Nat. Hazards Earth Syst. Sci. Discuss., 2, C3834–C3854, 2015
www.nat-hazards-earth-syst-sci-discuss.net/2/C3834/2015/


Natural Hazards and Earth System Sciences
Discussions



# *Interactive comment on* "Application of a fast and efficient algorithm to assess landslide prone areas in sensitive clays – toward landslide susceptibility assessment, Sweden" *by* C. Melchiorre and A. Tryggvason

**C. Melchiorre and A. Tryggvason**

caterina.melchiorre@geo.uu.se

Received and published: 30 June 2015

Dear Referees,

We would like to thank you for your precious comments to our paper. We think that they help us to improve the quality and the readability of the manuscript. Please, find our replies to your questions and comments herein. Please, find the revised version of the manuscript in the attached file.

Our Best Regards,

Caterina Melchiorre and Ari Tryggvason

P. Quinn (Referee) Specific Comments: Abstract (general) – the objective(s) of the paper is (are) not clearly stated Abstract line 5-6 – I don't know what is meant by ". . . able to detect soil and slope criteria guaranteeing a faster execution. . ."

We modified the abstract.

Introduction (page 7774), second sentence – this sentence contains two distinct, different and unconnected thoughts. I'm not sure why lives or transport corridors are threatened because the landslides occur in gentle terrain.

Fixed.

pg 7774 lines 3-4 – claim that ". . . do not show evident signs of . . ." needs a citation (or revision to soften / generalize the statement). While I agree there is generally little, if any, obvious advance warning of landslides in sensitive clay, there have been cases with documented movement for some years prior to eventual global failure (e.g. Demers, D., S. Leroueil, J. D'astous, 1999, "Investigation of a landslide in MaskinongeÌĄ, Quebec," Canadian Geotechnical Journal, Vol. 36, pp. 1001-1014)

We rephrased the sentence and cited this paper as evidence for creep sometimes being observed.

lines 16-17 – ". . . the terrain prone to landslides in sensitive clay can be discriminated from stable terrain using the ratio dH/dL..." I do not have that reference, do those authors claim precisely that, or have you paraphrased? I'm sure that nobody can discriminate stable from unstable sensitive clay terrain solely on the basis of slope angle.

Rephrased.

What we mean is that Berggren et al., 1991 suggested a methodology that uses a dH/dl criterion to identify areas at risk in a worst-case scenario. On a related note, you use



NHESSD

2, C3834–C3854, 2015

dH/dL throughout the paper, is there any reason you can't use a more intuitive term, like slope angle? It seems unnecessarily technical.

OK, we from now on use"cross-sectional angle".

line 25 – what is a "local visibility operator?" I think I understand this is some kind of viewshed interpretation, but I don't know this term and don't think it is in common use, so needs to be defined

We hope we have clarified a little by extending this sentence – but it is difficult to explain a full paper in just one or two sentences.

Pg 7776 lines 2-3 – ". . . there will be numerous areas that violate the dH/dL criterion. . ." What is the basis for using a dH/dL criterion, and what is a/the dH/dL criterion? The term and concept of its use come almost from nowhere and need a better introduction for the reader

We now talk about a threshold instead of criterion. And what we mean by "threshold" has been clarified in the extended description (see comment above) on Berggren et al., 1991.

line 10 – QCSI needs to be explained / described. You do this later. Here is appears without introduction, so at the least needs a citation.

We agree – we rephrase this in general terms here and wait with introducing the term QCSI.

lines 11-20 - the objectives presented here are not very clear, and this may be in part because the preceding text (introduction) is not very concise, and difficult to follow.

We have rephrased the objective.

line 21 – spelling Southwestern - Fixed Pg 7777 line 2 – ice-marginal - Fixed Line 3 – glaciomarine (mistake occurs throughout the paper) – Fixed



NHESSD

2, C3834–C3854, 2015

Line 4 – what is Holocene “transgression?” Maybe you mean regression? Transgression.

Line 8 – “... interbedded with fine-sand layers in their lowermost portions...” Does this mean they are ice-proximal (or anything else about genesis)? Is this somehow pertinent to the story you are telling or can it be deleted?

