Dear Reviewers and editorial team.

Thank you for the comments and remarks that helped increasing the quality of the paper. We hope our responses addressed the issues satisfactorily. Bellow is the point by point responses for both reviewers.

RC1 (Fritz Schlunegger):

This paper still needs some technical improvements before it can be published. In particular, the results section reads like a method chapter, and the first chapter of the discussion section is actually the presentation of some of the results.
Section 3 title is modified as "Methods" and title for section 4 modified as "Results and discussions". The final block from the initial section 3.2 is moved toward the beginning of the section 4.

A section presenting all results in full detail, however, appears to be missing (concentrations of cosmogenic 10Be and 26Al, uncertainties, ages, denudation rates etc.). Figures 3 and 4 could be part of the results. ***I also miss a table in the main text where all results are shown.***
I should have made this comment before, my apologizes to formulate this criticism so late in the review process.
We choose to provide all the results directly in the form of the figures 3 and 4 and not to add the whole table in the main text because it would lead to a 2 to 3 pages-long table or selected information, hence being a reduced albeit duplicate of the supplementary one. We believe that providing all the information (sites parameters, concentration, chemistry, etc.) in one table is best for a possible latter-use and reproducibility.

As a second criticism, there are very long sections (such as 4.2) without any reference to a figure or a table. Section 4.2 presents important information, but I don't see the related numbers of relationships on any figures. This would be important for me to verify if the statements are correct.
References to Fig. 3 (line 220) Fig. 4 (line 231) have been added.

As a third point, on p. 13, it is stated that the lower part of the Rocas cave has similarities to those reported for ghost-rock caves described before. However, the details are not presented. This is actually important, because the new model of cave formation bases on this comparison and also on the age pattern.
We have added a sentence to describe what is similar to ghost-rock caves described before: "In this younger part of the cave, passages show morphologies similar to those reported for ghost-rock caves by Dubois et al. (2014) and Rodet (2014), that is to say the cross section of the galleries are characterized by lens (or half lens) shape extending in the weathered strata while the non-weathered strata and the lower part of the galleries are characterized by scallops, potholes and incised meanders. Furthermore, the preserved ghost-rock at the type of the lens shape have porosities larger than 10 % (Fig. 5).

<u>Minor points:</u>
p. 2, line 41: it... physically erodes and transports insoluble sections. I think you mean: it transports particles that were eroded from insoluble sections. I think that this water does not transport entire sections (which would be too much).
"[...] it also simultaneously physically erodes and transports insoluble sections of bedrock. "
modified as

“it also simultaneously physically erodes and transports insoluble residues from bedrock sections.”

p. 2, line 57: ..as cave sediment infill. Please avoid the juxtaposition of more than two substantives and change to: as sediment infill of a cave.
“infill” modified as “deposits”.

p. 3, lines 82ff: The sentence starting with 'The latter has been used...' is not clear. Please rephrase and ev. make two sentences.
“Large water flow loops at depth have been proposed to explain some hypogene cases since the flow is upward on one end of the loop (Klimchouck, 2017) or used to explain ghost-rock formation and its subsequent drain, sometimes creating a deep sump at more than 100m below the base level (Dandurand et al., 2019). The latter has been used to invoke convective cells as a satisfactory explanation for primokarst formation, subsequent drain, and finally deep phreatic loops such as Fontaine de Vaucluse or Touvre spring.”
Modified as
“Large water flow loops at depth have been proposed to explain some hypogene cases since the flow is upward on one end of the loop (Klimchouck, 2017). Dandurand et al. (2019) refer to a similar process with large convection cells of water at depth to explain ghost-rock formation and its subsequent drain, sometimes creating a deep sump at more than 100m below the base level. In this model, deep convective cells are proposed as a satisfactory explanation for primokarst formation, subsequent drain, and finally deep phreatic loops such as Fontaine de Vaucluse or Touvre spring”

p. 4, line 101: Inherited from what?
“Inherited” modified as “Hercynian-inherited”

p. 7, line 168: we use the Lal (1991) scaling factors
Changed accordingly

p. 7, line 173: erosion that provide modeled concentrations that... avoid the use of nested sub-sentences.
“Both the minimal and maximal combination of burial age and erosion rate that provide modeled concentrations that are in the range of the measured one (± 1σ) are computed to estimate uncertainties.”
Modified as
“Both the minimal and maximal combination of burial age and erosion rate providing modeled concentrations in the range of the measured one (± 1σ) are computed to estimate uncertainties.”

