Earth **Surface**
Dynamics

Discussions

## Interactive comment on "Measuring Decadal Vertical Land-level Changes from SRTM-C (2000) and TanDEM-X (∼ 2015) in the South-Central Andes" *by* Benjamin Purinton and Bodo Bookhagen

**Anonymous Referee #2**

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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. How-

ever, 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.

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.

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).

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 De-

tection (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.

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?

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.

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.

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

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

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

P8L24. "Error factors" need to be explained.

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

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

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