Response to Reviewer 1

R1C1:

> *"In this study, the authors use a thermo-mechanical numerical model to investigate the thermal evolution, and in particular the thermal maturity, within forming accretionary prisms. The numerical model represents a mantle-scale subduction model and thus includes a (more) sophisticated thermal and isostasy model compared to higher resolution but dynamically simpler wedge models. Based on the thermal model and parameters specifically adjusted to fit borehole data from the Nankai through, the vitrinite reflection parameter %R_0 is computed based on three different existing models. The main conclusion of their work is that the evolution of %R_0 within accretionary wedges is strongly affected by thrusting, which is also observed in vitrinite reflection data from a borehole from the Nankai Trough. The general idea of the paper is intriguing and allows to interpret the temporal and spatial evolution of a parameter, here thermal maturity through vitrinite reflectance, that in field measurements remains one-dimensional. This implies that the strength of the work is its applicability to natural systems, which comes a bit short. Below, I comment on several points that in my opinion need improvement for the paper to be accepted. I also attach the pdf of the manuscript with individual comments. The main points circle around the introduction of the numerical model and the comparison to natural data. Furthermore, there are many small errors and lack of clarity throughout the manuscript and writing has to be improved. Based on the comments below and in the attached pdf, and my general impression, I recommend major revision before reconsidering the paper. I hope my comments are constructive and helpful*
> *Best wishes, Jonas"*

Response R1C1:

We would like to express our sincere appreciation to Dr Jonas Ruh, Reviewer 1 (R1), for conducting a thorough review of our study and providing invaluable insights. We agree with R1's summary of the manuscript and have diligently addressed his feedback through the following significant revisions.

1. We have included additional geological context in the manuscript to provide a comprehensive introduction to the Nankai subduction zone and previous observations regarding thermal maturity in the area. Section 2 (Geological setting and model generalization) and Section 5.4 (Comparison to natural wedges) were added to the revised manuscript to specifically address the lack of geological context in the original manuscript.
2. The method section(Section 3) has been expanded to encompass details on model setup, governing equations, and a comprehensive explanation of thermal maturity computation.
3. Several figures and supplementary figures have been updated, and we have included additional supplementary figures. For instance, we have added Fig 1 illustrating the initial model setup, Fig 2. Showing a general evolution of a typical model, Fig S4-S13 for lithological, thermal and thermal maturity of each model.

4. Furthermore, we have incorporated Table 3 to illustrate the parameters used for thermal maturity computation. We have added Section 3.5 to give theoretical details of the thermal maturity evolution in each model.
5. The discussion section has been updated with distinct sections to enhance clarity and organization as suggested by R1.
6. Additionally, we acknowledge that our previous manuscript contained inaccuracies resulting from incorrect computation of the arctan function. Consequently, the reported values for the angle of friction and surface slopes were erroneous. We have rectified these errors and now provide a comparison of the observed surface slopes with those computed using critical wedge theory.

We hope that these modifications have improved the quality and scientific rigour of the manuscript and we would be happy to address additional comments from the reviewer.

R1C2:

> *"1) Model setup. The paper consists to >90% of modelling results and therefore needs a proper introduction of how the model was set up and how the different routines are implemented. As the present paper is ultra-short, I see no reason why not to extend the model setup section by a proper introduction of the numerical but also the %R_0 model. For example, move the model setup part of the Supp Mat to the main manuscript and show a general tectonic evolution of such a model including isotherms so that a reader that is not that familiar with geodynamic modelling can understand."*

Response R1C2:

We agree with the reviewer and therefore have moved a lot of material from the supplementary section to the main text. This includes sections on governing equations(Section 3.1), the rheological model(Section 3.2), Boundary conditions(Section 3.3), Surface processes (Section 3.4) and Model setup(Section 3.5). Additionally, we have included added to the section on thermal maturity computation for Easy%Ro, Simple%Ro and Basin%Ro as well as the geological setting(Section 2). We have also added figures describing typical model evolution (Fig 2) and model set-up (Fig 1) to the main text. Please go through Lines 141-308 of the revised manuscript for more details.

R1C3:

> *"Also better introduce the %R models and describe their differences. I personally would also have liked to see how %R is calculated to later better understand their differences (what is the temperature dependence etc.). "*

Response R1C3:

We agree with the reviewer and therefore have added Section 3.5 on thermal thermal maturity computation for Easy%Ro, Simple%Ro and Basin%Ro detailing their computational methodology and their relationship to temperature. In addition, we have added Table 3 listing the parameters used for each method. Please go through Lines 238-265 of the revised manuscript for more details.

R1C4:

> *"Furthermore, there are some errors and ambiguities in the choices of parameters and the model description in the Supp Mat. First of all, décollement strength ranges from 2° to 22°, while internal wedge strength is unclear. This means, the wedge strength is defined by the faults, which after a strain of 1.5 have a friction angle of 15 or 20 (text and table differ)."*

Response R1C4:

We regret reporting an erroneous table in our earlier manuscript. We have updated the table now along with an expanded set of parameters. The décollement strength ranges from 4.5° to 14.5°. The friction angle after a strain of ~1.5 is 20°. Additionally, we have updated erroneous mentions of those values in the main text also. Please go through Table 1, Table 2 and Section 3.6 on Experimental strategy in Lines 294-308 for more details.

R1C5:

> *"Furthermore, the tables itself are contradictory. Table 1 says décollement 0.03/0.08, which is nothing close to 22°, and sediment strength with a friction coefficient of 4.64, which is out of range. Table 2 gives friction angles that are not matching those parameters."*

Response R1C5:

We again regret reporting an erroneous table in our earlier manuscript as stated in R1C4. We have updated Table 2 now and erroneous mentions of those values in the main text.

R1C6:

> *"Also, in the Supplementary material, the equation for Mohr-Coulomb friction is wrong. I guess it should be Drucker-Prager (P in equation 9 is mean stress, not lithostatic stress as in the Mohr-Coulomb formulation), in this case missing a cos(phi) multiplied with cohesion. Otherwise one gets wrong geometric fault angles."*

Response R1C6:

We are thankful to the reviewer for pointing out this error. We have corrected this by providing the correct Drucker-Prager formulation in the rheological model. As mentioned in

R1C4 we have also moved supplementary material on the rheological model in section 3.2. Please go through Lines 190-205 of the revised manuscript for more details.

R1C7:

> *"I was also pretty lost with the sedimentation process. Although nicely introduced in the supplementary material, it remains enigmatic when only reading the manuscript."*

Response R1C7:

We agree with the reviewer and have added the surface process in Section 3.4 mentioned earlier in the supplementary section to the revised manuscript. Please go through Lines 216-236 of the revised manuscript for more details.

R1C8:

> *"2) Comparison to natural systems. The authors argue that the strength of the paper is its application to natural systems, but the paper only mentions one borehole to which it compares well. Since thermal parameters are implemented from that borehole that is not unexpected. Although the borehole data occupies a prominent position in this work, it is not really introduced."In my opinion, the paper would gain a lot of strength if it presented a proper section in comparison to previous work and natural examples on the topic."*

Response R1C8:

We agree with the reviewer and have added the geological setting (Section 2) to the manuscript where we have also introduced the C0002 borehole. Additionally, we have introduced Section 5.2 in the manuscript discussing comparisons and implications of our model with the natural wedges. Please go through Lines 107-140 and 561-614 of the revised manuscript for more details.

R1C9:

> *"Also, a comparison to other numerical models that investigate thermal properties of shallow subduction zone dynamics is missing. For example Sepideh Pajang's work in Solid Earth, as a counterpart to mantle-scale models (just an example)."*

Response R1C9:

We agree with the reviewer and we have introduced Section 5.3(Comparison to previous numerical studies) in the revised manuscript. Please go through Lines 526-559 of the revised manuscript for more details.

R1C10:

> *"3) Discussion. Large parts of the discussion are rephrasing the results and redundant. I think the discussion would benefit from a separation of subsections that focus on different topics, for example Importance of thrusting on …., comparison to natural examples, comparison to previous work, and even implications for prospection or so, as it is mentioned to be of importance in the introduction."*

Response R1C10:

We agree with the reviewer and we have separated the discussion into subsections in Section 5.1 Thermal maturity distribution and importance of thrusting in wedges, 5.2. Implications Section 5.3 Comparisons to previous numerical studies and Section 5.4 Comparisons to previous natural wedges. Please go through section 5 of the revised manuscript for more details.

Minor comments:

Besides the suggestions above, I commented directly into the attached pdf.

R1C11 (L 51):

> *"I would add an introduction on the thermal maturity in the Nankai, or why would you take values from there?"*

Response R1C11:

We agree with the reviewer and we have introduced Section 2 in the manuscript about the geological setting of the Nankai subduction wedge. In the second paragraph, I also introduce the thermal maturity measurement in the Nankai accretionary wedge and the adjoining Shimanto accretionary wedge. Please go through section 2 of the revised manuscript for more details.

R1C12 (L 64):

> *the sediments are the metamorphic rocks, aren't they? It is phrased a bit unclear*

Response R1C12:

We thank the reviewer for informing us about the factual error in the manuscript. We have corrected the error in the updated manuscript. However, due to the heavy restructuring of the introduction section, we do not have the same sentence in the manuscript anymore.

