**Response to Paul Eizenhöfer (Referee 2)**

*We thank Paul Eizenhöfer for thoroughly reviewing our manuscript and his constructive comments. Almost all raised issues are minor and we can easily implement his suggestions in a revised version. Some of the issues, including two points that – according to the reviewer – require more attention, originated from the fact that we have not expressed ourselves clearly enough. We are very confident that we can solve the raised issues with clearer formulations and some additional explanations in a revised version of the manuscript.*

The study by Trost et al. is centred on the characterisation of drainage divides along the Salzach-Enns drainage systems in the Eastern Alps, and how the geomorphology along these systems reflect the past Alpine tectonic and climatic evolution since Early Miocene. In their approach the authors employ state-of-the art geomorphic metrics, i.e. $\chi$ stream profile analysis, swath profiling along the divides and Gilbert metrics in order to determine the potential mobility of drainage divides. Their main conclusion describes a general eastward migrating trend for the drainage divides in the Eastern Alps mainly as a result of Mid-Miocene extrusion tectonics. In this sense $\chi$ stream profile analysis indicates a rather long-term trend whereas the Gilbert metrics are partially influenced by the last glacial maximum. The geomorphic analysis is very detailed and thorough appearing overall robust to me. In this review I found rather few minor issues regarding the general approach. The study is in line with recent studies on Alpine geomorphology, e.g. Robl et al. (2017) and Winterberg and Willett (2019). Thus, its conclusions are not surprising from a fundamental note but add an important link between long-term and short-term drainage reorganisation by comparing divide mobility through $\chi$ analysis and Gilbert metrics, respectively. I am convinced the manuscript will find wider interest in the Alpine geomorphology community as well as add new constraints on effects of the eastern Alpine tectonic and climatic evolution on major rivers in the region. I am listing below some mostly minor issues and suggestions that may aid in improving the current version of the manuscript. However, this list contains also points on two topics that may require some more attention. These are concerned with general clarifications regarding processes at geologic time scales and more detailed background on the nature of Figure 7. **Reply:** *We are pleased that you think our manuscript will add new constraints on the Eastern Alpine tectonic and climatic evolution. We appreciate that you find our method and results robust and detailed. In the following, we address the comments to mostly minor shortcomings of the manuscript line by line.*

Line 14 – Perhaps also provide a rough idea on the relative time scales at which catchment geometries, headwater and hillslopes are operating. **DONE:** *Yes, but this would indeed be only a very rough idea. Thank you for this interesting but definitely not trivial comment. For large catchments, it is clear that propagation of disturbances takes millions of years, but for hillslopes it depends on the considered model which is less clear than for fluvial incision. So something like "covering time scales from millions of years to the millennial scale" would be possible. We will implement such a statement in our revised version.*

Line 25 – This reconstruction of the drainage network appears very prominently here in the abstract and I kept wondering throughout the manuscript how the respective Figure 7 has been produced (including the 'palaeo-stream profile analysis'). Maybe this should be improved. Further below a more detailed comment on this. **DONE:** *In the abstract, we could indeed state more clearly that these reconstructions are based on hypotheses on the location of drainage divides. We will implement such a statement in our revised version and explain our approach in some detail later in the manuscript.*

Line 30 – I think the phrasing might be somewhat misleading since 'link' also implies that the drainage system would also somehow influence climate and tectonics (I assume this was not meant this way here). In case it is meant as such I would regard this issue as rather controversial. See discussions on this in Willett et al. (2006) or Schlunegger et al. (2007). **DONE:** *We could change the word order to clarify this point, although we think the former formulation did not imply that the drainage system controls climate or tectonics.*

Lines 33-36 – Perhaps add works by Miller et al. (2007) and Willet et al. (2001) since horizontal advection is being mentioned here? Also, from a structural geologist's point of view I find the description on mountain formation and orogeny a bit too simplified. There is nothing itself wrong with a simplification. However, fault activity does play an important role in this study. Thus, I would perhaps include a more solid tectonic background here considering basic principles of structural geology. **Reply:** *Adding the two references about horizontal advection makes, of course, sense. However, we do not agree that fault activity plays such an important part in this study. As mentioned in the manuscript, it is definitely important for the large-scale drainage pattern, but this pattern itself is not a subject here. Our analysis of the drainage pattern only refers to drainage divides in the large-scale pattern. Of course, we could point out that our perhaps oversimplified explanation refers to the large scales only, while fault activity becomes more and more important at smaller scales. In general, we think that the simple orogen-scale view is a good tradeoff between an explanation for readers without a background in tectonics and keeps the focus of the study.*