p. 8, line 178: We present in Figure 3 -> In Figure 3 we present
Changed accordingly

p. 10, lines 229ff: The related values have not been presented yet.
A link the Fig. 4 is added.

p. 10, line 247: Based on the cave locations -> please specify what you mean here.
“Based on the cave locations in the vis River channel...”
modified as
“Because of the direct cave-entrances toward the Vis River channel, and the short distance (100s meters) between the sampling site and the cave entrances...”

p. 12, line 287: that are distant of ... the formulation doesn't sound correct to me, but I might be wrong. It just sounds strange.
We didn’t change it as it seems ok, albeit probably not of a common usage.

p. 14, line 330: rather than a river-related tiered cave.
Changed accordingly

p. 14, line 340: and that the evolution of the base level …
Changed accordingly

p. 15: The section of Figure 6 lists 4 items, but the figure itself shows only three sketches. Please harmonize, else it is quite difficult to properly understand this figure.
The caption is modified and the 4 items meaning are integrated inside the figure itself

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RC3 (Régis Braucher):

The paper of Malcles et al. presents burial ages obtained in karstic networks of southern Massif Central. The authors propose for networks far from the river valley flanks or cliff walls that the well accepted epigenic speleogenesis model (network is formed when water table is stable then abandoned when the river is lower due to incision) cannot be applied and propose a model based on speleogenesis controlled by regressive denudation towards inner part of the plateau. Despite I am a bit far from this topic but more attracted by the cosmogenic nuclide applications, I think that these data have to be published after rewriting with more explanations and simplification. At this stage, some parts of the paper are a bit fuzzy, and the cosmogenic methodology lacks important information. See pdf
Thank you, we hope we managed to make the purpose better explained ad that we did provide the cosmogenic-nuclide parameters that you found missing.

Part 3 : Please provide the types of spikes used and their concentrations.
We use in-house spike Abaz5870 with a concentration of 1025 µg/g. This information was added in the table caption.

Precise the spallation production rate used.
We 4.47 and 30.29 atm g^{-1} yr^{-1} as SLHL spallation production rate for ^{10}Be and ^{26}Al respectively. This was added line 161.

What half-lives have been used for ^{10}Be and ^{26}Al?
We used half-lives values from Korschineck (2010) and Chmeleff (2010): 1.387 and 0.705 Ma for ^{10}Be and ^{26}Al respectively. This was added line 124.

