Authors' response to reviewer comments on "Spatial distributions of earthquake-induced landslides and hillslope preconditioning in northwest South Island, New Zealand" by R. N. Parker et al.

We thank P. Meunier and the anonymous reviewer for their very fair, rigorous and constructive comments on our manuscript. We have addressed each of the reviewers' comments and in doing so, undertaken the recommended reanalysis that amounts to major corrections. Each comment is addressed individually, and we summarise the major changes made to the manuscript as follows:

- We have rerun our analysis using a landslide size threshold of 13,000 m^2, in order to eliminate uncertainties associated with censoring of smaller landslides due to the mapping technique and post-seismic vegetation regrowth.
- We have added the variable DIR, which accounts for the location of sites relative to the direction of seismic rupture directivity. This variable exhibits a significant influence on landslide probability and hence features in our probability models.
- We have added clear explanation of the process for comparing observed (a posterior values of 0 and 1) and predicted probabilities.
- We have added justification for the scale of elevation model resampling and generation of DEM derivatives.
- We have added more explanation of McFadden's pseudo-R^2
- We have added more explanation and justification of the method used to test for a signal consistent with hillslope preconditioning, along with an explicit discussion of the limitations. We also stress that although our result is statistically significant and has important implications for our understanding of the landslide distribution (now discussed), it should be treated as tentative due to unavoidable uncertainties in the available data for these events. Throughout the paper we have changed wording to clarify that this is a tentative, though potentially important, result. We emphasise that, were equivalent data available for more recent earthquakes, the methodology presented here may be used to further test our hypothesis.
- We have added a detailed discussion where we consider our results in light of other current work into the temporal aspects of earthquake-triggered landslides. We outline conceptually how the new findings may relate to other observations and are now able to better explore the contribution of this work to understanding distributions of landslides, in terms of the long-term evolution of failure in hillslope (substrate) materials.
- The conclusions have been updated to reflect additions to the discussion.
- Figures and tables have been updated with the updated results.
- We have added Appendix C, which includes a table of Variance Inflation Factors (VIFs), as part of assessing collinearity between predictor variables.
- We have added Appendix D, in which we compare landslide depths to uncertainty in the elevation data, as part of assessing the implications of using a DEM generated post-landsliding in our analysis

With these corrections, we find that the outcome of our test of hillslope preconditioning is unchanged. We are happy to accept, and emphasise to the reader, that further testing using data from other earthquakes is required to strengthen this finding.

Please note that page and line numbers refer to those in the updated manuscript PDF submitted with these comments.

R. N. Parker, G. T. Hancox, D. N. Petley, C. I. Massey, A. L. Densmore, N. J. Rosser

24 March 2015

Interactive comment on "Spatial distributions of earthquake-induced landslides and hillslope preconditioning in northwest South Island, New Zealand" by R. N. Parker et al.

P. Meunier (Referee)
meunier@geologie.ens.fr

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This paper investigates the influence of the damages accumulated during an earthquake on the rate and the pattern of mass wasting caused by the following earthquake through a process of long term weakening. This concept is illustrated by the analysis of the landslide patterns associated to two earthquakes with overlapping epicentral area and separated by a period of 39 years. Using a logistic regression technique to forecast the probability of hillslope failure associated to each of these earthquakes, the authors assess that the landslides caused by the 1968 earthquakes are promoted in the area of strong shaking of the 1929 earthquake. They deduce from this result that deep, long term weakening effects take place during the shaking, perturbing the erosion rate in the epicentral area during at least half a century. This is a very interesting and innovative result, which surely deserves a run in Esurf. The paper is clearly written and well presented and the figures are useful and well detailed. However, I'm not fully convinced by the strength of the result advanced in the study in its present state. I will present several remarks the author should address before considering this study for publication. I therefore ask for a publication via major revisions.

1. First, the authors should clarify what they call the observed probability of landsliding : "observed P_{LS}". By definition, this probability, being based on the observations a posteriori, should only be 0 (no landslide) or 1 (landslide). From the last sentence of the Fig.11 caption, it seems that they have extracted the cumulated landslide coverage in the area defined by the value n of the Predicted probability "Predicted P_{LS}" and then plotted the ratio of the landslide cover of this surface with n. If it is the case, they should explain it more clearly in the manuscript for it is fundamental to know which data is plotted against which model.

To clarify this, the following text has now been added to page 10, line 16:
Note that by definition the observed probability of landsliding, being based on observations a posteriori, is 1 for landslide sites and 0 for non-landslide sites. For comparison, observed and predicted probabilities are therefore aggregated (mean-averaged) across sites (pixels) that fall within equal quantile bins of the predictor variables. For each data point generated, the mean predicted probability represents the proportion of sites expected to fail, while the mean observed probability represents the proportion of sites observed to fail.

2. They should also explain why their P_{LS} never reach the value of 1 on Fig.11. Similarly, are there any landslides reported in the area of predicted P_{LS}=0 ?

The reviewers have highlighted an important point, which we now address with the following addition at page 16 line 11.

Note that in none of the models does predicted P_{LS} ever reach values of 1 or 0, as the predictor variables are not able to discriminate slopes where failure or non-failure is a certainty. This observation may be attributed to stochastic uncertainty in the model predictors, but also points to the possibility of important factors omitted from the model (epistemic uncertainty), of which the unconstrained damage legacy of past events is one possible candidate. While we do not have data to constrain the influence of all past events that have possibly conditioned hillslope materials, we are now able to test whether the *damage legacy of the largest recent earthquake (the 1929 earthquake) may be present in the distribution of landslide triggered by the 1968 earthquake.*

3. I don't understand why equ.(4) and equ.(5) still include NDS and CA. Fig 12 seems to demonstrate that pseudo-R2 shows no significant increase of the model predictability when they are included.

Following the updated analysis requested by reviewer #2 (in which landslides smaller than 13,000 m^2 were removed from the analysis), CA failed to produce a significant coefficient in the 1968 earthquake models, and has therefore been removed from the final result. We therefore limit our response comments to NDS. This variable is included in equations 4 and 5, because it has a statistically significant influence, albeit small, in the logistic regression models for both earthquakes, and so we report this variable for completeness. Although the model may be simplified by removing this variable, the fact that the influence is the same in both earthquakes suggests that inclusion of NDS slightly improves the predictive accuracy of the model. The same is also true of DIR, which now features in Equation 4, in response to the reviewer's later comment below.