We do not know if this puts constraints on ice-proximity. The reason we mention this is that it has been hypothesised by some (Torrance, Malehmir et al.) that the proximity to the sand is important for the subsequent leaching. We do not really go into this discussion, so it could be omitted, but on the other hand we subsequently mention the importance of leaching so it is not completely irrelevant either.

Line 11 – “. . . when the sea level was 125 m above present. . .” I don’t think you mean sea level was higher, I think you mean (?) the land was depressed and the marine limit is recorded at âĹij 125 m on the current (isostatically raised) landscape?

Well, how much was due to the landmass being depressed and how much was due to a difference in sea level is to our knowledge not precisely known. We agree that most of it was likely due to that the land was depressed. Is it OK if we say relative sea level?

Line 16 – need to defend (or remove) “. . . intense” with a citation.

Removed.

Line 23 – discusses / mentions “glacial fine clay” and “post glacial silt” and “glacial/post glacial clay;” can you say anything about the genesis, are these deposits lacustrine, glaciolacustrine, marine or glaciomarine? These seem to me to be important distinctions, much more important / relevant than “glacial” or “post-glacial”

We agree that genesis is important, but this info is not really provided by the map.

Pg 7778 line 8 – “. . . to detect areas above a specified dH/dL threshold. . .” is presented like the reader should know what it means. I think you are referring to the



NHESSD

2, C3834–C3854, 2015

use of an angle threshold to identify landslide susceptible areas?

With the improved explanation earlier on rational behind cross-sectional thresholds we hope that the reader will now know what is meant.

line 9 – “ArcGIS” needs a citation

ESRI, Redlands, CA – is this OK?

line 17 – what bearing on line of sight does “... information on depth to bedrock...” have? It blocks “line of sight”.

This has been clarified in the description.

Section 3.1 is, generally, unclear and hard to follow. The overview description of the algorithm is, to me, unintelligible.

We apologize. We have rewritten this section, hopefully making it clearer.

Line 21 – “surrounding cells” means what. . . immediately adjacent cells? Some (nearby?) neighbourhood of cells?

Now explained.

Line 22 – “. . . elevation of the cell is lowered. . .” I can’t process this description. If the cell is lowered, it’s angle with adjacent cells gets steeper, not flatter, no? Not sure what I’m not following, but this needs a clearer description.

We have completely rewritten this section taking this comment into account.

The last sentence on this page is unclear, I cannot make out what it means.

Rewritten.

Pg 7779 line 14 – what is “neck size?” This is obviously a critically important concept in the paper but is nowhere defined. In the same line, what is a “sample” in “. . . width of a few samples. . .?”



NHESSD

2, C3834–C3854, 2015

It is defined now.

Line 20 – use of QCSI is begging for some kind of explanation of what it is prior to this point

We did this later. This has been moved up and now comes earlier.

Line 25 – write out NNH in full before abbreviating it.

Done.

Line 7780 line 2 – what is granulometry? Do you mean texture, or grain size?

Grain size.

Line 4 – cite a source for the SGU product used as a bedrock map

We did this further down, but OK, we do it here too.

Line 5 – cite the data sources for the three different SGU databases

We think that is enough to cite the SGU-report Daneils and Thunholm (2014). We do not really see the point of providing the databases behind a specific SGU-product. The data in the product have been processed. If it is not sufficient, here is the web-addresses of Brunnsarkivet: http://www.sgu.se/grundvatten/brunnar-och-dricksvatten/brunnsarkivet/. The other types of data do not have a specific web-address.

Lines 13-14 – the "depth to bedrock map; is it generated as a raster with "depth to rock" used directly, or as a raster with "rock elevation" that is then used to infer "depth to rock" from available topography (by subtraction)? This is an important difference that leads to different uncertainties in the resulting map.

We understand that the two different methods may generate slightly different results. How this will effect uncertainties is more unclear. On the other hand – we are consistently talking about "depth to bedrock", thus we do not think we need to go into this



NHESSD

2, C3834–C3854, 2015



discussion here.

Related to the above, did you do any QA / processing to resolve anomalies in the bedrock depth map? I assume there must have been issues with this map given the various sources of data with different spatial densities of input data.

No, we did not process this information. And yes, there are issues with map resolution. But as no information implies large thickness the more sparse the information is the less likely it is to influence our results. A sentence about this has been added.

