a complete revision. The specific comments are mostly of two kind. Firstly, where I find reasoning of the methodology hard to understand or poorly justified. Secondly, where the authors are concluding much more from the data than they actually can, given the derived uncertainties (this is not a complete list, the remaining text free of such comments should also be critically revised, with this kept in mind).

Page 1, line 12: This first line does not tell the reader anything since glaciers are losing mass at very variable rate (even glaciers short distance apart).

Page 1, lines 18-19: The uncertainties here have large overlap. Assuming that the uncertainties where e.g. 95% confidence level (let alone lower confidence), you cannot state with great confidence that you show that the volume loss rate is higher now (even though it is more likely that it is, rather than the opposite).

Page 2, lines 8-10. Strange sentence, since you talk about examples of regional differences but only mention the upper limit values.

Page 2, line 15. Is “scientific debate” a good phrase to describe this, isn’t the common goal of everyone studying this just to obtain answer to the same scientific questions?

Page 3, lines 7-17. Here the authors seem to give observations and models the same weight. When you have models on one hand and on the other hand conclusive observations, which don’t fit the models, the reason for this is usually the incompleteness of the models, which in this case is probably the melting mechanism of the debris covered glacier.

Page 3, lines 15-17. I don’t understand this sentence. What melt is caused by the glacier emergence velocity? Are you maybe referring to emergence of debris to the surface but not the classical glaciological term emergence velocity?

Page 5, lines 11-17. The author don’t discuss at all the effects of seasonal changes on their geodetic results despite the fact that the DEMs (including the ones with most emphasizes, November 1974, October 2006 and February 2015) are from different time of the year. Can the seasonal effect be neglected? If so, based on what?

Page 6, lines 31-32. What about glacier motion, does your velocity data give any upper limit on what the motion of the GCPs could be within the time frame (if so state it)?

Page 8, line 1. Systematic errors in the glacier change map?

Page 8, line 4. Did Ragettli et al., (2015) do independent estimate on this or did they get the value from Sugiyama et al., 2013. If the latter Sugiyama et al., 2013 should be referenced for this. This ELA estimate, which presumably is just some average value for this catchment, is used in this paper to estimate accumulation area ratio (AAR) for each glacier. It is then repeatedly referred to in the paper like some actual observation of the AAR for the glaciers. It is not and given the unrealistically high variability of AAR in table 1 (15-86%) it is probably not even a good estimate for individual glaciers.

Page 9, line 1. I am not really following you here, when you mention the term automated flow accumulation process. Are you delineating ice divides between neighbouring ice catchments? Is the big difference for Langshisha glacier between Pellicciotti et al., (2015) and this study caused by some part of Langshisha glacier as defined in the former study, being considered as separated ice catchment in this study? If so state this clearly. I would also recommend that you revise Figure 1 to better reveal the coverage of each glacier with improved background image behind it. By doing so you can (hopefully) convince the reader that your delineation of the glaciers is the more appropriate one.

Page 9, lines 2-5. This is a huge difference and is bound to have great effect on the result. You compere these two studies later on for this glacier, without even mentioning this important difference.

Page 9, line 12. Standard deviation of deltah/deltat at given point calculated for the up 28 difference maps or is this calculated over a given window?

Page 9, line 16. Well here is the answer to the question above. Personally I don't find this a good way writing, when something is only partly explained in a sentence and the same sentence and the following sentence does not indicate that further explanations will be given, but then later on the missing puzzle suddenly pups up. When I read such text, I am always asking myself "did I miss something?"

Page 10, lines 12-15. Here a justification why this should be errors but not actual elevation changes are completely missing. The span of elevation change rate over an entire glacier can easily be greater than the DEM errors but this depends on the time span, DEM quality, glacier type, etc.

Page 11, line 4. Outlier correction uncertainty? Do you maybe rather mean sensitivity to outlier removal?

Page 11, lines 5-17. This is very confusing text. I don't really understand what you are doing including why the thinning rate 2006-2015 is appropriate proxy for the outlier removal (of all data sets or just the 2006-2015 difference map?).

Page 11, line 18. DEM adjustment uncertainty? Is the term uncertainty appropriate here? I do not see that the parameter explained in this section is really used in your uncertainty assessment.

Page 11, line 29. I have problem obtaining the same results as the authors from this equation. If n=8 making Ndeltat=28 and k=3, I get

C_2= (8 over 3)/(2*28)=(8!/(3!*5!))/(2*28)=56/56=1, not 6 as authors say one should get.

Page 12, line 7-14. It took me quite a bit of time to actually understand what you are doing. I think I do now. Again I can't see what is logical about using the thinning rate from October 2006 to October 2015 as a threshold value. Can you explain that?