Please note that the positive result in the test for preconditioning presented in Table 5 still holds true when NDS is removed from the fitting of Equation 5 (Table R1). The regression output is shown below. Note that FPD(1929) derives a significant negative coefficient, indicating that when other factors are controlled for the 1968 earthquake landslide probability decreases with distance from the 1929 earthquake fault.

Table R1 Logistic regression result for the 1968 earthquake with FPD(1929) added and excluding NDS.

1968 Inangahua Earthquake					
Number of observations		3181175			
Likeihood Ratio Chi2		29109			
Model p-value		0.00			
Pseudo R2		0.245		**95% confidence interval**	
Variable	**Coeffient**	**Standard error**	**p-value**	**Lower bound**	**Upper bound**
Intercept	-28.116	0.202	0.000	-28.512	-28.512
Log10(PGA)	11.036	0.101	0.000	10.839	10.839
SL(G=1)	0.097	0.001	0.000	0.095	0.095
SL(G=2)	0.110	0.001	0.000	0.108	0.108
SL(G=3)	0.115	0.001	0.000	0.113	0.113
SL(G=4)	0.154	0.002	0.000	0.150	0.150
FPD(1929)	-0.031	0.001	0.000	-0.034	-0.034

4. Fig. 13G shows that the 1968 model residual shows "partial correlation" with the 1929 Pls, not a strong one. The problem is that, from what I understand, the authors have use models using 2 different parameters (PGA in the 1968 model and FPD in the 1929) to construct it. The correlation should be done with models using the very same parameters (in this case FPD for the 1968 model). Otherwise, we don't really know what we are looking at.

Please see response to comment 6.

5. I somewhat fail to see any correlation on Fig. 13H.

For each plot in Fig. 13, we now provide correlation coefficients and p-values, attained by adding each x variable into a logistic regression of predicted P_{LS} and a posteriori observed probabilities. $P_{LS}(1929)$ and FPD(1929) both derive statistically significant coefficients.

6. Looking at Fig. 1, the landslide pattern of the 1968 EQ seems to be strongly influenced by the radiation pattern as the source initiated at the very northern part of the fault and propagated southward, increasing the shaking in the southwest quadrant.
   I'd suggest the authors to add a parameter for this effect in their 2 models. This could be done by subdividing the epicentral area into two subspaces (or four) with different weights (higher in southwest for the 1968 EQ). The limit of these two subspaces should be centered on the epicenter and should be oriented normaly to the direction of the rupture propagation. If this parameter is found to be controlling (through the pseudo R2), this might significantly change their final correlation between the 1968 model residuals and the 1929 Pls. In fact, it may strengthen it.

We have taken the reviewer's advice and have refitted Equation 4 for both events (1929 and 1968), using 2 subspaces. The new variable, DIR (Directivity), indicates sites in zones toward and away from which the ruptures propagated (now shown in Fig. 10). To clarify:

- In 1929 the rupture propagated north from the epicentre, therefore north of epicentre = 1, south of epicentre = 0
- In 1968 the rupture propagated southwest from the epicentre, therefore southwest of epicentre = 1, northeast of epicentre = 0

We find that this categorical variable derives a significant positive coefficient for both earthquakes, indicating that sites in the direction of the rupture propagation have a higher landslide probability. The addition of this variable improves the fit of the model. For the 1929 earthquake pseudo-R^2 increases from 0.183 to 0.201. For the 1968 earthquake pseudo-R^2 increases from 0.208 to 0.242.

With respect to the reviewer's comment #4, there is however an issue with using the Equation 4 to test for the effect of pre-conditioning. Now that the model has an additional degree of freedom to account for differences in landslide density across the two quadrants, any pre-conditioning effect (i.e.: higher than expected landsliding in the NE), will be masked by model over-fitting. Note when testing for pre-conditioning, this issue only applies to the 1968 model. While over-fitting may also occur in the 1929 model, we are not using this model to derive the residuals that would indicate pre-conditioning. We now stress clearly that the test for preconditioning depends on having a regional ground motion term that cannot be overfitted in this way (i.e.: the Shakemap model output, which is based on the Anderson et al. 1993,1994 fault model and ground motion intensities reported in Dowrick 1994.). This Shakemap model reflects the radiation pattern of seismic waves, and PGA(1968) also provides a better model fit than using the combination of FPD(1968) and DIR(1968).

We argue that we are justified in fitting the 1968 model using PGA, then testing whether the 1929 earthquake has an influence, using FPD(1929) as a proxy for the unavailable PGA(1929). We acknowledge that the scenario is not ideal and it would be preferable if PGA(1929) were available. However, in the absence of this data, or indeed data from a *better* pair of earthquakes to test these ideas, we know that a distance term provides a reasonable proxy for the spatial pattern of ground motions, and is the best available for this event. As noted below and in the manuscript we stress that in the case of these earthquakes our pre-conditioning result is significant but may be considered tentative in light of uncertainties in available input data.

Please note the following text that we have added in this regard:

Page 16 line 21:
We use Equation 5 to test our hypothesis of the influence of the 1929 earthquake on the landsliding resulting from the 1968 event. This model most accurately hindcasts the 1968 landslide distribution, using a ground motion term - PGA(1968) - that cannot be overfitted to the landslide distribution. Conversely, in Equation 4, DIR forms part of the ground motion term, giving the model an additional degree of freedom to account for any imbalance in P_{LS} between the northeast and southwest quadrants. As the northeast quadrant represents much of the area closest to the 1929 source, while the southeast quadrant is further away, the DIR variable absorbs and masks some of the preconditioning signal of the 1929 earthquake. Any test for preconditioning depends on having a regional ground motion term that cannot be overfitted in this way, for which we use the Shakemap PGA field. Based on observed ground motions, the Shakemap PGA also implicitly accounts for the effects of rupture directivity on ground motions represented by DIR in Equation 4. However, as the Shakemap data are subject to large uncertainties, we present the following result as tentative evidence of the effect of the 1929 earthquake on the 1968 landslide distribution, using the best available data for these events. To test whether the 1929 earthquake has influenced the 1968 landslide distribution, we use FPD(1929) as a proxy of the regional distribution of ground motion produced by the 1929 earthquake, in the absence of PGA data for this event. We acknowledge that this scenario is not ideal and it would be preferable if PGA(1929) were available, along with PGA(1968). However, in the absence of 1929 PGA data, a distance term provides a reasonable proxy for the spatial pattern of ground motions (Campbell and Bozorgnia, 2008).

