Comment to Referee #2

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 after 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**.

*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:*

**We thank the reviewer for his time she or he spent on reviewing our manuscript and that she/he agrees, that this topic is of interest for a large community.**

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

**It appears we did not succeed in conveying our main purpose of using this modeling approach. We comment in detail on this concern in our responses below and show how we will address this in the revised manuscript.**

*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*.

**These are interesting remarks that definitely need to be addressed. We provide a more detailed response in the comments below.**

*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)).*

**Thank you for pointing this out. We only used the thermochronometry data from seven samples in our modeling, because these seven samples are multi-chronometer samples, so that three or four thermochronometer systems (AHe, AFT, ZHE, ZFT) are available for each sample. We omitted the remaining samples, because for these only one or two thermochronometer systems are available (e.g. AHe and ZHe) or most of the thermochronometer systems are unreset. Hence the seven considered samples yield the best-constrained exhumation histories. Relying on these specific samples provides the most consistent framework for equal comparison of exhumation histories across the range.**

**Our purpose for providing influx calculations both in 2D and 3D was to tie in with previous calculations of flux steady-state, which were performed in 2D (Batt et al., 2001) and to test whether the assumed spatial geometry is important for the flux steady-state analysis. Indeed, the calculations both in 2D and 3D showed that different results are obtained, depending on the assumed geometry.**

**For the revised manuscript we will explain in more detail the reasons behind our sample selection for the inversion and why it matters, whether the flux analysis is performed in 2D or 3D.**

*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).*

**In section 4.3 we provide results from our quantitative influx and outflux calculations but limit our explanations to only reporting the numbers. Any further elaboration (e.g. direct comparison between influx and outflux volumes) would already be discussion, which can be found in section 5.4. This also pertains to the qualitative comparison between influx and outflux, based on variations on parameters like plate convergence rate, sediment thickness and exhumation rates.**

*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.*

**With our approach of using multi-chronometer samples we are able to report a remarkably rich temporal exhumation history that represents a significant new contribution. Besides the increase at ~2–3 Ma (already reported by Michel et al. 2018) there is a decrease in exhumation rates occurring at ~6 Ma due to the decrease in plate convergence rate. Since the contribution of both tectonics and climate to the evolution of mountain belts is still a matter of active debate in the geoscience community, this**

**result presented in the manuscript is of interest also to a broader readership.**

**In a previous study, Batt et al. (2001) concluded that the Olympic Mountains are in flux steady-state, because they successfully modeled their observed thermochronometer ages with a thermo-kinematic model (including horizontal velocities). However, a balance between influx and outflux is a basic precondition in deriving the mathematic equations for the model. Therefore, a flux steady-state balance is not directly tested, but deduced from their successful modeling. One might also argue that due to the non-uniqueness of modeling approaches (i.e. different models yield the same answer, depending on the considered parameter space), a direct corroboration of flux steady-state in the Olympic Mountains is still missing.**

**For our approach we tried to directly calculate both the influx and the outflux independently from each other, considering new constraints for parameters like sediment thickness, plate convergence rate and denudation rates. We also considered the temporal variations in these parameters, which has so far not been done. In addition to Brandon et al. (1998) and Batt et al. (2001) who focused their analyses on the long-term flux steady state over 14 Myr, we show that (at least qualitatively) strong temporal variations in influx and outflux occur on short timescales.**

**Hence, we believe that the results in this manuscript correspond to new results that complement the established literature and are of interest to a broad readership. As the reviewer also mentions in another comment below, we should more clearly explain how our methodology/approach differs from the established literature. We will do this in the introduction of the revised manuscript.**

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.*

**Thanks for this important remark. Indeed, as we showed with our flux steady-state analysis (performed both in 2D and 3D), the spatial geometry**

**considered for the analysis is important. We will be clearer in the revised manuscript, which spatial geometries we investigate.**

*# 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).*

**Michel et al. (2018) focused on presenting results from thermochronometric dating (AHe and ZHe) and thermo-kinematic modeling, documenting the role played by the subduction zone geometry in driving rock uplift and the impact of Plio-Pleistocene glaciation (increased exhumation rates due to glacial erosion). A direct test of flux steady-state was not the premise of that paper.**

**In the present contribution, we capitalize on multi-dating samples (additional AFT and ZFT ages), revealing an additional temporal variation in exhumation rates, which could not have been disclosed by Michel et al. (2018) due to the missing FT data. We also assess the flux steady-state balance by investigating both the influx and the outflux independently from each other.**

**To address the concerns of the reviewer we will convey more clearly in the revised manuscript what discerns the new contribution from previous work.**

*# 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*.