R1C13 (L 72-79):

> *"This is a long and complicated sentence. Why not something simpler, first introducing the effects of surface processes already known. There are quite a lot of papers that are here important, because they specifically investigated the effect of surface processes on the thermal evolution of accreting sediments. Meaning, elaborate on what has been done by the cited work. For example: "A number of studies investigated the effects of surface processes on the thermal evolution of sedimentary systems under convergence. For example, blabla " And then point out what is missing and why this work is necessary."*

Response R1C13:

We agree with the reviewer and we have modified the paragraph. The section now describes the work of each of the cited studies in detail and discusses its relevance to the presented study. The paragraphs(Lines 78-97) now reads as

"Although there is general consensus on the rate and extent of sediment trajectory transition from horizontal to vertical during accretion, the dynamic perturbations in sediment dynamics have yet to be adequately examined. For instance, while most studies show a great degree of correlation between the initial depth of incoming sediments and their final position in the wedge (e.g., Mulugeta and Koyi, 1992; Willett, 1992), a dynamic fluctuation in this correlation due to thrusting can result in non-stationary exhumation paths for accreting sediments in a wedge (e.g., Konstantinovskaia and Malavieille, 2005; Miyakawa et al., 2019). Much remains to be explored regarding the partition of high and low thermal maturity paths and how sediments travel inside natural wedges, given the conventional assumption that accreting sediments remain at the same relative depth and translate along the adjacent "layers" without vertical mixing throughout the tectonic evolution of the wedge (Hori and Sakaguchi, 2011).

Our assessment identifies a primary gap in existing research: the prediction and mapping of the initial sediment influx to their final location in the orogenic wedge. More specifically, the challenge lies in determining which portions of incoming sediment will predominantly constitute the core of the wedge and which will reside at comparatively shallower depths. Given that the maximum exposure temperature estimation from the thermal maturity is inherently reliant on the path of sediments inside the wedge, information on path diversity would inherently constrain the uncertainty in maximum exposure temperature used for the identification of paleothermal structures of subduction zones. Moreover, to better understand the time-depth paths of wedge sediments, their dependence on the initial state of undeformed sediments, and thus their thermal maturity, the factors that control the evolution of subduction-accretion systems, like sedimentation, erosion, and décollement strength, ought to be considered (Mannu et al., 2016; Simpson, 2010)."

R1C14 (L 72-79):

> *"No need for references here. May put them above when introducing what has been done yet"*

Response R1C14:

In accordance with the reviewer's feedback, we have revised the paragraph accordingly. Moreover, we have made a concerted effort to predominantly place the references at the end of the sentences, as suggested by the reviewer, to enhance the readability of the manuscript.

R1C15 (L 83):

> *"You may write that here but how is the presented model more realistic?"*

Response R1C15:

We agree with the reviewer that using the term realistic is not appropriate here and hence we have dropped it in the sentence. We have now restricted the use of realistic geothermal and thermal conductivity profiles where we emulate the empirical trends from the C0002 borehole.

R1C16 (L 93):

> *"is it conservative since 2019?"*

Response R1C16:

We thank the reviewer for rightly pointing this out about I2VIS. I2VIS has been conservative since 2003. We have modified our citations accordingly. The sentence(Lines 141-142) now reads "*We employ I2VIS, a conservative finite-difference 2-D thermomechanical subduction-accretion model with visco-plastic/brittle rheology (Gerya and Yuen, 2003a, 2003b).* "

R1C17 (L 104):

> *"this sentence can be phrased generally, not only focusing on wedges"*

Response R1C17:

We agree with the reviewer and we have modified the paragraph accordingly. The sentence (Lines 170-171) now reads "*In order to accurately assess thermal maturity, it is crucial to* *consider the temperature distribution, which necessitates a realistic thermal conductivity profile when modeling thermal maturity."*

R1C18 (L 105):

> *"Maturity?"*

Response R1C18:

We would like to extend our apologies to the reviewer as we did not fully comprehend the specific correction that was expected in this particular instance. However, we assure the reviewer that the updated manuscript has undergone substantial restructuring to address various aspects of the review, and we have made every effort to address the reviewer's intended corrections as per our understanding.

R1C19 (L 115):

> *"that sounds strange, since the other model is called "simple""*

Response R1C19:

We agree with the reviewer and we have modified the paragraph accordingly. The sentence (Lines 256-258) now reads "*In the interest of clarity, we have mostly illustrated Easy%R0, which is the most extensively used method for Vitrinite Reflectance computation and hereafter we refer Easy%Ro as %Ro, unless explicitly stated.*"

R1C20 (L 128):

> *"You never mentioned that there is a décollement layer with a lower strength"*

Response R1C20:

We agree with the reviewer and have now added in section 3.5 on model set-up where we mention the décollement layer with lower strength. The relevant sentence (Lines 276-279) now reads *"We simulate this with a predefined configuration at the interplate, with a 350-meter-thick weak décollement below a km thick sediment layer."*

R1C21 (L 130):

> *"the investigation of these parameters requests the introduction of their implementation in the main text, not only in the Supp Material"*

Response R1C21:

We agree with the reviewer and we have expanded the method section(Section 3) to include, Model setup, Governing equations, rheological model and surface processes.

R1C22 (L 130):

> *"values of friction angle? Also here, an intro is needed of how and what that means. Mohr-Coulomb? Or what does that mean? What is cohesion? What is the fluid pressure?"*

Response R1C22:

We agree with the reviewer and we have expanded Tables 1 and 2 to include information on all these parameters. Additionally, we have expanded the method section to detail the role of cohesion, angle of friction and fluid pressure in the determination of wedge rheology.

R1C23 (L 133-134):

> *only at the trench you mean only in front of the frontal thrust?*
>
> *This has to be introduced clearer.*

Response R1C23:

We agree with the reviewer and we have modified the Section on the Experimental Strategy(Section 3.6) to reflect this. The relevant sentences (Lines 300-303) added in the model set-up are as follows "*In all the models presented in this study, sedimentation is limited to the trench, extending from the sea to the land. Restricting sedimentation to the trench allows us to observe and analyse the length and frequency of thrust sheets, enabling comprehensive investigation of their role in determining sediment trajectories..*"

R1C24 (L 133-134):

> *Reason? same for décollements?*

Response R1C24:

We would like to extend our apologies to the reviewer as we did not fully comprehend the specific correction that was expected in this particular instance. However, we assure you that the updated manuscript has undergone substantial restructuring to address various aspects of the review, and we have made every effort to address the reviewer's intended corrections as per our understanding.

R1C25 (L 138):

> *the coefficient of friction or the friction angle? Not the same…*

Response R1C25:

We regret the mistake here and thank the reviewer for pointing this out. We have corrected the sentence (Lines 306-308) in the updated manuscript as *"Sediments used in the model have an angle of friction (φ) of 30° and a strain-softened value of 20° after a threshold of 0.5-1.5 strain. The coefficient of friction (tan φ) increases linearly between the strain thresholds."*

R1C26 (L 138):

> *in the table it is 15°*

Response R1C26:

We agree with the reviewer and we have modified Table 2 to correct the mistake.

R1C27 (L 140-142):

> *"from the text I thought sedi rate is varied between 0.1-0.9. But here you state that you cannot pre-define these rates. Then you have to re-phrase above, otherwise it is misleading"*

Response R1C27:

We agree with the reviewer and we have modified the paragraph on the surface process to reflect this. The relevant paragraph(Lines 224-236) now reads "*The shape of the basin and the resolution of the surface grid can lead to overfilling or underfilling when using the equation mentioned above to fill the basin. To address this issue, we calculate the volume of deposited sediments and adjust for any deficit or overfill in the subsequent step. This ensures that, over time, the total amount of sedimentation remains consistent with the prescribed* *value. However, it is challenging to ensure that all sediments added in a particular step are accommodated within the basins, especially in models with high sedimentation rates where significant runoff occurs. Therefore, the sedimentation rates mentioned in this study are computed as effective sedimentation rates after the model run, rather than being predetermined. We perform multiple model runs (approximately 100) with sedimentation rates uniformly distributed in the range of 0.1-0.9 mm/yr. From these runs, we select models that exhibit appropriate sedimentation rates.* "

R1C28 (L 144):

> *this sentence to me doesn't do much if there isn't a figure supporting it. Some videos in supp info are not enough.*

Response R1C28:

We agree with the reviewer and added Fig. 2 elaborating the thermomechanical evolution of a typical wedge. Additionally, we have added supplementary figures (Fig S4-S13)for the evolution of each model. Additionally, we have added a description of how each parameter such as slope angle measured in our models. We also have added a supplementary figure(Fig. S18) explaining the measurement of alpha, beta, Length of the wedge and the distance between the first and second frontal thrusts.

R1C29 (L 147):

> *in the table it is taper angle alpha. But Fig. 1 A1 shows a more or less flat surface slope. Is taper here the total taper (alpha + beta) or just the surface angle? I cannot really verify those numbers listed here*
>
> *now here it is surface taper then*

Response R1C29:

We thank the reviewer for pointing out this misuse of terminologies. We have changed all mention of taper angle to surface slope for clarity. Also as stated in response to R1C28, we have added additional resources to find out these measurements.

R1C30 (L 148):

> *And how is it measured? A1 shows rather a horizontal or even a negative slope. Needs to be explained better*

Response R1C30:

We thank the reviewer for pointing out as we found out serious errors in reporting surface slopes in our study. We recalculated the surface slope by fitting the best-fitted line to the surface between the trench and the backstop. We have also mentioned the updated values in the Table 2. Also as stated in response to R1C28, we have added additional resources to find out these measurements.

R1C31 (L 149):

> *Where do I see the analytical values that support that these values fit Davis? The last one has a stronger décollement than internal strength. Meaning that the décollement is probably not within the "décollement" layer*

Response R1C31:

We agree with the reviewer and have added the analytically computed values comparing the Davis with the one observed in our models in Table 2. As stated above there was a serious error in computing arctan values in our analysis that lead to misreporting of both surface slopes and internal friction angles. We have corrected this now across our manuscript. The correction in the angle of friction means that in all experiments sediment is stronger than the décollement. Additionally, we see the slopes of our wedges ~1.5 in excess of what is predicted from critical wedge theory. This is probably due to the percolation of weak decollement material in the fault gauges making them even weaker than the strain-softened shear zones. We have mentioned this in the result section now in Lines 319-321.

R1C32 (L 151):

> *what does that mean?*

Response R1C32:

We would like to extend our apologies to the reviewer as we did not fully comprehend the specific correction that was expected in this particular instance. However, we think the comment was specific to the following sentence "*Models with weaker décollements develop thrusts that are lengthier and remain active for shorter periods*" What we meant was that weak decollement facilitates relatively easy transmission of strain on the incoming blocks. As a result, even though the thrust sheets could be longer(especially in the case of strong wedge sediments), they still are active for a relatively small duration.