Page 13, Line 3:
Our analysis has sought to control for all major factors known to influence the spatial distribution of landslides, at (or close to) the scale of the whole earthquake-induced landslide event. Using the best available data for the 1968 earthquake, our model achieves this using variables with defined physical links to landsliding, while maintaining a low level of model complexity, which avoids overfitting. Once these steps have been taken to control for the influence of other variables, our results suggest that landslide probability in 1968 is higher for hillslopes that experienced strong ground motions in the previous 1929 earthquake.

7. If maintained after the above modifications, the result of this study is somewhat in contradiction with what has been observed in Taiwan where the prolonged mass wasting, measured as an excess flux of river sediments, seems to vanish after a few years [1]. O.Marc is also preparing a manuscript on the prolonged rate of landsliding in epicentral area of several strong earthquakes (see [2,3] for personal communications) and he's found similar time frames. These contrasting results need to be discussed briefly.

Considering this comment has greatly helped us understand the likely relationship between reported short timescales of decay in post-seismic landslide activity, and our reported result of a preconditioning signal several decades after an earthquake (which is also supported by other numerical modelling work we are involved in (if of interest, please see references to preliminary work in Parker 2013 and Parker et al. 2013, also Moore et al. (2012))). However, we urge caution against treating river sediment fluxes as a direct indicator of landslide activity or hillslope material damage. Sediment fluxes reveal the evacuation of failure material from an orogen, which is subject to the remobilisation of failed material and exhaustion of landslide material deposits well-connected to the fluvial network. Our analysis is concerned specifically with hillslope material damage and triggering of landslide sources. Our results should therefore be compared with landslide data and substrate damage indicators. Having limited information on work in preparation (Uchida et al., 2014 and Marc et al., 2014), we have attempted to explore this aspect in a general sense:

Page 20, Line 1

Further advances in testing our theory may be made where multi-earthquake landslide datasets are available for more recent events, where higher resolution (and multi-temporal) elevation models are available, along with data from more dense seismic networks. On the basis that future testing may further support our hypothesis, we discuss the implications of our results in light of current understanding of the temporal landslide response to earthquakes. Fundamentally, our results are consistent with the idea that seismic ground motion produces irreversible damage, such that the legacy of past earthquakes may be preserved to a greater or lesser degree as a loss in strength in hillslope materials, for longer periods of time than previously thought. Several studies suggests that following large earthquakes, prolonged rates of mass-wasting, and associated indicators of changes in hillslope material strength, return to background levels within timescales of less than a decade (Hovius et al., 2011, Uchida et al., 2014, Marc et al., 2014). However, our data suggest that even after several decades, when the next large earthquake occurs, there is still a signal of hillslopes weakened by the previous earthquake. Note that unless some healing or annealing process takes place in hillslope materials, or all damaged material is stripped from hillslopes by erosion, there is no reason why we should not expect this to be the case. We explain this observation further by considering groups of hillslopes in different states from a spectrum of earthquake-induced damage. During the 1929 earthquake a first subset of hillslopes is weakened to the point of failure (co-seismic landslides). A second subset of hillslopes is moved to states close to the point of failure, such that failure of these hillslopes is triggered during relatively moderate aftershocks and post-seismic rainfall events. Landslides produced by these two subsets of hillslopes generate sediments that take time to be evacuated from the orogeny by fluvial processes, at a rate that decays over sub-decadal timescale as landslide deposits are exhausted of mobilisable sediment (Hovius et al., 2011, Dadson et al., 2004). A third subset of hillslopes has also been weakened by the 1929 earthquake, but insufficiently for moderate post-seismic events (aftershocks and rainstorms) to trigger failure. Additionally, yield stresses in these hillslopes may remain too high to be exceeded by moderate interseismic events, such that continued permanent deformation and damage accumulation does not occur post-seismically. As a result, the post-seismic rate of landsliding decays, while the landscape maintains a subset of hillslopes damaged and in a state closer to failure that prior to the 1929 earthquake, but which may only be brought to the point of failure by another large earthquake. Both co-seismically and post-seismically, only a relatively small proportion of hillslopes in the landscape actually undergoes full failure. For example, within 10 km of the 1929 source, only 3% of hillslopes were mapped as 1929 landslides. Therefore the behaviour of hillslopes that fail during or soon after an earthquake only accounts for small subset of the landscape effected by the seismic ground motions. The result we present here, and numerical simulations using geotechnical models (Parker et al., 2013, Parker, 2013, Moore et al., 2012), support the hypothesis that there is a legacy of damage in the remaining apparently intact landscape that may not fail either during or after an earthquake. If this is the case, then at any point in time, each of these subsets exists along *a continuum from pristine hillslopes to those damaged almost to the point of failure, evolving with each event that generates damage-inducing stresses.*

This long-term perspective may reveal why correlation between the 1968 landslide and the 1929 earthquake is weak. Although our analysis provides spatial estimates of the effect of the 1929 and 1968 earthquakes on hillslopes, we lack information on the damage condition of hillslopes prior to the 1929 earthquake. Hence we can only expect to find partial or weak correlation with a single past event, even if the 1968 landslide distribution were the deterministic product of the accumulation of all past events. However, one would expect events added to the historical record to incrementally and cumulatively account for more unexplained variability in landsliding. Similarly, if landslide distributions are pre-determined by the legacy of accumulated of damage from past events, then data from neither the triggering earthquake, nor a single previous event, can provide an exact prediction of landsliding. In this way, the apparently stochastic nature of landslide occurrence and the inability of current models to identify the exact hillslopes that undergo failure may in part result from not knowing the condition of each hillsope at the onset of shaking. In future, if the damage condition of hillslopes can be correlated with the history of past damage-inducing events, then building historical data or proxies for damage into landslide models may provide a means of constraining this effect.

P.Meunier

[1] Hovius, N., Meunier, P., Lin, C.-W., Chen, H., Chen, Y.-G., Dadson, S., Horng, M.-J., and Lines, M.: Prolonged seismically induced erosion and the mass balance of a large earthquake, Earth Planet. Sc. Lett., 304, 347–355, doi:10.1016/j.epsl.2011.02.005, 2011.

