Answer to R1 (Fritz Schlunegger):

I enjoyed reading the paper (despite the hurdles listed below; these are not scientific but editorial) because the topic is interesting, and the data including the story and conclusions should be published. I thus congratulate the authors to have taken such an endevour.

Thank you. It was quite an endeavour as you have guessed it, but it was a pleasant one and we hope that the data will be helpful to the communities interested in the karst dynamic or the intraplate deformation.

However, the paper needs a substantial editorial improvement. This includes the English, which needs to be polished and carefully checked for typos/grammars (I am not native speaking, but I detected many sentences where the grammar is not correct).

English grammar and typos will be carefully checked. We'd like to point out that one of the authors is Australian (native speaker) and that three others have spent several years in Australia or in the USA.

The figures need to be improved as well they are hard to read, and sometimes it is not obvious how they have to be read. For instance, in Figure 3, I see abbrevations such as Vis, Herault etc. on the right side. Are these valleys or regions? Then I see labels such as Cales, Deves-Mejean etc. in the main part of the figure. Are these caves? I though so, but it is not well explained.

Vis, Hérault, etc. are indeed valley. Other cited labels (e.g. Cales) are indeed caves. We'll state it carefully not only in the main text but also in figure captions to avoid any misunderstanding.

I also thought that the photo of Figure 2 is not clear. I don't really see the Vis valley. The sunlight appears to perturb the impression of a dissection.

We will replace the picture with a better one showing more clearly the Vis canyon.

In the same sense, section 4 is not well organized. It starts with a discussion, but the data has not been presented yet - a results section is missing. I do see that the results are all presented in the discussion section, but this needs to be clearly separated.

We will do our best to present the results prior to the discussion to avoid confusion.

Finally, I understand that the Leicasse cave system records a different story than all the other ones, and the major arguments are put forward in section 4.3. This part, however, needs to be better and more carefully explained. For instance, I could not really follow the arguments about recycling of the quartz-rich sediments, but perhaps I missed the point here.

Not only the Leicasse cave deposits are recording a different story but the Fonctionnaire and the Rocas ones as well. However, the main argument supporting the ghost-rock dynamic is indeed brought by the Leicasse cave. Explanation in the main text will be extended for clarity. We will also add a figure to illustrate the recycling of the sediment.

One question I have not understood: Why did the burial of quartz pebbles stopped at 1 Ma?

The burial of quartz pebbles is an ongoing process and did not stopped at 1 Ma. For the cave entrance located in the valley, the sampling is biased by the known caves. For the "plateau" caves, the apparent burial age is controlled by the timing of the cave opening by regressive

erosion. We don’t have enough data to draw robust conclusions, but the farther from the canyon the cave is, the younger the burial age (this is based only on 3 caves so we won't mention it in the text, we first need to find more cave with sediments at different distances from the canyons).

My review might appear as critical, but nevertheless I really enjoyed this paper because it is a nice contribution, but there is space for making it more reader-friendly.

Thank you again, we appreciated your comments and suggestions that will help us to substantially improve our manuscript.

Please also cite Haeuselmann et al. (2009, Geology) as this is a classical paper addressiing the same topic.

We did not find Häuselmann et al. (2009, Geology) in Philipp’s publication list: http://www.sghbern.ch/praezis/publist.pdf, so we will refer to Häuselmann et al. (2007, Geology) paper since it is a classical paper on this topic.

Answers to R2 (Philippe Audra):

Overall, the article is well-written, the discussions are based on the results, and are supported by rather high-quality figures.

Nevertheless, some points, of varying importance, remain to be clarified or reformulated.

Thank you, here is how we propose these issues to provide an improved manuscript.

Minor corrections :

- 40: *...advocates of the conventional model postulate that...* => references ?

Excellent question! It appears that this removal of insoluble particles is implicitly assumed in the literature. Without going into details of the epigenic speleogenesis paradigm (impact of limestone purity, proposed thresholds for hydrodynamic behaviour linked with fracture size, etc. we will refer to Dubois et al. (2014) and Quinif (2010) who discuss the idea of soluble and insoluble products removal.

- 58-60: *while hypogenic and ghost-rock karstification occur below the base level and subsequent tiered karst geometries cannot be interpreted in terms of river entrenchment phases* => references? This is not entirely true, or at least not well formulated. Fantomization initially acts below the base level, but the second phase of removal is dependent on the base level, enabling solid particles to be washed towards the outlets. For hypogenic, the upward flow and corresponding caves are also below base level by definition, but the outlet and connecting conduits may present horizontal extensions connected to the base level, and thus considered as records of the progressive incision of the hydrographic network.

