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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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Dear Authors, dear Editors,

Please find below my review concerning the manuscript by Dixon and co-authors entitled "Glaciation's topographic control on Holocene erosion at the eastern edge of the Alps" (Paper # ESurf-2016-29).

This manuscript investigates the spatial variability and potential topographic controls on Holocene erosion rates in the eastern Alps. The authors present original catchment-wide erosion rates (26 new basins) from cosmogenic 10Be at the eastern edge of the Alps (Austria and Slovenia). They compare their erosion results with detailed topographic metrics to discuss any potential climatic, tectonic and/or topographic control on the spatial distribution they observe for catchment-wide erosion rates. Finally, they

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combine their new results with existing 10Be-derived catchment-wide erosion rates all over the European Alps to discuss the different forcings on Holocene erosion at the orogen-scale.

This is a very interesting, well-written and presented manuscript. The authors have really well introduced their work with a comprehensive review of the literature and presentation of the open questions concerning the late evolution of the European Alps. They propose new detrital 10Be-derived erosion rates that nicely complement previous observations in the Eastern Alps and have performed a detailed topographic analysis to discuss their dataset and the spatial variability of catchment-wide erosion rates over the entire European Alps. As such this study represents in my opinion a valuable contribution and will have potential of great interest for the community. I have some questions and suggestions, mainly to better clarify some of the results and their interpretation, and have outlined them in a set of general and specific comments below.

General comments: 1 - Structure: the present manuscript is overall well-structured, with clear “Introduction” and “Approach” sections. However, the merged section 3 “Results and Discussion” is sometimes quite difficult to follow between the different subsections. I would suggest the authors to separate the results from the discussion which may be easier to follow for the readers.

2 - 10Be-derived basin erosion rates: the new results for 26 basins presented by the authors are really interesting and nicely complement previous investigations of Holocene erosion rates across the European Alps. However, I would recommend the authors to provide more discussion about the possible bias in calculated erosion rates from local complexities and the resulting implications for their story. I have listed some detailed questions below, especially concerning the integration times, snow-cover correction, the sediment grainsize, the approach for floodplains and non-quartz bearing areas... Moreover, some discussion about the (previously) glaciated catchments would be helpful for ESurf readers concerning the re-mobilization of morainic/glaciogenic material, or the glacial perturbation on 10Be concentrations which might be non-negligible for

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slowly-eroding terrains (e.g. Glotzbach et al., 2014 Terra Nova). Finally, the compilation of existing 10Be-derived basin erosion rates from the literature is very interesting and nicely put the new dataset into a broader context. I am wondering if 10Be-derived basin erosion rates have been directly taken from published papers, or if the authors have recalculated the erosion rates with a uniform/updated production rate? If not, maybe discuss also this point and potential implications.

3 - Topographic metrics: the authors provide a detailed topographic analysis of the studied catchments with a special attention on mean basin slopes and slope distributions within the basins. I would suggest to also report other metrics, such as hypsometry or local relief to strengthen their message (see also specific comments below). Finally, the authors discussed in some sections the role of fluvial incision and river response to potential perturbations, but this would be better illustrated with some river profiles shown as a new figure. This will clearly help readers to evaluate the degree of disequilibrium in river profiles, and potential differences between regions/massifs or glaciated vs. non-glaciated catchments.

Specific comments, by line number: - Page 1, lines 26 and 27: “poor topographic indicators of controls” (l.26) contradicts with the end of the next sentence “its topographic legacy” (l.27). Maybe rephrase the sentence on line 26 (which also appears in contradiction with the manuscript’s title).

- Page 2, lines 14-15: “suggest that glacial forcings are the dominant control on landscape evolution in mountain belts”. This sentence reads vague and quite general, maybe rephrase to precise in which mountain belts (mid- and high-latitudes?) and over which timescales (Plio-Quaternary?).

- Page 2, line 26: “have been invoked as principle drivers of erosion and uplift”. Maybe cite also there Fox et al. 2015 (Geology).

- Page 2, line 28: “post-glacial erosion may explain rates of uplift in the region via isostasy”. Please clarify the timescales over which this correlation is valid, i.e.

Holocene for erosion rates from Wittmann et al. (2007) and historical/modern for uplift rates (leveling data from Schlatter et al., 2005).