R1C33 (L 152-153):

> *not English*

Response R1C33:

We agree with the reviewer and we have modified the paragraph to make it have more clarity.

R1C34 (L 161):

> *"In general, I'd focus on describing figures in the results section. Here, you list a lot of numbers and geometries that I cannot find anywhere in a figure of the main manuscript"*

Response R1C34:

We acknowledge the lack of figures for the general evolution of wedges in the earlier manuscript. To resolve this, we have added Fig 2, which gives an idea about the typical evolution of the wedge along with contours for thermal evolution. Furthermore, we have added 10 supplementary images (Fig S4-Fig S13) that show the thermomechanical evolution of each model. Alongside we have also included a better description in Table 2 for how these parameters are measured and Fig. S18 shows these measurements for an instance.

R1C35 (L 169-170):

> *"redundant. In general it is clear that a thicker pile of sediments increases the max temperature at the base, both enhanced by sedimentation or a steeper, thicker wedge"*

Response R1C35:

We agree with the reviewer that the sentence pointed out by the reviewer was redundant and hence have removed the sentence from the paragraph.

R1C36 (L 172):

> *"maybe rephrase"*

Response R1C36:

We thank the reviewer for pointing pot a language error and we have fixed the sentence accordingly. The sentence(Lines 347-349) now reads "*For wedges characterized by the same décollement strength but higher trench sedimentation, we observe that the rate of thermal maturity increases in a landward direction from the trench and remains consistent across these wedges (Fig. 4B).*"

R1C37 (L 183):

?

Response R1C37:

We have reformulated the sentence(Lines 362-363) indicated by the reviewer as "*In wedges with a higher décollement strength or sedimentation rate, sediments tend to follow high-maturity paths in larger proportions.* "

R1C38 (L 185):

to the end of the sentence

Response R1C38:

We agree with the reviewer and we have made a concerted effort to predominantly place the references at the end of the sentences, as suggested by the reviewer, to enhance the readability of the manuscript.

R1C39 (L 190-192):

which equals the spacing of thrust sheets, correct? Just for my understanding

Response R1C39:

Yes. The reviewer rightly pointed out the periodicity in the thermal maturity mapping of incoming sediments.

R1C40 (L 200):

??

Response R1C40:

We have removed the sentence indicated by the reviewer.

R1C41 (L 212): *?*

Response R1C41:

We agree with the reviewer and have reformulated the sentence. The sentence(Lines 396-397) now reads "*Moreover, we observe that incorporating information about the normalised depth of sediments (Y_n) significantly aids in constraining the maximum exposure temperature*"

R1C42 (L 212-213):

> *I guess that has somethiing to do with Y_n in the figure but it is neither indicated in the figure caption nor here in the text. What is Y_n?*

Response R1C42:

We are thankful to the reviewer for pointing out that Y_n in Fig. 4 (in the original manuscript; Fig. 6 in the updated manuscript) was not properly defined. Y_n is the depth of the marker from the surface normalized by the thickness(vertical extent) of the wedge at the location of the marker. For more clarity, we have added a figure explaining the computation of Y_n in panel B of Fig. S17. Additionally, we have also included the definition in the Fig. 6 description.

R1C43 (L 212-213):

> *Obviously thickness represents temperature represents maturity. The question is how fast a %R_0 is established. From the plots it seems that it is rather fast*

Response R1C43:

We are thankful to the reviewer for the observation and the insightful question. We agree that the thickness of the wedge leads to an increase in the temperature in the wedge and hence leads to higher thermal maturity. The second part of the comment which relates to the time it takes for thermal maturity to be established, depends on the maximum exposure temperature and time duration of exposure. As mentioned in section 3.5 the rate of kerogen conversion is exponentially dependent on temperature. Hence typically, the thermal maturity does not change significantly once the maximum exposure temperature is achieved. Please see the supplementary Figure S19 showing a change in %Ro with time for the constant set of temperatures computed using Simple%Ro which gives an idea about the time it takes for thermal maturity to set in for different temperatures.

R1C44 (L 218-219):

> *not English*

Response R1C44:

We have reformulated the sentence(Lines 402-403) as "*The usage of Easy%Ro, Simple%Ro, and Basin%Ro in our models provides us with a distinct perspective on the comparative (dis)advantages of each method in estimating thermal maturity values.*"

R1C45 (L 221):

> *in what?*

Response R1C45:

We acknowledge that the earlier paragraph lacked clarity on the uncertainty of maximum exposure temperature. We have added the following sentences to define the uncertainty more clearly. These sentences(Lines 403-407) read "*The non-uniqueness of maximum exposure temperatures for the same %Ro values arises from the variation in sediment trajectory and thermal exposure history. This diversity among sediment markers results in multiple markers attaining the same level of thermal maturity. We refer to the range of maximum exposure temperatures corresponding to the similar %Ro values as the uncertainty in maximum exposure temperatures.*"

R1C46 (L 226):

> *I don't really understand this entire section. Uncertainty here is the deviation of the mean, right? But that is not uncertainty, this is just a deviation because of whatever goes into the equations of the three different models for %R_0. If otherwise, then I didn't understand.*

Response R1C46

We thank the reviewer for the feedback on this section. We would like to inform the reviewer that the uncertainty mentioned here is not due to the use of the three models. Furthermore, in Fig. 6A we only show the values for Easy%Ro. Indeed the uncertainty in maximum exposure temperature is the result of diverse sets of sediments taking different trajectories and being exposed to different temperature fields, yet ending up having the same thermal maturity. We acknowledge that we did not explain this in the original manuscript to full effect leading to confusion. Therefore, we have restructured this paragraph to bring more clarity. The start of this paragraph(Lines 402-414) now reads "*The usage of Easy%R_o, Simple%R_o, and Basin%R_o in our models provides us with a distinct perspective on the comparative (dis)advantages of each method in estimating thermal maturity values. The non-uniqueness of maximum exposure temperatures for the same values of %R_o arises from the variation in sediment trajectory and thermal exposure. This diversity among sediment markers results in multiple markers attaining the same level of thermal maturity. We refer to the range of maximum exposure temperatures corresponding to similar %R_o values as the uncertainty in maximum exposure temperatures. Uncertainty for all three models increases with increasing %R_o from ~20–25℃ at ~0.3 to ~35℃ at %R_o=3.5 (Fig. 6b). Easy%R_o, probably the best-recognised method of thermal maturity computation, yields the best constraint on uncertainty for very small changes nearing <1 values. For the values of %R_o between 1 and 3, all models yield very similar uncertainty, with Simple%R_o yielding the most constrained exposure* *temperatures (Fig. 6b). However, beyond %R_o = 3, Simple%R_o becomes unreliable, with uncertainty in exposure temperatures as high as 55℃ at %R_o = 4. Easy%R_o yields an uncertainty range of ~37℃ till %R_o = 4.4, and starts to be unreliable above this value. Basin%R_o remains consistent until a very high value of %R_o ~ 6, and thus provides the best constraint on the widest range of values of thermal maturity (Fig. 6b).*"

R1C47 (L 229-230):

> *but after reading through the manuscript, I don't really know what is unique. I suppose it is the thermal conductivity. I mean, other numerical models have similar implementations of thermal evolution. So, is it a specific parameter? Or the entire model that is set up differently?*

Response R1C47:

We acknowledge that the novelty is primarily our integration of empirical thermal conductivity values in our numerical models. Other than thermal conductivity we also compute %Ro from multiple models. To emphasize both these points in greater detail we have included a detailed paragraph on this(Lines 171-185) which reads as follows "*In order to accurately assess thermal maturity, it is crucial to consider the temperature distribution, which necessitates a realistic thermal conductivity profile when modeling thermal maturity. Many geodynamic models assume that thermal conductivity decreases as temperature increases, following a defined relationship (e.g., Clauser and Huenges, 1995). These models typically predict a decrease in thermal conductivity with depth within accretionary wedges, as geothermal profiles tend to increase in temperature with depth. However, empirical data reveal a different trend: thermal conductivity increases with depth, primarily due to sediment porosity influencing shallow thermal conductivity (Henrys et al. 2003, Lin et al. 2014). Additionally, the thermal conductivity values calculated using the Clauser and Huenges model (1995) are significantly higher than those observed at shallow depths (< 3 km). To address these disparities, we incorporate the observed empirical relationship between depth and thermal conductivity from the IODP Site C0002 borehole in the Nankai accretionary wedge into our simulations. By adjusting the thermal conductivity formulation for sediments based on temperature and depth, we aim to replicate the empirical relationship observed in the core samples taken from the borehole at IODP Site C0002 (Sugihara et al., 2014) and account for the decrease in thermal conductivity near the surface caused by increased porosity. We modify the thermal conductivity formulation for sediments as a function of temperature and depth as follows.*"

R1C48 (L 231):

> *re-phrase, unclear*

Response R1C48:

We agree with the reviewer and we have modified the paragraph to make it have more clarity.

R1C49 (L 233-234):

> *I think it is possible to compare your results to natural examples, why not?*

Response R1C49:

We agree with the reviewer and now we have compared it to the natural examples in a separate section named Section ***5.3 Comparisons to natural wedges***.

