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Comment on esurf-2021-89

Jean Braun (Referee)

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-RC1, 2022

Review Mallard and Salles, ESURFD 2021

The manuscript by Mallard and Salles describes the results of a study aiming at explaining the observed increase in sedimentary flux from the Southern African craton documented by Baby et al (2018 an 2020) as resulting from dynamic topography (DT) or enhanced precipitation. For this the authors use a surface process model that is subject to a range of model-deduced DT estimates over the past 30 Myr and a rainfall function/distribution that has been reconstructed from various previous studies. The authors find that the sedimentary fluxes predicted by the model offer little resemblance to the observed ones. The observed amplitude of the change in sedimentary flux can be matched by varying rainfall but no its timing. The recent increase (i.e., in the past 10 Myr) is never reproduced by any model scenario. Despite this, the authors attempt to determine which of the imposed drivers is responsible for the increase in erosion rate (and thus in flux) predicted by the model, by computing correlation maps between erosion rate, DT and rainfall for each of the tested model scenarios.

Although I appreciate the efforts that the authors have put in this modeling exercise, I have to recommend that it be rejected as it contains major methodological flaws, and is not clearly presented. I justify my recommendation in the rest of this review and, where possible, I provide hints/suggestions on how this work and how it is presented could be improved.

My major concern include:

- A lack of clear focus/objective for the paper: what are the authors trying to demonstrate/prove? Are they trying to demonstrate that one DT model fits better than others? That DT or climate variations can or cannot be responsible for the observed increased in sedimentary flux in the Orange basin? Reading through the introduction I do not see any clear questions or hypotheses being stated that are later tested against each other. Similarly reading through the conclusions I do not see what the major findings are except that none of the tested DT scenarios or climate scenarios can explain the variations in sedimentary fluxes. The various scenarios are not ranked by their fit to the observations. No alternative scenario is offered apart from stating that some sediment may have been recycled within the marginal basin which implies that the sedimentary flux data cannot be used to say something about what happens on the continent.
- Like others before (Roberts and White, 2010; Paul et al, 2014), the authors have assumed that they know the erodibility coefficient (Kf) in the SPL. This is a major flaw of their approach as WE DO NOT KNOW its value within a few orders of magnitude. Where does the value of 1.6e-7 comes from? Note also that any given value of Kf is only meaning fully if we know what m (the area exponent in the SPL) is. To further assume, like the authors have done, a one-to-one correspondence between rock type and Kf is also rather misleading, especially with a precision that assumes that variations of a few tens of percents are meaningful. The authors should know that there are many factors that influence Kf such as fracturation, degree of weathering, model resolution, etc. To scale Kf, the authors have also used a present-day map of lithologies that, in my opinion, is irrelevant when applied to a model that is run over 30 Myr (the lithology of eroded material is relevant). To present results that depend on the absolute value of the coefficient Kf, one must use time-relevant constrain (thermochron ages, sediment fluxes, etc.) to calibrating Kf. This has been shown over and over (a great demonstration shown in Fox et al, 2014's work on inverting Taiwanese river profiles).
- The authors correlate predicted erosion rate, with the imposed rainfall rate and and DT variations in an attempt to infer which of the main drivers (DT or climate) is responsible for the predicted variations in erosion rate. First I do not understand the purpose of this exercise as, regardless of what is driving the erosion rate, the model cannot represent the observed sedimentary flux. Second, the authors seem to not take into account that, according to the SPL, there always exists a large time lag between variations in uplift rate (or in base level) and the resulting variations in erosion rate, whereas there is no such time lag between imposed variations in rainfall rate and erosion rate (see Whipple and Mead, 2006, Fig 4). Note also that the time lag mentioned above can be of the order of a few to a few tens of million years, depending mostly on the assumed value of the erodibility coefficient Kf, if n=1. The lower Kf, the longer the time lag.
- The way the DT models are incorporated into the LEM is such that very different scenarios (some predict overall subsidence while others predict uplift) all produce a very similar sedimentary flux prediction. This demonstrates that the model is almost insensitive to DT scenarios and that most of the erosion is driven by erosion of the short wavelength topography which is basically identical to the present-day topography.
- The model is poorly presented. There is no brief description of the main relevant equations; the value of key coefficients (such as the slope and discharge exponents, n and m, in the SPL) is not given, which makes the interpretation of the results very difficult to assess. There are major issues with some of the figures too (see below).