Lines 40-42 – A recent study by Eizenhöfer et al. (2019) discusses the impact of horizontal advection on drainage systems in convergent orogens, which might be of some general interest in this field. **DONE:** *Thank you for pointing to this very recent work, which fits well to this section. We read the study with interest and will add the respective reference. However, during the lecture of your manuscript, we felt some potential for misunderstanding. To clarify our point: With the term 'horizontal advection', we do not refer to any ramp structures (and any possible gradients in uplift rate), but rather mean the process of lateral extrusion. As we do not go into details of the tectonic evolution of the Eastern Alps, but rather look at drainage geometries, we do also not specifically include features like the Sub-Tauern Ramp.*

Lines 48-49 – It is certainly correct that previous disturbances in a drainage system are erased after a knickpoint has traversed through it, but I would add that this also involves a time component needed for a knickpoint to reach the headwaters, which is not trivial, especially in glacially influenced parts of the Alps. **DONE:** *We agree that this is indeed not trivial if $n > 1$ in the stream power law. For $n = 1$, the horizontal velocity of knickpoints is always constant in the $\chi$ representation of a river profile, regardless to the topography. For $n > 1$, flat areas, as they occur in formerly glaciated valleys, indeed cause lower velocities. However, it seems to be impossible to add serious information on this here, except for mentioning a reasonable range (0.0001 – 0.1 m/year) derived from van Heijst and Postma (2001). We will implement such a statement including the reference in our revised version.*

Lines 50-52 – I might be a bit more cautious here since horizontal advection has been mentioned and is also present in the Eastern Alps. Horizontal advection causes an asymmetry across drainage divides in the direction of advection with the geomorphic characteristics listed here. However, in most cases the divide remains immobile despite the presence of a lateral advection component (see, for example, Miller et al., 2007; Eizenhöfer et al., 2019). **Reply:** *This is in principle true. The best solution would probably to start this point from the other side. This means that migration of drainage divides is usually reflected in an asymmetric shape. In turn, asymmetry may indicate an unstable drainage divide, but there may also be other reasons, mainly variations in precipitation or lithology or advection. The discussion why we consider these as not very likely here could be given in the discussion part.*

Lines 56-58 – I am convinced that major tectonic phases do influence drainage patterns but would add some time notion to ensure that the reader understands that these drainage patterns reflect events at geologic time scales. **DONE***: We agree that it is not obvious which time scale is meant by our description. We therefore suggest adding the term "long-term" to tectonic phases, as the duration of single events also differs in duration. In this way, we aim to distinguish between long-lasting deformation and short-tectonic events.*

Line 64 – Please add a rough time information on the onset of topography information in the Alps. **DONE:** *We will add "Oligocene" as time marker in the revised version of the manuscript. We will also include an appropriate reference (e.g. Handy et al., 2015).*

Line 73 – I suggest being more precise with the term 'fault tectonics' since this does theoretically also involve thrusting, normal faulting and/or displacement along a décollement. I assume you refer to extrusion tectonics here. **DONE:** *In a revised version, "fault tectonics" will be changed to "extrusion tectonics" to be more precise.*

Lines 84-86 – Perhaps briefly elaborate from a more geological point of view (here or better in the next section) why sediment provenance is consistent with the drainage systems here. **Reply:** *We refer to this point in the next section*

Lines 99-100 – The exhumation of the Tauern Window and the tectonics behind this process are still debated. Since this is not a major concern in this study, I would rephrase this sentence by moving away from a causal relationship and perhaps just state a coeval occurrence. **DONE:** *We agree that the formation of the Tauern Window is still under debate and the intention of this study is the general evolution of drainage patterns in the Eastern Alps and not the Tauern Window. We therefore suggest rephrasing the sentence to:"During Early to Middle Miocene times lateral extrusion tectonics set in (confined by a set of crustal scale strike-slip and associated normal faults) and the Tauern Window exhumed rapidly."*

