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Interactive comment on "Determining the Plio-Quaternary uplift of the southern French massif-Central; a new insights for intraplate orogen dynamics" *by* Oswald Malcles et al.

Oswald Malcles et al.

oswald.malcles@umontpellier.fr

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Modified manuscript is provided in supplement

Reviewer 1

This article from Malcles et al., presents a nice example of how cosmogenic dating of burial sediments can be used for landscape reconstruction. The multi-methodological approach is particularly interesting, coupling cosmogenic and magnetostratigraphic data with geomorphological analysis and numerical model of lithospheric scale uplift. The article is globally well written and consistently illustrated, even if as I point

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out below, some of the figures can be improved to provide a better understanding of the different datasets The paper is reasonable organized the data are produced and interpreted state-of-the art and the line of arguments is generally consistent and convincing, and supports discussion and conclusions. However, some improvement is required with respect to a couple of problems, such as the erosion trend used as input data in the numerical model. The paper of Malcles et al. contributes substantially to reconstruct the landscape evolution and uplift history of the French Massif Central. The paper fits excellently in the profile of Solid Earth, and I would suggest publication after moderate modifications. Some general suggestions are listed below, and are complemented by specific comments in the attached pdf text file.

Q1: My main remark is related in the interpretation of the onset of the regional uplift. I find that the data well constrain the Plio-Quaternary incision rates but the onset of the uplift is not well demonstrated.

A1: Although we agree with the reviewer #1, the time period covered by our samples is unfortunately following the onset of the uplift and any conclusion on the onset of the uplift would be nothing more than an hypothesis loosely supported by the data. For instance, the area is part of the lithospheric-scale structure which is the Massif Central and it makes sense that the local evolution is linked to the regional evolution. We agree that this question is important to address for a better understanding of the regional dynamic, and hence, for stable continental area deformation driving processes, but it is out of the scope of the paper. We made it clearer in the paper. Line 59-60: adding “Further studies should aim to address the problem of uplift onset, giving more clues concerning the stable continental area but owing the data we presently have, discussing such onset is out of the scope of the paper.”

Q2: Introduction The introduction does not follow a classical organization, and introduction is merged with the tectonic setting and with the list of hypothesis to prove. I do not dislike it, but I suggest to separate in a sub paragraph the discussion of the hypothesis that the authors want to test.

A2: In order to make it clear, we split the Introduction in two parts: 1.1 Introduction (line 36) and 1.2: Working hypothesis (line 95)

Q3: About the three scenarios proposed I would like address your attention on the case of old uplift. The uplift could have started early and you could record only the last <5 Ma history of incision, a probably increase in the incision rates. The attached sketch explain the two alternative scenario and a possible relationship with the flat surface.

A3: First, we agree that the incision-rate is probably not linear if looking at higher frequencies, and could have started earlier. For example, such incision-rate variations have been proposed for the Alps (See Saillard et al., 2014; Rolland et al., 2017; Line 386). There is not any possibility to test, based on our data, if there was an increase in the last 5 Myrs, but we can not rule out a marginal increase over the last 5Myrs. Concerning the flat surfaces, we understand this comment as rising the question of possible apparent dip due to diffusion processes and not due to differential uplift. First, we point out that surfaces present southward dipping on both river-sides (if the dipping is due to diffusion processes, we expect dipping toward the river). Second, diffusion processes will mark the surfaces edges first, creating convex topography. This convex shape (and probable increasing dip) would be discarded either by automatic recognition or by manual control of the surface robustness. To take into account this remark we added for clarity: " Diffusion processes could create apparent tilt of remnant horizontal surfaces. However, we avoid that problem by completing the automatic selection and correction with a final check to make sure that the residuals are randomly distributed over the surface (see below)." (Lines 283-285).

Q4: The age and the geological meaning of the flat upper surface is relevant to reconstruct the onset of uplift.

A4: Agree, but because of the strong uncertainties concerning the ages of the upper surface (e.g. possible important time-lag between the surface formation and the uplift onset), we chose not to discuss it in the paper that deals mainly with the Plio-

Quaternary dynamic.

Q5: Moreover, the relationship of the cave galleries with the upper flat surface and with the geomorphological markers should be better described. Karst model One of the main point in using the cave galleries as ancient base level is to show that cave passages were really connected to the base level and that they are not only a perched level of preferable karst dissolution. For this reason I would like to see the profiles of the cave systems and its relationship with the river and eventually some photos testifying the phreatic style of the passages.