R1C50 (L 238-240):

> *What is the link here to seismic velocity models? Without figure I don't know what and how it compares. What in your experiments compares to P-wave velocity patters? The viscosity? A P-wave distribution from PerpleX?*

Response R1C50:

Past empirical studies have shown a strong correlation between Vp and thermal maturity estimates for depths of up to 4 km (Baig et. al, 2016, Mallick et al. 1995), however, the exact nature of this correlation may vary depending on the specific location. We have also added the same information in the paragraph(Lines 540-544) which read as follows "*Although empirical studies have shown a strong correlation between Vp and thermal maturity estimates for depths of up to 4 km (Baig et al, 2016, Mallick et al. 1995), the exact nature of this correlation may vary depending on the specific location. Nevertheless, the patterns of thermal maturity values in the wedge core in our models also correspond to the patterns of P-wave velocity observed in the Nankai and Hikurangi margins (Górszczyk et al., 2019; Nakanishi et al., 2018; Dewing and Sanei, 2009; Arai et al., 2020).*"

R1C51 (L 241-242):

> *Temperature distribution in sedimentary basins and acc wedges vary strongly, see Underwood etc.*

Response R1C51:

We agree with the reviewer and this can lead to misinterpretation of thermal maturity evolution. However, it is difficult to get a general trend of this change and models for C0002 borehole temperature profiles(e.g. Sugihara et al. 2011) which samples both the Kumano forearc basin and the wedge do not observe this difference either. Nevertheless, we acknowledge that this could potentially have a big impact on our inference and therefore we have mentioned this in the discussion to qualify our result. The relevant sentence(Lines 572-575) now reads "*However, our result is reliant on the empirical thermal conductivity profiles estimated for C0002 borehole, which does not show any large thermal discontinuity* *between the forearc basin and inner wedge that have been observed fossil accretionary wedges (eg. Underwood et. al 1989).”*

R1C52 (L 247):

> *I mean, it has to be either on the markers or on the nodes and I am not sure it would differ strongly. It is just a higher spatial resolution*

Response R1C52:

Due to heavy restructuring of the discussion section we have removed this sentence in the updated manuscript.

R1C53 (L 247):

Wording

Response R1C53:

Due to heavy restructuring of the discussion section we have removed this sentence in the updated manuscript.

R1C54 (L 251):

Wording

Response R1C54:

Due to heavy restructuring of the discussion section we have removed this sentence in the updated manuscript. Also, we have made a concerted effort not to add references in the middle of the sentence.

R1C55 (L 251-252):

> *you cite always Miyakawa, but there are of course 100's of studeis that investigated the thermal evolution of acc wedges based on nature, numerical modelling, etc*

Response R1C55:

We agree with the reviewer and now we have compared it to the natural examples in a separate section named Section ***5.3 Comparisons to previous numerical studies.***

R1C56 (L 258-259):

which is how?

Response R1C56:

Due to heavy restructuring of the discussion section we have removed this sentence in the updated manuscript. We have also added a description of why underthrusted material will end up having a higher thermal maturity. Please go through Lines 422-441 of the revised manuscript which reads "*Collectively, our results support a general increase of thermal maturity with depth and landward in accretionary wedges. The thermal maturity increase with depth is primarily the result of progressively larger exposures to higher temperatures as depth of burial increases. On the contrary, the landward increase in thermal maturity is caused by the long-term deformation of sediments accumulated at older times and the exhumation of sediments that were underthrusted as they meet the backstop. Our models demonstrate that the rate of landward thermal maturity increase is faster for thicker wedges, both for the case of sediment near the surface and deep inside the wedge (Fig. 4). This can be attributed to a larger proportion of sediments being exposed to higher temperatures over an extended duration within thicker wedges, but validating this result with natural observations remains challenging, given to the very limited availability of thermal maturity data across natural wedges. Accretionary wedges in our models can be simplified as a system where the subducting oceanic plate acts as the primary heat source, while the seafloor acts as a heat sink. The heat generated through other sources such as shear heating, radioactivity, and advection is relatively insignificant compared to the heat originating from the younger oceanic plate. In our simulations, we consider a relatively younger and hotter oceanic plate of approximately 20 Myr, which is consistent with the accretionary wedge in the Nankai region adjacent to the Kumano forearc basin (Zhao et al., 2021). Given that the convergence rate remains constant across all models, the heat received from the oceanic plate should remain relatively similar. However, as the wedge thickness increases, the temperature gradient between the boundaries of the wedge must become gentler, resulting in a larger portion of the wedge experiencing elevated temperatures. Moreover, frequent advection from the subduction channel also results in elevated temperatures in the core of the wedge. Finally, models with thicker wedges typically exhibit higher décollement strength, leading to increased shear heating at the base of the wedge.*"

R1C57 (L 262):

other examples?

Response R1C57:

After restructuring the discussion section we have added multiple examples in the manuscript both from the numerical studies and natural wedges in relevant sections. Please go through Section 5.4 of the revised manuscript for more details.

R1C58 (L 264-265):

> *Really? Isn't it mainly because of lesser fault offset? It would be worth demonstrating your argument with data*

Response R1C58:

We express our gratitude to the reviewer for posing such an insightful question. Both thicker faults and lower offsets can potentially contribute to the observed phenomenon. However, we believe that, in this particular instance, the former is more likely. The reason for this preference lies in the fact that vitrinite reflectance, which serves as an indicator of thermal maturity, is influenced by both the maximum exposure temperature and the duration of exposure. As previously explained in response to R1C43, the relationship between temperature and the rate of %Ro increase follows an exponential pattern. This characteristic allows %Ro to function as an estimator of maximum temperature. The limited uncertainty observed in Fig. 6 further supports this argument. A comprehensive examination of the impact of fault thickness and %Ro values on this matter can be found in Fulton and Harris's (2012) extensive study. Our models demonstrate that these thick faults facilitate rapid heat diffusion, resulting in lower %Ro values. We have also included a reference to Fulton and Harris's study in the manuscript.

R1C59 (L 266):

> *Throughout the mauscript, the space in front of references is missing? Did you have a numbered citation style before?*

Response R1C59:

Yes. We agree with the reviewer and we have modified the manuscript accordingly.

R1C60 (L 298-300):

> *indicate figure and what is shown*

Response R1C60:

Due to heavy restructuring of the discussion section we have removed this sentence in the updated manuscript.

R1C61 (L 321):

> *That is a bit too dramatic*

Response R1C61:

We agree with the reviewer and we have modified the manuscript accordingly.

R1C62 (L 351-353):

> *is this a result of this study or already clear from the implemented equations?*
>
> *It remains unclear on what basis one would be less well-constrained than the other only from results presented here. Ok, answering my previous question: It has nothing to do with the presented simulations*

Response R1C62:

We agree that the particular assertions come directly from the basic formulations of Easy%Ro and not a particular result that can only be observed in our models.

R1C63 (L 508):

> *the strongest decollent has friction angle of 22.... Completely wrong here*

Response R1C63:

We are grateful to the reviewer for identifying the inaccuracies in the parameters presented in Tables 1 and 2. We have made the necessary corrections in the updated manuscript.

R1C64 (L 540):

> *values taken where?*

Response R1C64:

We acknowledge that the thermal maturity values displayed in Fig. 6 do not specify the locations within the wedge from which these temperature values have been obtained, and any location change would indeed influence the depth versus vitrinite reflectance map. In the revised manuscript, we have updated the figure description to incorporate this information. The new description for Fig. 6 is as follows: " *Depth vs Thermal maturity (%R0). The shaded (in violet) region shows the range of observed Ro% (mean±1SD) from the C0002 borehole, colored lines represent the values in models sampled from a 10 km wide hypothetical borehole 20km seaward of the backstop.*" Additionally, we have included an illustrative sub-image to enhance visual comprehension.

R1C65 (L 540):

> *“This could also be depending on an initial geotherm and it would probably not differ as you tweaked your thermal modelling based on data from the Nankai”*

Response R1C65:

We concur that the thermal maturity values presented in Fig. 6 are influenced by the geotherm used in the model, which has been adjusted to resemble the present-day empirical geotherm for the C0002 Wedge. However, we would also like to emphasize that the impact of the current geotherm on the thermal maturity observed near the backstop is minimal, as these sediments have been exposed to a range of temperatures and geotherms due to changing spatiotemporal positions. Given that our model does not incorporate past-geotherm information, the similarity of thermal maturity trends between the synthetic boreholes in our model and the C0002 borehole in the Nankai margin is noteworthy.

R1C66 (L 566):

> *“Why eroded? In the panel below it is obvious that it is not eroded”*

Response R1C66:

We appreciate the reviewer for drawing our attention to the potential ambiguity in the earlier description of the figure, allowing us to revise the figure described in the updated manuscript. In Fig. 7, Panel A depicts the value of vitrinite reflectance of markers at 7 Myr. Nevertheless, their spatial location in the figure remains at their position at 4 Myr. This assists in identifying regions of the wedge that contribute sediments to high maturity paths. The eroded regions in Panel A are still present at approximately 4 Myr, as evidenced by the lithology map in Panel B. However, by 7 Myr, these regions have been eroded, resulting in an undefined thermal maturity value indicated by dotted grey lines. To enhance clarity, we have amended the Figure 7 descriptions to be "*Mapping of eventual thermal maturity (vitrinite reflectance at 7.5Myr) to the location of same markers at ~4Myr in the model. Panel A shows the values of thermal maturity for the markers while the lithology of the wedge is shown in panel B. The half arrow represents the active frontal thrust. The sediments which were eroded by 7.5Myr but exist at 4Myr have been markers eroded using dotted grey points.*"

Response to Reviewer 2

R2C1

> *“The article is a very dense, and compact summary of an extensive series of numerical experiments which broady speaking, make realtively realistic simulations of accretionary prism evolution. The models are “full cycle” with a standard, pre-subduction initiation, initial condition, a “natural-forced” subduction initiation (triggered by a pre-placed weak zone between the converging plates) and subsequent “conveyer belt” type subduction-accretion of sediments to the wedge from the oceanic plate. The accretion process is well-described by the mechanical model, which is extensively documented both here and in other publications, and is generally a robust method for tackling this problem. More simply put, the equations chosen conserve mass and momentum across all accretion and subduction-related processes. They also have a rheology that produces a reasonable facsimile of the faulting process. This arises entirely “naturally” from the internal conditions of the model.”*

Response R2C1:

We thank the reviewer Dr David Hindle (R2) for a careful summarization and evaluation of our paper. We concur with their summary of the manuscript and have diligently worked to make revisions accordingly. Consequently, we present a list of significant modifications made in the updated manuscript.

