Response to Adam Forte (Referee 1)

We thank Adam Forte for his detailed and constructive review. We are happy to take up all of his suggestions to improve our manuscript. The review does not question our methods, results or interpretation. We are confident that we can revise the manuscript within a short period.

I have completed my review of 'The destiny of orogen-parallel streams in the Eastern Alps: Salzach-Enns drainage system' by Trost et al. In this contribution the authors consider the stability of drainage divides in the Eastern Alps with a variety of metrics that have been recently proposed/codified and then consider the results of this with respect to the tectonic history of the Alps and expected future evolution of the drainage network. In doing so, they present an interesting new take on how to use some of these metrics and point out some important considerations for the applicability of these metrics (especially for the Gilbert metrics) especially in places with recent histories of glacial modification. I don't have a ton of comments and most of them are largely editorial (i.e. wording and such). I have one semi major point toward the end of the paper (which I think shouldn't be too hard to deal with and I hope will help to strengthen the applicability of what they discuss beyond this particular use case). Ultimately, I think this paper will make a nice contribution to Earth Surface Dynamics. **Reply:** *We are very pleased that you find our work interesting and important. It is very nice to hear that you consider our approach as a nice contribution to Earth Surface Dynamics. In the following, we will address all suggestions line by line:*

L37 – I think you can remove 'abundantly' here. **DONE**: *We will remove 'abundantly'.*

L46 – Think 'conditions' not 'conditioning' might make more sense. **DONE:** *We will change the wording in the revised version of the manuscript as suggested.*

L75 – Add direction that material was extruded to help those without a lot of familiarity with the geography of the region. **DONE:** *As suggested, we will add "to the east" in the revised version of the manuscript.*

L80-83 – Might be helpful to specifically mark the location of some of the features on a figure, maybe figure 1? Or as an inset? **DONE:** *We agree that it might be useful to annotate those features. We therefore will add some more annotations to Figure 1, especially emphasizing the knee-shaped bends and T-shaped river junctions of Salzach and Enns drainage systems.*

L203 – Good to see consideration of the choice of base level, but could you maybe elaborate on specific rationales as to those choices? I.e. is there anything special about those, e.g. is 400 m approximately the elevation of the foreland as rivers exit the mountains? Something else? **DONE:** *The lower base level is set to 400 m, representing the outflow of the main rivers to the foreland. We will add this note to the method section. The increase of base level up to 1000 m follows equal steps in order to narrow down the spatial impact of tectonics and climate. We will also add a short paragraph to the method section.*

L304-306 – Might expand this to include references that address the extent to which spatial/temporal statistics of rainfall translate into spatial/temporal statistics of runoff which is related to the question of changes in channel geometry that you highlight here. **DONE:** *Maybe it was not a good idea to start the discussion about catchment sizes and discharges here. In a revised version, we will point out more clearly that neither climatic data at geological timescales nor the theoretical concepts (would the stream power law in terms of maximum discharge look the same?) allow for taking into account climate seriously on this level.*

L309-310 – Which was a primary conclusion of Forte & Whipple, 2018. **DONE:** *Thank you for pointing to that. We did not intend to claim the conclusion – We will add the reference.*

L331-333 – This is an interesting approach (i.e. advocating for setting the base level just down stream of the channel heads for the χ calculation to isolate what is 'being felt' by the near divide portion of the channels). What comes to mind however is wondering if that is significantly different than (1) looking at the channel steepness of the area near the headwaters (with a 'base level' a little downstream of the channel head, a χ anomaly across a divide will mainly be a reflection of a difference in channel steepness directly downstream of the channel head, I think) or (2) the Gilbert metrics at a larger accumulating area (i.e. with this high base level χ value you're kind of taking the same approach as the Gilbert metrics to focus on what's happening near the divide, but in this case you're considering an area slightly bigger than what you were with Gilbert metrics because of the choice of threshold area for defining channel heads). This is not to imply that there is anything wrong with your approach (I rather like it!), just that I think to make this a more complete contribution, it would be good to consider if these other two would be equivalent or not (I'm definitely not sure they would give you the same answers, but my initial guess would be yes). **Reply:** *Thank you for this encouraging comment! You are absolutely right that all these approaches are similar in their spirit. It is all about some kind of slope and about how to measure it in such a way that it is still related to the erosion rate (and not limited by a critical slope) and not too strongly affected by noise of the DEM (the problem of the rivers with rather low channel slopes). We even did some experiments with your idea (1) some time ago. There we started at 1 km² catchment area and moved downward by a given increase in χ and measured the elevation vs. χ slope over this range. Theoretically, maybe even the best way to measure the headwater steepness index, but we found that the results had strong variation along drainage divides, so that across divide variations were hard to interpret - we did not publish it so far. The other idea, (2), would probably still have much weight on the upper part of the hillslopes as the relief of the river profile is much lower. We would therefore guess that we do not get rid of the problem that the relief of the uppermost part of the hillslopes might not be a good proxy for the erosion rate. So we would say that the race for the best topographic proxy of erosion rate is still open.*

L434-435 – It might be better to couch this in terms of 'glacially modified' mountain ranges instead of mid-latitude as (1) while certainly latitude is going to play a big role, moisture availability and detailed local climate will also control the extent of glacial activity and (2) your observations would generally be valid anywhere glacial modification of the landscape has been significant. If you choose to make this change, I would suggest similarly changing it elsewhere in the manuscript. **DONE:** *We agree that the main issue we tried to cover with "mid-latitude mountain ranges' is the glacial overprint. As you have already mentioned, the terming "glacially modified" includes the applicability of the approach to other mountain ranges. We therefore agree on changing "mid-latitude" to "glacially modified" throughout the manuscript.*

Figure 2 – This doesn't really matter and is just a point of clarification, but the χ values displayed on this map seem high if a reference area of 1 was used as is implied in the text. It seems more like a value of 1e6 was probably used? Doesn't change anything, but could be a point of confusion for some (if trying to replicate what you've done). **DONE:** *Thank you for pointing to that. Unfortunately, there was a typo in the methods section. We used for our calculations, as already assumed, a reference drainage area of 1 km². We will change the value of 1 m² to the correct value of 1 km² in the revised version of the manuscript.*