Lines 104-106 – Echoing my earlier comment on sedimentary provenance, I would be a bit more precise geologically in terms of location of sources (e.g., Austroalpine units covering the Tauern Window) and indicators that were used to identify provenance. **DONE:** *We will add some more detail to the location of units in the reversed version of the manuscript (as suggested).*

Line 113 – 'recent geological past' is a fairly vague term. Please be more precise regarding timing. **DONE:** *We will specify the time range to Pleistocene.*

Section 2.1 – I do not think it is required to go into such detail here regarding $\chi$ transformation and/or the derivation of channel steepness since the equations shown here do not contain any modifications and/or update on already existing literature. I would simply describe their use and theoretical background and refer to the literature in order to keep this section short and focused. **Reply:** *We could indeed streamline the recapitulation of the theory a bit and focus it on the aspect that we need – the relationship between the slope in the $\chi$ plot and erosion rates and how this can be used in $\chi$ mapping without considering profiles explicitly. However, we learned in many discussions with colleagues, that the interpretation of the $\chi$ transform and in particular of $\chi$ mapping is challenging, so that a short recapitulation of the theory behind $\chi$ in order to point to advantages / disadvantages and pitfalls should be given. This goes in line with the location of section 4.1 at the beginning of the Discussion (please see also our comment below) dealing with limitations of the method.*

Line 131 – In the case of n = 1 (as assumed further below to derive the ratio U over K directly, and very often used in the literature), this relation is linear. **DONE:** *This is, of course, true. Power-law relations become linear if the exponent equals 1. As nothing relies on the nonlinearity, it would be the best to remove the statement about the nonlinearity.*

Line 162 – I think it would be good to show (perhaps only as supplementary material) in a simple slope/area plot that this threshold has been chosen meaningfully in excluding hillslope processes (e.g. the brake-down of a linear slope/area relationship). **Reply:** *The cutoff size of 1 km$^2$ is some kind of tradeoff between the amount of available data (river segments) and validity of the stream power model. Hergarten et al. (2016) investigated the effect systematically for the topography of Taiwan and found a moderate deviation in slope of about 20 % at 1 km$^2$ from that predicted by the stream power law, while there was still a strong correlation between slope and catchment size. The deviation may be a bit stronger for the Alps due to the lower precipitation. However, we should keep in mind that we did not consider any river profiles quantitatively. Choosing a higher cutoff value would just cut the river segments in Figs. 2 and 7 a bit, but without affecting the lower part. So nothing in the interpretation of those figures would change. Of course, it would be possible to make a slope-area plot for the considered region if required, but we are not convinced that it would really merit a supplementary figure. So we think it would be the best just to discuss it briefly.*

Line 167 – Generalization of the Gilbert Metrics and applying the stream-power relation. **DONE:** *Thank you for the suggestion. We will add your formulation to the revised version of the manuscript.*

Lines 178-179 – Just a minor quibble, but I would ensure that this is phrased in a way that the reader is aware that exactly across divide channel head elevation, hillslope gradient and local relief are the Gilbert Metrics. **DONE:** *We will add the clarification: "channel head elevation, hillslope gradient and local relief which represent the Gilbert metrics)" for the reader.*

Line 194 – I suggest mentioning early on to which base level elevation you are referring to when discussing stream profile analyses. This is not right away clear at the beginning of this (and actually throughout) the paragraph. **DONE:** *We agree that the chosen base level is not clear. We therefore will add information about the base level to the method section.*

Line 203 – It needs to be clear at which base level you started (0? or something higher?). This is not trivial otherwise you would not have computed $\chi$ at different base levels). I recommend, however, to consider in your selection for the lowest base level the bedrock/alluvial transition as done by Winterberg & Willett (2019) and briefly explain why you chose this lowest base level. **DONE:** *We chose 400 m as base level and successively increased in equal steps towards the height of the channel heads. 400 m represent thereby the outflow of the main rivers to the foreland. We will add a short paragraph to the method section to clarify the approach.*

