Response to reviewer comments

Original reviewer comments are in *italicized text*.

Our replies are in plain text.

Bold text shows small corrections within the original text.

We (K. E. Clark and co-authors) are providing a revised version of our manuscript for consideration and would like to thank the two referees (anonymous referee #1 and Ken Ferrier) for their reviews, which we feel helped us to substantially improve our paper during revision. In particular, we now include an expanded methodological discussion and a table detailing the soil carbon stock data, in response to the request from both reviewers for more information about these calculations. We provide detailed responses to these and other comments below, and our revision incorporates our thorough effort to address each of these points.

Detailed response to interactive comment from reviewer #1 (anonymous)

In their paper "Storm-triggered landslides in the Peruvian Andes and implications for topography, carbon cycles, and biodiversity", Clark et al. present a largely remote sensing-based investigation of landslide distribution in space and time. They draw on field-derived measurements of soil properties and carbon content to derive carbon yields. They draw a number of conclusions about the degree, timing, and distribution of erosion and carbon export in the Kosñipata valley. This is a well-conceived and well supported study that has incremental, but important, implications for geomorphic studies. In particular, the authors make some very good points about landslide inventory biases by spatial-temporal variability, and the possible control on biomarkers.

We thank the reviewer for these positive comments on our paper.

***General comments**:*

More information is needed around the calculation of soil organic carbon. A table in the supplemental data would be appreciated here. Is a single density used for each pit? Seeing the depth intervals and carbon/density values would help the reader to understand why the calculated carbon stocks here are 2x (give or take, according to Figure 7a) the previous estimates. I would like to see an explanation of why these values are higher, not just that this dataset is more complete.

Reviewer #2 raised a similar concern, and we appreciate both reviewers making the point that the manuscript needed more background information about the soil pits and associated soil carbon stock calculations. As suggested by the reviewer, we now include a supplementary table (Table S3) that describes the location of plots, forest type, number of pits dug per plot, average depth of plot and average soil carbon stock. In addition, in the revised main text (section 3.3) we have clarified the calculation of soil organic carbon content, as follows:

"Carbon stocks were determined by multiplying interval depth (m) and measured soil organic carbon content (%) by bulk density (g cm^{-3}) **for each soil layer. %OC was measured in each layer for every pit. For each plot one pit was measured for bulk**

density at the following intervals: 0-5, 5-10, 10-20, 20-30, 30-50, 50-100, 100-150 cm, and the depth-density trend from this pit was applied to other pits from the same plot. Soils were collected and processed following the methods Quesada et al. (2010). An average SOC stock (in tC km^{-2}) for each plot was determined from the mean of individual pit SOC stocks (Fig. 7a; Table S3).

The reviewer also asked for more complete consideration of why the soil OC values are higher in this study than reported in previous papers. In our original Discussions paper, we attributed these differences primarily to the different depths over which C stocks are integrated (prior studies calculated stocks over only the top 30 or 50 cm, while we calculate over the full depth of mobile soil material, varying by plot from average depths of 33 to 158 cm, as now reported in Table S3). The reviewer's comment stimulated us to consider the comparison to prior work in more detail. We do not have access to the original data from prior studies (only the final C stock values), so we cannot provide detailed comparison of depth-by-depth C concentrations, as suggested by the reviewer. However, we can calculate an inferred C stock for the pits reported in our study over the top 0-30 cm and 0-50 cm intervals, matching the depth intervals used in prior work. The resulting values are similar to those reported in the prior respective studies, lending confidence to our interpretation and emphasizing that the differences can be attributed primarily to integration depth. We have included this new comparative analysis in new figure in the supplement (Figure S2) and we discuss this comparison in more detail in Section 3.3.2, as follows (all new text):

"Our soil C stock values are a factor of 1.2 to 1.7 higher than values reported in these previous studies (Girardin et al., 2014a; Zimmermann et al., 2009). For the same soil pit data (i.e., density and %C) used in this study, calculation of soil C stocks over depths equivalent to those used in the prior studies (i.e., over the top 0-30 cm and 0-50 cm) yields values in close agreement with those previously reported (Fig. S2). This consistency indicates that the differences between the full-depth values used here, versus the partial depth values reported previously, are attributable predominantly to the integration depth used."

The mapped landslides include both scars and deposits. Please discuss the implications beyond the inclusion of low slopes. This would make some, but possibly not all, landslide areas too large. Might there be a topographic bias associated with this?