Line 18 – the bedrock map "resolution" is 50 m, but what is its spatial accuracy and how does this vary across the map? I assume the 50 m resolution relates to your choice of pixel size, and not to the spatial density of the input data. Am I wrong?

You are correct. The spatial accuracy is highly variable. However, we did not go into this as it turned out that the influence of depth to bedrock map had very little influence on our results. Our speculation is that "the depth to bedrock map" product may be derived from data sources that are geographically located where they are likely not to affect the map (nobody drills a well near a slop or high up on a hill) .This is touched upon in the Discussion and Conclusion-section.

Lines 24-25 – the landslide scarp map's development needs to be described (or a reference about its development cited) in order to be understood and accepted by the reader, not simply presented as a "product of SGU." Was it produced as part of the present work? If so, it needs to be fully described in this paper or in a preceding, related paper or document.

The landslide scarp map was produced by SGU, it was not developed under our work. There is no reference about its development, just a product description. We have provided the reference to the product description.

Line 29 – what are the "time-dependent factors?" and how/why are they not important?

Changes in groundwater level, pore pressure. We have rephrased the sentence.



NHESSD

2, C3834–C3854, 2015

Page 7781 line 1 – add “occurrence” after “probability of landslide. . .” Further, without knowing more about the cited model (perhaps some basic explanation) it is difficult to know what is meant by “. . . probability of landslide (sic) is basically the probability of failure.”

This has been rephrased.

Line 7-8 – Retrogression distance is, I think, only very weakly linked to sensitivity, and several authors draw correlations to other factors. See for example: Bjerrum, L., 1955, “Stability of natural slopes in quick clay,” Geotechnique, Vol. 5, pp. 101-119; Carson, M.A., 1977, “On the retrogression of landslides in sensitive muddy sediments,” Canadian Geotechnical Journal, Vol. 14, pp. 582-602; Lebuis, J., J.-M. Robert, P. Rissmann, 1983, “Regional mapping of landslide hazard in Quebec,” In Symposium on Slopes on Soft Clays, Swedish Geotechnical Institute Report No. 17, Linkoping, pp. 205-262; Carson, M.A., G. Lajoie, 1981, “Some constraints on the severity of landslide penetration in sensitive deposits,” GeÌĄographie physique Quaternaire, Vol. 35, No. 3, pp. 301-316; and in particular, Tavenas, F., P. Flon, S. Leroueil, J. Lebuis, 1983, “Remolding energy and risk of slide retrogression in sensitive clays,” In Symposium on Slopes on Soft Clays, Swedish Geotechnical Institute Report No. 17, Linkoping, pp. 423-454

We are aware that other parameters have been linked to retrogression distance. Some of those parameters might have been used, others would have been more difficult to use in this type of study.

Line 10 – “small scale” = “regional scale?”

Yes.

Line 15 – how do you extract geometry “before” the landslide from post-landslide Li-DAR?

It was extracted from a side of landslides, where we judged that the slope geometry



NHESSD

2, C3834–C3854, 2015



could represent the slope geometry of the landslide. That is why it was done on a subsample. We added an explanation of this (not here, but further below where it is discussed in more detail).

Line 17 – again I find the term "the ratio dH/dL" hard to get my brain around, could you call this something intuitive? Similar to travel angle, although that term would extend to the toe of the debris, which you presumably cannot measure for most of these landslides.

We use cross sectional angle now.

Line 18-19 – what is the interpreted "maximum value of QCSI" and how is it determined? Is this the maximum value enclosed within the footprint of the landslide?

The maximum QCSI is simply the max value of QCSI inside the 'scarp' polygon.

Line 19 – what do you mean by "landslide scarp?" From context I assume you mean the whole perimeter of the landslide, although "scarp would not normally be used in this sense, as the toe of the landslide, for example, is not a scarp. If you are talking about the perimeter of the landslide feature you have traced, perhaps you should call it the perimeter or outline of the body of the landslide (and emphasize it does not include the displaced debris, unless it does).

No, we mean the landslide scarp, not the perimeter. We discuss this in slightly more detail now.

Line 24 – provide a citation to defend that these two statistical measures are "mainly used" as I will admit I've never seen them used before.

We have rephrased it that we choose to use those two terms. They are defined in Table 1.