**It is important to emphasize that models such as HeFTy and QTQt are purely thermal models. These models are designed to be very good in calculating temperature-time paths based on thermochronometry information. However, for the purpose of this study we are exploring kinematic information (e.g., exhumation), and therefore, require a thermal-kinematic model such as Pecube. Furthermore, as we try to discuss in section 5.1 of the manuscript the information that can be obtained from our**

**age-elevation transects is only limited due to the horizontal extent of the transects and the strong lateral gradient in exhumation rates within the Olympic Mountains. So, although vertical profiles can be directly included in QTQt, this might not yield ambiguous results.**

**Previous exhumation rates reported by Michel et al. (2018) for the Olympic Mountains were also derived by thermo-kinematic modeling using Pecube (however by using a 3D model). To tie in with previous work, we again used Pecube, but due to the suspected temporal variations in exhumation (as suggested by the thermochronometry data) we used the 1D model option coupled with a Monte-Carlo algorithm. This approach has successfully been applied in other studies to derive exhumation rate histories, e.g. Ehlers & Thiede (2013), Adams et al. (2015), Avdievitch et al. (2018).**

**The issue mentioned by the reviewer regarding the timestep discretization is a good remark and requires more explanation. Our detailed response to that point can be found below and will be included in the revised manuscript.**

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

**We comment in detail on these points in our response below.**

*# 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.*

**We thank the reviewer for pointing this out. This indeed is an aspect that represents a limitation to our approach. As we described in section 2.3 of the manuscript, the sediment currently entering the accretionary wedge is a mixture of sediment with different source regions (e.g. Olympic Mountains, Vancouver Island, Canadian Cordillera and in case of the Astoria fan the interior USA). Particularly with the onset of Plio-Pleistocene glaciation this effect became more pronounced, due to increased detrital input from the Cordilleran Ice Sheet. Hence, our influx/outflux calculations for the Olympic Mountains do not represent a closed system, where increased denudation in the Olympics (so a higher outflux) directly results in a higher influx (due to an increased sediment thickness).**

**However, our calculations indicate that on long timescales (i.e., the 14 Myr considered in the calculations) flux steady-state is reached, which might seem surprising given that the sediment thickness is governed by contributions from different source regions. We suspect that processes during sediment deposition, like redistribution by turbidity currents and re-deposition in more proximal parts of the Juan de Fuca plate, play an important role in the final sediment budget. In other words, the amount of**

**sediment eroded from the Olympic Peninsula in a given time period (the outflux) is dispersed as it enters the ocean, so that for the same time period only a fraction of the sediment thickness (governing the influx) is composed of material originating from the Olympic Peninsula.**

*# 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).*

**Our apologies, if our wording caused some misunderstanding. With this sentence we wanted to state that our 1D model is well suited to resolve temporal variations in exhumation rates, because the model covers a large range of possible exhumation histories. In comparison, the exhumation rates used in calculating the denudational outflux (the rates presented in Michel et al. 2018) have a good spatial coverage, but the 3D model is not suited to investigate a large range of possible exhumation histories. In the revised manuscript we will change the wording, so that our intension for using the exhumation rates during the flux calculations is more evident.**

**Nonetheless, the concerns of the reviewer regarding our chosen time-step interval are justified and need to be addressed. We will perform tests for a sample using different time steps. However, the time period resolved by our thermochronometer ages varies between 5 and 10 Myr for the seven considered samples, placing a threshold to possible durations of the time steps (e.g. durations longer than 3 Myr seem not to be reasonable). We also note, that although our reported exhumation rate histories in Figure 5 seem very detailed and suggest a high temporal resolution, these step-like patterns would likely be much smoother in reality. Hence our summary of exhumation rates in Figure 8 uses a more smoothed shape.**

**In the revised manuscript, we will present results from the sensitivity analysis of the time-steps.**

*# 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.*

**See our response to comment 4).**

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

**A good suggestion by the reviewer. We will include a section in the revised paper, which addresses the aspects mentioned by referee #1 and #2 (e.g. limitations of our influx and outflux calculations, limitations of the exhumation rates).**

*# 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.*

**We agree with the reviewer that a line can also fit the ZHe ages (without a change in slope). In the revised manuscript, we will change this. Our further interpretation that the age elevation transects only yield limited information on exhumation rates should still be valid.**