We have expanded our consideration of the implications of mapping landslide scars and deposits together, pointing out that this approach should still capture the extent of landslide effects on biomass. We have added the following paragraph to Section 3.1, in the Methods (all new text):

"The landslide areas visible via spectral contrast in the Landsat images include the regions of failure, run-out areas, and deposits. In some of the high-resolution imagery, we were able to distinguish scars from deposits, but not systematically enough to separately categorize these for the full landslide catalogue in this study. One 2007 landslide was coupled to a particularly large debris flow and stood out within our inventory, with the 1.7 km long debris flow comprising ~5% of the total landslide area for the total inventory from 1988 to 2012. With this one exception, we consider all areas with visible contrast outside of river channels as being "landslide" area (e.g., see Fig. 2a and inset photo). For the purposes of quantifying biomass disturbance and organic carbon fluxes associated with landslide activity, the convolution of scars and deposits is justified on the basis that all of these areas were covered in forest prior to landslide occurrence. However, the fate of carbon from scars vs. deposits may differ, as discussed below, and when considering the slope distribution of landslide areas, the role of deposit areas introduces some bias (see further discussion in Section 4.2, below). Future landslide mapping work, taking advantage of even higher resolution imagery than available in this study, would benefit from the effort to explicitly distinguish scars and deposits for full inventories."

As suggested by the reviewer, we have also clarified the potential implications for the relation between landslide occurrence and topography, with the following text added to Section 4.2 (all new text):

"Since our mapping did not distinguish landslide scars from deposits (see Section 3.1), systematic changes in the ratio of scar to deposit area with elevation could influence apparent patterns of landslide occurrence. For example, larger deposit areas at low elevation would increase calculated susceptibility even if the total landslide scar area were not larger. However, our anecdotal field observations do not suggest that landslides at lower elevations have consistently longer run-out or larger deposit areas, so it is unlikely that such bias explains the observed relations between landslide occurrence and topography within our inventory."

The relationship between erosion and topography is not clear. Based on the landslide inventories, the authors suggest that erosion rates are highest at low elevations and decrease with elevation. They also state that the low elevation plateau may be a result of high erosion rates not yet propagating onto the plateau. Based on their mapping, the knickpoints in the streams representing this boundary occur at ~1400-1600 m a.s.l. The landslide-derived erosion rates peak at least 1000 meters higher. I understand that part of the paper shows the importance of the single event, but it seems that something is missing from the discussion.

Landslide susceptibility, which is the relevant metric for describing the extent of landslide-associated erosion, increases below the elevation range around 1500-2000m (Fig. 5b), which is coincident with the observed fluvial knickzone, at least within our ability to resolve this zone. These two features are not offset by 1000 m, as the reviewer states. The reviewer may be referring to Fig. 5a, which shows total landslide area as a function of elevation, which does indeed peak at elevations around 3000 m. But the catchment area at these elevations is much larger, so the effective depth of erosion associated with landslides is lower (as reflected in the landslide susceptibility in Fig. 5b). We apologize that this distinction was confusing in our original text and have revised Section 5.2.2 in an effort to clarify this aspect, as follows (mostly new text):

"A second set of information comes from the Kosñipata Valley topography and its relation to implied erosion associated with landslide activity. Although total landslide area in our Kosñipata dataset is greatest at mid-elevations, these mid-elevation landslides are distributed over a relatively large catchment area (Fig. 5a). Effective landslide erosion is greatest where landslide susceptibility on a unit-area basis is highest (Fig. 5b), so our inventory implies focused landslide erosion at lower elevations (< ~1500-2000 m) in the Kosñipata Valley, specifically associated with the 2010 storm (Figs. 2a, 5). This focused erosion appears to spatially coincide with the observed transition in the river channel profile at ~1700 m elevation, marked by the vertical step knickpoint (Fig. 10a)."

***Specific comments*:**

pg. 637 – bottom: More landslides should result in lower concentration of cosmogenic nuclides in quartz. If sediment cosmogenic nuclide-derived erosion rates are lower than the landslide rates, then they have 'higher' concentrations than they should. This implies that there is either significantly storage of material (that does not make it into the river system), or that there is total bypass and poor mixing (i.e. cosmogenic nuclide-derived rates are local and not representative of catchments as a whole). This is not crucial to the paper, but it is an interesting topic.

