Comment to Referee #1

Note for readers and the editor: Our intention with this reply is to provide a quick response to the points raised by the referee and how we will modify the manuscript. We will provide a single revised manuscript at the end of the open discussion period (expected to end 01.11.2018). This version will address all the comments and suggestions of the two reviewers and possible, upcoming short comments by the community.

Referee comments are in italics, **our response is in bold text**.

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.

We thank the reviewer first for the time she or he has spent on revising the manuscript, and second for acknowledging that the results from this work are new and contribute to this topic. We understand that it seems counterintuitive to present new thermochronometry data and to not include them in the influx and outflux calculations. However, we still use the results obtained from the new data in our qualitative discussion of flux steady-state, where the modeled exhumation histories demonstrate a strong temporal variation in exhumation (equating to a variation in the outflux). However, as discussed in the paper, we can't use these results for our quantitative assessment. We also hesitated to include all of the new data and published data in a new 3D Pecube model for reasons further outlined below.

In the revised manuscript, we will develop the implications of the new data in more detail, so to demonstrate that our findings are stronger with their inclusion (to avoid a splitting into two papers)

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?

These are good and helpful remarks by the reviewer. Indeed, we did not explain in detail why we used 1.5 km as minimum sediment thickness. A more detailed comment on this is provided below and a thorough elaboration on this will be included in the revised manuscript.

This comment highlights that we did not convey clearly enough our approach for performing both a 2D and 3D flux calculation. Our intention with providing both 2D and 3D flux analyses stems from the fact that the previous attempts for estimating the flux balance in the Olympic Mountains were performed in 2D (e.g. Batt et al. 2001). As mentioned by the reviewer, the spatially variable exhumation rates suggest that the outflux in this mountain range is highly variable, depending on the actual position in the mountain range. Therefore, our intention was to perform an analysis with the established 2D setup and to compare this with an analysis based on a 3D geometry and to see whether different outcomes are obtained. As we describe in the manuscript, on long timescales flux steady-state is only achieved using a 3D geometry, whereas the 2D geometry requires unrealistic parameters (or is an oversimplification). We believe, that this is an interesting outcome, given that the previous analysis has been done in 2D.

In the revised manuscript, we will explain in more detail our approach in the methods section and discuss in more detail what is gained from performing both 2D and 3D analyses in section 5.2.

Introduction: Lines 56-61: you don't need to summarize the conclusions here in the introduction.

This seems to be a choice of style. Since the manuscript has not reached a space limit, we prefer to leave the introduction as it is.

Methods: 150-153: Would be great to introduce here already what methods are used for the flux calculations.

Thank you for pointing this out, we will insert a brief introduction to our used flux calculations at this section of the revised manuscript.

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.*

Indeed, the prime purpose of Pecube is to perform full 3D inversions of thermochronometric datasets. After inspecting our thermochronometric dataset (particularly the samples with multiple thermochronometer systems available), we suspected a possible strong variation in exhumation rates, because of the observed ages (e.g. young AHe ages, overlapping, 5–7 Ma old AFT/ZHe ages). Hence, our prime intention was to evaluate this hypothesis. To perform a full 3D model in Pecube, an exhumation rate pattern, an exhumation history (so temporal variations in exhumation) and also topography need to be prescribed in advance. In the 1D model, thousands of possible exhumation histories are explored with the Monte-Carlo algorithm, yielding a best-fit history for the observed thermochronometer ages for the respective sample. The advantage to this method is that histories with variations in exhumation rates can be robustly constrained (and do not have to be guessed in advance by the modeler in a 3D approach, which could introduce a bias). We use the results obtained by this approach for our qualitative assessment of the flux steady-state, where we argue for temporal variations in the outflux, because also the exhumation rates are temporally variable.

As mentioned by the reviewer, we could use all existing thermochronometric data and use the temporal evolution of exhumation rates obtained from the 1D modeling to set up a 3D model. However, the differences in exhumation rates between the seven samples modeled with 1D (e.g. the timing and magnitudes in variation of exhumation rates) would be difficult to account for in a 3D model, and large misfits between modeled and observed ages would be expected, if all existing thermochronometer ages (from this work, as well as literature) are included in a 3D model. Such misfits could be expected given that samples are located in different parts of the accretionary wedge, which are probably separated by faults. Setting up a 3D Pecube model with faults on top of the ellipse exhumation rate pattern in order to simulate different parts of the accretionary wedge would be a paper on its own and many additional parameters would need to be constrained. Hence, we did not include a model like this in the current manuscript.