Page 7782 line 9 – mentions that you use two maps to validate the models: the landslide scarp map and the landslide probability map. I don't believe the later parts of the



NHESSD

2, C3834–C3854, 2015

paper discuss any comparison between your models and the landslide probability map, unless I've forgotten or missed it.

The coefficients (e.g., Gilbert) were calculated on the probability map.

Line 22 – replace "from" with "of"

Done

Page 7783 line 16 – what is meant by "Receiver Operating Characteristic?" You mention it here, and later you note that you tested the model against it, but it is not cited or described, and you don't present any actual results. I suggest you either describe / cite this fully so the reader can understand and accept it, or delete any mention of it.

We agree that it adds very little to the manuscript, thus we have deleted it from the manuscript.

Lines 17-20 – you give a very brief overview of aspects of the Gilbert skill and the Heidke skill, which are subsequently relied on a great deal in your analysis. Some additional detail is needed; for example, how do you " remove true positives due to random chance" and how / why do you remove samples that are "correctly classified due to random chance?"

It is actually shown in Table 1 what random chance means, but we now mention it also in the text.

Lines 21-23 – in the ROC curve which quantities are plotted on the x and y axes? It's not obvious which orientation of these noted measures determines the interpreted area. My instinct on first read is to assume (1 – specificity) is plotted on the y axis but I don't know that is correct.

ROC is not used anymore.

Page 7784 line 5 – "landslide scarp map was converted from a vector to a raster..." Before reaching this point I assumed (incorrectly?) the scarps were polylines. However



NHESSD

2, C3834–C3854, 2015

I now assume you must be talking about polygons? I'm still not clear. . .

We now state when the data is presented earlier in section 3.2 that it is converted from vector data to a raster.

Line 8 – how do you determine the "height of the slope before the landslide event" from post-landslide topography? Needs an explanation.

We have to live with that the dH today is a "conservative" estimate of dH before the landslide. We now state this to improve clarity on the assumptions we have to make.

Line 9 – how do you estimate dL, "the maximum retrogression distance" from post-landslide topography? Again, needs an explanation; I assume this could not be automated but rather was based on expert judgement?

We hope that with the explanation above it will be clearly in what context we estimate dL.

Line 11 – "71 scarps" is not very many, or does not seem like very many. How big is the whole set from which you selected 71, and how were these selected (randomly? Based on some selection criteria?)? Recognizing it was not the intent of this work, I think you could obtain immense value from interpreting every landslide scarp possible from the high resolution topography.

The total number of scarps is around 250. They were not randomly selected, but manually after visual inspection of the DEM.

Line 13 – how was a point automatically selected? This strikes me as a very difficult process to animate and needs some explanation.

No, not very difficult as both maps were converted to raster (pixel) images – but fairly computer intensive. Simple point-by-point comparison, and we kept the maximum for every scarp.

Line 15 – ". . . the maximum value of QCSI was extracted. . ." Why (and how) did



NHESSD

2, C3834–C3854, 2015

you choose the maximum value? Why not some other measure, possibly the average value, possibly some other statistical measure?

Again, as both maps have been converted to raster representations we just search for the maximum QCSI within each scarp. Why maximum? Other statistical measures were also computed. Since the results were very similar, we chose to show the highest QCSI representing the weakest point.

Line 18 – did you look at other measures of QCSI (mean, median, etc) to see if there was a better relationship? Yes, but no.

Line 21 – what does "AA.VV." stand for? This is an odd name for an author.

Latin for Auctores Varii – (we found no instructions how to refer to such a report with many authors, but maybe the editor can inform us about the preferred standard for this journal).

Line 25 – you mention using best/worst case soil maps, but unless I'm mistake, your results only discuss the use of the best case soils map. You should either present both sets of results, or delete mention of unused work, or explain why you don't show both sets of results.

We remove the worst case soil class and now it is not mentioned in the manuscript.

Line 25 – I don't think the term "QCSI-dependent soil class map" really describes what you used the QCSI map for, rather I think you simply divided the QCSI map into different slices / ranges of QCSI value. I don't think you should try to tie that back to "soil class" since the QCSI map is not a soil class map.

We now call it QCSI-dependent soil map, since it still show soil (the best case soil class).

Page 7785 line 2 – what is the "classification system used at SGU" and why should the reader accept it as a useful or meaningful basis? Provide a description and/or citations



NHESSD

2, C3834–C3854, 2015



and defend its use in your work.