1. Additional Geological setting to the manuscript to give a thorough introduction of the Nankai subduction zone, and previous thermal maturity observation in the area. Section 2 (Geological setting and model generalization) and Section 5.4 (Comparison to natural wedges) were added to the revised manuscript to specifically address the lack of geological context in the original manuscript.

2. Expansion of the method section (Section 3) to include model-set-up, governing equations as well as an in-depth introduction to thermal maturity computation.

3. Updating of several figures as well as the addition of several more supplementary figures. We also added Table 3 to illustrate the parameters used for thermal maturity computation.

4. Updating the discussion section with distinct sections on Thermal maturity distribution, implications and comparisons to previous numerical models and previously studied natural wedges.

5. We have added Section 3.5 to give theoretical details of the thermal maturity evolution in each model.

We hope that these modifications have improved the quality and scientific rigour of the manuscript and we would be happy to address additional comments from the reviewer.

R2C2:

> *"Development of faults in space and time during the evolution of the accretionary prism is quite critical for core material of the paper – the evolution of the thermal maturity of the wedge sediments. The paper neatly demonstrates a number of phenomena in this context."*

Response R2C2:

We are again thankful to the reviewer for the careful observation of our paper and have diligently addressed his feedback through the following significant revisions.

R2C3:

> *Heat transfer through the evolving wedge is also a major component of the paper's results. From my point of view, perhaps the most significant and somewhat mysterious resuldécollement horizon on which the wedge forms and/or b) the amount oft is the overall increase in thermal maturity (given as an average value) for the system according to a) the strength of the sedimentation on top of the growing wedge. However, the extensive quantification of material paths in the wedge the paper provides is also very interesting.*

Response R2C3:

We agree with the reviewer that the original manuscript did not clarify the thermal maturity computation in enough detail. We have added section 3.5, which details the thermal maturity computation using all Easy%Ro (Burnham and Sweeney, 1989, Sweeney and Burnham 1990), Simple%Ro (Suzuki et al., 1993) and Basin%Ro (Nielsen et al., 2017). Please go through Lines 238-265 of the revised manuscript which reads

"*The model computes the %Ro of each marker to estimate the thermal maturity of sediments during the model run using three widely used methods of thermal maturity modelling Easy%Ro (Burnham and Sweeney, 1989, Sweeney and Burnham 1990), Simple%Ro (Suzuki et al., 1993) and Basin%Ro (Nielsen et al., 2017). All the models presented here employ a simplified parallel Arrhenius reaction model, which accommodates an array of activation energies for every component of the kerogen, allowing it to estimate thermal maturity under varying temporal and thermal scales. The Easy%Ro model by Sweeney and Burnham (1990) can be described using the following equations*:"

Hopefully, this will make the paper more accessible.

R2C4:

> *"A few of the problems with the paper begin here. There is little discussion of the actual process of heat transfer.*
>
> *The paper focuses, perhaps understandably, but perhaps too much, on the mechanical evolution of the wedge and the different styles of thrusting that develop. Ultimately, the problem being tackled is one of an actively deforming wedge with heat being pushed into it from its base, as well as "advected" into it from the "side" (and also thermal blanketing from the top in the cases involving active sedimentation.) Perhaps this is a reasonable, physical analogy for what is happening (to a first order at least). The wedge material is being both heated up and also stirred or mixed with differing amounts of stirring/mixing depending on the wedge properties (effectively the strength of the décollement). When discussion of heat flow comes, it is only of local phenomena around individual faults. Whilst obviously important, this does not really capture the thermal behaviour of the model as a whole. The model actually implements radiogenic heat, shear heating, adiabatic heating, and of course, in general, advection. All of these different components of heat transfer/generation as well as the approximate conditions of the deeper, more stable parts of the system ought to be discussed. Instead, these phenomena are treated as side effects, subsumed to mechanical processes and ignored."*

Response R2C4:

We are extremely grateful to the reviewer to inform us about the lack of discussion on thermal evolution. In the updated manuscript we have added some discussion on the evolution of temperature inside the wedge and the resulting thermal maturity trends in L 424-L441 which reads "*Collectively, our results support a general increase of thermal maturity with depth and landward in accretionary wedges. The thermal maturity increase with depth is primarily the result of progressively larger exposures to higher temperatures as depth of burial increases. On the contrary, the landward increase in thermal maturity is caused by the long-term deformation of sediments accumulated at older times and the exhumation of sediments that were underthrusted as they meet the backstop. Our models demonstrate that the rate of landward thermal maturity increase is faster for thicker wedges, both for the case of sediment near the surface and deep inside the wedge (Fig. 4). This can be attributed to a larger proportion of sediments being exposed to higher temperatures over an extended duration within thicker wedges, but validating this result with natural observations remains challenging, given to the very limited availability of thermal maturity data across natural wedges. Accretionary wedges in our models can be simplified as a system where the subducting oceanic plate acts as the primary heat* *source, while the seafloor acts as a heat sink. The heat generated through other sources such as shear heating, radioactivity, and advection is relatively insignificant compared to the heat originating from the younger oceanic plate. In our simulations, we consider a relatively younger and hotter oceanic plate of approximately 20 Myr, which is consistent with the accretionary wedge in the Nankai region adjacent to the Kumano forearc basin (Zhao et al., 2021). Given that the convergence rate remains constant across all models, the heat received from the oceanic plate should remain relatively similar. However, as the wedge thickness increases, the temperature gradient between the boundaries of the wedge must become gentler, resulting in a larger portion of the wedge experiencing elevated temperatures. Moreover, frequent advection from the subduction channel also results in elevated temperatures in the core of the wedge. Finally, models with thicker wedges typically exhibit higher décollement strength, leading to increased shear heating at the base of the wedge.*"

Although they still are not sufficiently detailed, we refrained from going beyond the scope of the present study. Additionally, we have included the heat equation used in the study and the modification to thermal conductivity in section 3 of the manuscript.

R2C5:

> *I understand the conclusions of the paper to show that somehow more sediment reaches greater depths in the stronger décollement models, hence leading to higher overall thermal maturity. I am not entirely sure why this doesn't show up more clearly in the various figures presented (for me at least, this is not the case). I would also be interested in questions such as whether the total heat flux through the model – in other words, the total amount of heat energy pumped into the accretionary wedge as a whole – is greater in a case with higher average thermal maturity?*

Response R2C5:

We are extremely grateful to the reviewer for the insightful comment. In the first part, we discussed the reasons for this in the updated manuscript in L424-L441. Please see the response to R2C4.

However, the second part of the comment is much more interesting to me, and I believe the answer is it depends on wedge evolution. This is because, in our models, higher thermal maturity is achieved by sediment by attaining a higher temperature and not necessarily the quanta of heat energy. This is because the rate of kerogen conversion increases exponentially with temperature. This can be understood by taking a model with high decollement strength, where wedges are thicker but smaller. Exposure of the wedge to the oceanic plate is thus smaller but the temperatures attained in the core of the wedge are higher. Larger wedges with high sedimentation could have an overall higher heat energy being pumped into it, but at a lower temperature and hence a smaller thermal maturity in comparison to thicker wedges with high décollement strength. Nevertheless, more investigation is needed to understand the thermodynamic aspect of heat exchanges in the wedge system and would be a fascinating topic to investigate in the future.

R2C6:

> *Nevertheless, another broad conclusion is that the model "can" be used as a predictive/interpretative tool for thermal maturity data (and potentially other things too) from accretionary prisms in general. This is, I think, largely true. One of the only caveats remaining, I am still not entirely sure why it (the model) makes the predictions it does. Being more clear about why/what exactly is happening would be very helpful. Another caveat. I think the authors believe they have already explained this, but I think there is another, less superficial, deeper level of explanation possible.*

Response R2C6:

We are again extremely grateful to the reviewer for the insightful comment. A major missing section in the original manuscript was the in-depth mathematics of the thermal maturity computation. We have added this part now(L 238-265), which also makes the temperature dependence of the rate of thermal maturity increase quite evident. Please also see the response to R2C3 for some details. We hope that the addition of a new section will resolve this conundrum. The predictive power of our algorithm resides in the multitude of thermal maturity computations across different sediment trajectories which informs us about the uncertainty in the maximum exposure temperature estimates. Hope this makes the paper more understandable for the readers.

R2C7:

> *In terms of structure/text/figures, the article is reasonable. I have noted later that I would like to see 2 or 3 more "cartoon" type figures to help the reader better understand stuff in the text. Currently, it is very difficult to follow in places because concepts/terms are used without adequate definition. The English language usage is sometimes difficult to follow, and seems quite heterogeneous in quality. I have made a number of technical points in this regard too. It requires quite large improvements in places. I think overall the paper represents a significant amount of work, of fundamentally high quality. However, it is currently difficult to understand in places. With some work and extra thought, it should be possible to change that.*

Response R2C7:

We thank the reviewer for pointing this out. We have expanded several sections to make the manuscript accessible as mentioned in response to R2C1. Additionally, we have tried to make cartoon-like figures to show the placement of horizons(Fig 4), boreholes(Fig. 9), measurement of different properties of the wedge(Fig 9, S17-S18) as well as definitions and conventions used throughout the manuscript. We hope all of this makes the manuscript easier to read and resolves the points raised by the reviewer.