We agree that the comparison of landslide and cosmogenic erosion rates is interesting, and the reviewer makes an insightful argument about some of the potential implications. Nonetheless, our paper is not focused on determining or interpreting cosmogenic erosion rates, so we view more detailed comparative analysis along these lines as beyond our present scope. We have modified the text to acknowledge the possibility for further work on this topic, particularly noting the potential for comparing the two sets of data collected from exactly the same catchment (which is not possible with current information). Our revised text reads as follows:

"The difference between the landslide-associated erosion rates measured in Bolivia (Blodgett and Isacks, 2007) and the catchment-averaged denudation rates typical of this region has not been widely considered, **and a more systematic comparison including data paired from identical catchments could offer fruitful avenues for further investigation. For the purposes of this study,** the observation of relatively high landslide rates suggests at the least that landslides are the primary mechanism of hillslope mass removal, as they are in other active mountain belts (Hovius et al., 2000; Hovius et al., 1997)."

Pg. 647 – middle: If the landslide inventory also includes depositional areas, then this is not a conservative estimate.

As noted above, areas associated with landslide deposits are also cleared of vegetation as a result of landslide activity, and we think this biomass should be included in the overall landscape-wide calculation of the amount of carbon stripped from hillslopes by landslides. We have added a note to clarify that the fate of carbon associated with landslide scars vs. deposits may differ, but that such differences are not well known (all new text):

“Calculated fluxes include carbon that was residing both on landslide scars and in areas of runout and deposit. The fate of carbon from each of these areas may differ, but such differences are not well known and we consider all to contribute to the loss of previously living biomass as a result of landslide occurrence.”

Pg. 647 – bottom: Why New Zealand? Give some justification for this comparison.

We have provided a comparison to data from Guatemala and New Zealand because these are the only studies to estimate landslide-associated carbon fluxes from montane systems over annual to decadal timescales, at least as far as we are aware. We have reworded the text to clarify that we are trying to make a general comparison:

“The area-normalized landslide carbon yield in the Kosñipata Valley is similar to the upper end of landslide carbon yields **for other mountain sites around the world where landslide carbon fluxes have been evaluated.**”

(Note also that we have moved this comparison to the Discussion, consolidating what was previously repetitive text).

Pg. 649 – middle: You might want to distinguish the ‘work done’ by landslides (which is removal of material here) from the geomorphic work done by landslides in the topographic sense (steep lower slopes).

The reviewer raises a good point. We have changed the text in section 5.1 to read:

“Here we define geomorphic work, *sensu* Wolman and Miller (1960), **as total landslide area, reflecting the removal of material from hillslopes (rather than, for example, the work done by landslides to modify slope angles).**”

Pg. 650 – lines 8-12: does not make sense.

We thank the reviewer for bringing this typo to our attention; we have changed “at” to “to”, as follows: “The notable shift from low **to** high landslide susceptibility above 30-40° (Fig. 6b) is consistent with the hillslope angles that reflect rock strength expected for the metamorphic and plutonic bedrock (Larsen and Montgomery, 2012).”

Pg 653 – top: this is too speculative. You could discuss the potential controls on topography, but should avoid discussions of erosion.

We have removed “erosion” from this sentence.

Detailed response to interactive comment from reviewer #2 (K. Ferrier)

We thank the reviewer, Ken Ferrier, for a thorough and positive review. We have modified several aspects of the manuscript to cover the issues he raised.

General comments:

The central aim of this manuscript is to measure landslide-derived organic carbon fluxes out of the Kosñipata catchment in the eastern Andes. The authors computed these fluxes from the size of landslides, which they mapped using annual satellite images over the period 1988-2012, and the abundance of organic carbon in the material eroded by the landslides, which they estimated from new measurements of organic carbon stocks in soils and vegetation. These measurements took considerable effort: The authors analyzed a large number of satellite images and dug a large number of soil pits to estimate organic carbon stocks and fluxes. Their analysis implies that landslides in the Kosñipata basin have been responsible for large fluxes of organic carbon out of the catchment, with roughly three quarters of the flux coming from soil carbon and the rest from vegetation. These measurements are likely to be of interest because organic carbon fluxes from continents to the oceans are an important link in Earth's carbon cycle, and because the extent to which these fluxes are influenced by large erosional events is not well known. These new measurements are the manuscript's greatest strength.