We described the classification criteria in the paper. We refer to SGU to let the reader know that we have not derived this classification to suit our study. No reference is available, there is a product description that is not published. Moreover, SGU has previously used the classification for similar purposes. We used it because: 1) it provides additional constrains to the assessment of landslide susceptibility; 2) it is provided by the same institute that provides the soil class map; 3) it gives some kind of reference system (what we previously called standard procedure) to assess the results of our analysis.

Lines 2-6 – your process of assigning soil classes is not clear to me. Lines 7-10 – your process of assigning "soil classes" to the QCSI map is not clear to me. We added a sentence at the end of the paragraph to better clarify the procedure.

Line 11 – this paragraph jumps into identification of optimal filters, but you haven't yet really explained (or the explanation wasn't sufficiently clear) what you will do with the soil maps, so it feels jarring to jump to the filtering procedure without yet knowing the basis analytic methodology.

We added a sentence to better clarify what we did with the soil map that was simply a reclassification based on the QCSI value (table 2).

Line 16 – why use only 1:10? Is this used solely to test model performance (i.e. you are isolating this parameter to understand variability of other parameters) or is this intended as an eventual model feature?

The cross-sectional angle is not a filter parameter (this we hope is now clear from the revision of the text above). The reason 1:10 was used should now also be clear – it is the recommended value from the literature. We also verify in this study that a value close to 1:10 is reasonable based on the QCSI-dependent cross-sectional angle.

Line 24 – the results of the Gilbert score are not shown..." I don't think you should



NHESSD

2, C3834–C3854, 2015



discuss methods or results you don't intend to share, but rather should focus on core results essential to the work only. Otherwise it becomes distracting to read about other non-essential tests you performed, that were, in the end, irrelevant to the outcome

We have rephrased this – but we do find it worth mentioning that two (slightly) different statistical test give almost identical results. If we show both readers may be confused as they show the same results, or if we do not mention it they may suspect that we only showed the results supporting our case.

Page 7786 line 14 – you need to present / cite / defend the "standard procedure used at SGU" and not expect the reader to accept it as valid.

This has been removed as it is explained above.

Line 19 – "decreased according to the QCSI. . ." What value was used to determine the slope angle, the maximum/mean/median/mode in the landslide body? Some value at the river's edge? The actual value at the pixel under consideration?

Here we refer to Table 3. The 13 cross-sectional angle thresholds are shown in Table 3. We run the algorithm using the 13 thresholds in descending order from 1:1 to 1:22. As previously explained, those thresholds are derived by analysing the relationship between QCSI and the ratio dH/dL. Anyway, we have rephrased this sentence and the following ones in order to clarify the procedure better.

Line 20 – what is "the same" for both the first and second part of the analysis? I can't follow this sentence.

Rephrased

Lines 26-28 – after reading this sentence many times, I have no idea what it means. Please clarify what you did. Perhaps this requires several short, clear sentences, not one long sentence.

We have left the sentence almost invariant, since we think that correctly summarizes



NHESSD

2, C3834–C3854, 2015

what we did. We added a sentence that, we hope, adds clarity by giving a concrete example of the procedure.

Page 7787 line 5 – "the performance of the model deteriorated. . ." I can't see this from the figure, since its meaning isn't adequately explained. Also, what is the sensitivity threshold? c

We added a sentence in the section describing the curve (Model evaluation section) that clarifies the meaning of the curve and of the sensitivity threshold. We also added a sentence to explain why the performance deteriorates.

Lines 19-20 – garbled sentence "... the not filtered and the filtered and unfiltered maps. . ."

Corrected.

Lines 20-21 – your use of "high values" and "low values" is confusing (and probably wrong?); I think you mean 1:22 is a "high value" right? That is a very low angle, so should be a "low value" of cross-sectional angle, not a high value. Low angles have high dL/dH values but not high angles. This terminology is used (and needs to be corrected) elsewhere also.

Corrected. We also double-check it elsewhere in the manuscript. Thanks to point out to the mistake.

Page 7788 line 3 – "ROC curves" mean what? They are not well defined, and if they won't be shown, should not be mentioned, in my view. If they were considered critical to your core purpose you would likely be inclined to show them.

Removed.

Lines 10-13 – I cannot understand what has been done / is being described in the sentence starting with "By looking. . ."

