Earth Surf. Dynam. Discuss., referee comment RC2
https://doi.org/10.5194/esurf-2021-89-RC2, 2022

Comment on esurf-2021-89

Anonymous Referee #2

Referee comment on "Landscape responses to dynamic topography and climate change on the South African source-to-sink system since the Oligocene" by Claire A. Mallard and Tristan Salles, Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2021-89-RC2, 2022

General Comments:

This manuscript aims to test the effects of dynamic topography and climate variation on the sediment flux in southern Africa using a landscape evolution model. The authors aim to test whether any of these factors can produce the increase in sedimentary flux rates observed in some recent reconstructions (as shown in Baby et al., 2018; 2020 – it is not clear to me that previous reconstructions had the resolution to detect an uptick in flux). They first force the landscape evolution model with dynamic topography from four different geodynamic scenarios and find that the model predicted sedimentary flux rates do not match the observed increase in flux rates. They then introduce variability in rainfall based on climate reconstructions and find that rainfall variation can induce a wider range in modelled flux rates but that the patterns still do not match the observations. The correlation between instantaneous dynamic topography, erosion, and precipitation in the simulations is then used to argue that climate is a stronger driver than dynamic topography for erosion since 30 Ma.

I appreciate the modeling effort, but I have some serious issues with the manuscript as presented. Some aspects of the modeling methods and choices are not clear, and many of the methods are incompletely explained. The landscape modeling methods, parameters and choices for the LEM, and what sedimentary data they are trying to match and how various sedimentary datasets combined (or not) all need to be more clearly explained. For example, I think that the observation of increased sediment flux that they are trying to reproduce is that of Baby et al. (2018, 2020) since that is the one displayed on their comparison figure (fig 3) that shows an increase in flux, but this could be much more clearly stated in the beginning. I also think the landscape model should be explained more clearly and some of the many, many choices that go into selecting some of the fixed parameters justified (what are the slope and area coefficients of the stream power model, for example). Additionally, the purpose of the modelling exercise and what, if any, implications exist beyond this study are also not clearly articulated. Was the purpose to demonstrate that dynamic topography sourced from deep in the earth is not a major driver of erosion in the last 30 Ma in southern Africa? If so, I think they have demonstrated that successfully. However, but I find it a bit of a strawman argument and I am not sure what the wider implications are given that the maximum increase in dynamic topography of any of the models over the period of study was ~100 m, and two of the models are subsiding throughout this period. I think they have demonstrated that climate variability could be playing a key role over this period and that the system is complex with interactions between uplift, erodibility, and precipitation. However, have not adequately demonstrated that climate replicates the observations or that it is the only explanation, and the complexity of the system is not a surprise in my opinion. I think it is a neat test to try to drive the landscape model with the dynamic topography predictions, but at this stage I think the manuscript requires extreme revision to be publishable. I outline some more specific points below.

Specific Comments:

Line 12-13: Unless I have misunderstood the methods, this statement seems wrong or misleading. My understanding is that the numerical simulations were driven with the dynamic topography uplift from geodynamic models and inferred precipitation maps that outputs were compared with Earth data not "a series of numerical simulations forced with Earth data". The distinction is important, and if they are truly forcing their simulations with Earth data that needs to be explained much more clearly.

Line 16-17: I am not sure I agree that the statement "paleo-rainfall regimes are the major forcing mechanism that drives the recent increase of sediment flux in the Orange basin" is fully supported by the manuscript. Paleo-rainfall could be a major forcing mechanism, but it hasn't been demonstrated that it matches the observations. Also, the logic here seems to be that if dynamic topography is not the forcing mechanism that precipitation must be. It certainly could be, this ignores other possible drivers that haven't. In this particular setting, many have proposed upper mantle variability may be responsible for recent uplift/erosion (e.g. Burke & Gunnell, 2008; Paul et al., 2014). The authors recognize that the lack of upper mantle input might be affecting their conclusions about dynamic topography in the conclusion (line 251-3) but don't acknowledge that this could also affect the robustness of the conclusion about the importance of climate.

Line 23: This first sentence and to some extent the first two paragraphs of the introduction are somewhat misleading. Most, if not all, of the previous studies linking source and sink in southern Africa have focused on replicating the major pulse(s) of sedimentation observed in the Cretaceous in the Orange River Basin and off the southern coast (Tinker et al., 2008a, 2008b; Rouby et al., 2009; Guillocheau et al., 2012, Braun et al., 2014; Stanley et al. 2021). As written the first sentence of the paper implies that previous studies have focused on the post 30 Ma increase in sedimentation, which is not the case. This paper focuses on the post-30 Ma history, which is a worthwhile exercise, but it shouldn't be directly juxtaposed with the previous work without acknowledging that the previous work was focused on a longer observation period. The Cretaceous pulse of sediment in the marine record is much larger than the post 30 Ma increase in sedimentation, especially in the Orange River basin (Baby et al., 2020) and it seems disingenuous not to mention this (it also provides support for one of this manuscript's conclusions that much of the topographic uplift pre-dated the Oligocene, even though that hypothesis was not directly tested here).

Line 46-47: It seems odd to cite Partridge & Maud 1987 here but not the new geodynamic models

Line 47-50: It would be helpful to show these scenarios on a figure. I assume they correspond to the four scenarios shown in Figure 1, but it is not immediately clear which is which

Line51-53: I am not clear what is meant by flexural uplifts – be more specific?