[2] Constraints on post-earthquake elevated landslide rate: towards forecasting of a general mechanism? T Uchida, O Marc, C Sens-Schönfelder, K Sawazaki, P Meunier, N Hovius. EGU General Assembly Conference Abstracts 16, 7392.

[3] Geomorphic and seismic coupled monitoring of post-earthquake subsurface weakening. O Marc, K Sawazaki, C Sens-Schönfelder, N Hovius, P Meunier, T Uchida. EGU General Assembly Conference Abstracts 16, 7212.

Interactive comment on "Spatial distributions of earthquake-induced landslides and hillslope preconditioning in northwest South Island, New Zealand" by R. N. Parker et al.
Anonymous Referee #2
Received and published: 11 February 2015

General comments:
Parker and co-authors present a very interesting study of the factors controlling landslide failure during two earthquakes in New-Zealand that have occurred in the same area in 1929 and 1968. Using a logistic regression based on many potential factors driving failure, they demonstrate convincingly that for each earthquake seismic ground motion (or more precisely distance to the fault for the 1929 EQ) and hillslope gradient (with an influence of lithology) are the main predictors of failure probability. This first result is well supported by the data and analysis. By itself, it is worth being published in esurf as it adds new constraint on EQ triggered landslides. I have only few comments on this part, which is very clearly written.

The authors build on this first result by exploring whether the 1929 EQ could have pre-conditioned hillslope material to make the area closest to the 1929 fault line more susceptible to rupture (all other predictors being accounted for). This is a very interesting question, which has hardly been addressed in the past. After some quick statistical analysis, the authors argue that the 1929 EQ has likely increased the failure probability during the 1968 EQ. However, I do not think the data analysis clearly support their conclusion. There might be a small effect, but this part of the paper lacks a more robust statistical treatment, and a more objective analysis of the results to be really convincing. The authors are trying too hard to see preconditioning in their data and are overlooking a careful discussion of the limitation of their approach. I'm afraid the uncertainties regarding the 1929 event (the landslide inventory is made 38 years after the 1st EQ , there is no PGA data nor DEM prior to the event) are too large for the effect this EQ could have on the 1968 event to be computed precisely enough to. I hope I'm wrong as I'd really like preconditioning to be important, but for the moment the data and results do not support this notion. I have many detailed comments on this aspect amounting at a large revision of the MS.

Apart from this, the paper is well written, clearly organized easy to follow and the bibliography is adequate. On top of the major comment that I have regarding the demonstration of preconditioning, I have many minor comments that should not distract from the fact that this a very good paper worthy of publication in esurf once properly revised.

Congratulations to the authors for a very interesting idea.

In addressing the reviewer's many helpful comments, we have particularly taken on board the point regarding the uncertainties in our input data and regarding 'trying too hard' to see preconditioning in the data. As outlined below, we have bolstered the statistical tests of preconditioning and are now more explicit about the data limitations. We also emphasise that, while we are using the best available data for these events and our result is statistically significant, due to uncertainties in ground motion and topographic data, the reader should treat our result as tentative. We suggest our workflow as a means to further test the preconditioning hypothesis, where multi-earthquake landslide data exist for more recent events, with dense seismic networks and improved topographic models.

Detailed comments

1. P9L12: have you checked for any correlation between the size of the landslide and any other parameters studied in your logistic regression?

Assuming that this relates to the reviewer's later comment, we have rerun the regression with a larger landslide size threshold (13,000 m^2). Please see below.

2. P10L24: that ~ 18 % of the landslides occurred in over-steepened source areas (and the logistic regression results) shows that local slope is a critical element. Yet, you do not discuss your choice of the scale of measurement of the slope (why 90 m ?), nor the fact that you don't have a DEM prior to the 1929 and 1968 EQ.

Please see response to comment 4.

3. P13L10-24 , Figure 9c: while I understand that the residuals could be correlated to Pls(1929) for high Pls(1929), the fact that there could be a trend at low Pls(1929) does not make sense to me. Should it not be uncorrelated here ? If the 1929 had little probability of failure, it should not reduce the probability of failure for 1968, but simply not change it.

The reviewer is correct, we would expect the 1929 earthquake only to increase the 1968 landslide probability, and not actually decrease it in regions of low failure probability. The effect the reviewer highlights is due to fact that we are looking at a pattern in the residuals. As the residuals of fitted regression models sum to zero, any trend in the residuals must start < 0 and end > 0 (or vice-versa). The negative residuals indicate where a model that does not explicitly account for pre-conditioning will in relative terms over-predict landsliding, because this variable has not been included in the model.

In order to clarify this, we have added the following to the end of the Fig. 9 caption:
Note that positive and negative residuals are relative to the prediction of a model that does not explicitly consider the effect of hillslope preconditioning, but is fitted using landslide data that is subject to the effect of hillslope preconditioning. Therefore it is the direction of the trend, rather than the absolute (positive or negative) residual values, that is of importance in the test of preconditioning.

4. P13L25: if I remember well the 10 m resolution is not a great DEM (e.g. staircase effects due to the contour lines of the 50000 maps it is derived from). Resampling it at 30 m is a good idea, however you should justify the choice of this spatial scale as it has many implications for the subsequent analysis (slope calculation, aspect etc...).

The following text has been added:

Page 11, Line 18:
The elevation model was resampled at this scale to remove fine scale noise, while ensuring that the characteristics of individual landslides are resolved. Using a 30 m grid, we ensure that more than ten sample points fall within the smallest landslides included in our analysis. Additionally, 30 m is much less than typical hillslope lengths in the region of 500 m, ensuring that multiple hillslopes are not contained in a single pixel.

Page 12, Line 7-12:

In the calculation of elevation derivatives, using a spatial window size (3 pixels or 90 m) smaller than the smallest individual landslides included in our analysis, we minimise the risk of overgeneralising the characteristics of individual landslides.