Lines 270-271 – Besides climate and lithology, I would also add tectonics. **DONE:** *We will make this sentence more general: "In the simplest case with overall uniform conditions, erosion rate increases with channel steepness in the drainage and topographic gradient in the hillslope domain." Challenges and Complications are explained in some detail in the next section.*

Line 276 – I think the nature of this 'signal', especially its origin (i.e., climate or tectonics) needs to be more precisely elaborated. A brief description on how such 'signals' traverse through the channel up to the hillslope domain should be included. **DONE:** *Thanks, we will add "expressed by an upstream migrating knick point or knick zone" to clarify the nature of the signal.*

Lines 277-279 – Are there additional references that dealt with lowering base levels due to glacial erosion (perhaps not necessarily an example from the Alps) to underline the generality of this hypothesis? **DONE:** *Thank you for pointing out the missing references. The process of glacial overdeepening is well described in many publications (e.g. Hallet et al., 1996; Whipple et al., 1999; MacGregor et al., 2000; Brocklehurst and Whipple, 2002; Montgomery, 2002; Anderson et al., 2006; Haeuselmann et al., 2007; Züst et al., 2014). We added the missing references, which includes also global examples, to enhance our point. In the case of the Salzach valley,* *the glacial overdeepening, and hence the lowering of the baselevel, is described in Bleibinhaus and Hilberg (2012) and Pomper et al. (2017). We will add the required references to the revised version of the manuscript.*

Section 4.1 – This section might be better located towards the end of the manuscript (I would rather expect early on a discussion on your results followed by a description of the limitations), but this might be a matter of taste. **Reply:** *We definitely understand your concerns with the location of this section. However, due to the pitfalls and difficulties in interpreting across divide $\chi$ gradients, we made a conscious decision to clarify the use and expectations towards the $\chi$ analysis in advance of the details of our results. Overall, we think this practice helps – in this particular case – to keep the manuscript short and precise, as many clarifications are already done and discussing the results becomes straightforward.*

Lines 281-282 – From a geological point of view, I struggled with this argumentation, since mm/yr translates over geologic time scales to km/Ma, which I regard as rather significant and geologists deal with this magnitude of rates on a daily basis. Over such time period a drainage divide might have migrated over kilometre distances (Eizenhöfer et al, 2019, provides a, perhaps more theoretical, example of a drainage divide that migrates significantly over geologic time). So, determining divide migration might be challenging, but probably not 'unfeasible'. Since the scope of the study ranges over geologic time scale (starting at initial collision at ca. 30 Ma) I think this phrasing needs to be modified. **DONE:** *We agree that "feasible" is very pessimistic and we can change the wording to "challenging", as suggested. However, our intention was to point to the "direct determination of divide migration rates", in which case we think "feasible" is quite fitting. We fully agree that divide migration rates in order of a mm/y result in a total divide shift of several kilometers over geologic time scales. Evidence is found in topography and the process can be modelled by employing LEMs (although this issue is not trivial). However, we do not think that there is a direct method to measure the yearly rate of divide migration in field.*

Lines 288-289 – I would elaborate in some more detail on these tectonic phases and spatial and temporal changes in uplift patterns since these are very distinct, i.e. Early Miocene collision followed by Middle Miocene extrusion tectonics. A good start are works by Frisch et al. (1998) and Kuhlemann (2007). **Reply:** *We agree on the importance of the tectonic phases. We therefore refer to the introduction (Section 1.2 Co-evolution of topography and drainage system of the Eastern Alps), where the main processes are mentioned, including the work of Frisch et al. (1998) and Kuhlemann (2007). We think a detailed description in the Discussion part potentially exceeds the scope of this work. However, as the reconstructions of the mentioned authors do neither include any information on topography nor elevation, we think that a detailed description of the tectonic processes is not necessarily helpful.*