We are pleased that the reviewer enjoyed the paper and appreciate his succinct summary of our analysis.

I have a few suggestions for strengthening the manuscript. Most importantly, I suggest adding an explanation for how the landslide-derived fluxes were calculated.

We have considerably revised Section 3.3 of the Methods, splitting this into three subsections. The first subsection comprises two new paragraphs that set out the methodological approach for determined landslide-derived carbon fluxes and put this approach within the context of prior related work (this text is not repeated in full here, for brevity, but is included in the manuscript showing tracked changes).

We have also revised the text of what is now Section 3.3.3, to help clarify how landslide-associated fluxes were calculated. We have also taken the opportunity to make a minor but important revision to these calculations. We now only quantify the above ground biomass, soil and below ground biomass, which dominate the carbon stock and show variability with elevation which can be described by a linear model with quantified uncertainties (see revised Fig. 7). This allows us to propagate the uncertainty on the elevation vs carbon stock models through the calculations which use landslide area to quantify carbon flux and carbon yield (e.g. see Table 2). We do not include the epiphytes and wood debris in this calculation because they make up a minor component of the carbon stock (<~10%) and their carbon stocks are not clearly linked to elevation. We instead provide an estimate for how much epiphytes and woody debris are likely to contribute in Section 4.4.

These revisions do not impact any of the conclusions of our study, and so do not impact the revised version in a major way. However, they do help strengthen our findings by providing a robust estimate of uncertainty to the carbon yields, which we include throughout the main text.

At present, the organic carbon fluxes are reported in section 4.4 without an explanation *for how they were calculated. An important aspect of this is landslide thickness: How did the authors estimate the thickness of eroded material in each of the landslides? Landslide thickness is required to estimate the mass of soil eroded in each landslide, which in turn is required to compute the organic carbon flux as the mass of the eroded soil multiplied by the organic carbon concentration in the soil.*

The reviewer raises an important point about the relevance of landslide thickness to the calculated carbon fluxes. We mentioned briefly in our original Discussions paper that we assume all landslides strip the full soil depth, but we agree that this aspect could have been better developed. Previously we justified this assumption on the basis of field observations that most Kosñipata landslides clear soil to bedrock. We have now added an analysis based in geometric scaling relationships for landslides. This analysis indicates that the vast majority of landslides in our inventory are deeper than the deepest observed soils (covering >98% of landslide area, and maybe more), supporting our inferences from field observations.

We have added a new paragraph to the end of Section 3.3.3 discussing the importance of landslide thickness, as follows (all new text):

"In our calculation, landslides are assumed to strip all above ground and root biomass from hillslopes, based on field observations from the Kosñipata Valley that landslides are cleared of visible vegetation and roots. We also assumed landslides completely remove soil material to full depth, again consistent with field observations that landslides in the Kosñipata catchment are typically bedrock failures that remove the entire mobile soil layer. To test this latter assumption, we used geometric scaling relationships for landslides in mountainous terrain (Larsen et al., 2010) to estimate landslide depths. We calculated landslide volume from the area (A)-volume (V) relationship, $V = \alpha A^{\gamma}$ where α and γ are scaling parameters (we used $\alpha = 0.146$ and $\gamma = 1.332$, from the compilation of global landslides in Larsen et al., 2010, but also tested other literature values). We estimated depth by dividing volume for each landslide by the respective landslide area."

And in Section 4.4, we now describe the results of this additional analysis:

"On the other hand, our values may overestimate fluxes from soil OC if landslides are shallower than soil depths, since we have assumed complete stripping of soil material to full soil depth and since soil OC stocks depend on depth of integration (see Section 3.3, above). Using the average scaling parameters for global landslides (Larsen et al., 2010), the minimum landslide depth in our inventory would be 0.74 m. Average soil depths at most plots were deeper, with the deepest being 1.58 m. However, for the same scaling parameters, only 99 landslides in our inventory, equating to 0.06 km^2 total landslide area (or ~2% of total landslide area), would be shallower than deepest soils at 1.58 m. Using scaling parameters for bedrock landslides only ($\alpha = 0.146$ and $\gamma = 1.332$; Larsen et al., 2010), yields only one landslide shallower than 1.58 m. This analysis corroborates our field observations that most landslides in the Kosñipata Valley clear soil and expose bedrock. We thus view our calculation of fluxes on the basis of complete stripping of soil as providing a reasonable estimate."