A5: We agree with possible alteration-driven karstification but, we do not propose to discuss the cave formation. Indeed, to our knowledge, there is no way to date the void created by karst galleries, therefore we use them only as empty pockets trapping sediments without any doubt, and now located on canyon wall as already done by Granger et al., (1997, 2001). This is the case at least for the Rieutord caves and the Garrel, for the Leicasse cave we refer the reader to Camus (2003) where the link to fluvial transport has been shown. Therefore, we think that adding more pictures of the caves and figures would not bring a significant gain in clarity. We refer the reader to publication where these relations are discussed e.g. Audra et al., (2001) or Moccochain (2007) and the link to fluvial transport has been shown (Camus, 2003). To allow the reader to have a look at the geometry of the caves we added a link to a university hosted database with a doi where the caves are available in 3D. Line 1548-154: Karst3D (2019). Karst3D data base. https://doi.org/10.15148/940c2882-49f1-49db-a97e-12303cace752

Q6: Why haven't you dated samples from the Leicasse cave, that is placed at high elevation? Higher levels exist (betwen 600 and 700 m) in the same region suggesting an old history of the uplift and incision.

A6: Dating of other samples is in progress and should be subject to publication when obtained. Furthermore, we point out that to our knowledge quartz bearing infilling are not present at 600-700m a.s.l. In the explored caves. Highest known quartz cobbles in the Leicasse cave are located c.a. 450 a.s.l.

Q7: To show the sampling sites within the caves and the location of the caves on the topographic maps could help the reader.

A7: See above for topographic survey database. (Sampling site will be added).

Q8: Geomorphological analysis I found this part interesting and useful to put quantitative data in a regional scenario. I have some doubts about the paragraph organization: the three working hypothesis shown at the beginning seem a bit extraneous in a paragraph where the authors should explain how to extract the data and show the results. I suggest to rethink this organization.

A8: The paragraph structure has been rewritten in order to make it clear with in order: methods, expectations and results. Line 257 to 267 have been moved to Line308-320.

Q9: Also the title of the paragraph seems a bit out of context, better “analysis” instead of “evidences”, for example.

A9: Changed to “Geomorphometrical approach” Line 276.

Q10: I wonder to see some pictures of the analyzed and discussed markers.

A10: We added as supplementary some pictures of the discussed flat surfaces.

Q11: The limit of 2âŮę of slope is questionable. For me the problem is the topographic gradient of the entire margin. Along a NNW-SSE directed profile from Aigoual summit to the Cevennes fault the mean gradient is about 2âŮę, with important local variations that show topographic gradient up to 4âŮę (few kilometers SE to the summit of the Mt Aigoual for example). I would like to see if tilted plans between 2âŮę and 4âŮę exist. The limit of 2âŮę is reasonable for Plio-Quaternary marker, but it is possible that older geomorphological marker could be more tilted.

A11: We agree that using 2° as slope cut-off will limit the detection of some surfaces

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and lead to miss some markers. But we focus on the Plio-Quaternary evolution, therefore the 2° cut-off could even be seen as a proxy for older (and polyphased) surface filter. For instance, old surface in the Larzac plateau have been proposed by Bruxelles (2001) but we didn't include them in our analysis because of their expected old ages and so forth, possible strong alterations, plural-deformation registration, etc.

Q12: How the slope of each marker have been calculated?

A12: As explained in lines 280-302 we use automatic and manual delimitation of surface, iterative plan fitting using extracted DEM points and statistical outliers suppression, and robustness criterions filter. We have modified these lines to make it clearer.

Q13: Numerical modeling The approach is interesting and perfectly reasonable even if I am not the right person to evaluate the details. However, I have same remarks on the input data for the modeling. The authors used a regional distribution of erosion that do not correspond exactly to the published data. In figure 11 the maximum of erosion is placed at the top the Mt Aigoual, with value of 0.08 mm/yr (or 80 m/Myr) (please, change the dimension to homogenize text and figure). But, the values on top reach the minimum values testified also by the oldest thermochronological ages (long-term erosion) of Barbarand et al., 2001 and Gautheron et al., 2009. Also the cosmogenic denudation rates of Olivetti et al. 2016 suggest that the erosion on the top of the massif close to the margin is very limited (values of about 0.04 mm/yr). The values increase along the flank, toward the lower elevation samples, confirmed by your new data set of incision rates.