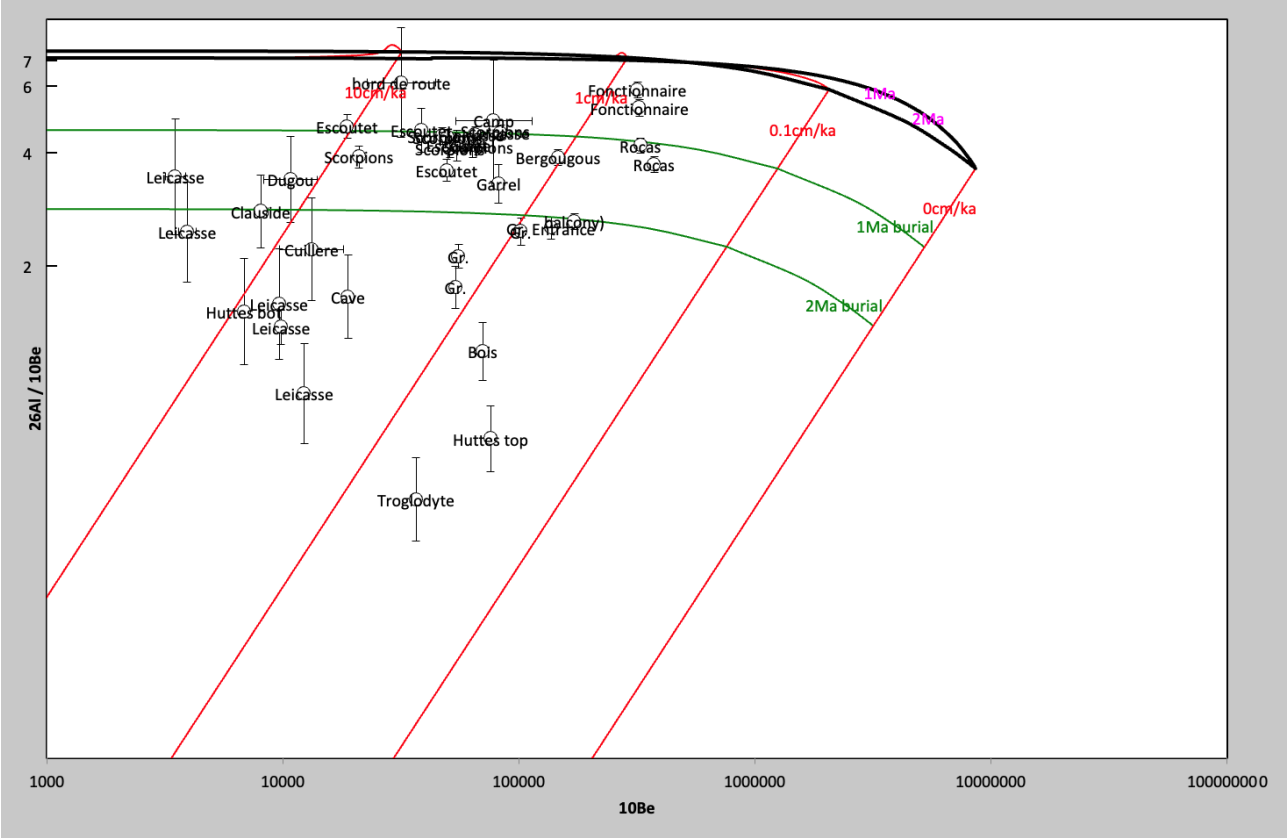
What is the spallation production rate ratio used for $^{26}Al/^{10}Be$ (6.75?)
Yes, we use the value of 6.75. We indicate the total value SLHL instead (line 162 and associated references for both nucleonic and muonic productions)

Line 109 – 123: the age calculation explanations are not clear and difficult to understand.
We modified and completed the explanations, we hope it is clearer now.

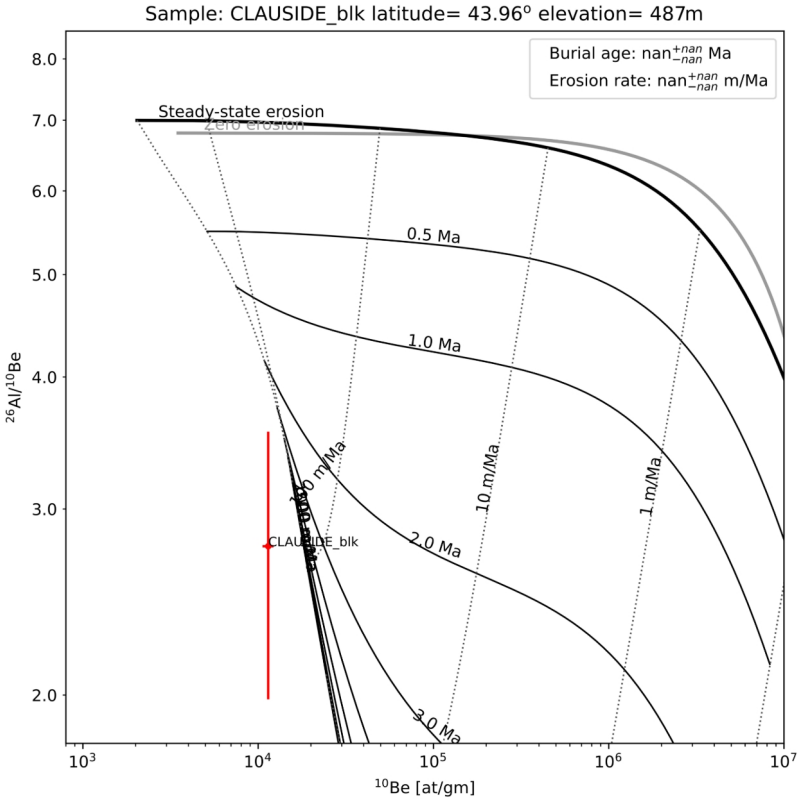
Using the data set provided I have recalculated all ages and paleo denudation rates (see excel table at the end of this review) using a normal approach sample by sample, ignoring postproduction. The clauside amalgam can be modelled (2.04 ± 0.46 Ma and 147.8 ± 33.16 m/Ma).

