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Earth Surface Dynamics Discussions

Interactive comment on "Glaciation's topographic control on Holocene erosion at the eastern edge of the Alps" *by* Jean L. Dixon et al.

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Dixon et al. provide new detrital cosmogenic 10Be data to constrain erosion rates of nearly 30 catchments in the easternmost Alps (Austria and Slovenia). While earlier studies in this area have argued for a tectonic control on erosion rates, with catchments influenced by recent uplift recording higher rates than catchments to which this recent phase has not (yet) been communicated, the extended dataset presented here shows that the main controlling parameters on erosion rates are basin relief and mean slope, which the authors argue to be influenced by glacial preconditioning.

This study provides interesting new data that significantly tone down previous interpretations, and provides an integrated view of Holocene erosion rates in the Alps. It is therefore timely and definitely suitable for publication in Earth Surface Dynamics. While

my overall evaluation of this manuscript is thus positive, I recommend it be returned to the authors for moderate revisions before final acceptance. These pertain to some apparent misconceptions or imprecisions in the writing, as well as the intriguing slope-area relationships that may merit some more discussion. As most of my comments are rather specific, I will list them tied to page and line numbers below:

Page 1, line 13: the Hergarten et al. model is based on a fundamental misconception: it mistakes a glacial imprint on topography for a transient tectonic signal. It would be preferable if this fundamentally flawed study were not perpetuated in the literature any more than it needs to; I would thus suggest the authors to refrain from citing it, particularly in the abstract.

Page 1, line 23: Although Legrain et al. do invoke "deep lithospheric processes", it is not sure these are required for the easternmost Alps. In contrast to the west, convergence is still active in the East (e.g. Serpelloni et al., Geophys. J. Int., 2005) and the inversion of the Pannonian basin can be linked to a change in crustal stress fields from extension to compression (itself possibly linked to a deep lithospheric cause, however).

Page 2, line 7. The process of valley deepening and widening described above is not, in fact, the cause of the "glacial buzzsaw". The generation of widespread low-relief surfaces at elevations around the average Quaternary ELA (the topographic fingerprint of the "buzzsaw") is rather linked to efficient cirque retreat, possibly aided by periglacial (frost-cracking) processes. See Mitchell and Montgomery (Quat. Res., 2006) and Egholm et al. (ESurf., 2015) for discussions of these processes.

Page 2, line 9: Isostatic rebound will cause rock uplift but will not in itself increase relief. Relief increase is due to the fact that glacial erosion is strongly non-uniform or "selective", deepening valleys while having limited effects on higher parts of the landscape.

Page 2, line 14: Norton et al. (Geology, 2010) would be a good complementary (or alternative) reference here.

Page 2, lines 26-27: This presentation of the findings of Wittmann et al. (2007) is slightly misleading. In fact, their regression of denudation rates versus rock-uplift rates gave a slope of 1.0±0.25, i.e. erosion rates could be either higher or lower than rock-uplift rates, and these authors include a lengthy discussion of the potential implications of this finding. Champagnac et al. (2009) did subsequently argue, based on a subset of this data, that rock-uplift rates were lower than denudation rates, but even their analysis is not equivocal on this point.

Page 3, line 1: again, why do you need to invoke deep lithospheric processes in a region where convergence is still ongoing?

Page 3, lines 12-14: there are several regional names here (Styrian (Alps?), Levanttal Alps, Gleinalpe, Koralpe, Schladmig Tauern, Seckauer Tauern, Pohorje) that are not know to a non-Austrian readership. They should be indicated on the map of Fig. 1.

Page 3, lines 16-18: a geological map might make this description of the regional geology easier to follow.

Page 3, lines 27-28: "but appears conspicuously unrelated" to what? This is unclear . . .

Page 4, line 24: Norton et al. (2008) is not in the reference list.

Page 4, line 25: it is laudable that the authors try to take snow shielding into account in their calculation, but how reasonable is it to extrapolate a snow-depth – elevation relationship determined for central Switzerland to eastern Austria? The most comprehensive climatology database to date that I know of (Frei and Schaer, Int. J. Clim. 1998) shows that both mean-annual and winter precipitation is significantly lower in eastern Austria than in central Switzerland.

Page 4, lines 25-27: Can you provide some information on the geology of the sampled catchments, at least reporting the aerial percentage of quartz-bearing lithologies in

Table 2? Were topographic and relief measures only calculated on the quartz-bearing part of the catchments or the entire catchments?

Page 5, line 25: the Roering et al. (2001) model was actually designed to model shallow landsliding, not really hillslope creep.

Page 6, line 1: "higher erosion rates in general" is unclear: what erosion rates are you discussing here?

Page 6, lines 5-9: it could be useful here to show a plot of the combined datasets (the current dataset and that of Legrain et al., 2015).

Page 6, line 25: "segmented" rather than "segmenting".

Page 7, line 1: A similar relief structure was described for glacially influenced catchments in the western Alps by van der Beek and Bourbon (Geomorphology, 2008).