Rephrased



NHESSD

2, C3834–C3854, 2015

Line 14 – what is the “total area classified. . .” I don’t believe it has been defined or illustrated. At this point in the paper, I am left wondering what role the landslide probability map has played in the work, I don’t believe its use has been mentioned in the discussion of the work.

We rephrased the sentence containing ‘total area classified’. The probability map has an important role, since it allows the calculation of the prediction coefficients (e.g. Gilbert’s)

Line 26 – what do you mean by “. . . the model has very good performance. . .” I don’t believe you have defined “good performance in a precise way or demonstrated good performance in an intuitive way. Page 7789 line 5-6 – The sentence beginning with “Also, it should be noted. . .” begs the question “so what?” How does that selection bear on your results? You need to complete this thought to show the reader why that selection was important.

We clarified what we mean.

Line 7 – what is your basis for claiming the filtering procedure will “slightly” decrease the selection of the positive sample?

Fig 6.

Lines 21-22 – you say the QCSI-dependent cross-sectional angle thresholds did not improve model performance, but I found it very hard to follow what you did with those inputs to judge whether your approach was valid. Is there a better way to use that map?

I hope that what we did is clearer after the revision.

Page 7790 line 3-5 – “optimal” in what sense? I think you are saying the statistics you used are “best” in some sense when you use those thresholds. This might be “best” in the sense of capturing most area of past landslides with minimum excess, but would not be “best” in the sense of determining worst case limits for possible retrogression,



NHESSD

2, C3834–C3854, 2015

for example.

Optimal in the ‘sense of capturing most area of past landslides with minimum excess’. We do not think that we claimed that we wanted to find the worst case limit for retrogression. With ‘analysis’ we mean what we did in the manuscript.

After having read the paper, I’m left with the impression that your work shows that only a simple soils map is needed to identify landslide-susceptible areas, and nothing you explored offers an improvement on that. I’m also left with the impression that your work shows that a retrogression angle of 1:13 is the “best” in some sense, for use with the soil types. I feel like this is a disappointing conclusion to the work, and is largely indefensible, since we know with certainty that retrogression distance can be much longer than that, and in your database many examples were traced with longer retrogression distance. I may, however, be misunderstanding the intent of the work. If it is simply to build an approach for statistical analysis, moving the yardsticks forward a little, it’s probably good work. If the intent is to offer this as a robust method for landslide susceptibility mapping, as is, then I would be wary of its use, except perhaps for attracting the eye to areas with broad soil classes that might include landslide-susceptible sensitive clay (presuming some topographic limitation is also applied to focus within areas of marine invasion).

The algorithm is mainly based on elevation data and soil data. The results are dependent on both of them. The main purpose of the paper was to test the use of the algorithm in landslide susceptibility mapping, not to suggest a methodology for landslide susceptibility assessment. The algorithm provides fast execution time (which means the possibility to use DEM with high resolution), a filtering procedure and the possibility to use local information (depth to bedrock and local cross-sectional angle thresholds). The filtering procedure, the use of the depth to bedrock data and local cross-sectional angle are mainly used to redefine the area selected from elevation and soil data, but the two main sources of information (or landslide susceptibility factors) are soil and elevation. It is therefore obviously that the results mainly depend on those two data and



NHESSD

2, C3834–C3854, 2015



we never expected or claimed something different. Specifically, the filtering procedure and the depth to bedrock data are designed to remove areas erroneously assessed as 'susceptible'. In the paper we showed that both of them improved the susceptibility maps. Concerning the local cross-sectional angle thresholds, we derived them from the QCSI map. As we wrote in the paper, it was surprisingly to find out that they did not improve the results, despite a relationship between the QCSI and the cross-sectional angle was found (If I knew the results, I would not research). Summarizing, 2 out of 3 of the proposed approach improved model performance. The angle 1:13 is the 'best' in the sense 'of capturing most area of past landslides with minimum excess' as previously mentioned by the reviewer. We do not think that we claimed something different in the paper. Keeping this is mind, the 'choice' of 1:13 is supported by the results of the analysis. As previously said, the purpose of the work was not to provide a method for susceptibility assessment.

Table 1 – true positives, true negatives, false positives/negatives need to be defined

We did in the text, where sensitivity and specificity are defined.