R2C8:

> *"Whilst I'm satisfied the numerical model is a perfectly reasonable one, there is something I have noticed that needs to be pointed out. The equations used in the model represent all layers as viscous materials. Looking at the movies, which is the only place you would see it, there is quite clearly significant "flexing" of the two plates, in particular the oceanic subducting one. Of course, this cannot be "flexure", since the material has no elastic strength. The problem this creates is that the décollement angle is being repeatedly modified by wedge growth primarily, but this is something that is coupled with and feeds back into, deformation and redistribution of mass of the overlying sedimentary wedge material. In simple terms, thinking of a critically tapered wedge, a change in basal angle changes the wedge surface angle. This is more complicated when there is a continuously varying basal angle below the wedge, as is assumed in the case of subducting oceanic plates for instance. In a time-dependent, viscous model, transient states will develop which will in turn drive some of the faulting. This isn't necessarily a problem. However, the usual understanding of such a process is that there is probably some elastic support of wedge material. The viscous model will instead give a relatively fast evolution towards an Airy isostatic condition. There will probably be a difference between the two "basal surface geometries" that either model would predict. The (more) correct solution is likely to be one involving some elastic support. And so on. More generally, the "load" condition on the supporting substrate is a complicated thing, also influenced by the evolving thermal state of the subduction system. This point comes back to my earlier concern about the lack of discussion of the thermal evolution of the model."*

Response R2C8:

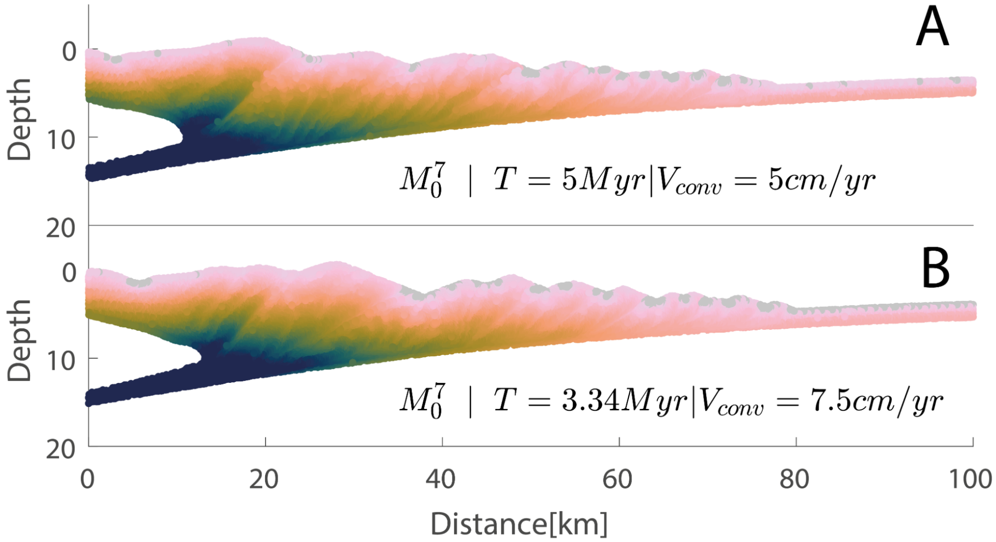
We fully agree with the perspectives expressed by the reviewers. Incorporating an elastic base or, even better, overall elasticity in our wedge model would enhance its realism. Additionally, we acknowledge that adopting a visco-plastic model would result in a more immediate change in the basal dip. An elastic model would be more appropriate for incorporating poroelastic responses related to fluid flow. However, we regret to mention that these aspects were beyond the scope of our current study and model. We would also like to emphasize that the change in the dip angle of the wedge is minimal, with a maximum standard deviation of 1° between 2.5 Myr-7.5 Myr (Table 2). The theoretically expected variation in surface slope due to critical tapering with such a basal dip also has a maximum standard deviation of value of 0.7° (Table 2). Nonetheless, we strongly believe that further investigation into these aspects would be worthwhile in future studies.

R2C9:

> *Concerning the mechanical modelling, relatively little attention is paid to rates. The displacement rates on individual faults, as opposed to the overall convergence rate might be interesting to consider. That would be for the existing series of models. A further thought would be, what happens if the convergence rate changes? Is the change in thermal maturity broadly a linear function of rate or a non-linear one? This might be a bit much for one paper, as I have little feel for how difficult it is to set-up and run, although it would involve a fundamentally identical starting condition and simply run the convergence faster.*

Response R2C9:

We express our gratitude to the reviewer for raising such an insightful question. Indeed, running the model is not a challenging task. As an example, in the Figure S20, we present the results of running the model with a higher convergence velocity of 7.5 cm/yr (Panel B), which is then compared to our original model (Panel A). However, the primary change observed after modifying the convergence velocity is an increase in incoming sediment. When we compare two models with different convergence velocities but the same value of T*Vconv or an equal amount of incoming sediment, we observe minimal differences. However, we believe that a more thorough investigation is warranted, as there are significant changes in temperature exposure. It is likely that with a longer evolution, we would start to observe more pronounced variations. Nevertheless, conducting a deeper investigation exceeds the scope of this paper, but it presents a fascinating subject for future studies.



R2C10:

> *Concerning fault, rates and heat transfer. This is a combination of things, in a submarine wedge, that surely leads to groundwater flow and hydrothermal systems. This is turn would, in real systems modify temperature, heat flow and potentially long term thermal maturity. Again, the discussion of heat transfer in general could be added/extended substantially in this direction too.*

Response R2C10:

We acknowledge that the fluid flow through in a real system would modify the temperature field considerably. We partially account for it by assigning a higher thermal conductivity to the weak décollement material that also permeates the faults in the wedge. However, our model is not capable of modelling the multi-scale fluid flow and thus also fails to reproduce very high thermal maturity in faults. Although this is out of the scope of our present work, this would definitely be something that we would be interested to explore in future.

R2C11:

> *The paper oscillates somewhat between generalised models to represent accretionary systems of different physical characteristics, and specific references to site(s) within the Nankai accretionary prism. Sometimes this makes it a bit confusing to know whether the model is actually simulating the Nankai, or whether these are general models simulating non-specific "type" examples of prisms. I would make this a bit clearer in the text in places.*

Response R2C11:

We thank the reviewer for pointing this out. We clarify this in detail in the Section 2 Geological setting added in the updated manuscript. Our model takes key parameters from Nankai, however, these parameters have a similar range of values for many accretionary wedges across the globe. Hence the trends observed in our models can also represent a generalised state of thermal maturity in wedges.

R2C12:

> *line 159-161: I suspect that it isn't too difficult to get this degree of correspondence between a measured temperature profile and a calculated one, just based on top and bottom boundary conditions, reasonable total lithospheric thickness and reasonable thermal conductivity values, as well as any heat sources. Relatively small differences in temperature at these depths are actually already generally quite significant when considering a lithospheric geotherm.*

Response R2C12:

We concur that the geotherm presented in Fig. S16 is influenced by the thermal conductivity profile in the model, which has been adjusted to resemble the present-day empirical profile for the C0002 borehole. However, we would also like to emphasize that the impact of the current geotherm on the thermal maturity observed near the backstop is minimal, as these sediments have been exposed to a range of temperatures and geotherms due to changing spatiotemporal positions. The correspondence in geotherms between the observed and the model generated is strictly for preliminary validation. What we are finally interested in is if we can generate similar thermal maturity in depth. Fig 9 shows the correlation between the observed values and our models. Given that our model does not incorporate past-geotherm information, the similarity of thermal maturity trends between the synthetic boreholes in our model and the C0002 borehole in the Nankai margin is noteworthy.

R2C13:

> *line 23: support that – replace with "show that".*
>
> *line 26: must take account of the length and frequency*
>
> *line 29: factual – replace with "measured"*
>
> *line 67: propose subdividing these …*
>
> *line 72-75: incomprehensible*

Response R2C13:

We are thankful to the reviewer for informing us about the language-based error in the manuscript. We have now updated these sections in the manuscript in the interest of better readability.

R2C14:

> *paragraph lines 72-82: this paragraph introduces fundamental, existing concepts in understanding thermal and mechanical evolution of wedges. The verbal description is hard to impossible to follow. It seems to me a cartoon figure could help the reader enormously however.*

Response R1C14:

We would like to inform the viewer that we have expanded the methods section and added 2 figures(Fig 1 and Fig 2) to illustrate various concepts used in the study. We hope the expanded method section will now give readers ample details about the modelling method. Additionally, Fig S4-S13 has been added to show the model evolution in each experiment. We have also added cartoon figures such as Fig 8 to illustrate the main findings of our study. Hopefully, this improves the clarity of the manuscript.

R2C15:

> *line 99: not clear what this means, Do you mean that the geotherm is "fixed" and the model migrates through it? (Clearly not, because of the equations used). Then is it "better" because you have better surface and basal boundary conditions, or better thermal conductivity parameters or better ways of tracking their evolution or a combination of all 3 of these?*

Response R2C15:

We acknowledge the lack of clarity in this sentence. We intended to emphasise that the empirical thermal conductivity profile used by us leads to a realistic geo-thermal gradient, even when it is dynamic. This is as illustrated in the updated manuscript (L172-L185) "*Many geodynamic models assume that thermal conductivity decreases as temperature increases, following a defined relationship (e.g., Clauser and Huenges, 1995). These models typically predict a decrease in thermal conductivity with depth within accretionary wedges, as geothermal profiles tend to increase in temperature with depth. However, empirical data reveal a different trend: thermal conductivity increases with depth, primarily due to sediment porosity influencing shallow thermal conductivity. Additionally, the thermal conductivity values calculated using the Clauser and Huenges model (1995) are significantly higher than those observed at shallow depths (< 3 km). To address these disparities, we incorporate the observed empirical relationship between depth and thermal conductivity from the IODP Site C0002 borehole in the Nankai accretionary wedge into our simulations. By adjusting the thermal conductivity formulation for sediments based on temperature and depth, we aim to replicate the empirical relationship observed in the core samples taken from the borehole at IODP Site C0002 (Sugihara et al., 2014) and account for the decrease in thermal conductivity near the surface caused by increased porosity*."

R2C16:

> *line 107-108: (English) – replace with “It is generally rare to have data on both thermal maturity and thermal conductivity from the same borehole. To our knowledge, the C0002 borehole in the Nankai accrretionary wedge in the Kumano forearc basin is the only place where this can be found in an accretionary wedge.”*

Response R2C16:

We thank the reviewer for informing us about the typographical error in the manuscript. We have corrected the error in the updated manuscript.

