Responses to Anonymous Referee #2 for manuscript (esurf-2018-51) submission to Earth Surface Dynamics:

Measuring Decadal Vertical Land-level Changes from SRTM-C (2000) and TanDEM-X (~2015) in the South-Central Andes

We appreciate the review and the improvements suggested by close reading of the manuscript. Highlighted in **bold are the reviewer comments** followed by our reply. All changes will be made to the final manuscript submission following completion of the interactive review period.

Response to Anonymous Referee #2

My primary expertise with respect to this manuscirpt lies in the technique, and subsequent interpretation, of geomorphic change detection. At the outset, I would therefore like to emphasise that my review focuses upon the overall form of the manuscript and the technical component of the DEMs of Difference analysis. I do not have the technical expertise to scrutinise the detail of the remote sensing data processing; other reviewers should be sought for this elements. Overall, this manuscript presents an interesting and novel demonstration of how spaceborne radar DEMs can be used to detect vertical change in the Earth's surface. However, in my opinion, for this journal the manuscript needs to be reorganised to present a clearer research question/aim at the outset that is focused upon the geomorphological problem that is being investigated. There are also elements of the context, methods and results that are not organised in a classical research paper order. For the material that is presented, I do not see a reason why the context, methods and results can't be split into separate sections. I elaborate on these two items below, in addition to identifying further major and minor points.

We acknowledge the reviewers' statement regarding their expertise and nonetheless appreciate a very thorough and helpful review of the manuscript regarding the geomorphic questions. We do, however, disagree that the paper needs significant reorganization to frame the study around a geomorphic question, since we intended the work as a primarily methods paper. As mentioned in the response to Referee #1 our initial motivation for the study was to establish a full-catchment vertical land-level dataset. However, during processing we realized the constraints and considered it more useful to elaborate on the methodological processing steps than a sediment budget. Thus the focus has been shifted from the specific geomorphic question to a clear (we think) description (including some basic code) that allows other users to perform similar analysis in other terrain. We chose to submit the study to Earth Surface Dynamics given a focus on methods (e.g., Grieve et al., 2016a; Dietze, 2018) and data quality (e.g., Grieve et al., 2016b; Purinton and Bookhagen, 2017) in this journal. Therefore, we do not feel that any restructuring or major reframing is in order, but we have tried to accommodate the below suggestions.

Major comments

1) A clear geomorphic research problem needs to be identified at the outset and backed up with appropriate context. P2L29 describes what will be included in the paper but there is a need for a more explicit geomorphic aim and associated set of objectives. The data processing methodology to generate a DEM of Difference is novel and far more could be made to contextualise this in the literature review. For example, by critically analysing a greater diversity of previous work on DEMs of Difference (P1L25) a stronger case could be made for the need to scale-up the typically small-scale topographic surveys that are acquired using terrestrial / airborne geomatics techniques to generate DEMs.

We clarify the manuscript contents at P2L29 to read:

"In this submission we discuss the errors associated with each of these datasets and the corrections applied to mitigate uncertainties in their differencing for *dh* detection outside of the cryosphere. This is primarily a data quality and methods focused study. Geomorphic change detection is applied via correction and differencing of the TanDEM-X and SRTM-C over the south-central Andes in northwestern Argentina (Fig. 1) to identify and measure areas of *dh* in gravel-bed channels specifically and then across the landscape."

Regarding the literature review of *dh* studies, we feel that the paragraph on P2L3-15 clearly contextualizes the research with regards to previous studies of geomorphic change detection relying on small-scale or sparse datasets.

2) Context, methods and results need to be appropriate separated. For example, P2L20-28 is primarily methodological detail but in the introduction section. Much of the material on P4 is context for the research question (introductory material). Some of the material in section 3 is discussing methods or presenting results but this section comes before section 4 (methods).

As noted in the response to Referee #1, we have removed Section 3 and moved this to the beginning of Section 4. It is common in remote sensing studies to briefly introduce the datasets used early on, hence the brief description of the TanDEM-X and SRTM-C at P2L16-28, which follows well the contextualizing paragraph mentioned in the last reply at P2L3-15. By setting this up early on, we avoid any confusion about the datasets we are referring to, since a number of TanDEM-X and SRTM-C products exist and are often called the same thing despite different processing. Section 2 on P4 is important for contextualizing the correction technique and dataset details. We feel that the study is laid out well as is and clearly indicates why we are using the specific versions of SRTM-C and TanDEM-X data we mention in the introduction.

3) The description of how "trunk channels" (P8L22) were digitised is confusing. Within the braided rivers literature, the term "trunk channels" is not widely used. Do you mean primary anabranch or the active width (i.e. Peter Ashmore's term)? This explanation (section 4.2.1) of the methods used to detect channel change needs to be improved (see also comments listed below). Fundamentally, it is not clear why a Level of Detection (LoD) approach for DEM differencing, rather than the now more widely used approach of probabilistic thresholding (see article by Wheaton 2010 that is cited in the manuscript). At the very least a clear justification of why a LoD approach was applied is needed. However, a stronger analysis could be presented if the DEMs of Difference were regenerated using a probabilistic approach.

We change "trunk channel" on P8L22 to "active width of the primary channel branch (herein, trunk channel)". We apply statistical cutoffs and a final LoD given the coarser (30 m) nature of our data, as opposed to the fine (meter to sub-meter) lidar and SfM-MVS data used in other gravel-bed river studies. We refer to this as a hybrid of the two techniques. The following is added at P8L18 to clarify the above concerns:

"Previous change mapping over gravel-bed channels has relied on level of detection cutoffs and probabilistic thresholding (e.g., Lane et al., 2003; Wheaton et al., 2010). These studies have, however, been developed for meter to sub-meter photogrammetric or lidar data. Here we use a hybrid approach of statistical outlier detection on the entire distribution of pixels followed by a level of detection cutoff for remaining pixels well within the bounds for expected noise between the datasets. Remaining uncertainties are primarily caused by speckle noise and terrain characteristics, with the biggest impact from slope."