5. P14L23: for many landslides the scale at which the local hillslope gradient (SL) is measured is actually smaller than the landslide itself. Given that the DEM is post-1968, this means that the local hillslope gradient used in the logistic regression is posterior to landsliding and would typically be either smaller than before the EQ (in the center and at the base of the landslide) or much steeper (in the boundary of the landslide). Given that (SL) is a critical discriminant factor of the logistic regression, I think the authors should explore other values of the hillslope gradient (e.g., derived from unruptured area immediately close to the landslide), or estimated at a much larger scale. At least, they should discuss potential biases induced by using a DEM posterior to the EQs.

We agree that this is an important issue for discussion and have added the following text, which also emphasises the uncertainties in ground motion data:

Page 19 Line 13:
We stress that this suggestion must be treated as tentative due to uncertainties in our analysis variables. This particularly applies to the ground motion proxies and the PGA field, which relies on interpolation from limited observations, using ground motion prediction equations (Wald et al., 2006). Additionally, as the elevation model used in our analysis was derived following both earthquakes, there is the possibility that hillslope gradients measured at landslide sites may not accurately reflect slope characteristics at the time of landslide triggering. However, depths of most mapped landslides are likely to be smaller than uncertainties in the elevation data, suggesting that the 1929 and 1968 landslides are unlikely to have produced surface changes detectable in the elevation model (Appendix D). As our analysis explicitly considered only the source area of landslides, any bias is likely to involve over-estimation of gradients, in source areas where headscarps have been steepened by landsliding, or no effect in cases of translational failures. In our probability modelling, underestimation of gradients for landslide sites produces over-prediction of landslide probability for steeper hillslopes. The residuals of Equation 5, plotted against slope gradient (Figure 13 B) may indicate very slight over-prediction at high gradients, reflecting this effect. However, as post-landslide topographic changes are small relative to the elevation model uncertainty, and as slope gradient appears to be well-fitted to the data, this suggests that the use of post-landslide elevation data has little effect on the outcome of our analysis.

We are cautious of the reviewer's suggestion of using 'unruptured' areas adjacent to landslides or gradients measured at larger scales, as this relies on the assumption that neighbouring hillslopes have not already failed as landslides in a past event. We feel this may not be a safe assumption to make for this seismically-active mountain range.

6. P15L24: I'm not familiar with McFadden's Pseudo R2 . Maybe giving a bit more details on how it is actually computed could help the audience to better understand its meaning.

In addition to Equation 3, we have now added additional explanation of the pseudo-$R^{2:}$
Page 13 Line 24:

The pseudo-R^2 is designed to look like a conventional R^2 goodness-of-fit, derived from ordinary least square regression, with values ranging from 0 (no correlation) to 1 (perfect correlation or in the case of logistic regression, perfect separation of true (landslide) and false (non-landslide) categories). As logistic regression is fitted through an iterative process of maximum-likelihood estimates, the conventional R^2 approach to goodness-of-fit does not apply. However, like conventional R^2 values, pseudo-R^2 can be seen as an indicator of explained variability and the level of improvement offered by the full model over the model without its predictors (McFadden, 1974).

7. IMPORTANT COMMENT: it is not clear how the observed Pls (which varies between 0 and 1) is estimated from the map of hillslope failures (binary values). This should be explained in detail.

Reviewer #1 also highlighted this issue. To clarify, the following text has now been added to page 10, line 16:
Note that by definition the observed probability of landsliding, being based on observations a posteriori, is 1 for landslide sites and 0 for non-landslide sites. For comparison, observed and predicted probabilities are therefore aggregated (mean-averaged) across sites (pixels) that fall within equal quantile bins of the predictor variables. For each data point generated, the mean predicted probability represents the proportion of sites expected to fail, while the mean observed probability represents the proportion of sites observed to fail.

8. P16L13: remove "this".

Thank you. 'This' has been removed.

9. P17L18: I don't understand the notion of size in the context of this sentence. Is it the magnitude, is it the size of the landslides ? Rephrase.

'size' has been replaced with 'magnitude'.

10. P18L10: "topographic amplification": this is an interpretation. You're just measuring the position with respect to the divide. Hillslope geometry (convex vs concave) could generate a dependency to NDS by the sole hillslope gradient dependency. This is why exploring the cross-correlation within your predictors could be interesting to reduce the number of meaningfull ones.

Page 15 line 28 now reads:
Although each predictor explains a component of the variance in the spatial distribution of failures, not all variables contribute equally. Figure 12 presents the predictor variables in rank order of their importance in each model, determined by sequentially removing the predictor contributing least to the fit of the model. In all three models, the regional ground motion proxy (distance from the fault plane or PGA) and hillslope gradient rank as the top two variables, followed by geology, position on hillslope, and location relative to rupture directivity in the case of Equation 4. In all three models, the regional ground motion, hillslope gradient and geology account for over 80% of the total model fit, while position on hillslope (NDS) and location relative to the location relative to rupture directivity (DIR) are secondary in defining the spatial distribution of landslides. The position on hillslope (NDS) relationship is consistent with ridge-to-valley scale patterns of amplification and damping of seismic waves found by others (Davis and West, 1973, Bouchon, 1973, Wu et al., 1990, Benites et al., 1994, Meunier et al., 2008).

The issue of cross-correlation is addressed below.

11. P18L15: the following section is a bit weak as it does not objectively describe the results and try to push the idea that the variation in the residuals are determined by Pls 1929.

We have now rewritten section 5.2 based on the reviewers' recommendations.

12. There's also one thing not really clear in Figure 13: the amplitude and values of the residuals should theoretically be the same in each graph. We're looking at Y=[Observed Pls1968-Eq(5)] as a function of various predictors and Pls1929 and Distance from 1929 fault. I suppose that the variations in the amplitude of the residuals (from a range of 0.006 in fig. 13G to less than 0.00001 in fig. 13E) comes from the binning that varies with the chosen predictor. But this is far from being clear in the text or the legend, and suggests that there are actually very large variations in the residuals that the 'mean' is hiding (hence my request for displaying the standard deviation of each bin).

The reviewer is correct, the difference in the amplitude of the residuals is due to binning and aggregating the probabilities with the different predictor variables (Note that this is necessary as the observed a posterior probability of landsliding has values of 0 and 1).

We have added the following sentence to clarify this at the end of the Fig. 13 caption:
Note that the amplitude of the plotted residuals varies due to binning and aggregating probabilities with the different predictor variables.

Unfortunately, as the observed a posteriori probability has values of 0 and 1, the standard deviation of residuals is not particularly useful for understanding the variability in each bin.