For our quantitative outflux analysis, there would also be no benefit from an updated exhumation map (with an additional temporal variation in exhumation). The integrated amount of exhumation considered during the 14 Myr period (this value is used during the outflux calculations) would still

be the same as with the current exhumation rate map (i.e., even a more sophisticated model will still result in the same amount of exhumation).

In the revised manuscript, we will include a more thorough discussion (including the remarks above) about the concerns raised by the reviewer.

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.

See our response to the reviewer's comment above.

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?

Thank you for pointing this out. We agree with the reviewer, that the minimum sediment thickness of 1.5 km needs to be better justified. A more thorough way of constraining this parameter is explained in the following, and we will modify the manuscript to explain this better. More specifically, during the entire history of sediment accretion at the deformation front since ~40 Ma, the sediment thickness was likely different from the reported 1.5 km, but to get constraints on the sediment thickness for this entire period is difficult, as most of it has already been incorporated in the accretionary wedge. However, we focus our analysis on the past 14 Myr, where obtaining constraints is easier. As shown in Figure 3 of the manuscript, the oceanic crust subducted at present is very young (~6–9 Ma). The young age together with the fast subduction rate (which was even faster prior to 6 Ma) prevent the accumulation of a thick succession of sediments on top of the oceanic crust (e.g. the pre-Quaternary sedimentation rates obtained from the ODP boreholes are around 80–110 m/Myr, Table 1). Assuming it takes the oceanic crust about 9 Myr to reach the deformation front (the oldest oceanic crust at the deformation front is currently 9 Ma old) would yield a sediment thickness of ~700–1000 m at the deformation front. However, this is likely an underestimation of the actual thickness, because the inherent assumption for this calculation is that the spreading rate and convergence rate stay constant over time. Furthermore, the sedimentation rate likely increases closer to the deformation front, because more detritus is delivered through submarine canyons and turbidity currents (the ODP boreholes were drilled on the deep sea plain or on a submarine fan ~120 km away from the deformation front, Figure 3). Therefore, using a minimum sediment thickness of 1500 m for pre-Quaternary times should correspond to a good estimation of this otherwise difficult to constrain parameter.

As the reviewer mentions, we observe high exhumation rates at ~6 Ma, which consequently yields a higher sediment supply to the ocean. But the high exhumation rate period is followed by a period of very slow exhumation rates from ~6–2 Ma (Figure 8), so the sediment supply to the ocean will also decrease. Furthermore, the sediment material entering the accretionary wedge at the deformation front is a mixture of material delivered from different sources (e.g. Olympic Mountains, Vancouver Island, Canadian Cordillera). Variations in exhumation rates in these different source regions will also result in variable amounts of sediment from the respective source region, however the effect of these variations on the sediment thickness is difficult to constrain.

A more thorough elaboration on the used sediment thickness will be included in the revised manuscript.

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.

We understand the concern of the reviewer, to not use the presented data in all of our analyses. As outlined in the comments above, we are unfortunately not able to include the data in a new 3D Pecube model, to obtain an updated exhumation pattern (for the quantitative flux calculations). However, we still use information obtained from the new data for qualitative flux analysis, clearly showing variations in exhumation rates. The change in exhumation rates at ~6 Ma due to a change in convergence rate is also a new observation, that has previously not been reported for the Olympic Mountains and we will clearly state this in the revised manuscript.

Results: 312-318: I don't see AFT ages mentioned here?

Thank you for indicating this, the AFT ages will be included in the revised manuscript.

345: should it not be OP1551 to be consistent with figures?

Indeed, this typo will be corrected in the revised manuscript.

357: I would argue that the volumes vary with the location in the wedge geometry, latitude is irrelevant.

We will consider the suggestion and replace "latitude" in the revised manuscript.

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. . .”

Our intention was to quickly introduce the classic way of interpreting an age-elevation transect (which requires the absence of a lateral gradient in exhumation). However, this is not possible in the Olympic Mountains (due to the spatial gradient in exhumation rates). We will rephrase this paragraph in the revised version of the manuscript, to make it clearer.

Lines 401-402: references are needed here, or rephrase to avoid passive voice.

References to Michel et al. 2018 and Brandon et al. 1998 will be included in the revised manuscript.

Lines 480-482: more likely a lower outflux outside of the profile, is it not?

Thank you for indicating this, we will include this in the revised manuscript.