Lines 88-95: It is a little confusing to me why one would hypothesize sedimentation rates to increase from the two subsiding scenarios, and even the two uplifting ones have relatively low magnitudes of uplift. Braun et al. (2013, 2013) showed that one of the reasons that dynamic topography can cause so much erosion is because it causes widespread tilting that can cause drainage rearrangement and steepens slopes over large regions, and Stanley et al. (2021) showed that the shape of the uplift (whether tilted or uniform) strongly controlled the erosional response for a given magnitude of dynamic uplift. It is difficult to tell what shape and variabilities these dynamic topography models have based on the information in Figure 1 and whether it is reasonable to hypothesize that they could be causing an increase in sedimentation in a complex system. Clearly stating why these scenarios might cause the increase in sedimentation rate observed (and perhaps which are more likely) might help clarify some of the purpose of the modeling.

Line 115-116: This exercise alone and the assumptions made suggest that the 1st order topography/uplift of the plateau existed at the start of the model. This means that topographic development isn't something that's really being tested by the landscape modelling exercise (even though the suggestion of pre-Oligocene uplift is highlighted as an implication in the abstract). It seems that these paleotopography maps (i.e. the initial condition for the models) could be affecting the fluxes as much (more?) than the dynamic uplift driving the models, but this isn't really tested.

Line 161-164: Two things about these comparisons. First, I guess that some of the reason the modelled flux matches the flux rate is because the choice of some parameters in the model was made to match well, and this should be acknowledged. In particular, the erosivity parameter in the stream power law, is not very well constrained and can be affected by many factors. If a base erosivity had been chosen an order of magnitude larger or smaller, that could have been justified within the range of reasonable values for erosivity and would have affected the flux rates, potentially substantially, I think. This should be acknowledged/discussed. Second, it is a little hard to compare the flux rates because the observational data is averaged over much larger time periods because of limited age resolution in the sedimentary record. The model outputs can resolve 100 000 year variations, but the natural data never could so a more nuanced discussion of the comparison is needed.

Line 185-187 “This can be explained”: what does “This” refer to? Statement needs more explanation

Line 197-8: This statement seems somewhat circular/obvious, what is this statement trying to convey?

Line 228-9: “Smoothed” seems like an odd choice of adjective here. The precipitation changes seem to be overwhelming any dynamic topography signal – the correlation coefficients have changed sign for nearly every comparison with dynamic topography. This also seems true when comparing the dynamic topography only fluxes and the ones with precip (Fig 3). I’m somewhat curious what the fluxes would look like simply starting with the “paleotopogrpahy” inputs that you started with and having no dynamic topography forcing. The flux patterns in 3a are fairly similar between the models, and TX08 has the largest flux despite subsiding throughout the model run. It also has the highest elevations in the input topography so it seems that this starting topography and then the isostatic uplift in response to erosion is swamping any erosion signal driven by dynamic topography. This isostatic, erosion driven uplift is then only enhanced by precipitation increases (and of course modulated by erodibility).

Lines 238-242 and Figure 5: These statements are very hard to evaluate without a comparison to the actual observations. Also, Figures 5a-d look quite similar overall to me, so some way of more direct comparisons to highlight differences (if they exist) is necessary to support the statement that only AY18 and TX08 show preferential Orange River mouth deposition.

Lines 251-254: This mention of the upper mantle here is in important caveat that does not come up at all until the conclusions, and then is supported by a new test and appendix figure that is also not described or discussed until the conclusions section. This merits more discussion in the main text.

Line 271-272: What is meant by a “new framework to fill the data gap”?

Line 273: I realize the model did vary sea level, but there is little to no discussion of the effects on the erosion / sediment flux. This should be explored, especially if it is mentioned here in the conclusions

Lines 292-302: This description of the landscape model is inadequate. A reader should be able to get at least a general sense of what was done without needing to become intimately familiar with the Badlands code. For example, what are the parameters for the stream power law? Only erodibility is shown in the table A1, what about the slope and area exponents? Are the processes of wave induced transport and growth of coral reefs included in this study? If so, what are the parameters involved? More of a description of the marine processes and transitional/coastal areas is needed – what are the units on these parameters given? Is diffusion the only process acting in the marine environment?

Lines 304-309, Table A2 and Figure A2. There seem to be some conflicts between the figure A2 (scale from 0-1 for erodibility) and Table A2 with values of 1-3.2 when figure A2 is stated to be based on table A2. I also find this map a bit confusing because the areas where metamorphic Precambrian basement are exposed look blue implying high (I think?) erodibility similar to the shelves where I would expect (and table A2 would imply) that these areas should have low erodibility (e.g. the Namaqua-Natal belt near the Orange River mouth, Zimbabwe Craton area in the northeast).

Figure A1: Black coastline is not on the map and would be helpful.

Figure A3: Many of the references used to create the sediment thicknesses in part b are not in the reference list (Intawong, Maystrenko, Koopman, Kuhlman). Since this is a major input to creating the paleo topographies and also one of the comparisons for the model outputs, a description of how these were estimated/combined is needed.

Figure A7: Two rows are shown as "Rainfall uniform at 0.6 m/yr, Erodibility uniform" but have different coefficients. One is clearly must be mislabeled.

Table A1: There are a lot more parameters that seem like they should be included here (see comments on landscape modelling methods above). Also, how did you choose 1.6e-7 for the base erodibility? Did you try others and what was the effect?

Technical Comments:

Line 41-44: This sentence was somewhat confusingly worded – I think it would make more sense if it started with "While" rather than "If"

Line 251: I think this should be “allow us to generate”

Note: I completed the above review without reading the previously posted reviewer comment as I think it is valuable to have two independent reviews/read throughs that are not biased by one another. However I have now read the other reviewer’s comments and I agree with much of what he says. His point 3 about the differences in landscape response time to a change in uplift vs a change in precipitation is not one I had appreciated while reading the paper, but it seems like would make it rather difficult to compare the uplift and precipitation correlations with instantaneous erosion directly. I also agree with many of his points about he erosivity parameter - something that was also an area of concern in my reading of the manuscript.