Thanks for the recalculations, we also did sample-by-sample computations, but we don't know what you mean by "normal approach" here. The mostly insignificant discrepancy between both methods in term of burial ages comforts the proposal of this paper albeit the results for paleo-erosion rates seems sometimes statistically different (at one σ). Because only the order of magnitude should be considered for current river-sand estimation of recent denudation rate (e.g. Sassolas-Serrayet et al., 2019), and given the increased number of unknown for past conditions (source elevation, watershed morphology a few Ma ago, etc.), we do not consider this statistical discrepancy as meaningful and, as stated in the paper, we do consider only the order of magnitude as a useful indicator.
The assumption of no-secondary production for the Clauside site is wrong because of the too small overlying rock thickness with less than 10 m and probably a lower mean density due to alteration or fracturation of the overlying rock. This latter effect might be small. Indeed, we recognize that the secondary production during 10^5 to 10^6 yrs is reasonably smaller than the current one but because the time-production-rate path is not known we prefer not to provide any constraint that might be misleading for further user. A dedicated study using overlying carbonate denudation rate and high-resolution DEM could be performed in order to provide a sounded estimation.

A banana plot will help to have in one figure the entire dataset.



Thank you for the banana plot it shows that we provide all the information for anyone to reprocess our data. Unfortunately, since post-burial production cannot be neglected for some caves, as for example the Baume Clauside, we rather choose not to plot all the samples on one banana plot but provide all the data so anyone willing to have the approach followed by Régis Braucher can do it. We provide here the banana plot for Baume Clauside to illustrate that given the long burial of the sample and the rather low thickness of limestone above, it is impossible to give a constrained burial age.



Regarding the production rate used in the calculation we do not know if it the one of the cave location or the one of the sources of the sediment (mean production rate of the watershed). This will not alter the burial ages but will highly influence the paleo denudation rate determination.

Agree, we use the location of the sampling site (elevation, etc.) for the computation of the scaling factors. For the low-elevation cave with young samples, this assumption is probably wrong and one can assume that the paleo-watershed should have displayed the same kind of geometry and elevation than the current one (if no process as drainage capture or transient dam happened during this time). For older sample, the paleo-elevation or even mean latitude of the watershed is not known because of the regional dynamic (Massif-central uplift, Mediterranean watersheds being aggressors of the Atlantic ones, etc.). For these reasons we choose to use the sampling site parameters bringing at least a processing consistency.

"Theses scaling factors use the sampling site parameters (e.g. elevation)." was added line 170 to make it clear for the readers.

Fig.3. try to use different symbols for a given site. This will help the reader working on black and white paper sheet. In this figure you have plotted two Rocas ages and two Fonctionnaire ages corresponding to two measurements on the same samples. If this is true do not present both data as this will give artificial more weigh to these ages. You can do this when working on different samples.

We do not use the Rocas and the Fonctionnaire samples for incision rate computation, so they do not bring any artificial weight. We think that it is important to illustrate the repeatability of the measurements, and since they don't bring any artificial weighting we prefer to keep them. We have modified the symbols to help readers working on black and white paper sheets.

Line 140-144: the use of isochron approach is not helpful here.

We agree with the reviewer, but we think that we have to show the results of the isochron approach so the readers can make their own minds.