A13: Dimensions in figure have been changed for consistency. We extended the explanations in the text to explain the erosion-profile setting (lines 352-360): "This profile is a simplification of the one that can be expected from Olivetti et al. (2016) and do not aim at matching precisely the published data because of, first, the explored time-span ($\sim$ 1 Myrs) is not covered by thermochronological data (> 10Myrs) or cosmogenic denudation rate (10s-100s kyrs). Second, we assume that erosion rates are correlated to the first order to the local (10s km2) slopes, that are higher near the drainage divide. This allows to include any kind of erosion processes (e.g. landslides). Third, the model supposes a cylindrical structure perpendicular to the cross section, this implies to average the high-frequency lateral variations of slope, elevation, etc. to derive the actual denudation rate based on these proxies. Concerning this erosion profile, a parametric study with highest erosion rate ranging from 1 to 1000 m.Myrs-1 led to the same first order interpretations.."

Q14: Therefore, the input data of the erosion distribution that the authors used for the modeling is a bit different from the measured data. I think that an erosion trend resulting bigger at lower elevation and minor at high elevation is not consistent with a process of isostatic uplift induced by erosion, supporting event more clearly the authors conclusions.

A14: Using numerous erosion profiles, but with always a pattern of erosion on the relief and deposit off-shore won't change the first order results since they are mainly controlled by the Elastic parameters. We extended description of the choice of the erosion profile to make it clear (lines 352-360. (See A13 above).

Q15: I suggest to be more rigorous in the description of the geodynamic model: for instance the flexural response to the gulf of Lion extension is complicated to invoke, for the distance between the high topography and basin. The role of the mantle upwelling has been proposed by many authors (that have to be cited) that worth to be discussed a bit more in detail (dynamic or isostatically supported, the Massif Central thin lithosphere suggests, in my opinion, a clear contribution of the mantle in the present topography).

A15: We agree that mantle or deep processes are involved in the uplift onset. However, we developed a conceptual to test the role of the erosion-induced isostatic adjustment and not to elaborate more complicated models with too many parameters compared to the constraints that we have. For instance, we cannot decipher if the thin and slightly hot lithosphere is related to dynamic topography with ongoing mantle upwelling or if it is supported by thermal isostasy being a remnant of past processes like the opening of the Gulf of Lion.

Discussion Q16: Lines: 376 is not clear for me.

A16: “Hence, the incision rate has to be balanced to the first order by the uplift rate”. Explanation has been added for better clarity Lines 420 – 424: “Hence, back to the three conceptual models presented in part 1 (Fig.2), we can discard, at first order, the models A (Old uplift-recent incision) and B (Old uplift-old incision) because the obtained incision rate shows recent incision and surface tilting tend to prove a current uplift. Therefore, the incision rate has to be balanced to the first order by the uplift rate. Eustatic variations magnitudes are of too low (100-120 m) to explain the total incision (up to 400m). “

Q17: The only way that I know to re-equilibrate a river profile is a regressive erosion that move from the base level upstream. If the river is full equilibrated means that regressive erosion reached the uppermost part of the profile. Moreover the lack of knickpoint does not prove that incision rate and uplift are in equilibrium, if the landscape undergoes a long topographic degradation.

A17: We agree that the lack of knickpoints do not prove an equilibrium state (and that in general the term “equilibrium” is subject to debate) but it allows to dismiss a strong impact of regressive erosion due to recent sea-level variations or tectonic. To address the concern of long topographic degradation (assuming no uplift?), we point out that such degradation will lead to mass export, then lithospheric unloading and then isostatic-adjustment uplift, which is why we developed our conceptual model to test if this process could be responsible for our observations.

Q18: It could be interesting to know why the rivers profiles from northeastern margin of the Massif (Olivetti et al.) and from Ardeche (personal data) show knickpoints and Cevennes rivers not. If the authors want to discuss about the river profiles it could be interesting to show some data.

A18: This subject looks important and further study should be addressed in this sense. But discussion about the river profiles are beyond the scope of this paper and seems to us unnecessary, especially in a manuscript that is already long and complex. Indeed, for drawing robust conclusions, we should, as suggested by Reviewer #1 enlarge the study to other rivers surrounding the Massif Central and not only the ones in our study. As Reviewer #1we noticed the difference in knickpoints between the southern and eastern margin of the Massif-Central and we think that ot deserve a more complete study, notably to discuss a possible role of karstic dynamic given the major lithological difference between these two regions.