13. P18L19: this is not what I observe. In figure 13C there's a pattern (i.e. there's no random variation in the residuals) but you choose to ignore it, while you choose to see a trend in figure 13G and a correlation in fig13H !. You need to beef up the statistical analysis of these graphs.
    P18L26: I don't see any correlation in Fig. 13h...quite the contrary.

As noted below, this section has been rewritten with additional statistical tests added.

14. P19L2: you could also mention that the pseudo-R2 increases from 0.246 to 0.247 when refitting the model. However, I'm not sure I would qualify this as a significant effect.

In our updated model (using the large landslide area threshold recommended by the reviewer), the increase in R^2 is actually larger. We have now added the following sentence to draw attention to this.
Page 17, Line 29:
By adding FPD(1929) into Equation 5, we are able to improve the fit of the logistic regression model from $R^2=0.246$ *to* $R^2=0.251$.

15. Figure 10: a bit difficult to read and evaluate. At first order, it seems that pd3, pd6, SL,ES, ER are highly correlated. Any logistic regression would probably not been able to separate the effect of these 5 predictors. I'd suggest to explore cross-correlation and keep only the predictors that are truly independent of each other.

Multicollinearity was investigated during the fitting process and the final fitted models only include variables that are found to be independent of each other. To summarise this we have added a table of variance inflation factors in Appendix C, and the following text on page 13 line 11.

In order to avoid the problem of over-fitting regression models and predictor covariance, an issue particularly characteristic of automated fitting procedures (e.g.: Hosmer and Lemeshow, 2000), model fitting was undertaken manually, and based on the following criteria:

[…]

2. Predictors variables included in the model must not exhibit multicollinearity, as determined by variance inflation factors (VIF):

$$VIF = \frac{1}{1 - R^2}$$

where R^2 is the linear coefficient of determination of the relationship between any two predictor variables. VIF values greater than 10 indicate a high level multicollinearity, and are avoided in our model (Kutner et al., 2004). The matrix of VIF values is given in Appendix C, and indicates no high multicollinearity among the variables that feature in our final models.

Figure 11: I'd suggest to invert the color scale for Pls such that it matches (to some extent) the observed map of hillslope failures.

Thank you for the suggestion. We have tried both ways and prefer to keep the more conventional dark=low, light=high colour scale here.

16. Comparing figures 11F and 11G, it seems that the residuals for 1968 are not much larger than the residuals of 1929 and are both patterned. This led me to 2 suggestions:

- the first one is to plot the residuals of 1929 against the predicted Pls(1929), to evaluate how much variability there's "naturally" in the 1929 model. The whole demonstration of preconditioning relies on the assumption that Pls1929 is a good predictor of observed failure probability, but what if it is not good enough because you don't have the pga for 1929 (which seems a critical element in 1968) or a DEM prior to the event?

This is not quite correct. The demonstration of preconditioning relies on the following:

1. The assumption that the 1968 model (Equation 5) accounts for variability due to factors known to influence landsliding

2. That residual variability unexplained by the 1968 model correlates with the expected distribution of ground motions (or effect on hillslopes) of the 1929 earthquake.

For the latter, the distance from the mapped 1929 seismic source provides a reasonable approximation (either as a single variable or when combined with others variables to hindcast 1929 landslide probability). In terms of the relationship between predicted and observed 1929 landslide probability, this is shown in Figure 11 and displays a reasonable fit to equality. For completeness, a plot of the 1929 residuals (Equation 4) is given here in the response letter (Figure R1). The fit of this model is not as good as Equation 5, and we see larger residuals and greater non-linearity. However, $P_{LS}(1929)$ still provides a reasonable predictor of 1929 failure probability, with a monotonic relationship between observed and predicted landslide probability. For this reason, we argue we are justified in using $P_{LS}(1929)$

to test for the influence of the 1929 earthquakes on the 1968 landslide distribution. Note that even if we did not have landslide data for the 1929 earthquake, it would still be possible to test whether unexplained variability in 1968 landsliding was correlated with distance from the 1929 earthquake seismic source. As outlined elsewhere, we are explicit about the tentative nature of this result.

We address the issue of post-landslide elevation data below.

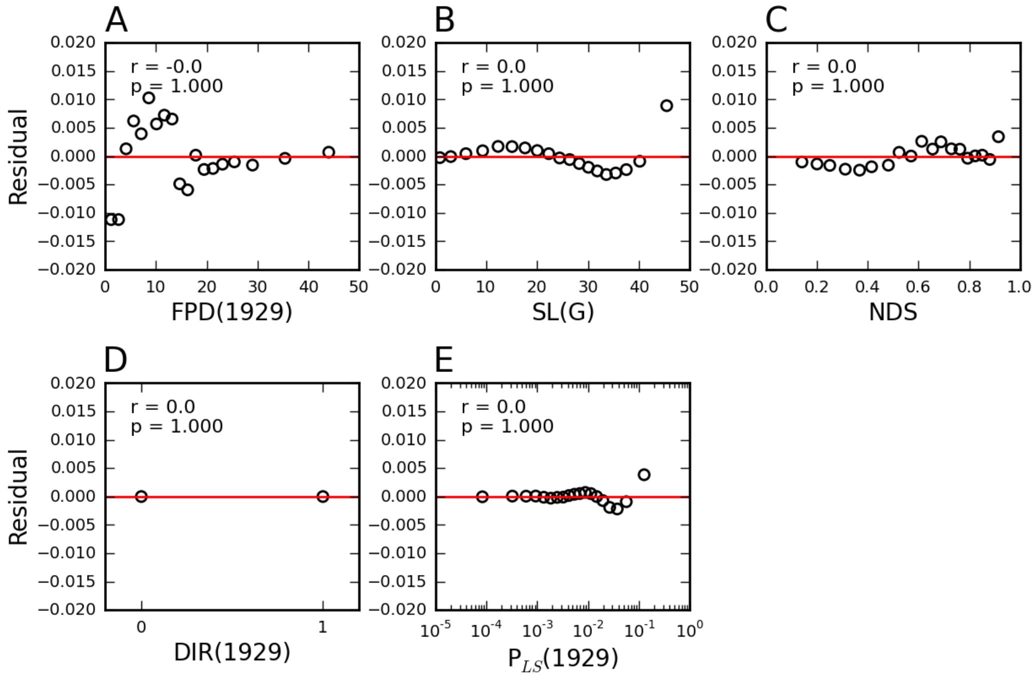

Figure R1 Distribution of P_{LS} residuals for the 1929 earthquake, hindcast using Equation 4. Residuals are calculated by aggregating probabilities across 20 equal quantile bins of the x-variable. Positive residuals indicate that the model under-predicts P_{LS} and negative residuals indicate that the model over-predicts P_{LS}. Note that the amplitude of the plotted residuals varies due to binning and aggregating probabilities with the different predictor variables. For each plot, the coefficient (r) and significance (p) of correlation in the residuals are given. These were derived by adding each x variable into a logistic regression analysis of P_{LS} predicted using Equation 4, and observed landsliding. In this respect we test the coupled significance of SL and G as they feature in the model.