Lines 289-290 – Please clarify whether these hillslopes are at critical slopes following glacial erosion, or potentially something else (e.g., deep mantle processes as discussed by Schlunegger & Castelltort, 2007). **Reply:** *As we are in the section about challenges and limitations, we wanted to point out that the relationship between slope and erosion rate is lost, if the slopes reach their limit of stability, so that the slope itself is no longer useful in the context of erosion rates and drainage divide migration. Why they are so steep does not matter much here. It is of course either glaciation or just the decrease of fluvial erosion due to decreasing catchment size, when approaching the drainage divide. We will point out this aspect more clearly in a revised version.*

Lines 293-300 – I suggest rephrasing this paragraph by focusing on and more systematically discuss the effects the three parameters (climate, tectonics and erodibility/lithology) can have on $\chi$ stream profile analysis. Here climate and tectonics are emphasised in the beginning of the paragraph while lithology appears as some 'side effect'. This issue might appear a bit nit-picky, but I would prefer to have the limitations of $\chi$ stream profile analyses clearly outlined. **Reply:** *Although the variability in erodibility due to lithology may be smaller than* *variations in uplift rate and precipitation, our wording should not imply that lithology is some side effect. We have recently even published a study on lithological effects controlling the shape of topographic features (Baumann et al., 2018). However, while climate and tectonics are active drivers of landscape evolution, substrate properties are a passive control. Therefore, we started describing the active part followed by the passive part. Nevertheless, we could try to state more clearly here under which conditions $\chi$ maps might be misleading, although we think that we have already provided a detailed section on limitations of $\chi$ mapping.*

Lines 309-310 – Perhaps elaborate this 'future divide mobility' aspect a bit more? **DONE:** *The argumentation for $\chi$ to represent rather future divide migrations is shown in Forte and Whipple (2018). Thank you for pointing to the missing reference. We will add the reference and refer to it in a revised version of the manuscript.*

Lines 312-314 – This base level strategy adopted here might be strategically better placed in the methodology section. **DONE:** *As suggested, we will add a short paragraph to the methods section. However, we decided to keep these two lines in the manuscript, as they demonstrate our approach to counteract some of the limitations.*

Lines 334-337 – Perhaps go even one step further: what is / could be the nature and potential origin of these signals with respect to these different amplitudes and time scales? **Reply:** *The origin of the signals is given in two examples (the development of escarpments as short wavelength – high amplitude signal; tectonic processes as high length scale low amplitude signal). We are aware that there are many more examples, but here we refer to section 4.3, were the observed (mainly high-length scale – low amplitude) signal is interpreted in terms of tectonic evolution of the Eastern Alps. Our intention here is to show the general origin of the signal. We agree, that it is rather uncommon to start the discussion section with general statements, but think that this is appropriate in the context of $\chi$ signal interpretation (see also reply to section 4.1).*

Line 343 – These simplifying assumptions (i.e., uniform climate, lithology, tectonics) should be emphasised a bit more in the limitations section. **Reply:** *All these assumptions (deviations from those) are explained in section 4.1.*

Line 358 – Even though glacial erosion stopping divide migration appears trivial, is there any reference out there that would support this and could be added here? **DONE:** *Thank you for this comment. We will add a reference. From a geometrical point of view, this is indeed a straightforward implication of glacial erosion. The formation of cirques with vertical cirque faces locally changes the geometry of hillslopes at the divides and temporally removes asymmetry, which may have existed prior. The impact of glacial erosion on drainage divides is discussed by (Robl et al., 2017a) and is nicely illustrated by a movie in the supplement.*

Line 363 – I suggest being more precise regarding this 'peculiar west-east directed migration', and directly implement your results (i.e., a divide migration from W to E). **DONE:** *Thank you for inviting us to be more precise. We will rephrase the sentence as suggested.*

Lines 372-373 – I do not think basic background on $\chi$ stream profile analysis needs to be repeated here and should belong to the methodology section. **DONE:** *We will remove the mentioned paragraph and think the relationship is sufficiently described in the methods section.*

Lines 374-377 – It is not entirely clear from the way it is phrased here whether basic background regarding $\chi$ stream profile analysis is being discussed or implications of your analyses in the Eastern Alps as a whole. Perhaps add some broad geographic location indicators? **DONE:** *Indeed, we aimed to refer to our observations from the Eastern Alps and not to a basic background. For clarification, we will add the suggested locations to the revised manuscript.*