We agree that the second phase is dependent on the base level but only incidentally, when the surface cut through the primokarst. Once the primokarst has been emptied from its alterite, the geometry of the karst network will follow the primokarst geometry with secondary modifications of the network according to the hydrodynamic regime and evolution depending on local conditions. Clearly, our main point is that the overall geometry of the karstic network is controlled by the primokarst and not the base level. In other word, the location of a horizontal level is expected to be linked with the location of a horizontal ghost-rock area and not by a hypothetical stability of the base level. The location of the horizontal ghost-rock area itself being induced by any heterogeneity promoting alteration due to slightly higher permeability. As mentioned by other authors (e.g. Dandurand et al., 2019), fantomization can be induced by convection cells and later on solid particles can be washed away even if they are below the base level, as long as the outlet is lower than the entrances and that potential energy is sufficient to move the particles. In this case, as mentioned by Klimchouck (2017), fantomization could be part of hypogene processes, or we might wonder if hypogene processes could be part of Fantomization! This debate is beyond the scope of this manuscript, but we will modify the text to describe Dandurand et al. (2019) ideas in the introduction to refer to it later in the discussion. As a matter of fact, we are afraid that we might agree to disagree with Philippe Audra on the fact that horizontal extensions might be representative of the progressive incision of the hydrographic network, since it is one of the main results of this study, at least for our study area, and likely for some other areas as proposed by Dandurand et al. (2019). We would like to remind that we are

not pleading for a paradigm change where all the horizontal conduits formation could not be linked to base level evolution, but we show that in our study area it is not likely the case.

- Fig1: the allochthonous alluvial deposits give the impression that they are injected from the valley into the caves, which is possible but limited to the entrance areas. I assume that the sediments are transported from the plateau and stored in the horizontal level => 2 figures distinguishing fluvial alluvium and subterranean sediments would be required.

We agree that in the general case, sediments can be flushed down the cave if rivers flow from crystalline mountains toward limestone plateaus. To add clarity about sediments recycling as requested by reviewer #1 we will add a figure which will also address this comment. In our case, the sediments that can be used to estimate the river downcutting rate have to come directly from the river. This is supported by the fact that cave deposits are usually near the entrance and that the cave geometries (Scorpions, Escoutet) are probably associated with endokarstic loops. We choose this representation for the sake of clarity given the fact that the precise source of the sediment will not change the main conclusion of our study. However, we will add a comment, at least, in the figure caption to make it clear that two sources of quartz rich sediments can be found in the area.

- 155 : *Reported layer thicknesses are usually less than 15m* => reference?

We will modify the text to refer to the geological map of the area and to our field observations where we have never seen layers thicker than a few meters.

- 180: *The low differential incision rate between the two populations of ~40m/Ma, if focused on the CFZ, could lead to earthquakes with long recurrence times, consistent with the unexpected 2019 Mw 4.9 Teil earthquake (Ritz et al., 2020)* => better explain the relationship between the low dispersion of incision rates along the CFZ and low occurrence seismicity, it's not clear at all. Similarly, this point in the conclusion needs to be clarified.

We will expand this discussion in the revised manuscript based on the following rational: if we convert the 40m/Ma incision difference in differential uplift (supported by low river slope gradients of the area), and if this is all accommodated by the CFZ (which has yet to be proven, it could be distributed over a 10-120 km width band long the CFZ), the fault slip rate would be 0.04 mm/yr. According to Wells and Coppersmith (1994), this slip rate would produce for example magnitude ~6.5 earthquake earthquakes with recurrence time of ~10 000 yrs, if off fault or distributed deformation occurs, the recurrence time would greatly increase. We will also point out that it is consistent with numerical models showing that the flexural response of the lithosphere due to erosional unloading (Malcles et al., 2020) could explain this difference in incision rates without needing the CFZ to slip.

- Fig4: the legend mentions "*unfilled data points*" which are not visible on the graph.

Unfilled modified to “white filled” for consistency with the “black filled” ones.

- Primokarst: make it clear in the introduction what this term means in relation to ghost weathering, and ensure that their alternative use actually corresponds to a difference, if any. Otherwise, just use the term ghost.

A brief description of the primokarst will be added in the introduction. We consider that the ghost weathering is more tightly linked with the processes itself of isovolumetric alteration, and

therefore can be local or isolated, while the primokarst refers to the incipient karst network geometry not yet created.

- Fig6: reword legend: 2. Karstic network originating from the focused removal of ghost

We will reword the legend as requested.