- Add for each graph of figure 13, the standard deviation for each quantile bins of the x variable. This would help in deciphering whether a trend in the average is statistically significant or not.

We found that the best way to deal with this problem was to display correlation coefficients and p-values for each plot, which evaluate the strength and significance of the relationship in the residuals. These values were attained by adding each x variable into a logistic regression of predicted versus observed probability, to test whether the x variable has a significant influence. The predictors already included in the model are well fitted, and therefore derive no significant coefficients. However, the variables representing the 1929 earthquake do derive significant coefficients, supporting our results.

As noted above, the observed a posteriori probability has values of 0 and 1, the standard deviation of residuals is not particularly useful for understanding the variability in each bin.

17. P20L1-3: you should first try to discuss the limitations of your approach, rather than first trying to push the idea that the 1929 EQ has influenced the 1968. I see at least three points to discuss:

- at this stage of the paper, it seems that the authors have forgotten that they are working on a very "strange" landslide inventory, taken 38 years after the first EQ over which the 1968 have superimposed new landslides. Clearly, the 1929 data is censored of small events (as the authors acknowledge themselves), but how these limitations could play a role in the model regression is never discussed. I suggest for instance to redo the calculation by censoring the 1929 with a very large minimum landslide size and see how it affects the subsequent calculations.

In the updated manuscript we have run our analysis using a threshold landslide size of 13,000 m^2, where no censoring of 1929 or 1968 landslides is apparent in the magnitude-frequency distribution (Fig. 8). The updated result continues to support our reported findings. In fact, as noted above, the increase in R^2 from adding FPD(1929) to our 1968 model is now larger than when using all landslides in the analysis.

The follow text has been added:
Page 11 line 13:
In order to undertake logistic regression analysis, we first removed landslides with areas less than 13,000 m^2 from our dataset, to eliminate biases arising from small landslides censored by the mapping resolution and post-landslide vegetation regrowth.

- given the small changes in probability you're looking at, how accurate and precise is Eq. (4) to predict the "impact" of the 1929 EQ on the 1968 EQ ? Given the lack of predicted pga for 1929, is it really realistic to assume that Pls1929 is good enough to detect a very small effect on the 1968 data ?

- what could possibility be the impact of building a logistic model using hillslope gradient measured after the EQ. and use it to predict failure probability on non-ruptured hillslopes ? That probably means that the sensitivity to hillslope gradient built into the 1929 logistic model is likely incorrect, or not as precise as it could be. I understand there's no easy way to sort for this effect, but it must be discussed given the importance of hillslope gradient.

We have added the following section to the discussion in which we are explicit about the limitations in our analysis and tentative nature of our result:
Page 19 line 9:
This behaviour is consistent with our hypothesized influence of damage accumulation, where failure occurred in brittle hillslope materials. Our results suggest the possibility that in the case of the 1929 earthquake, damage in unfailed hillslopes persists, resulting in regions close to the 1929 seismic source enhanced sensitivity to landslide triggering in 1968. We stress that this suggestion must be treated as tentative due to uncertainties in our analysis variables. This particularly applies to the ground motion proxies and the PGA field, which relies on interpolation from limited observations, using ground motion prediction equations (Wald et al., 2006). Additionally, as the elevation model used in our analysis was derived following both earthquakes, there is the possibility that hillslope gradients measured at landslide sites may not accurately reflect slope characteristics at the time of landslide triggering. However, depths of most mapped landslides are likely to be smaller than uncertainties in the elevation data, suggesting that the 1929 and 1968 landslides are unlikely

to have produced surface changes detectable in the elevation model (Appendix D). As our analysis explicitly considered only the source area of landslides, any bias is likely to involve over-estimation of gradients, in source areas where headscarps have been steepened by landsliding, or no effect in cases of translational failures. In our probability modelling, underestimation of gradients for landslide sites produces over-prediction of landslide probability for steeper hillslopes. The residuals of Equation 5, plotted against slope gradient (Figure 13 B) may indicate very slight over-prediction at high gradients, reflecting this effect. However, as post-landslide topographic changes are small relative to the elevation model uncertainty, and as slope gradient appears to be well-fitted to the data, this suggests that the use of post-landslide elevation data has little effect on the outcome of our analysis.
Further advances in testing our theory may be made where multi-earthquake landslide datasets are available for more recent events, where higher resolution (and multi-temporal) elevation models are available, along with data from more dense seismic networks.

18. P20L27 and end of discussion: to really make a case for considering preconditioning in landsliding susceptibility, you should give the reader an order of magnitude of how incorrect would be eq. (4) if it did not take into account previous effects : e.g. what would be the surface affected by landslides with or without preconditioning. As much as I like the idea, it seems your data are showing this is only a third or even fourth order parameter compared to pga, hillslope gradient and lithology (fig 12C). Maybe a more balanced and less arm-waving discussion would be better.

We have now added information on the order of magnitude of the preconditioning effect:
Page 17, Line 13:
To put this result in context, for regions within 15 km of the 1929 fault plane, observed P_{LS} is 56% higher than P_{LS} predicted by Equation 5. A predicted landslide area of 2.4 km^2 and an observed landslide area of 3.7km^2, amounts to a 1.3 km^2 (56%) underestimation of the total landslide area in this 1648 km^2 region.