Page 7, lines 11-12: The Vernon et al. (2009) reference is inappropriate here, as these authors did not discuss frost-cracking as a potential mechanism controlling spatial variations in erosion rates (moreover, these authors were looking at long-term exhumation rates from thermochronology data, on which the influence of frost cracking would be much harder to substantiate).

Page 7, lines 12-16: these arguments to rule out frost cracking as a mechanism controlling the variation of erosion rates are not completely convincing. First, it would be good to show the correlation between mean catchment elevation and erosion rate and to show that this correlation is weaker than that between mean catchment slope and erosion rate (this is what the authors appear to argue). Second, the fact that basins of the same average elevation show large differences in erosion rate does not necessarily rule out frost cracking, as this process depends on mean-annual temperature and its variation rather than elevation (which is just taken as a convenient proxy). The aspect of the basins (north- versus south-facing) as well as their geology may play a major role in modulating frost-cracking efficiency.

Page 7, line 30: slab detachment has become the preferred “deus-ex-machina” mechanism to “explain” uplift rates in the Alps. The data reported by Qorbani et al. (2015) provide only a very indirect indication for possible slab detachment. In the absence of more clearly resolved seismic tomography imagery for the European Alps, I feel we should be careful in invoking this mechanism . . .

Page 8, lines 8-15 (and Figure 6). There is something in this Figure I do not understand. Apparently (unless there is a problem with the x-axis) this slope-area plot is for extremely small catchment areas (<1 km2), i.e. for the most part within the hillslope domain (the hillslope – fluvial transition typically occurring at catchment areas of 105-106 m2. At these small catchment areas, the data should show either increasing slope with area (diffusional hillslopes) or no relationship between slope and catchment area (landslides, debris-flow domain). Yet the data show very good slope-area scaling, with larger concavities for glaciated than for non-glaciates catchments (as expected). So either the area axis is in km2 rather than m2 (which would make sense) or something very curious is going on.

A more general comment on Section 3.4: possibly your best potential argument for a control by glacial preconditioning on erosion rates would come from your 5 catchments in the Seckauer Tauern. There are 3 unglaciated and 2 glaciated catchments, with for the rest fairly similar characteristics (at first glance at least). There are also 2 catchments that have significantly higher erosion rates than the other 3. Are these the two formerly glaciated catchments? If so, bingo!

General comment on Section 3.5: the fact that erosion rates appear to systematically increase toward the west is, however, not easily explained by a mechanism of glacial preconditioning of topography. On page 9 (lines 11-28) the authors attempt to invoke paleo-climate variations and possibly thicker ice cover in the western Alps, but in the absence of any data this remains somewhat speculative. Several studies have reported average LGM ice thickness for the studied catchment areas; it may be interesting to have a closer look at this, compile this data where it is missing and see if there is a

relationship with millennial erosion rates. However, a more simple relationship may exist between present-day rock uplift (as inferred from GPS studies) and erosion rate – GPS-derived rock-uplift rate data have now been published for most of the Alps, including the western Alps (cf. Nocquet et al., Scientific Reports 2016). If a strong relationship with rock-uplift rates exists (and uplift rates are similar to or higher than erosion rates) then a tectonic or geodynamic control on these laterally varying rates should be invoked.

Page 9, line 4: the arguments used by Persaud and Pfiffner (2004) to suggest active ongoing tectonics in the part of the central Alps they were studying were not particularly convincing. Not sure it is worth citing this here.

Page 9, lines 23-25. This is a long and complex phrase. It is important for your arguments though; you may want to reformulate it.

Comments on Figures

Overall, I’m not sure the organisation of the figures is the most logical and effective. Some could be merged; others appear to be missing.

Figure 1: Needs to show the different regions sampled (Styrian Alps, Levanttal Alps, Gleinalpe, Koralpe, Schladmig Tauern, Seckauer Tauern, Pohorje). A simple way to do this would be to color-code the catchments and add a legend (in that case Fig. 2a would not be needed anymore). An additional panel with a simplified geological map could also be useful here.

Figure 2: (a) can be combined with Figure 1. If you want to keep a map with the catchments here, it may be more useful to color-code them according to rate, so that the reader can see the spatial variation in erosion rates easily. An additional plot of erosion rate as a function of mean-catchment elevation would be useful (see specific comments above).

Figure 6: Check the scale for this plot (see comment above)!

Figure 7. Not sure the erosion rate versus slope plot is the most effective here. An interesting plot could be simply erosion rates versus longitude (to show whether there is really an east-west increase or this is only apparent); otherwise suggested plots (see above) would be erosion rate versus average-LGM ice thickness and/or erosion rate versus present-day rock uplift rate (GPS data).

Grenoble, 7 July 2016 Peter van der Beek

Please also note the supplement to this comment:
http://www.earth-surf-dynam-discuss.net/esurf-2016-29/esurf-2016-29-RC2-supplement.pdf

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