Additional text that describes this would also provide a basis for reporting uncertainties on the organic carbon stocks and fluxes, which the manuscript currently lacks and would benefit from.

We have now included an uncertainty analysis, propagating errors on the elevation trends in OC stocks through to the calculation of landslide-associated carbon yields and fluxes (see reply to earlier comment). These uncertainties are reported in the revised section 4.4 and in table 2. As noted above, we also now report in Section 4.4 a lower bound on carbon fluxes based on assuming shallow landslide depths.

In addition, I suggest adding a section that describes the connectivity of the mapped landslides to the channel network. What fraction of the landslide-mobilized material made it to the channel network? This connectivity can vary substantially among regions. The authors briefly mention this issue in other studies on p. 656, but the manuscript does not explain how they estimated what fraction of the landslide material actually reached the channel network.

We thank the reviewer for this suggestion. In section 5.4 of the revised text, we now discuss landslide-river connectivity in the study area, as follows (all new text):

"The extent to which landslides connect to river channels exerts a first-order control on the fate of landslide material (Dadson et al., 2004), and thus on the fate of carbon. We identified landslides as connected or unconnected to rivers by manually inspecting high-resolution imagery and following landslides to their termination (i.e. to their lowest elevation point). Connected landslides terminated in river channels, identifiable by the absence of vegetation. We found that, for the Kosñipata Valley during our study period, greater than 90% of landslides were directly connected with rivers, similar to the high connectivity found for other storm-triggered landslides (e.g., West et al., 2011). However, even with high connectivity, it remains uncertain in the case of the Kosñipata how much of the material stripped by landslides is actually removed by rivers and exported out of the valley."

Lastly, it would be useful to provide more descriptions of the soil pits, perhaps in the supplementary material. Specifically, it would be useful to see maps that show where the soil pits were dug, and profiles on organic carbon concentrations in each pit. These would put the calculations of organic carbon fluxes in context, by showing how representative the soil pits are likely to be of the catchment as a whole.

Reviewer #1 raised a similar point. In response, we have included a table of the soil plot data used in this study (Table S3), providing the location of the plots and associated carbon stocks. This reviewer also suggested including maps of soil pit locations and profiles for each pit. Although we agree that such further analysis would be interesting, we view it as outside the scope of this already lengthy manuscript, and as likely target for future manuscripts from co-authors, considering in more detail the patterns of variability in soil OC within and across the plots.

In summary, this manuscript presents some new estimates of landslide-derived organic carbon fluxes based on an extensive series of new measurements. I believe that the manuscript would be strengthened by considering the issues I listed above, which I believe the authors could address in a moderately revised version of the manuscript. Below I list more a few more suggestions for improving the manuscript.

We hope the reviewer agrees that our changes have addressed his comments and strengthened the manuscript in the process.

***Specific comments**:*

p. 633, lines 10-11: This states that "landslides may completely turnover hillslopes every ~1320 years". This is strictly true only if landslides occur in every part of the catchment (do they?), and if landslides do not recur in the same place until the entire catchment has been resurfaced. By Figure 2 it looks as if some portions of the catchment did not experience any landslides during the observation period. I'd suggest rephrasing this slightly to reflect that.

We have rephrased the abstract to address this important point:

"Catchment-wide landslide rates were high, at 0.076% yr-1 by area. As a result, landslides **on average** completely turn over hillslopes every ~1320 years**, although our data suggest that patterns of landslide occurrence varies spatially, such that turnover times are likely to be non-uniform**. In total, landslides strip 26**±4** tC km-2 yr-1 of soil (80%) and vegetation (20%)."

p. 636, line 11: What does spp stand for in the units for species richness? I suggest defining that here.

spp stands for species, a term we use in full in the revision.

p. 641, lines 23-25: It would be useful to specify in the text not only that the carbon stocks estimated in the present study differ from those in previous studies, but also to state that these estimates are bigger, and to quantify how much bigger.

In response also to comments from Reviewer #1, we have re-written this section and now include a quantitative comparison, now stating that, "Our soil C stock values are a factor of 1.2 to 1.7 higher than values reported in these previous studies (Girardin et al., 2014a; Zimmermann et al., 2009)."

p. 644, line 8: It would be appropriate to cite some older papers here, especially Zhang and Montgomery, 1994, Water Resources Research, v. 30, p. 1019-1028.

