'This is an original study on the effect of large valley incision on lower crust exhumation in the context of a continental plateau underlain by a thick continental crust with very low ductile resistance. The authors conclude that, under certain conditions (high plateau elevation, thick crust, large incision rate and prolonged incision history), river incision can lead to exhumation of the ductile lower crust beneath the valley axis. The article is well-written, has high-quality illustrations, and I agree with the main interpretation of the model results. However, I have several criticisms that should be addressed prior to publication, this is why I ask for major revisions although I think that they will be relatively easy to address. My comments to the authors are as follows'

Dear Reviewer#1,

We sincerely appreciate your valuable feedback on our manuscript entitled "Deformation and exhumation in thick continental crust induced by valley incision of elevated plateaux." Your insightful comments have provided valuable guidance in improving our study and sharpening the clarity of our arguments. Below, we provide a detailed answer for each of your comments. Modifications added in the manuscript are also shown:

- *'I understand that in nature, crustal thickness and plateau elevation can vary widely. However, in your case, since you assume constant crust and mantle densities, there should be a linear relationship between these two parameters due to isostasy. This means that you cannot arbitrarily choose both crustal thickness and plateau elevation independently. For example, if we assume a mean crustal thickness of 35 km for sea-level elevation, then local isostasy (neglecting density changes due to temperature and pressure and including the plateau in the total crustal thickness) would give approximately the following values: a 1 km-high plateau corresponds to a total crustal thickness of 42 km, 2 km corresponds to 49 km, and 3 km to 56 km. How, then, can you justify a 65 km-thick crust with a 3 km-high plateau, as shown in Figure 2a, using your chosen densities, without introducing a significant initial isostatic imbalance? Am I missing something here?'*

We agree with the reviewer that, assuming a reference elevation of 0 km for a 35 km thick continental crust and using the densities implemented in our models, there is a linear relationship between crustal thickness and plateau elevation. This relationship would typically prevent testing different plateau elevations for a given crustal thickness. However, in our models, the surface of the continental crust is initially flat and does not experience lateral pressure gradients that would drive isostatic re-equilibration. Although we tested various plateau elevations for the same crustal thickness, our models (black dots in Figure 2a) align closely, in particular the reference model, with the expected linear relationship between crustal thickness and plateau elevation whatever the chosen crustal thickness. This approach allowed us to more comprehensively explore the range of plateau elevation/crustal thickness combinations observed in nature. Relying strictly on the isostatic relationship would have limited our ability to disentangle the individual effects of crustal thickness and plateau elevation on model behavior as the isostatic linear relationship would lead to only one possible elevation for each crustal thickness.
To clarify and justify this modeling choice, we now include the theoretical isostatic relationship in Figure 2a and have added a corresponding explanation in the Methods section 2.3 (line 219 to 228).

- *'I find one of your results particularly interesting — that surface uplift can be decoupled from the lithospheric response, and that crust-mantle decoupling explains why the Moho remains stable while the lower crust migrates toward zones of lower pressure. Could this behavior explain the very low effective elastic thickness often inferred from the isostatic response of the lithosphere to surface processes? In other words, could this be explained by a situation where only the upper crust effectively responds?'*

We agree with the reviewer's comment that the decoupling between the crust and the mantle in our models lead to a scenario in which only the crust responds to valley incision. This decoupling would affect the calculation of the effective elastic thickness (EET), as the response would reflect crustal behavior alone rather than that of the entire lithosphere. Consequently, the computed EET in the context of erosional processes in orogenic settings would be lower, potentially explaining the systematically low EET values observed in such regions. To fully address this point, we have added a dedicated discussion section 4.2 in the manuscript (line 453 to 463):

- *'Yield stress envelope in Figure 2b: there appears to be no strength in the mantle, which seems surprising. With the dry olivine rheology you use, I would expect some resistance.'*

The absence of strength in the mantle is consistent with the thermal gradient chosen in our models. At the crust-mantle boundary, temperature is ~ 1100 °C. The figure below shows the computed differential stress for the dry olivine rheology (from Hirth & Kohlstedt, 2003) as a function of temperature. In such a configuration, it is normal to have a very low strength in the mantle (see Figure 1 below).