We have also added a substantial discussion section of how our observations and other current work in to temporal aspects of earthquake-triggered landsliding may link back to the dynamics of the evolution of failure in population of hillslopes (Please see response to reviewer #1, comment 7)

19. P21L8: no, you do not directly demonstrate the topographic amplification effects, you postulate it.
20. P21L9: as above you do not demonstrate that hillslope weathering is the predictor. It is just hillslope orientation (which in itself is a great result that you could emphasize better in the discussion!). You could also emphasize in the conclusion that many tested parameters do not appear critical for predicting EQ triggered landslides!

We have edited Conclusion #1 to read:
The 1929 and 1968 earthquakes reveal a consistent spatial pattern of landslides that can be modelled probabilistically as a function of spatial variability in seismic ground motion, hillslope gradient, lithology and position on hillslope (which we postulate is a proxy for ridge-slope-scale amplification and damping).

21. P21L24: point 1 of the conclusion clearly states that failure probability is explained at 90% by pga and hillslope gradient (with litho effect). How come that the current

> damage state of hillslopes represents a significant source of uncertainty? Again this looks like unnecessary arm waving given the uncertainties in the data and subsequent analysis.

Following our reanalysis, 90% has now changed to 80%

Please note that we state:
"Statistically, the seismic ground motion and hillslope gradient (where the influence of hillslope gradient is lithologically dependent) account for the majority (>80%) of the explanatory power of the model."

This 80% refers to the relative contribution of PGA and hillslope gradient (+litho) to the total fit or explanatory power of a model with all significant predictor variables included in it. In other words, PGA and gradient (+litho) are the best of our currently available predictors. However, the absolute fit of the model still has substantial uncertainty, as we are a long way from being able to accurately discriminate between landslide and non-landslide sites.

Our extended discussion also addresses the concerns raised in this comment, by exploring the meaning of our findings in terms of landslide failure processes. Please see page 20 line 1 to page 21 line 25.

Interactive comment on Earth Surf. Dynam. Discuss., 3, 1, 2015.

Additional minor corrections:

Page 1 line 28:
We then assess whether this variability can be attributed to the legacy of past events
Has been changed to
*We then assess whether this variability **may** be attributed to the legacy of past events*

Page 1 line 27: Sentence removed
Our results suggest that the 1929 Buller earthquake influenced the distribution of landslides triggered by the 1968 Inangahua earthquake.

Page 2 line 4:
While our results are tentative, the findings emphasize that a lack of knowledge of the damage state of hillslopes in a landscape potentially represents an important source of uncertainty when assessing landslide susceptibility. Constraining the damage history of hillslope materials, through analysis of historical events, therefore provides a potential means of reducing these uncertainties.
Has been changed to:
While our results are tentative, they suggest that the damage legacy of large earthquakes may persist in parts of the landscape for much longer than observed sub-decadal periods of post-seismic landslide activity and sediment evacuation. Consequently, a lack of knowledge of the damage state of hillslopes in a landscape potentially represents an important source of uncertainty when assessing landslide susceptibility. Constraining the damage history of hillslopes, through analysis of historical events, therefore provides a potential means of reducing this uncertainty.

Page 2 line 21:

In other words, the predicted number of landslides triggered by any given trigger event will not vary through time.

Has been changed to:

In other words, the predicted number of landslides triggered by any given trigger event, or the susceptibility to landsliding in that event, will not vary through time.

Page 3 line 26:

If previous earthquakes do influence patterns of landsliding in subsequent earthquakes, then it is reasonable to hypothesize that spatial distributions of landslides should be at least partially correlated with the ground motions from past earthquakes.

Has been changed to:

If damage from previous earthquakes does influence patterns of landsliding in subsequent earthquakes, then it is reasonable to hypothesize that spatial distributions of landslides should be at least partially correlated with the ground motions from past earthquakes.

Page 4 line 27:

This model assumes a fault plane striking 015^o, and dipping at 45^o from the surface to a maximum depth of 12 km, with a dip direction of 100^o.

Has been changed to (correction to fault strike):

This model assumes a fault plane striking 010^o, and dipping at 45^o from the surface to a maximum depth of 12 km, with a dip direction of 100^o.

Page 5 line 4: Sentence added:

Earthquake parameters for both events are summarised in Table 1.

Page 8 line 29

where x_{min} is the minimum size of landslide modelled by the function and α is the power-law scaling exponent.

Has been changed to:

where $p(x)$ is the probability of a landslide having a given size, x_{min} is the minimum size of landslide modelled by the function and α is the power-law scaling exponent.

Section 5 (from page 14 line 16) has been largely rewritten to accommodate the additional and updated analysis.

Page 18 line 11:

Our results both support the findings of previous work into modelling earthquake-induced landslides, as well as providing new insights into how past earthquakes influence future landslide distributions.

Has been changed to:

*Our results both support the findings of previous work into modelling earthquake-induced landslides, as well as providing new insights into how past earthquakes **may** influence future landslide distributions.*

Page 18 line 19:

This particularly concerns factors influencing the aspect of landslides, which implies that patterns observed in other earthquakes may be regionally specific or confounded by the influence of other more 'powerful' predictors that might not have been controlled for.

Has been changed to:

This particularly concerns factors influencing the aspect of landslides. Neither the orientation of hillslopes relative to the seismic source, nor relative to hillslope-scale variations in received solar radiation, were found to exhibit a significant influence on landslide probability. This implies that patterns observed in other earthquakes may be regionally specific or confounded by the influence of other more 'powerful' predictors that might not have been controlled for.

Page 19 line 3:
While time-independent variables provide useful constraints on the spatial distribution of landslides, our results suggest that previous earthquakes also impart a significant influence on future landsliding.
Has been changed to:
While time-independent variables provide useful constraints on the spatial distribution of earthquake-triggered landslides, our results suggest that previous earthquakes may also impart an influence on future landsliding.

Page 19 line 6: Clarification added:
(or those closer to the 1929 seismic source)

Page 19 line 11:
Our results suggest that in the case of the 1929 earthquake, damage in unfailed hillslopes persists, resulting in regions close to the 1929 seismic source having enhanced sensitivity to landslide triggering in 1968.
Has been changed to:
Our results suggest the possibility that in the case of the 1929 earthquake, damage in unfailed hillslopes persists, resulting in regions close to the 1929 seismic source enhanced sensitivity to landslide triggering in 1968.

The conclusions have been updated to reflect the updated results and discussion.

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