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Earth Surface Dynamics Discussions

Interactive comment on "How steady are steady-state mountain belts? – a re-examination of the Olympic Mountains (Washington State, USA)" *by* Lorenz Michel et al.

Anonymous Referee #2

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This paper by Michel et al. aims at assessing the degree of steady-state of the accretionary prism of the Olympic Mountains. The authors used existing and new thermochronological data, to assess by an inverse model, the evolution of exhumation and denudation during the last 14 Myr that they equate to material outflux. For influx, they consider the rate of plate tectonic convergence multiplied by sediment thickness at the wedge front, and it is implicitly assumed that only frontal accretion contributes to material influx (a largely questionable hypothesis).

The question addressed by this paper is of interest for a large community, and I thank the authors for the efforts they have put in this manuscript. However:

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Discussion paper

1) The method used (inverse model based on Pecube with only 1D temperature and discretized at 1 Myr) is not optimal.

2) The influx reconstruction is not constrained or discussed well enough, in particular the contribution of fluxes other than frontal accretionary fluxes (such as isostasy, etc) or temporal changes in the structure of the wedge are not inferred.

3) The paper is sometimes confusing (e.g., some thermochronological data are not all used in the inversion, the 2D or 3D accretionary fluxes are not necessary (only in 3D or in 2D)).

4) The organization of the paper itself could be better to help its readability (for instance there is no results section on steady-state itself, which is surprising having read the title of the paper).

5) The addition of this paper, compared to established literature (Batt et al., 2001; Brandon et al., 1998; Michel et al., 2018) that already demonstrated a global steady-state over the last 14 Myr and change of exhumation rate at the onset of the glaciations, is not clear to me.

Detailed comments:

L 32-35: A very minor comment: steady-state is time-scale dependent, but it is also spatial-scale-dependent. The likelihood of obtaining a steady-state, for a given time-scale, is likely decreasing when going to finer spatial-scales due to heterogeneities and variabilities in landscape dynamics or tectonics that might become more and more dominant in controlling averaged or integrated values (i.e., mean topography or mountain range sediment discharge). This applies to topographic and flux steady-states. This dual dependency of steady state to time and spatial scales (if correct), implies that defining a steady-state over a time-scale can be a correct or incorrect assumption, depending on the associated spatial-scale. So please be clear and explicit on which spatial-scale you investigate here.

L 51 - Please add a sentence to clearly state in the introduction, what are the main differences, in terms of methods and expected results, of this new manuscript compared to Michel et al. (2018) at Geology. Their abstract ends with: "However, the youngest AHe ages require a 50–150% increase in exhumation rates in the past 2–3 m.y. This increase in rates is contemporaneous with Pliocene-Pleistocene alpine glaciation of the orogen, indicating that tectonic rock uplift is perturbed by glacial erosion." Having read Michel et al. (2018), the differences are not clear to me at this stage of the paper (without having read the following sections).

L 202-207: I don't understand the need to use Pecube if neglecting topography and considering only a 1D model. An inverse modelling strategy using for instance QTQt (Gallagher et al., 2005 and so-on), that can jointly inverse samples from the same vertical profile, seems more appropriate here. This approach also has the benefit of not requiring any a priori time discretization. These advantages prevent the potential for both over interpretation and the introduction of artifacts in the inferred thermal histories.

L 230-236: What about isostasy or dynamic topography: are they not considered in the material influx? This need to be discussed.

L 249-251: There might be some circularity in the rational (to assess steady-state), as sediment thickness depends on the outflux and controls the influx.

L 295: "Our exhumation rates presented in this paper (Fig. 5) have a high temporal resolution". This statement might be overstated. No test was performed on the sensitivity of the inversion scheme and results to the time-step used (only 1 Myr was used). The data, especially individual samples, do not necessarily inform on a temporal evolution at a 1 Myr resolution. Is the inversion misfit better when changing temporal resolution? I would like to see some tests to determine the best time-step for the inversion (at least performed on one sample).

L 510 – section Results - Please add a sub-section at the end of the Results section, to present the results concerning flux steady-state analysis. This is the main ambition

of this paper, and yet there is no result section on steady-state. This does not help the reader to get a clear message from reading this paper.

L 367 – section Discussion - Please add a sub-section to present the limitations of the approaches used in this paper.

L 385-390: For the vertical profile with ZHe data, the change of polarity of the slope is not a robust feature. Models can be defined, satisfying all the ZHe ages and their uncertainties, without leading to a change of slope. However, for AHe, the change of slope seems robust.

L 451: It is assumed in this paper that the geometry of the accretionary wedge is constant and that other processes than tectonic accretion are negligible (an implicit assumption). Could you please discuss: 1) if there were some potential changes in the extent, volume and geometry of the Olympic Mountain accretionary wedge? 2) how you integrate isostasy or dynamic topography in your comparison of in- and out-fluxes? The isostatic response to erosion can generate uplift (with no associated influx in the presented model) and induce additional erosion. This need to be discussed in this manuscript (not as a perspective L533-536).

L 505: “In summary, the assessment of flux steady state in the Olympic Mountains is non-trivial and many scenarios are possible.” The used datasets (thermochronological data, sediment deposits, geometrical structure, etc) are not sufficiently well resolved to offer a robust assessment of temporal changes in fluxes or in steady-state. Therefore, one could question the real addition of this paper compared to Batt et al. (2001), Brandon et al. (1998) or Michel et al. (2018) that have already demonstrated 1) a global steady-state over the last 14 Myr and 2) a potential change in exhumation rate with the onset of Plio-Pleistocene glaciations.

Minor edits:

L 14 : “We present 61 new thermochronometric ages” - Please add: mainly obtained

from 21 new samples (or the correct number).

L 37: “tectonic parameters” – please change by “tectonic conditions” (a parameter implies a quantitative framework/model that has not been defined yet).

L 39: “Plio-Pleistocene glaciation” - There is probably no need to limit the scope to the onset of Plio-Pleistocene glaciation, as older glaciations (for instance at the Eocene-Oligocene transition; Bernard et al., 2016; Thomson et al., 2013) might have also led to variations in denudation and exhumation.

L97: ref “Ehlers et al. 2005” : The closure temperature of these thermochonometers has been constrained in older papers than Ehlers, 2005. For instance: Gallagher et al., 1998; Farley, 2002; ...

L 162: “three/two” - What does three/two mean here?

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