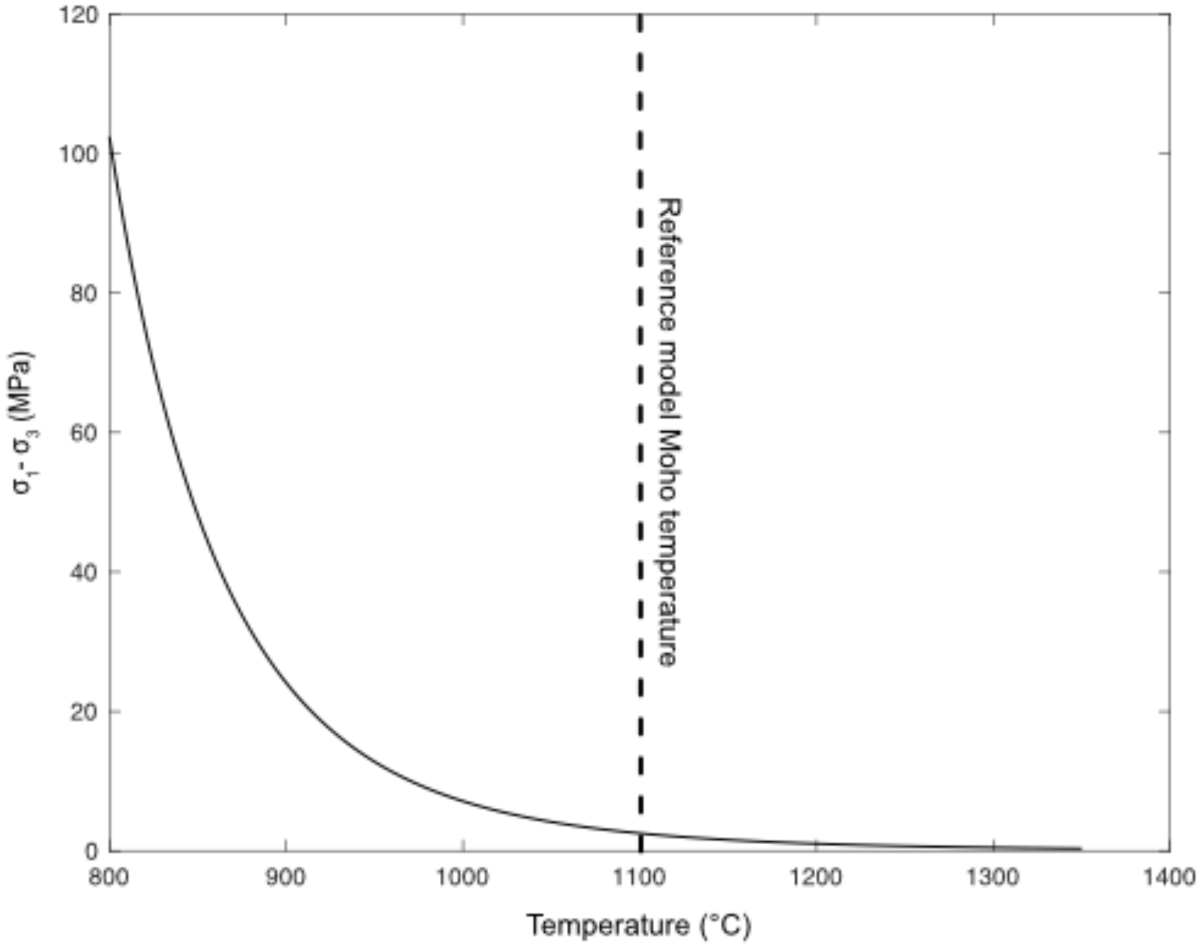

Figure 1: Differential stress evolution as a function of temperature for the Dry Olivine rheology.