- Page 2, line 29: "youthful tectonic features such as river knickpoints". Please rephrase, river knickpoints can have several origins and as rightly stated after ("correlated with previous glacial cover. . .") these are not tectonic markers.

- Page 3, line 8: "erosion rates". Please specify the timescale "post-LGM/Holocene" for these erosion rates.

- Page 3, lines 13-14: Please indicate the massifs, basins and other important locations on Figure 1 for clarity.

- Page 3, line 17: "that has uplifted some 300 m above sea level in the last 7 Ma". Please add a reference here if possible.

- Page 3, line 18: "Massifs" corrected by "massifs".

- Page 3, line 27: "This timing coincides with inversion and uplift of the Styrian and. . .". Please clarify which timing is considered here. Is it 4 Ma (l.27), 7 Ma (l.17) or 10 Ma (l.16)?

- Page 4, line 9: "250-500 um size fraction". In Table 1, some sample sizes are higher (500-800 um). Are they replicates? Please clarify, and maybe also discuss potential implications when comparing the resulting erosion rates for different grainsizes.

- Page 4, line 25: "elevation-snow depth relationships previously determined in the Swiss Alps". I am not sure to what extent the data from Auer (2003) in the Swiss Alps can be extrapolated to the Austrian/Slovenian Alps (this extrapolation might depends on the local precipitations and moisture patterns). How comparable are the two climatic settings? Please discuss this extrapolation and the potential implications for calculated erosion rates.

- Page 4, line 26: "we set production rates equal to zero in parts of drainage basins with

carbonate terrains". These areas should be excluded from the calculations (since they do not contribute quartz) and not set to zero-production, otherwise this would introduce a bias in the catchment-wide erosion rates. Please correct or clarify. Also, how did the authors consider low-gradient areas such as floodplains (figure 1) in their calculations? Are they included in the integrated 10Be erosion rate calculations? Please clarify.

- Page 5, line 14. The (previously) glacial setting for these catchments may imply potential biases in the 10Be concentrations and thus in the calculated erosion rates, with input from morainic/glaciogenic material (e.g. Delunel et al., 2014 ESPL) or the impact of former glaciation (Glotzbach et al., 2014 Terra Nova). I would suggest the authors to discuss these points further and to what extent they may perturb the inferred catchment-wide erosion rates.

- Page 5, line 21: "Measured erosion rates also generally increase with increasing mean basin elevation". Can the authors provide a figure for this correlation?

- Page 5, line 22: "Measured erosion rates also generally increase with [...] slope". Looking at Figure 2b, we can also see two clusters: 1) non-glaciated basins with no correlation between slope and erosion, and 2) (previously) glaciated basins where there seem to be an inverse correlation between slope and erosion. Please consider discussing this potential alternative observation.

- Page 5, line 28: "We note that both erosion rates and catchment mean slope correlate with the proportion of the catchment that exceeds 35° (Fig. 2b)". This is not shown on figure 2b, or maybe I missed something. Please correct or clarify.

- Page 6, lines 9-14: "therefore likely reflect this erosional response to river incision and tectonic processes across the range". In the previous section (3.1, lines 11-12), the spatial variability in erosion rates was suggested to reflect lithological variations. Please clarify. - Page 6, line 25: "We segmenting". Please correct.

- Page 7, line 1: "consistent with characteristic glacial and non-glacial slope-elevation

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curves predicted by Robl et al. (2015)." This specific slope distribution with elevation for glaciated terrains has already been observed in other places (e.g. van der Beek and Bourbon, 2008 Geomorphology). Please cite some references.

- Page 7, line 7: "other climatic controls such as precipitation rates". How variable are the mean precipitation rates between the different studied catchments? Please discuss this point.

- Page 7, line 11: "correlations between elevation and either rock uplift or erosion rates". Unclear, please clarify. I think this would rather be "correlations between elevation or rock uplift and erosion rates". Also, correlations from Vernon et al. 2009 are based on thermochronology and thus imply much longer timescales than Holocene, and they do not consider frost-cracking. Please correct.

- Page 7, line 13: "elevation poorly correlates with the abundance of steep (>35ÌŁ) slopes". On Figure 3, there seem to be some non-linear correlation between elevation and fraction of the basin >35°. Please rephrase or discuss further this point.