Other points that need to be addressed:

- Line 51: What do the authors mean by "different flexural uplifts"? Different flexural thicknesses? Flexure is a response to another mechanism such as thinning or thicknenning of the lithosphere/crust or to denudation/deposition. It cannot be an uplift in its own.
- Line 60: How did the authors obtain absolute values for paleo rainfall rates? In the appendix, they provide references where paleo indicators are given. They need to explain to the reader how they have been able to go from indicators to actual rainfall rates.
- Line 69: As explained earlier I have major concerns about the erodibility. The value that is given in the relevant table is very low and should be justified.
- Line 94: The authors should compare the various DT models graphically in an appendix/supplementary section. They should also discuss the origin of their differences. Some predict uplift while others predict subsidence… So what is the point in trying to use constraints that do not agree with each other? Alternatively the authors should have as an objective to differentiate between them. But this is not stated/proposed.
- After the paragraph ending at line 96, the authors should show to the reader what the predicted fluxes from Baby et al (2018, 2020) look like. It would be interesting to see how they correlate with the different dynamic topography history estimates. We also need to know what the flux estimates correspond to: are they just from the mouth of the Orange Basin? Or from what Baby et al call the Cape area too? I also believe that Baby et al produce uncertainty estimates of their fluxes. This is really important as any model that would fit the data within uncertainty should be considered as equally adequate to reproduce the data. This is not provided or discussed here.
- Line 110: I am surprised that the authors included the weight of the sediments removed from the continent but not the sediment accumulated in the margins. In theory (by mass conservation) they should be equal in volume so that the weight of the deposited sediment should be much more localized and may therefore cause a much greater flexural isostatic effect along the margins. Remember that for the LEM, the variations in the height of the base level is the only way it can feel the perturbations caused by DT; it is therefore very important that any other process that can affect the relative position of the base level is taken into account.
- Paragraph ending line 116: What about using an optimization/iterative scheme to determine the optimum initial topography, in which whatever is predicted to have been eroded/deposited is used to readjust the initial topography. This should converge very quickly.
- Line 125: The authors should mention the values they have used for the slope (n) and area (m) exponents of the SPL. These are critical to understand what they have done/interpret their results. The relationship between impose variations in rainfall rate and predicted erosion rate depends strongly on m.
- Line 140: It is not supposing that the predicted topography looks like the present-day topography as you simply add DT to an initial topography that is computed by removing DT from the present-day topography… This is not a result but is part of the model setup.
- Line 150: Why discuss first how the model matches cosmogenic-derived erosion rates (that have not been presented in the data/introduction part of the paper) rather than sediment fluxes that have been mentioned earlier.
- Figure 3: There are major issues with this figure: It says that the dashed lines correspond to values calculated from the landscape evolution model whereas one of them is labeled "Baby et al, 2020" which seems to imply that it corresponds to observed values. I also note a second dashed line with a reference to Kuhlmann et al, 2010, which is not mentioned anywhere else in the paper (not even the reference list). The authors should be more precise.
- Line 161: This implies that the model or the setup the authors have used (substracting first DT then adding it) are insensitive to the assumed DT: whether South Africa has gone up or down over the past 30 Myrs has almost no effect on the predicted

sedimentary flux. This implies that the flux and thus the erosion that the model predicts are caused by a slow "downwearing" of the topography that would also likely take place if one had no DT applied to the model.