- *'I'm generally not in favor of requesting additional model runs in modeling papers, as this can easily become an endless process. However, I am somewhat puzzled by the fact that you don't really discuss your choice of an extremely weak and thick lower crust, which leads to strong convection and very rapid ductile flow. While I understand this may be a deliberate choice, I think it would be helpful to include a comment on how this specific rheology — which possibly resembles that of an orogenic crust — may not represent the "average" continental crust. Out of curiosity, I would be very interested to see how the system behaves with a more resistant (mafic) lower crust and/or a colder lithosphere. For instance, you could add another dimension to your parameter space in Figure 11, for instance by representing the* *effect of the thickness and/or average viscosity of the ductile crust and the comparison to natural settings.'*

In order to address this comment, we performed two additional models: one using the same rheology but with a colder thermal structure (temperature at the Moho of 800 °C), and another employing a diabase rheology for the lower crust. Both models have the same plateau elevation and crustal thickness as the reference case. The methodologies and figures for both models are presented in the supplementary materials (Figs. S8 and S9), and their results are now discussed in section 4.2 (line 481 to 489).

- *'By the way, you should clearly define in the main text what you mean by "lower crust." In your model, you designate crust below 10 km depth as the lower crust, but it shares the same rheological properties as the upper crust. In the literature, "lower crust" can refer either to the ductile portion of the crust — as you do here — or to the more mafic and mechanically stronger part of the continental crust. While this is briefly explained in a figure, it would be helpful to clarify this choice explicitly in the main text to avoid confusion.'*

You are right. We now give additional information on the definition of the lower crust in our models in the Method section 2.2 (line 184 to 186).

- *'Along the same lines, I'm not sure that such an overthickened and weak crust could remain stable without collapsing, unless it's being artificially supported by the model boundaries. This issue is not visible in your setup because you impose a constant crustal thickness and therefore remove any lateral pressure gradients (except the ones due to valley incision). But from a large-scale geodynamic perspective, the configuration might not be entirely realistic — especially if we consider that real-world plateaus are not laterally infinite. That said, since your model already shows lower crustal flow driven solely by valley incision, I can only imagine how much flow would occur if this plateau were adjacent to a region of much lower elevation and much thinner crust.'*

We agree with the reviewer's comment that, in a natural setting, if a plateau is adjacent to a region of significantly lower elevation, the flow of lower crustal material would preferentially migrate toward this area, potentially altering the overall lower crustal response to valley incision. However, in our modeling strategy, we deliberately choose to isolate the effect of valley incision itself, independently of any surrounding topographic or tectonic gradients. To achieve this, we imposed fixed lateral boundaries, preventing material from flowing out of the model domain.
Furthermore, our simulations indicate that the lower crustal material mobilized by valley incision originates from a relatively narrow area around the valley. To illustrate this, we performed a model in which the total width of the model domain was reduced from 500 km (reference model) to 200 km (see Figure 2 below).

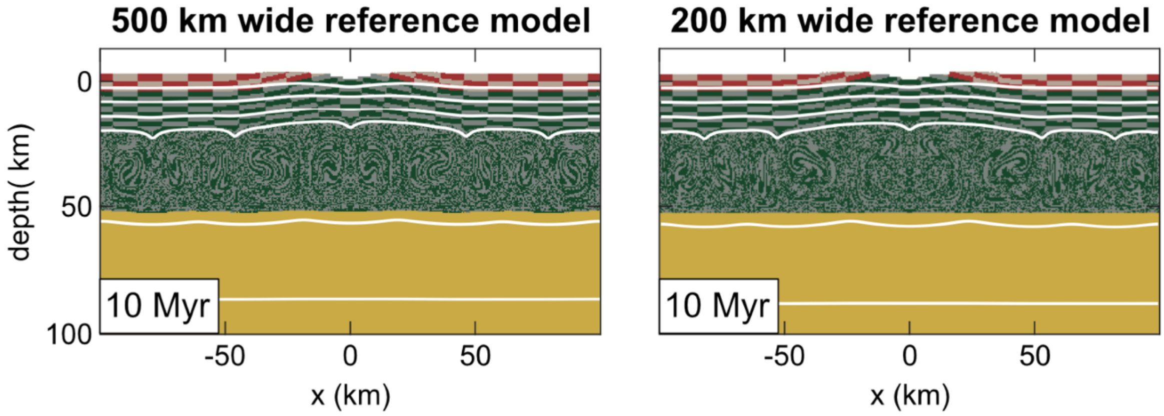

Figure 2: Material and thermal distribution after 10 Myr for a 500-km wide model (left) and a 200-km wide model (right).

The results show that the overall crustal response, as well as the timing and magnitude of lower crustal exhumation, remain very similar to those observed in the original 500 km-wide reference model.