- 260: *We suggest that the already proposed ghost-rocks process for large karst networks genesis (Dubois et al., 2014, Quinif and Bruxelles, 2011) can also be applied at the scale of large limestone plateaus and could be the first stage of large void opening prior to the high waterflow hydrodynamic phase* => by definition, ghost karstification doesn't create "large voids", it prepares them: reformulate

We agree, and this is what we meant by "the first stage". We will reformulate accordingly to make more it evident.

Major corrections :

- Fig5a: a much more precise description of the sedimentary profile is needed, rather than a photograph where nothing in particular can be seen, with the caption mentioning levels with or without quartz. What is the nature of this sediment? Fluvial, debris-flow? More generally, all sampled sites should at least be described (profile of the cave with location of the sample as shown in Fig. 5, detailed description of the profile of the section studied, nature of the dated elements (quartz pebbles or amalgam of sands?). This aspect is crucial for assessing the representativeness of the age and the type of interpretation that can be made of it. Generally speaking, this lack is the article's main negative point. Descriptions do not necessarily need to be included in the text and can be referred to in supplements.

We will provide in a supplementary file the sample locations on the cave maps, the link to the 3D cave models if available on the Karst3D data base (https://data.oreme.org/observation/karst3d). All the sites are related to fluvial deposits, but we will also add a brief description of the outcrops where the samples were collected, as well as pictures of the sites. In the already provided supplemental table we will add the nature of the samples (i.e., single cobble of gravel amalgam).

- Similarly, it is essential to provide all geochemical dating data in a table accessible in the supplements. No article should be accepted without the provision of objective data, which can also be later used to critically analyze the results.

The relevant data were already provided in the supplementary table (https://egusphere.copernicus.org/preprints/2023/egusphere-2023-765/egusphere-2023-765-supplement.pdf). The nature of the sediment dated (i.e., single cobble, or gravel amalgam) will be added (see also answer to the previous comment).

- 240*: The main difference with the widely admitted ESP model being that the network geometry is defined by the hypogene/ghost-rock phase and not by the base level time evolution*. I disagree with this conclusion, which ties in with my comment on lines 58-60. The fantomization phase determines the presence of discontinuities (below the base level), which will later be used for mechanical removal of the fantoms along the karst flow axes, but this occurs 1/ according to the position of the base level (not below it), and 2/ as the authors suggest, regressively from the valleys towards the center of the plateau, with the local establishment of horizontal conduit levels clearly correlated with the base level (the blue conduits on Fig. 5 show this very well).

One again we will have to agree to disagree on point 1, as already proposed by Dandurand et al. (2019), removal of ghost rocks can occur below the base level and form sumps or long conduits deeper than 100m below the base level. And even though we would agree that ghost rock was removed only at the base level and above, the horizontal segment of the conduits would have nothing to do with the base level elevation, hence they could not be used as a marker of the base level steadiness. As mentioned in the answer to comment on lines 58-60, we will expand the introduction to describe Dandurand et al. (2019) ideas about drowned conduits formation related to removal of ghostrocks deep (i.e., >100m) below the base level.

- 230: *This regressive erosion works its way from the canyon walls toward the center of the plateau following the primokarst structures, and possibly creating deep sump (>100m) rather than river related tiered cave*. This assertion about the origin of drowned conduits at great depth should mention the sites concerned (source of the Vis? Others?), and must clearly state that it is a suggestion, unverified at present, incidental to the subject of this article.

One again we will have to agree to disagree with Philippe Audra, many drowned conduits are known and dived at more that 100m below the base level in our study area (e.g. Gourneyras spring with explored drowned galleries down to 100 m, while the network shows mostly a horizontal geometry (https://www.plongeesout.com/sites/roussilon-pyrenees/herault/Gourneyras.htm), Vis spring, Durzon spring). These deep phreatic conduits are hardly explained by any other processes. We will expand the description of the context to describe these drowned conduits together with Dandurand et al. (2019) ideas about other drowned conduits explained by the ghost-rock pre-structuration as the Fontaine de Vaucluse or the Touvre spring that are not in our study area. Given that it has already been proposed for other regions, and that it is likely a substantial process in the speleogenesis in our study area, we think that it is not incidental to the subject. To contrary, it is closely related to the subject and ask the question: is the paradigm of conduit development related to base level steadiness verified at the present? We are not willing to push as far since a lot of work is required before drawing any conclusion, but we are not the first ones to state it and we strongly believe that our study brings elements of discussion to this ongoing debate.