Q19: Onset of volcanism is placed about 13 Ma and even earlier if we consider the synrift volcanism (Michon and Merle 2001).

A19: We agree, according to Nehlig et al., the volcanism is started 65 Ma ago in some places. One of the many issues they highlighted was the diachronism of the volcanic activity throughout the Massif-Central. For example, they shown an activity spanning from 13 to 2Ma with a paroxysm at 8.5 Ma for the Cantal stratovolcano. Dautria et al. (2010) proposed younger ages of volcanic structures southward, etc. We chose 5 Ma as an average of increased activity throughout the area but not as the onset of the volcanisms which is on the other hand is of major importance in the discussion of the uplift onset, as previously discussed.

Q20: Figures: In general the figures are good, but sometimes they lack of useful information such as topographic names (summits, cities, ect). I would appreciate to see the location of the analyzed caves in a map (in the figure 9 for example) and also the profile in a vertical view of the caves to have a look of the general topographic trend, its relationship with the incised river, and to show the sampling sites.

A20: Changed accordingly with some geographical information, trying not to overload the figures. See modified figure.

Q21: Coordinates are lacking.

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A21: Lacking coordinates are now provided in the figure 1 caption.

Figure 3 and 4 could be merged. Please also note the supplement to this comment: https://www.solid-earth-discuss.net/se-2019-99/se-2019-99-RC1-supplement.pdf Modifications included, and answer in the section below.

Supplementary review 1:

SQ1: Line 43: I agree with the recent uplift, but it is still under debate. The topography could be also interpreted as a longlasting degradation of an ancient topography.

SA1: We agree. This hypothesis is presented latter in this section. We changed the sentence in order to present it as our chosen hypothesis but not the unique one possible.

SQ2: Line 58: 9 kms seem a bit too much, anyway big thickness is found in the depocenter basin, such as the center of Ales Basin, while along the Cevennes margin the thickness progressively decrease toward the NW. It is not proven that the entire Cevennes region was covered by Mesozoic sediments.

SA2: Noted and slight changes in the sentence ("reach several kilometers" instead of "be more than 9km". The 9 kms are for the overall SE basins (southward of Ales Basin). Anyway, exact spatial coverage or thickness will not change our point given that the first order is sufficient to our study.

SQ3: Line 60: The uplift event is called Durancian uplift event while the Isthmus is the topographic high formed as a consequence, I think.

SA3: Noted and changed accordingly.

SQ4: Line 69: do you refer to Sanchis and Seranne?

SA4: Indeed, as an example of evolution induced by the extensional period, not as direct study of the watershed evolution.

SQ5: Line 70: To be meticulous, the events are three: the Mid-cretaceous uplift, the Pyrenean compression and the Oligocene extension.

SA5: We agree. The Durancian event (Mid-Cretaceous uplift) is presented before in the section but should mentioned here. Changed accordingly.

SQ6: Line 93:

SA6: Tilt –> Tilting

SQ7: line 55-56: I suggest to use Ma instead of Myrs ago

SA7: Changed accordingly.

SQ8: Line 132: It could be useful for the readers a briefly description of the morphological setting of the area, with the plateau, canyons ect.

SA8: Short descriptions are provided Line 54 to 59. We added call to figure 1 (for the provided topographic map) for visual insights into first order morphology.

SQ9: Line 136: sequence

SA9: Changed accordingly

SQ10: Line 140:

SA10: References writing changed

SQ11: Line 154:

SA11: Some precisions concerning the sediments protolith area are now provided.

SQ12: Line 594: fig. 9.

SA12: Changed accordingly

SQ13: Figure 1: It is not very clear the location of the Massif Central. It could be interesting to see the sampling location divided for methods, where you performed cosmogenic analysis and where paleomagnetism.

SA13: Context map was changed with zoom over western Europe. More information added into the figure caption. See revised figure.

SQ14: Figure 11: change mm.yr-1 in m.Myr-1 "the studied area that include the studies zones" sounds a bit as tautology. It could be better to clarify. the simbol v is gone

SA14: Dimensions changed for consistency to m.Ma-1. Unclear sentence was removed. Symbol v has been added.

Please also note the supplement to this comment:
https://www.solid-earth-discuss.net/se-2019-99/se-2019-99-AC1-supplement.pdf

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