*# 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).*

**We thank the reviewer for raising this issue and we will discuss it in more detail. For our quantitative assessment of flux steady-state we try to calculate the actual volumes of influx and outflux during the 14 Myr period. The two calculated fluxes should be viewed as pure volumes and separately from each other. Flux steady-state is attained, if both volumes (calculated for the 14 Myr period) equal themselves. We base our outflux calculations on the spatial integration of the exhumation rate pattern presented by Michel et al. (2018) and also consider the increase in exhumation due to the Plio-Pleistocene glaciation. As influx we consider all sediment resting on the subducting oceanic plate (so the influx is governed by the sediment thickness and the plate convergence rate). We assume that the mechanism of actual accretion (i.e., frontal accretion vs. sedimentary underplating) is not important, because finally all sediment is incorporated into the accretionary wedge and contributes to the influx volume, irrespective whether accretion takes place at the front of the accretionary wedge or at depth. As we discuss in the manuscript there are no estimates available on how much sediment might be transported into the mantle, so this is a limitation to our approach.**

**The impact of isostasy to the influx volume is not obvious to us. Any isostatic response (e.g. due to increased erosion during the Quaternary)**

**affects the mountain range itself (so to say the volume of already accreted sediment material), but does not directly affect the influx volume. A possible increase in erosion related to isostatic uplift of course affects the outflux, as the reviewer noted (and in turn increases the influx, due to a higher sediment yield). If there was a significant change in rock uplift rates created by a change in isostatic compensation, then this would have likely changed exhumation rates, which would be recorded by our chronometers and seen in our modeling results. Therefore, there is no reason to directly account for isostatic compensation in our thermo-kinematic models.**

**Effects of the Plio-Pleistocene glaciation are already included in our calculations (see Table 5), like an increase in sediment thickness (affecting the influx) and an increase in denudation rates (affecting the outflux). So one could argue, that a possible contribution of the effects of isostatic uplift is already included in our calculations (but we can't quantify the exact contribution of isostasy). In total, the observable effects of Plio-Pleistocene glaciation are likely the combined effects of increased sediment supply (particularly by the Cordilleran Ice Sheet), and increased exhumation rates (due to glacial erosion).**

**The reviewer also asks, if there were some potential changes in the extent, volume and geometry of the accretionary wedge. These parameters are affected by the amount of accreted sediment and the dip of the subducted plate. As shown in Figure 1 the subducted plate displays a bend below the Olympic Mountains, resulting in a flatter angle of subduction compared to areas in the north or south. If bending the plate occurs through time (the reason for bending is hypothesized to be extension in the Basin and Range Province, Brandon & Calderwood 1990), then this indicates that the angle of subduction below the Olympic Mountains is likely variable through time and hence the volume and extent of the accretionary wedge is varying through time as well. Our reported cross sections (Figure 7) show that the volume and shape of the accretionary wedge is spatially variable. As stated in our manuscript, a change in the width and mechanics of the offshore part of the wedge has been observed due to increased sedimentation during the Quaternary (e.g. Adam et al., 2004). So, the temporal and spatial evolution of the accretionary wedge is complicated. However, our flux steady-state calculations should be independent from any changes in the shape of the accretionary wedge. As we outlined above, we only look at the calculated volumes of influx and outflux, which do not depend on any parameter affecting the shape of the accretionary wedge.**

**For the revised manuscript, we will include a discussion of the above points, likely in a section about the limitations of our approach, as also suggested by the reviewer.**

*# 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.*

**In our response to the reviewer's comment 5) we already outlined what we view as new contributions. We fully understand that the reviewer is critical on this point. The Olympic Mountains have long been viewed as a case example for a (flux) steady-state mountain range. As we tried to convey in our manuscript our approach shows that both influx and outflux are subject to temporal variations (but as the reviewer notes, we can not provide a full, quantitative assessment on small temporal scales). We believe that the results reported in this paper will stimulate further investigations of (flux) steady-state, so that in the future better and further constraints are available for parameters, which we could not include in detail (e.g. sediment data, margin parallel velocities). This could contribute to a new perspective and understanding of steady-state in active mountain ranges.**

*Minor edits:*
*# L 14 : "We present 61 new thermochronometric ages" - Please add: mainly obtained from 21 new samples (or the correct number).*

**As requested, we will change this in the revised manuscript.**

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

**Good remark, will be changed in the revised manuscript.**

*# 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.*

**In the revised manuscript, we will no longer limit this to the Plio-Pleistocene glaciation and include references to earlier glaciations.**

*# 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; ...*

**Additional references will be included in the revised manuscript.**

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

**This indicates the number of samples collected for this study, which have been dated with AFT and ZFT, respectively. We will reword this in the revised manuscript.**