R2C17:

> *line 111: this is quite a jump for anyone trying to follow what's going on. The "maarker" concept is due to an Eulerian formulation (fixed grid with stuff moving "through" it). Your model state is tracked by "markers" that you track as they migrate through the grid. You really need to explain this in the main text, and not just the supplement.*

Response R2C17:

We thank the reviewer for the suggestion. As a result, we have included the detailed model description in Section 3 in the updated manuscript which includes Governing equations, the Rheological model, surface processes, Model- setup and experimental strategy.

R2C18:

> *line 115: repetition of the sentence here.*

Response R2C18:

We thank the reviewer for informing us about the typographical error in the manuscript. We have corrected the error in the updated manuscript.

R2C19:

> *line 119-120: "using data from the International Argo Program" (unless you mean the data was actually requested by you and then given to you, in which case, say "using data made available to us by the…)*

Response R2C19:

We agree with the reviewer, however, the language itself has been specified by the International Argo Program and hence in the interest of citation and discoverability we have left it unaltered.

R2C20 :

> *lines 121-126: repeated/garbled sentences*

Response R2C20:

We thank the reviewer for informing us about the typographical error in the manuscript. We have corrected the error in the updated manuscript.

R2C21:

> *line 144: This is a bit of an abrupt way to start the section. It sort of follows from the preceding one, but it would be nice to make it a bit smoother. For instance, In our models, subduction initiates at...*

Response R2C21:

We thank the reviewer for the suggestion and we have made appropriate changes suggested by the reviewer. The sentence now reads "*In our models, subduction begins at 0.1 Myr as the weak material between continental and oceanic plate fails (Fig 2, Fig S4-S13, also see supporting information movies).*"

R2C22 :

> *Beyond that, the age of 0.1MYr - is it always at this moment and is that in any way significant?*

Response R2C22:

0.1 Myr is not significant but it is only the time when after some initial deformation the subduction begins in earnest. Also, it is not a fixed number, for example, ideally, the models with stronger décollement should have a bit delayed start.

R2C23 :

> *line 156: In the preceding description of thrust sheet length, it isn't really obvious how this is measured. I assume it must be from a footwall cut off forwards, to the hanging wall cut off of the same unit? But I could be wrong. You must mean some particular geometric parameter here and it needs a bit more clearly defining. Also, the table alone is a bit of a stretch for readers. Figure 1 is not really clear enough to see what you are looking at when it concerns individual "faults" and "thrust sheets". Some sort of figure to show both how these lengths are defined/determined and where faults are present in the models (higher resolution needed) would be really helpful.*

Response R2C23:

We thank the reviewer for the suggestion as we also think it would be helpful for the reader. As a result, we have added a supplementary figure S18 which details the measurement of L(length of wedge), (surface slope), β(basal dip and, D(Distance between the first and second frontal thrust). We have also added the description of these measurements in Table 2.

R2C24:

> *"line 156: thrush – replace with "thrust"."*

Response R2C24:

We thank the reviewer for informing us about the typographical error in the manuscript. We have corrected the error in the updated manuscript. The sentence(L331-334) now reads "*Steeper surface slopes with increased décollement strengths and change in thrust sheet length with sedimentation and décollement strength are well-known effects that have been confirmed by previous numerical and analytical.*"

R2C25:

> *"line 158: analogue"*

Response R2C25:

We are thankful to the reviewer for informing us about the typographical error in the manuscript. We have corrected the error in the updated manuscript.

R2C26:

> *"line 167: you need to show exactly what you mean by this. Where is your trench? Which way is "landward" in this case? Where is you forearc high? You haven't actually defined them anywhere else. These are key results overall in this paper. Some sort of cartoon figure to summarise them (thermal maturity trends only) would maybe help readers."*

Response R2C26:

We agree with the reviewer that in the interest of clarity, these terminologies should be first clarified. To this end, we have added the following note in Section 3.5, the model set-up (L288-292) ".*The terms "landward" and "seaward" indicate the relative direction towards the continental plate or the oceanic plate, respectively. The "Backstop" refers to the edge of the continental plate that buttresses the wedge and acts akin to an indenter for the accretionary wedge. The "forearc high" represents the highest point in the forearc zone, which includes both the accretionary wedge and the forearc basin..*" Additionally, we have also added a cartoon pointing out these terminologies in the supplementary section.

R2C27:

> *"line 172: how do you define this horizon? Especially in an already deformed model? When you say "horizon" it has geological connotations, like a formation boundary or similar. But in the context you are using it here, it must be some sort of relatively flat (another problem, everything is curved with the plate) line that cuts through whatever it encounters. Again, a cartoon figure showing what is meant would be helpful, probably as an inset in fig. 2. The results themselves in fig 2 are very convincing! It would just be nice to get them in clear context."*

Response R2C27:

We agree with the reviewer that Fig 2 (Fig 4 in the updated manuscript) lacked any information on which horizon is the value of thermal maturity sampled. We have tried to rectify this issue by demonstrating the landward increase in thermal maturity across two horizons, 1. In the 2 km band along the surface of the wedge 2. A 2 km horizontal band at the depth of the trench. The representative position of these horizons is indicated in the inset provided in Fig 4. To correct the different wedge lengths, we have normalized the horizontal spatial position with the length of the wedge. As also stated now in Fig 4 *"Horizontal distance 0 represents the fixed backstop and 1 represents the trench."*

R2C28:

> *"line 173: attains the – replace with "reaches a"."*

Response R2C28:

We are thankful to the reviewer for informing us about the language error in the manuscript. We have corrected the error in the updated manuscript. The sentence(L 349-352) now reads "*Comparing the values of %R_o (Fig. 4) along an arbitrary horizon in several models emphasizes this result; the model with the highest décollement strength reaches a maximum %R_o of 1.25, and has the highest rate of landward increase in thermal maturity (Fig. 4A).*"

R2C29:

> *"line 200: none of the top half m...etc. incomprehensible"*

Response R2C29:

We are thankful to the reviewer for informing us about the typographical error in the manuscript. We have corrected the error in the updated manuscript. The sentence now reads(L 382-384) "*In wedges for the model without sedimentation (M_0^9.5), the top half of the incoming sediments fail to achieve %Ro > 1, as opposed to ~ 15% of them reaching %Ro > 1 in the models with a sedimentation rate of 0.9 mm/yr (M_0.9^9.5).*"

R2C30:

> *"line 218: As our models…."*

Response R2C30:

We are thankful to the reviewer for informing us about the typographical error in the manuscript. We have corrected the error in the updated manuscript.

R2C31:

> *"line 229-230: It isn't clear what the advantages are that you mean!"*

Response R2C31:

We agree with the reviewer that these advantages were not laid out. We, therefore, have reformulated the sentence (L 144-146) as follows to make it clearer.

"*Our numerical approach has several advantages over earlier attempts to simulate thermal maturity in an accretionary wedge, such as a more realistic geothermal profile, variable particle paths, and thermal evolution*"

R2C32:

> *"line 230-233: doesn't make sense. Is the resolution of your model too low or the others, or all of them?"*

Response R2C32:

We would like to clarify the statement in a bit of detail. Each simplification is due to a different reason. For example, we do not model elasticity in our model. Elasticity, Compaction and fluid flow were avoided due to both lack of resolution to accurately model these processes as well as the lack of fundamental governing equation being considered in our models. Having no erosion was a deliberate choice to lower the number of parameters we needed to attribute the observations to. We concur that if included these would make our models more realistic but we avoided it as it was beyond the scope of our present work.

R2C33:

> *line 234: "We are confident of the thermal maturity patterns of our models" – then I am very happy for you…but this also shouldn't be written in this way in a paper…*

Response R2C33:

We are thankful to the reviewer for informing us about the language-based error in the manuscript and we have now removed this unnecessary line in the manuscript.

R2C34:

> *line 238: how can your models "correlate" with a P-wave velocity!? They can surely only correlate with some parameters derived from P-wave velocities?*

Response R2C34:

Past empirical studies have shown a strong correlation between Vp and thermal maturity estimates for depths of up to 4 km (Baig et. al, 2016, Mallick et al. 1995), however, the exact nature of this correlation may vary depending on the specific location. We have also added the same information in the discussion section.

R2C35:

> *"line 241: evolve over long time intervals"*

Response R2C35:

We are thankful to the reviewer for informing us about the typographical error in the manuscript. We have corrected the error in the updated manuscript.

R2C36 :

> *"line 243: see specific comment. Are you sure of what you are saying about the model? Even if you are, the observation needs much more discussion/explanation than one line in the text and only being visible in the movies and there, without comment or annotation."*

Response R2C36:

In accordance with the reviewer's comment, we have expanded on the discussion section when comparing the natural wedges to our result. We have added a section 5.4 Comparision to natural wedges.

R2C37:

> *"line 246-248: bad English."*

Response R2C37:

We agree with the reviewer and have restructured the whole paragraph around the section pointed out by the reviewer.

R2C38:

> *"line 249: the question is here, from what point on, and at what depth? Total depth is different for the different models. So is the width of the accretionary wedge. Fig 1 shows a large spread of results. Also quite evenly layered thermal maturity for low basal friction cases."*

Response R2C38:

We acknowledge that, as also rightly pointed also by the reviewer that in Fig 1(in the original manuscript and Fig 3 in the updated manuscript), it is difficult to discern the landward increase in thermal maturity. Also, Fig 2 (Fig 4 in the updated manuscript) lacked any information on which horizon is the value of thermal maturity sampled. We have tried to rectify this issue by demonstrating the landward increase in thermal maturity across two horizons, 1. In the 2 km band along the surface of the wedge 2. A 2 km horizontal band at the depth of the trench. The representative position of these horizons is indicated in the inset provided in Fig 4. To correct the different wedge lengths, we have normalized the horizontal spatial position with the length of the wedge. As also stated now in Fig 4 *"Horizontal distance 0 represents the fixed backstop and 1 represents the trench."*

R2C39:

> *"lines 255-259: these are very "broad brush" statements dropped rather out of nowhere and just left there. They need far more serious discussion."*

Response R2C39:

We agree with the reviewer and have expanded on the discussion related to the increase in thermal maturity in thick vs thin wedges. The relevant paragraph (L422-444) given below includes the simple model earlier suggested by the reviewer in addition to large shear heating and advection from the subduction channel.

