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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 #1

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The manuscript presents new thermochronological data from the Olympic Mountains in Washington State, USA. In addition, the paper presents a quantification of the influx and outflux of material to the mountain range over the last 14 million years, in order to discuss whether this accretionary wedge range is in a flux steady state. The influx of material into the accretionary wedge is based on knowledge from offshore sediment volumes and plate velocities, whereas the outflux is based on an exhumation map from previous thermochronological work in the range.

The topic is interesting and the overall finding represents a new scientific contribution. However, I find it peculiar how the authors present newly obtained data, apparently

without using them in the following calculation of the denudation pattern/outflux. As it is presented now, the paper appears quite fragmented, and one wonders what is really gained form including the new data. In this sense, the paper could just as well be split into two separate papers. One on the newly obtained thermochron data, and one on the flux in and out of the range.

Regarding the flux calculations, more justification is needed for the choice of sediment thickness. It seems like the 1.5 km is taken out of the blue. Also related to this, it should be better explained what the gain is from doing both the 2D and 3D approaches. Why not just do the 3D, and test this with the 2D cross sections? Isn't it obvious that a 2D approach is not ideal when a strong spatial gradient exists in exhumation perpendicular to the 2D section?

In addition, I have several comments outlined below:

Introduction: Lines 56-61: you don't need to summarize the conclusions here in the introduction. Methods: 150-153: Would be great to introduce here already what methods are used for the flux calculations. 182-: Although I appreciate that a 1D approach can give valuable results, I cannot stop to wonder why the authors did not take the full 3D approach, which is the core purpose of Pecube. To the best of my knowledge, all the authors are doing could be done in Pecube in 3D. Using all existing data, they could make an updated exhumation map for the region to be used in their calculations of the outflux. You should as a minimum discuss why it is not feasible to do a full 3D inversion in Pecube. 220-236: this section could be clearer and more up-front about the 2D vs. 3D approaches. Why not just use the 3D approach? What is gained from the 2D? this should be made clear. 252-255: the use of 1.5 km as the minimum for the previous thickness of offshore sediment is not properly justified. It is stated specifically that this is the largest unknown, and right now it seems you have grabbed this number out of thin air. Could there not have been more sediment earlier where you argue for a much higher exhumation rate? Lines 295-298: as mentioned above, it seems odd that you don't want to actually use the data you present here. Either you should use

all available data together, or you can just as well leave the new data out. Results: 312-318: I don't see AFT ages mentioned here? 345: should it not be OP1551 to be consistent with figures? 357: I would argue that the volumes vary with the location in the wedge geometry, latitude is irrelevant. Discussion: Lines 378-382. This is unclear. You start with: "In the absence of a strong lateral gradient"... and end with "due to the strong spatial gradient..."

Lines 401-402: references are needed here, or rephrase to avoid passive voice. Lines 480-482: more likely a lower outflux outside of the profile, is it not?

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