4) A stronger geomorphic interpretation of the results (e.g. P19L1) could be achieved if there was a clearer geomorphic hypothesis to underpin the research at the outset. P19L21 mentions "field work" undertaken over the last decade. Is there supplementary field data that could be used to evaluate the remote sensing results from a more quantitative perspective?

As noted, this is chiefly a data quality and methods study, and we do not wish to over-emphasize the geomorphic implications. Rather we intend to point out some applications of the method and interesting results that can be attained. We do not have quantitative field data for the entire catchment or over the entire time-span (back to February 2000 when the SRTM-C was collected), which could aid in this analysis, and the statement is merely intended to emphasize that field observations (in addition to GoogleEarth historical imagery), support the growth of these gravel piles.

5) The conclusion argues that "previous" measurements are constrained by high signal to noise ratios to detect vertical change. However, the noise magnitude reported from the satellite radar approach is significant. In my opinion contemporary approaches to DEM differencing are all challenged by difficulties separating geomorphic signals from noise when the vertical magnitude of change is relatively small compared to the elevation variations typically associated with particular geomorphic units that are under investigation. The

conclusion would also benefit from a clearer summary of the actual method presented; the statement on P21L19 require more context within this section.

Previous measurements refer mostly to those using ASTER DEMs, which had much higher noise compared with TanDEM-X. In fact, there is very little noise in the TanDEM-X data, and it is emphasized in the study that the remaining signal to noise ratio in the measurements is primarily diluted by the lower quality SRTM-C data (see also our earlier publication: Purinton and Bookhagen, 2017). We modify P21L3-6 to read:

"Previous measurement of land-level changes at the scale of entire mountain belts has been restricted to the cryosphere, where the signal of snow and ice change outweighs the noise associated with DEMs used for differencing (typically ASTER or single TerraSAR-X / TanDEM-X CoSSC DEMs). On the other hand, studies outside of the cryosphere have relied on high-accuracy meter to sub-meter data at much smaller scales to measure height changes in rivers and hillslopes."

Furthermore, we add the following sentence to the end of the first paragraph of the conclusion:

"Noise from imperfect datasets continues to hinder signal detection in low magnitude geomorphic change detection, however, this study continues to push the envelope of the potential for change mapping using the data currently available to many scientists."

We feel that aside from these clarifications the conclusion is appropriately concise, without providing a detailed method summary aside from mentioning the correction of SRTM-C orbital biases followed by differencing and statistical avoidance of noise.

Additionally, we would like to point out that published DEM uncertainty values are typically an average over many land-cover types and terrain characteristics (slope, curvature, aspect). These values are usually not representative of individual situations, and we've tried to do this justice by separating change detection and analysis between steep and vegetated (hillslopes) and flat and vegetation-free (gravel-bed channels).

Minor comments

P1L3. The first sentence is focused on the cryosphere yet the paper is primarily focused with changes in terrain (rock / sediment). A more appropriate initial sentence is required.

We disagree and wish to contextualize the method in terms of the cryosphere, where most of the remote sensing change detection is carried out. Essentially we are presenting a snow and ice technique to the rock and sediment community.

P1L25. A greater diversity of refs is required for the rivers and earthquake examples.

The earthquake example is unique, but we have added to the rivers the citation for Lane et al. (2003), Wheaton et al. (2010), and Cook (2017).

P8L22. I think "hand picked" should say "digitised"

Changed.

P8L29. Was there no vegetation at all? This is context dependent for gravel-bed rivers.

The channels were vegetation free aside from a few sparse bushes of about 1-2 m height occurring in the furthest upstream reaches. This is a semi-arid environment and the climatic and hydrologic regime has been characterized in previous publications (Castino et al., 2017; Bookhagen and Strecker, 2012).

P8L24. "Error factors" need to be explained.

These are elucidated as the binning values on P8L31-P9L3, but a change is made to clarify this line earlier:

Before: Change mapping was done by separating the in-channel dh values into bins of contributing error factors and applying 5th and 95th percentile cutoffs to each bin, thus only taking the top (positive=aggradation) and bottom (negative=incision) 5% of outliers.

After: Change mapping was done by separating the in-channel dh values into bins of contributing error factors (local relief and TanDEM-X individual scene consistency) and applying 5th and 95th percentile cutoffs to each bin, thus only taking the top (positive=aggradation) and bottom (negative=incision) 5% of outliers.

P19L23. A comment is required about the 0.2m/yr average rate to state that this assumes geomorphic work is constant each year.

Added the sentence:

"This rate represents an average for the entire measurement period and assumes constant geomorphic change, whereas the true rates are more stochastic, following rainfall and anthropogenic activity variation."

P20L27. A clearer explanation of how field / auxiliary data could be used is needed.

Before: In either case, field knowledge or auxiliary data (even in the form of GoogleEarth™) is necessary for accurate assessment of true change signals versus noise.

After: In either case, field data (e.g., repeat total station or GPS surveys), field knowledge (e.g., via observations of incising reaches or roads damaged by aggrading channels), and/or auxiliary data (e.g., GoogleEarth™ historical imagery change mapping) are necessary for accurate assessment of the location of true change signals versus noise.

Sincerely,

For both authors,

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