Page 12. Do I understand you right that the last outlier detection you do is the catchment scale outlier detection? Wouldn't be more appropriate to do that before you the do glacier scale outlier detection?

Page 12, lines 23-24. Here we are left with the question "how?" until half a page later. Again, this is not a good way of writing, it makes the paper hard to read.

Section 3.4.3. Here you come up with three outlier criterion. Why this complexity? It is not really justified in the paper.

Sections 3.4.2-3. It seems to me that the glacier catchment scale outlier removals are not likely to function appropriately when the time interval between DEMs is so variable and you do the outlier detection on deltah/deltat. deltat is ranging from < 1 year up to 32 year. This means e.g. for the last criteria that the DEM error for the 1974 DEM causing the 1974-2006 delath/deltat to be considered as an outlier would need to be 32 times larger than the error in 2009 DEM causing the 2009-2010 deltah/deltat to be considered an outlier. DEMs over short interval off course need to be very accurate to have informative value for volume change estimates, hence this is logical from that perspective. If my understanding of the outlier removal procedure is correct it does however result in very weak outlier criterion for the 1974-2006 interval. If the authors rely entirely on this automatic outlier removal, it may result in erroneous result for this period, which to me, seems to be the case when looking at Figure 6 a. This is very unfortunate given that the main focus of your results and discussion is on the difference between the periods 1974-2006 and 2006-2015.

Page 14, line 5. I find the problem with your bias or trend correction approach manifest in this equation (I guess you are not the only one doing this). If this study had been only on one of these glaciers the data used for trend or bias correction would (presumably) only have been from the neighbouring area of this glacier resulting in MED$\sim$=0. But since you do the trend correction for the catchment as a whole (which I think is fine if you are studying the catchment but not individual glacier), MED$\sim$=0 is often not true for individual glacier, hence you will get different value for a given glacier than if you had focused the study only on that glacier. You are trying to compensate for this by adding this effect here into the uncertainty, but you are still left with the fact that the probabilistic mean of the actual average elevation change is likely not well represented by the centre of the given error bars. This becomes particularly awkward since your discussion of the results almost neglect the derived uncertainty limits and focuses on the centre of the error bars.

Page 14, lines 8-10. Are you saying that you use n=1? If so state it clearly, you could add to the sentence (i.e. n=1). Your usage of i.e. is not appropriate here (if I understand the sentence correctly). The fact that you use n=1 implies only that all pixels within the elevation band are fully dependent on one another (which truly is a conservative estimate). It does not however implies that there is no dependence between elevation bands. Since no attempts has really made to quantify the effect of the spatial correlation of your data (see e.g. Rolstad et al., 2009 or Magnússon et al., 2016, for further info) we don't really know if your assumption of no error compensation across elevation band is likely to lead to a conservative estimate of the uncertainty.

Section 3.6. It seems to me that your surface velocity could do with some more masking

of errors and outliers e.g. with correlation threshold. The masking that you are carrying leaves almost the entire velocity field intact as revealed by Figure 11 even though it is clear that much of it is just errors. The level of errors seen outside the glaciers is such that it is not clear if the signals on the glaciers are real or just errors as well. The figure itself is very hard view.

Page 15, line 9. Outlier and uncertainty assessment? Confusing. Wasn't this already done?

Section 4. This is all rather confusing. You calculate a lot of quality proxies used for outlier detection, mostly to convince yourself that the data that you derive your results from is of good quality. This is all good if one also reviews critically the outcome, which seem to be lacking in this study. A lot of these proxies are referred to as uncertainties apparently without being used to estimate uncertainty of the presented geodetic results. It is also not clear if all the DEM available during the period 2006-2015, apart from the initial and the final DEM, were really used to narrow down the uncertainty of volume change during this period. If not it seems to me that this paper would be much clearer if the focus of this paper were only on three DEMs, the ones from 1974, October 2006 and February 2015.

Page 18, line 18. Well if you think this is due to remaining systematic error, did you consider that your outlier removal is maybe not functioning so well?

Page 18, line 23-24. This is very true. Unfortunately you seem to forget it repeatedly in your discussion. Given that your uncertainties will be the same after revision of this work, much of the discussion on the results can be omitted because it is meaningless due to the large uncertainties.

Page 19, line 6. This is a good example of what I am talking about regarding the author neglecting the uncertainty in their discussion of the results. You cannot state here that the thinning rate increased by more than 100 %. If we know that John owns between 0 and 4 cars and Mike owns between 2 and 6 cars can you state that Mike owns at least twice as many cars as John? No and the probability of such statement being true is only 14/25=0.56 (given even probability distributions for the car ownership in both cases).