Table 2 – is the worst case soil class list used in the work? If so it's not clear to me and needs to be better explained. If not, delete the column.

Deleted.

Figure 2 – are there any points above your threshold line in the other landslide "scarps" outside your subsample of 71 features? What about higher values of QCSI?

Most probably there are some landslides in the entire database that have the cross-sectional angle lower than 1:22. If we correctly remember, the max QCSI for the entire area is around 0.7. Assuming that we have correctly understood the reason of the question, we would add that the possibility that some landslides have longer retrogression distance than the ones in the subsample has relatively small importance to our work, that aimed at testing the use of the QCSI map to improve model performance



NHESSD

2, C3834–C3854, 2015

not to exhaustively analyse the relationship between the QCSI and the cross-sectional angle distance.

On a related note, I think it would be a very valuable statistical effort to obtain angles and QCSI statistics for every landslide visible in the LiDAR; I think that could generate a very useful contribution to the profession, or at least have the potential to strengthen the meaning of the QCSI map (or suggest other ways of strengthening it).

We cannot but agree with you, but as previously said, the purpose of this specific work was another one.

Figure 3 – how is map area defined, is this a length in pixels, or area. . . for example, does 24 p mean 4 x 6 pixels or the like? Needs better explanation. Figure 4 – you suggest that lower area p.t.l. means "better" model performance, but this is only true if the model isn't eliminating more area with potential landslides. This measure has to be used carefully.

This is why p.t.l. is shown together with the sensitivity and not alone.

Figure 4 – part B appears to be missing?

The letter A and B got missed under the pdf creation and that was not noticed. The caption is not correct. We apologize for that. The new caption is: Sensitivity (A) and area prone to landslides (B) obtained by varying the elevation difference criterion. Results are shown for four pre-filtering options (i.e. neck size). The elevation difference criterion is given in meter (i.e. m).

Figure 6 – I can't figure out what this graph means, requires better explanation.

We have provided a better explanation in the text.

Figure 9 – what about "worst case" soil class? What is the "sensitivity threshold?" Needs definition / explanation.

We have removed all references to the worst case soil class, since no results were



NHESSD

2, C3834–C3854, 2015

shown. We have provided a better explanation of sensitivity thresholds in the text.

The paper needs a Figure showing the map resulting from the best case model output, with traces of mapped landslides shown for a qualitative comparison.

We think that such a map would not provide any additional information. An example of model output is shown in Fig. 5.

Anonymous Referee #1 *At the end of the Introduction the description of the aim of the paper is hard to follow. You introduce several different concepts, which in my point of view have to be explained with more accuracy. For example when you example the ratio dH/dL a figure could help to make the text more comprehensive and to help the readers. I suggest you to state in a more clear way the objectives of you work and to leave any specific descriptions of the methodology to section 3.

We have modified this part taking specific comments of the other referee into account and changing the terminology. For example, we now use 'cross-sectional angle' that is more intuitive definition than dH/dL. We think that introducing part of the methodology helps to clarify the objectives of our work.

*Still concerning the Introduction I would suggest you to enrich the bibliography on landslides susceptibility assessment that it is too poor for an original scientific paper.

In principle we could agree with the referee but we decided to cite only the papers that are strictly related to our work. As we said to the other referee, the purpose of our work was not to propose a method for landslide susceptibility assessment.

*In the text you often make use of abbreviations but sometimes you don't define them, For example what is NNH and SGU? Please define them the first time you mention.

We now do.

*Even though the description of the methodology is fine (section 3), section 4 concerning the analysis and results is again not really comprehensive. My suggestion is to



NHESSD
2, C3834–C3854, 2015

reduce the length of this section, to simply report the results and to leave any discussion of the results for the following section. Section 4.1 can be moved to section 3 concerning the methodology.

We have modified this section. We hope that it is now more clear. Despite we could agree that the section is relatively long, we think that we just present the results and the discussion is done in section 5.

*In figure 1 and 5 I would suggest to better highlight landslide scars with another color, maybe red.

In Figure 1 we used the legend provided with the map by SGU, in Figure 5 a color that we think is relatively good to show the scarps. Using the red color in Figure 5 would definitely worsen the readability of the map.

Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/2/C3834/2015/nhessd-2-C3834-2015-supplement.pdf

---

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 7773, 2014.