- Page 7, line 16: "While frost-cracking may enhance erosion at alpine sites, it does not appear to explain the patterns and variability in erosion rates across our catchments". Maybe frost-cracking is occurring (or had occurred in the Lateglacial period) for previously-glaciated catchments, but for non-glaciated catchments the mean elevations are too low to consider this effect. Would it be possible?

- Page 7, lines 24-25: "If catchment erosion were driven by increased river incision, then we should observe higher area-normalized stream gradients in rapidly eroding catchments". What are "area-normalized stream gradients"? Also, did the authors study the river profiles to identify such perturbations. I would suggest the authors to show some river profiles and/or hypsometric curves for the studied catchments to illustrate the discussion about river incision (see also my general comment). Same question for the sentence on Page 8, line 7-8. This is difficult to see the fluvial domain on figure 6, so I would encourage the authors to show some figures focused on the

fluvial part of catchments (river profiles, slope-area diagrams).

- Page 8, lines 3-6. How about lithological variations (and thus erosional resistance) as a potential control on the spatial variability in erosion rates?

- Page 8, line 15: “processes solely within the hillslope domain”. Did the authors also look at local relief as a potential topographic metrics for erosion rates?

- Page 8, line 22: “Compiling previously reported cosmogenic 10Be-derived rates across the Alps”. Did the authors report here the original erosion rates or did they recalculate erosion rates with uniform/updated production rate? Please clarify and discuss potential implications for comparing erosion rates across the Alps.

- Page 9, line 1: “we might expect Holocene erosion to reflect exhumation and uplift. . .”. How can erosion reflect exhumation and uplift? This appears unclear, please rephrase.

- Page 9, lines 4-7: “Long-term exhumation rates from thermochronometric ages are largely attributed to deep tectonic processes that increased during the Cenozoic”. Please rephrase, long-term exhumation rates are also driven by Plio-Pleistocene changes in erosion following climatic forcing as well as drainage modifications, not only tectonics.

- Page 9, lines 12-13: “with highest modern and LGM precipitation occurring in the northern slopes of the Alps and decreasing to the south and east”. Moisture patterns have changed between the LGM and modern (Florineth and Schluchter, 1998) so I am not sure that precipitation maxima have always been on the northern Alpine slopes. Please clarify.

- Page 10, line 3: Please correct “mm/ka” by “mm/ky” for consistency.

- Page 10, line 5: “49 mm/yr” would rather be “49 mm/ky”. Please correct.

Tables and Figures: - Figure 1: I would suggest to add main massifs, basins and maybe river names on Figure 1b to help the readers following section 2.1 and to link

with subsequent figures.

- Figure 2: Please indicate replicates on Figure 2b with a star or different symbol. Also, I would suggest to also add the data from Legrain et al. (2015) on this figure, that may be helpful to compare them already at this stage, no (as they appear on figure 3)?

- Figure 3: Maybe use different symbols (or open/filled) to differentiate between glaciated/non-glaciated basins?

- Figure 4: Why is the “basin erosion” legend reversed in panel a? Panels c and d are nice and informative, I am wondering if similar panels with elevations would be informative? On panel b, what are the criteria for “partially glaciated” and how does it relate to figure 2 with glaciated/non-glaciated? Please clarify.

- Figure 5: Would it be possible to use different symbols for glaciated/non-glaciated catchments?

- Figure 6: This figure is difficult to read at present. I think that the slope distribution for the hillslope domain is already illustrated by panels c and d of figure 3, so I would recommend to show here only the fluvial domain (>103 m2) to better highlight any differences between the different rivers.

- Figure 7: I would be curious to see if there is any correlation between mean basin elevation and erosion rate across the Alps. Did the authors look at this or can add the corresponding figure if informative?

- Table 1: I would suggest to also indicate the “integration time” for the reported denudation rates. In the footnote, please correct “negative and fast muons” by “slow and fast muons”.

- Table 2: In the footnote, please replace “Pleistocene” by “LGM”. What is the maximum ice coverage during the Pleistocene (do we have evidence ofr more extended glaciations before the LGM in this part of the Alps)?

I hope these comments and suggestions may be useful for revising the manuscript, and I look forward to seeing it published.

Sincerely,

Pierre Valla

Lausanne, 29 June 2016

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