Section 5.2.1. This comparison between debris covered and debris free glacier looking at “Explanatory variables” is rather primitive. For one thing it is rather inappropriate to refer to some of them as variables. I would rather refer to outcome of processes, which in some cases probably show correlation since they are dependent on the same physical variables. It is also strange that only Yala is included as candidate for the debris free glacier. The behaviour of Yala is then compared with 5 other debris covered glaciers. Even though the difference between Yala and each of the other 5 glacier is sometimes visually clear it is misleading to calculate the r-value for all the 5 debris covered glacier at ones and compare with a value calculated for a single glacier. When using data from several glaciers, various variables which effect the glaciers in different manner is bound to reduce the studied correlation compared to having data from just a single glacier.

Page 22, line 1. You mean April 2015.

Section 5.3. It is probably of interest for some to know the volume of these enormous avalanches. It however seems clear that avalanche falling on debris covered glacier (particularly the low insulated part of the glacier) is only going have minor and short last effect on the mass balance since it is going to melt much faster than the debris covered ice beneath it.

Page 24, line 25. What is numerical evidence?

Page 25, line 14. Is there no uncertainty in the area change?

Page 25, lines 16-17. I don’t understand what you specifically mean by correlation between areal changes and surface elevation height.

Page 25, lines 23-26. Again not promising for your outlier removal, even though it is

better to admit it does not work to well. It would however be even better to justify why you think this is an error, e.g. by pointing out that local lowering of 60-100 m over 32 years (as indicated by figure 6a) on such small glacier at such high altitude is very unlikely to say the least.

Page 25, line 28-29. Even though it is likely that hypsometry plays a crucial role here this statement is far too bold given that it is based on very limited and apparently erroneous data (according to Figure 6a).

Page 25, line 31. See my previous comment regarding the AAR.

Page 26, line 26. Here and at other places in this paper, some temperature data (if available) would support your discussion.

Page 27, lines 16-18. You have far too little data with far too great uncertainty to make such statement.

Page 27, lines 20-23. You can say that Kimoshung glacier has higher hypsometry than Yala. The staggering difference between AAR values (which here are treated as some kind of truth but not as estimates based on the assumption of fixed ELA=5400 m a.s.l. for the whole catchment) is however misleading.

Page 28, lines 16-17. I am confused, is this in accordance with previous statement in this section (page 27, lines 16-18).

Page 28, lines 27-28. There is completely insignificant difference between these values. There is no point in trying to explain the “difference” between them.

Page 29, lines 6-10. I am very puzzled here. You need to justify here why this data is now suddenly considered as usable data, when the one processing the data rejected it in recently published paper. Why has he/she as the third author of this paper changed his/her mind?

Page 29, line 13. How can you state this? Does including apparently erroneous data make the uncertainty estimate more realistic?

Page 29, lines 15-16. What other data did they use? They were hardly using GPS in 1982.

Page 30, lines 21-23. Sorry, I don't think many will agree on this statement.

Page 32, lines 5-9. This text does not fit into conclusion. If the authors think this text should be in the paper, it would be more appropriate to include it in the introduction.

Page 40, Table 1. See previous comments regarding the AAR.

Page 42, Table 5. It is not clear how the uncertainty of the average elevation change over the entire Langtang glacier catchment is calculated. Given its value it seems close to being basically (-)/(A*2), which basically corresponds to assuming that errors between glacier are completely dependent. Such assumption gives really conservative estimate, even too conservative causing the results to be downgraded. I also recommend that you stick to the same order of glaciers in the table as given in Table 7 with the glacier id.

Page 43, Table 7. Why no uncertainties? Are they within the digit of the given value, or did you simple not think about it? You are discussing these area changes in the paper without giving the reader any confirmation that these changes are significant.

Page 43, Figure 1. See my previous comments regarding this figure.

Page 44, Figure 2. The data on the debris covered glaciers is the most convincing part of this manuscript.

Page 45, Figure 3. It seems to me that using all the 6 proxies result in the same outlier removal as when you just use med2 and sigma2.

Page 46, Figure 4. 50% confidence level? What would the error bars be for a reasonably strict confidence level like 95%?

Page 47, Figure 5. Do you mean: a) A whiskers plot showing the distribution of uncertainties for the (up to?) 28 deltah/deltat maps. What do the red crosses indicate?

Page 47, Figure 6. See various previous comments on this figure. Should also be enlarged for better readability.

Page 48, Figure 7. The order of panels for the glaciers should be kept the same as the numbering of the glaciers in Table 7.

Page 50, Figure 9. Something went wrong with the altitudinal distribution for Yala.

Page 51, Figure 10. See previous comment regarding this figure.

Page 52, Figure 11. Very hard to read. The arrows are e.g. very hard to detect. Results do not appear very reliable (see previous comment).

References

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