We thank the reviewer for suggesting this additional reference and have added it to the text in section 3.5.

p. 647, lines 2-3: Is this a lot of carbon or a little? I suggest providing context for these numbers here by comparing them to carbon stocks in other places. Also: What are the uncertainties on these values? That would be a valuable quantity to report for the carbon stocks (and for other quantities too), because it'll aid comparisons to future studies.

We now include the follow sentence which qualifies how these soil and vegetation stocks compare broadly with other ecosystems:

"Overall, **the vegetation carbon stock values from the Kosñipata Valley are slightly lower than lowland tropical forests, and the soil values higher (Dixon et al., 1994), which is** consistent with broad trends in the tropics in which soil carbon stocks increase with elevation and are frequently greater than vegetation carbon stocks (Gibbon et al., 2010; Raich et al., 2006)."

We have also incorporated an uncertainty analysis through error propagation and it is included in the revised manuscript in section 4.4. We have addressed his comments on error in the general comments section.

p. 647, lines 7-11: These fluxes are likely to be conservative because they implicitly assume that landslides are the only means of conveying organic carbon to the channels. How much carbon is eroded to the channel network by other processes (e.g., soil creep)?

Total erosion rates are not yet known for our study site, so we are not able to quantify rates of soil creep and other erosional processes. However, we agree that these may also be relevant mechanisms of transporting carbon and have now added a sentence to the end of Section 4.4 that brings this additional process to readers' attention (all new text):

"When considering carbon budgets at the landscape-scale, the landslide-associated carbon fluxes we report here should also be viewed in the context that other processes such as soil creep may additionally contribute to the transfer of carbon from hillslopes to rivers (e.g., Yoo et al., 2005)."

p. 648, lines 13-14: I was a little confused by the wording in this sentence. Instead of writing "where RI_i is the return time for a year characterized by the landslide magnitude of year i", I suggest replacing it with something like, "where RI_i is the return interval for the ith largest landslide in the record."

We thank the reviewer for pointing out this confusing explanation in our Discussion paper. We have modified the text in section 5.1 of the revised manuscript, although we were concerned that the reviewer's specific suggested wording could be misleading. Instead, we now state: "where RI_i is the return **interval for the year with the i^{th} largest total annual landslide area**."

p. 650, line 11: Angle of repose pertains strictly to granular material, so I'd suggest replacing "angle of repose" with something like "hillslope angles consistent with the strength of the local bedrock."

The reviewer makes a good point. We have modified the text in section 5.2.1 as suggested by the reviewer, though we preferred wording as follows: "The notable shift from low to high landslide susceptibility above 30-40° (Fig. 6b) **is consistent with the hillslope angles that reflect rock strength expected for** the metamorphic and plutonic bedrock (Larsen and Montgomery, 2012)."

p. 661, line 10: For the editors: the doi link appears to be broken.

We thank the reviewer for bringing this to our attention and will double-check the doi links in the final publication PDF.

p. 686, Figure 11: This is a busy figure, and the individual panels are quite small, which makes them difficult to read. Could this be split into multiple figures? Also: What is shown in panel c? The caption's only description of panel c is a reference to Figure 10, but it would be more helpful to state what it is directly here.

We apologise that the individual panels of figure 11 were difficult to read in the Discussion paper. We intend for this figure to be printed in a horizontal layout for final publication, which we hope will address this issue. Although we agree it remains a busy figure, the correspondence between the location of the fluvial transitions in (a-c) and the geology, precipitation, and cloud frequency is directly related to the interpretation, and we therefore prefer not to divide the panels into separate figures.

We have revised the caption to clarify the description of panel c, as follows: "Figure 11: (a-c) Analysis of river profiles analogous to those in Fig. 10 (shown here as River #3, in cyan), for rivers throughout the Alto Madre de Dios region (d). In (b), data are binned by upstream area and means are shown by black circles. Arrows in (a) refer to locations along the profile of observed transition in the area-slope plots (b). **In (c), hillslope angles (from STRM DEM) are separated between regions upstream (blue) and downstream (red) of the transitions.** Transition locations are displayed as red dots in (d-g), which show regional elevation (Farr et al., 2007) (d), geology (INGEMMET, 2013) (e), TRMM 2B31 annual precipitation (Bookhagen, 2013) (f), and Modis cloud freqency (Halladay et al., 2012) (g)."