Lines 381-383 – Echoing my earlier comment on time scales, I think it is problematic to use the slow mm/yr rate of divide migration as argument for the longevity of geomorphic features (this needs to be tested) in the context of tectonic forces that operate at these time scales. **Reply:** *We fully agree that there is more work needed to test the longevity of geomorphic features. However, our aim was to work out and emphasize the difference between long-lasting continuous migration processes and sudden stream capture events, also presented by Goren et al. (2014) or (Robl et al., 2017b), which potentially work on the observed signal in our results.*

Figure 7 – There are a couple of issues I had with this figure (technically and geologically/geomorphologically). 1. How has this figure been produced? 2. Does it show some kind of 'model'? What are the model assumptions in detail? 3. How has $\chi$ been produced after changing the geometry of the drainage basins, especially that of the drainage divides? Have all elevations been the same as present-day? 4. Since Oligocene time uplift patterns and the geometry of tectonic units in the Eastern Alps have considerably changed (e.g., Frisch et al., 1998). Assuming that the drainage geometry and river courses are largely the same since then (perhaps with exception of the major strike-parallel drainage systems) and only locations of divides are changed, I find rather problematic. 5. It is very likely very challenging to accurately depict the fluvial geometry of the Eastern Alps in Early Miocene simply because numerous tectonic and climatic events (Messinian crisis, Pleistocene glaciations, Middle Miocene Optimum, significant changes of the drainage basin of the Danube far to the east, rapid Miocene exhumation of the Tauern Window, switch from convergence to extrusion-dominated tectonics, just to name a few examples) would have had a deep impact on the drainage system. Thus, Figure 7 is from my point of view an overly optimistic simplification that might be rather misleading than helpful. In applying $\chi$ analyses across the present-day Eastern Alps a number of simplifying assumptions have already been made and we somewhat already turn a blind eye to this (for good reasons), but doing the same back in geologic time, I find, is rather problematic.
**Reply:** *It was not our intention to reconstruct the topography of the Eastern Alps for different time steps. We would love to do that! However, uplift patterns, rates for horizontal advection, exhumation rates and so on are only very roughly constrained. It is even not clear, when the topography formation in the Eastern Alps happened and there is a vivid debate whether topographic pattern observed in the Eastern Alps relate to glacial imprint or fluvial prematurity caused by an uplift event about 5 Ma ago. Thanks to the comprehensive work of several authors (e.g. Frisch et al., 1998; Dunkl et al., 2005; Kuhlemann, 2007), there is at least some information on drainage systems and topography at certain time slice. The idea of figure 7 was just to roughly mimic the plan view geometry of drainage patterns of the Eastern Alps. As $\chi$-mapping in contrast to $\chi$ profiling does not explicitly require information on elevation, we show changing $\chi$ anomalies across divides for different catchment geometries. We could have done this also by employing one of our landscape evolution models, but think that it is more intuitive for the reader to alter the drainage system of the Eastern Alps according to the geometries based on the restorations of Frisch et al. (1998).*
*From a technical perspective, the maps were produced as described in the method section ($\chi$ mapping). The original DEM was dammed and the outflow of rivers rearranged to wind gaps, numerously described in the literature (e.g. Robl et al., 2008). A further change of the DEM is not necessary, as $\chi$ mapping, as aforementioned, does not require any information on elevation.*

Lines 389-391 – Exactly these assumptions stated here I regard as problematic, and Figure 7 is probably not the best approach to simulate palaeo-drainage geometries. **Reply:** *We would agree if it was our intention to provide a serious reconstruction of the drainage pattern or even the topography at distinct times. But as discussed above, this would not only be much more complicated, but would also require much more information, which is not available. The assumption is that topography adjusted to changes in tectonics by small changes in network topology and keeps as much as possible of the valley pattern. With regard to the early work of Hergarten and Neugebauer (2001) and the recent work on river capture that has raised much attention, this concept should be a reasonable tradeoff between available information and what we can read from it.*

Lines 442 – Since Figure 7 is indeed a very rough restoration, making many in my opinion rather oversimplifying assumptions (see my comment on this above), I would be very cautious in drawing conclusions based on this figure. **Reply:** *see previous comment.*

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