"*Collectively, our results support a general increase of thermal maturity with depth and landward in accretionary wedges. The thermal maturity increase with depth is primarily the result of progressively larger exposures to higher temperatures as depth of burial increases. On the contrary, the landward increase in thermal maturity is caused by the long-term deformation of sediments accumulated at older times and the exhumation of sediments that were underthrusted as they meet the backstop. Our models demonstrate that the rate of landward thermal maturity increase is faster for thicker wedges, both for* *the case of sediment near the surface and deep inside the wedge (Fig. 4). This can be attributed to a larger proportion of sediments being exposed to higher temperatures over an extended duration within thicker wedges, but validating this result with natural observations remains challenging, given to the very limited availability of thermal maturity data across natural wedges. Accretionary wedges in our models can be simplified as a system where the subducting oceanic plate acts as the primary heat source, while the seafloor acts as a heat sink. The heat generated through other sources such as shear heating, radioactivity, and advection is relatively insignificant compared to the heat originating from the younger oceanic plate. In our simulations, we consider a relatively younger and hotter oceanic plate of approximately 20 Myr, which is consistent with the accretionary wedge in the Nankai region adjacent to the Kumano forearc basin (Zhao et al., 2021). Given that the convergence rate remains constant across all models, the heat received from the oceanic plate should remain relatively similar. However, as the wedge thickness increases, the temperature gradient between the boundaries of the wedge must become gentler, resulting in a larger portion of the wedge experiencing elevated temperatures. Moreover, frequent advection from the subduction channel also results in elevated temperatures in the core of the wedge. Finally, models with thicker wedges typically exhibit higher décollement strength, leading to increased shear heating at the base of the wedge. Observational studies conducted by Yamano et al. (1992) on the thermal structure of the Nankai accretionary prism have further highlighted that the landward increase in prism thickness is the most significant factor contributing to temperature variations within the wedge. Consequently, the sustained higher temperatures within thicker wedges over time would lead to a higher rate of landward thermal maturity.*"

R2C40:

> *"line 262: such as for.. (also this citation needs more context directly in the text)."*

Response R2C40:

We acknowledge the lack of context in referred part of the discussion as pointed out by the reviewer. Due to heavy restructuring of the discussion section, the relevant paragraph fall in *Section 5.4 Comparisons to natural wedges*. The paragraph(L 586-603) related to the citation mentioned by the reviewer in the updated manuscript reads as follows "*On-fault increases in vitrinite reflectance is well also documented in nature, as for boreholes C0004 and C0007 drilled during the IODP Expedition 316, NanTroSEIZE Stage 1, which samples the megasplay fault in Nankai accretionary margin (Sakaguchi et al., 2011). The vitrinite reflectance data from the Megasplay and frontal thrusts in Nankai indicate the faults reach a temperature well above 300°C during an earthquake, much larger than the background thermal field. Therefore, on-fault increases in thermal* *maturity are comparatively smaller in our simulations and lack the marked increase in %Ro observed at fault sites in nature. This is primarily due to our models developing wider fault zones than their natural equivalents and the subsequent acceleration in the thermal diffusion occurring in simulated thrusts. For instance, the location of mega splay fault in the C0007 borehole exhibits an uneven within the high-reflectance zone with a maximum %Ro ~1.9 (Sakaguchi et al., 2011). However, a smoothening of the %Ro of the same dataset to yield consistent dataset results in only a fraction increase of %Ro to ~0.4. This is in line with the prediction by Fulton and Harris (2012) about the impact of fault thickness on change in vitrinite reflectance. Natural observations also exhibit a much higher incidence of on-fault increase in thermal maturity compared to our simulations as our models do-not capture a plethora of thin faults that exist inside the wedge due to the lack of sufficient spatial resolution. Natural examples of fault-block inversion have been well-documented in natural settings, providing evidence of past thrust activity preserved in the shallower sections of the Nankai accretionary wedge. Sakaguchi (1999) reported the presence of step increments of thermal maturity, similar to increments in vitrinite reflectance in Fig. 4 across the faults. Other examples include Ohmori et al. (1997) who documented fault block inversion along the Fukase Fault in the Shimanto accretionary wedge, while Fukuchi et al. (2017) reported such inversion beneath the forearc basin in the Nankai accretionary wedge. "*

R2C41:

> *"line 293: contradictory"*

Response R2C41:

We are thankful to the reviewer for informing us about the typographical error in the manuscript. We have corrected the error in the updated manuscript.

R2C42:

> *"line 297: what do you mean by "crossover paths"?"*

Response R1C42:

We acknowledge that we did not provide a clear definition of the term "crossover paths" in our models. We define crossover pathways as non-stationary sediment trajectories that vary with time and cut across trajectories of sediments previously located at the same spatial position. We have also included this definition immediately after the first use of the terminology in the manuscript.

R2C43:

> *"line 300: what do you mean by "laminar paths"? In fluid mechanics we have laminar and turbulent flow. Is it something like that? Please define."*

Response R1C43:

We acknowledge that we did not provide a clear definition of the term "laminar paths" in our models. Our intention was to use "laminar paths" to describe the absence of mixing between incoming sediments at the same horizontal location but different vertical locations within the accretionary wedge. This indicates that these two sediments do not intersect each other's paths. However, as we mentioned in our response to R2C50, it is important to note that these paths are not consistent over time. Subsequent sets of sediments will follow different paths, even if they exhibit laminar behaviour. In the interest of clarity, we have dropped these terms and have stayed with labelling the trajectories consistent or stationary.

R2C44:

> *"line 309: Many fossil "accretionary prism" deposits exist. They still show large areas of quite consistent bedding in many cases, suggesting relatively large chunks of these systems remain coherent, not chaotic. At least that was my impression. Perhaps these obserations are at a smaller scale than those implied in the models. In any case, fossil natural examples would be a useful citation in this context. What do they show in general? Are outcrops of them at a scale similar to what you model can show?"*

Response R2C44:

We agree with the reviewer that the large-scale processes in the accretionary wedges are inherently slow and mostly laminar with consistent beddings and do not reflect chaotic behaviour. We intended to convey the non-stationarity of sediment trajectories in the wedge but we ended up using the terminologies that can potentially lead to erroneous interpretations. As a result, we have changed this sentence(L 526-532) in the manuscript. Now the relevant paragraphs read "*The thermomechanical models presented in this study offer a dynamic representation of trajectories within the wedge. Although the averaged trends in thermal structure and sediment trajectories remain consistent, there are short-term dynamic fluctuations near the frontal thrust. These fluctuations contribute to a diverse range of sediment paths along the depth of the incoming sediments"*

R2C45:

> *“Fig 2: add cartoon inset figure (see specific comments)”*

Response R2C45:

We express our gratitude to the reviewer for bringing to our attention the lack of clarity in Fig. 2 (referred to as Fig. 4 in the updated manuscript). To enhance the figure's clarity, we have added an inset that illustrates the various horizons being sampled to depict the landward trends in thermal maturity values.

R2C46:

> *“Fig 3: this is not easy to read or understand. It probably needs better explanation. Moreover, this figure is key for the whole concept of “peridocity” in the paper’s results. Again, a cartoon explaining what/how periodicity is/arises would be helpful.”*

Response R2C46:

We are thankful to the reviewer for pointing this out as the method for determining periodicity was not mentioned in the original manuscript. We update this by adding the following line (L 370-371) to the manuscript “*The periodicity of mean %Ro was computed by finding the average wavelength of the auto-correlated mean %Ro.*” Additionally, we explain this phenomenon in the discussion section. The relevant lines(L463 - L468) read “*This is expected, given that thrusts are active over longer mean times, and they thus channel material toward the décollement more efficiently, in wedges with stronger décollement or increased sedimentation. While sediments at internal and higher structural positions of the wedge are translated towards the surface and have a lower thermal maturity, sediments at external and lower structural positions are translated towards the décollement and have a relatively higher maturity. The whole cycle repeats with formation of new in-sequence thrust.* ”

R2C47:

> *“Fig 4: What is the color bar scale on the right (Yn)?”*

Response R2C47:

We are thankful to the reviewer for pointing out that Yn in Fig. 4 (in the original manuscript; Fig. 6 in the updated manuscript) was not properly defined. Y_n is the depth of the marker from the surface normalized by the thickness(vertical extent) of the wedge at the location of the marker. For more clarity, we have added a figure explaining the computation of Y_n in panel B of Fig. S17. Additionally, we have also included the definition in the Fig. 6 description.

R2C48:

> *"Fig S5: what are the length scales on this figure? What are the "Boreholes" and how can one wedge represent "Positions" from all the different models simultaneously?"*

Response R2C48:

We apologize for the lack of description in the figures that cause this misunderstanding. Fig S5 in the original manuscript is Fig. S17 now. The wedge presented in this figure is only a representative image and does not represent the length scape of the samples shown below. It is meant to convey that we present 4 sets of boreholes(each containing 3 boreholes separated by a km), one of which lies in the wedge itself at 2.5 Myr and 3 lie in the incoming sediments as a distance of 1, 50 and 100 kms from the trench. We have

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R2C49:

> *"Fig S7 – what is Yn?"*

Response R2C49:

We are thankful to the reviewer for pointing out that Y_n in Fig. S7 (in the original manuscript) was not properly defined. Due to the restructuring of the manuscript, the same figure is now Fig. S17. Y_n is the depth of the marker from the surface normalized by the thickness(vertical extent) of the wedge at the location of the marker. For more clarity, we have added a figure explaining the computation of Y_n in panel B of Fig. S17.