- Line 165: Here the authors envisage a set of possible reasons for why the model cannot reproduce the sedimentary flux, but they do not envisage the possibility that the DT models are wrong or that DT may not be the driver of the increased flux. It could be the southward propagation of the South African Rift swell, for example.
- Line 175: from the amplitude of the imposed increase in rainfall (4.4) and that of the resulting increase in sedimentary flux (2.2) I can derive that the value of m that the authors have used must be close to 0.5. This relationship between m and the relative changes in driver/response should be discussed (see Braun et al, 2015, EsurfD, Erosional response of an actively uplifting mountain belt to cyclic rainfall variations).
- Line 182: proposed by whom? The authors of the current paper? Baby et al 2018? The authors should be more precise by inserting a reference at the right place.
- Line 230: This is because DT can only be felt in the model as a base level change. And it takes time (according to the SPL) to propagate information from base level to high elevations areas. This should be added to the text to explain this difference between the response they observe.
- Line 237: The first sentence of this paragraph should refer to a figure as it is not clear where the reader should look to see that climate is a major driver of sedimentary fluxes.
- Line 239: It is very difficult to extract this information from Figure 5. What we see is a slightly darker reddish patch near the mouth of the Orange River. But the overall depositional thickness is very similar across the entire margin for the four models. Shown this way, it is impossible for the reader to appreciate that there is a noticeable difference between the models and (most importantly) that one of the model predictions is closer to the "real world" of which we have no display here. The author should provide a more quantitative estimate of these differences and comparison with data (using a cross section or a basin integrated value)
- Line 250: The authors have actually shown that the model's predictions are almost insensitive to the applied DT, despite major differences between the DT scenarios they tested. I do not think that their work can be used to say anything about whether DT is responsible for the recent increase in sedimentary flux as observed. As suggested by Baby et al, it would be more interesting to test the effect of the east African rift opening (as suggested by many, Burke, for example). It would be interesting to see whether any of the DT models shows the well documented southward propagation of the rift system. But we cannot appreciate this as the authors do not propose maps of the various DT models. This could be done by showing (maybe in an appendix) four snapshots at (30, 20, 15 and 0) of DT for the 4 models.
- Line 266: I do not see how an earlier period of uplift could have helped generate faster uplift in the most recent past. The authors should explain this better. Are they suggesting that there might be some recycling of sediment deposited on the shelf? If this is the case, the sediment flux information becomes useless to determine the timing of what happened on the continent (uplift/climate change, ...). This should be discussed in the introduction.
- Line 297: from this description of how the authors estimated their initial topography, it appears that the isostatic effect of deposited sediments was taken into account, whereas the authors state the contrary earlier in the text of the manuscript. This should be made clearer.
- Line 349: this is the only place where the authors specify which sea level curve has been used (of if any has been used); this should be included in the model description/setup.
- Figure A1 and many others below: We cannot see the black line so this figure is difficult to appreciate. It is also difficult to see why varying rainfall in this way (with no noticeable large increase in spatially averaged rainfall between 20 and 15 Ma) the model predicts such a large pulse of sediment flux over that period.

- Figure A3a: what is the purpose of using such a high resolution map of flexural thickness. In our case flexural response is only going to be important around the margins where the distribution of the load (eroded and deposited) is small enough for flexure to matter.
- Figure A3b: what is shown here? Estimated sediment thickness from the model, from observations? Over which period of time?
- The model resolution is very coarse. I do not think I have seen SPL models run at this low resolution since the early days of LEMs. Minimum resolution typically used even for continental scale computation should be of the order of 1x1 km. There are good theoretical reasons for this including how slope estimates are affected by spatial resolution and the scale at which the slopes relevant to SPL should be measured.
- Finally, the authors should compare their results to those obtained by Stanley et al (2021): JGR Solid Earth: Constraining plateau uplift in southern Africa by combining thermochronology, sediment flux, topography, and landscape evolution modeling. This study focuses on finding the best uplift history for the South African Plateau that can reproduce observed flux estimates using an LEM. In view of the commonality of objectives and methods, the results of this manuscript and those of Stanley et al (2021) should be compared.

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