Line 145-162: This par is hard to understand!! You are explaining that samples might have been already buried prior to they are deposited in the network; this yields to a scattering in the age distribution. How can you know the sample position in the alluvium cover before its burial in the network? (Line 52-155:" This sample with the younger age, was the one located closer to the surface in the surface deposited alluvium layer prior to burial. The older age (~4 Myrs) is a better measure, equal or younger, of the emplacement of the alluvium layer that was subsequently buried into the cave. This sample was the one located deeper in the surface alluvium layer before cave burial")

Indeed, the main problem was the ~ 3 Ma of burial differences between the different cobbles in the Leicasse cave system. Because of their current location they do have a final burial in common.

First, this final burial stage can not be longer than the younger age (albeit it can take any value between this youngest age and "0").

If we consider the final burial period as equal to the youngest age (~ 1 Ma), it implies that this cobble did not endure burial prior to the final cave deposit, hence it stayed at the surface with an "infinite" exposition. Consequently, if the true final burial period is shorter than 1 Ma, it means that this cobble was partially buried close from the surface in an alluvium layer before its final burial in the cave. The true depth and residence time can not be properly estimated though.

Second, the total burial (subsurface partial or complete burial + cave) has to be equal or greater than the oldest age.

Consequently, this "4 Ma old cobble" was buried more than the "1 Ma old one". The logical explanation, using a parsimonious approach, is to consider a larger initial depth for the "4 Ma old cobble". But this cobble could have been also partially buried, although deeper that the youngest buried cobble, and therefore it represents a lower estimation of the age of the emplacement of the alluvium surface layer priori to its burial in the cave.

We added, line 246:

"We point out that theses point are relatives, that is to say, if it seems logical that the oldest being initially the deepest and the youngest the shallower, the absolute depth prior to the final burial, however, is unknown. A few constraints can be brought by the fact that the

Line 179-180: What is the mean displacement rate of the CFZ fault, and the mean offset after earthquakes? In Ritz et al. one can find max offset values of 20 cm and it is also mentioned in the same paper that no surface deformation was observed during historical seism. Can you thus conclude that this fault can be responsible of the incision of the studied valleys? What about a global uplift due to Massif Central Mountains?

We do think that the incision is permitted by the Massif-Central uplift and that the CFZ is a key element permitting a rather strong localization of the differential uplift, hence of the deformation. However, if the CFZ dynamic or the precise regional/local uplift rate are interesting questions, they are far out of the scope of this paper and our data only point toward a difference in incision rates, and are only supported by a few points.

Going further toward CFZ dynamic wouldn't be properly supported. For instance, prior to being able to discuss a hypothetical mean offset, the CFZ activity should be thoroughly demonstrated for other parts and shorter time scales. Then, it would only provide informations relative to a more or less constant differential uplift but not toward the repartition of this offset on different faults, or the proper rheology of such a system (fault locking, creeping, lateral variations etc.).

Therefore our aim was only to point out that our data suggest at least a gradient of incision rates across the CFZ. But we are unable to say if this is a regional tilt or a localized deformation on the fault. Given the Teil earthquake on the north-eastern end of the CFZ, proper studies should be conducted in our area too given our results.

Fig. 4; change symbols and change police type for network far from the river cliffs.

Changed accordingly, consistently with the Fig. 3

Line 188: What do you mean by "The unexpected result of diminished burial ages shown in Figure 3…"?
Our point here is that, given the elevation of the Larzac caves relatively to the river, and given the regional trend, we expected ages older than the obtained one (e.g. ~ 3 Ma for the Rocas samples). We added; line 291:
"(when compared with the expected one using the regional trend of ~ 90 m Ma^{-1})".

Line 198. Can you explain you approach here:" speleogenesis paradigm (ESP) which would predict ages 2 to 4 Ma older - or alternatively, a cave level elevation 150 to 250 m lower than recorded compared to the regional base level at the; me of the deposit)?
Given the ESP, and without any ad-hoc complications due to paragenetism, etc. the age-elevation relationship is expected to follow a more or less regular trend: the higher the deposit, the older the age. When using the regional ~ 90 m Ma^{-1} of incision rate we can predict an age for the Rocas or Leicasse, etc. deposit. This predicted age (~ 3 Ma) is way too old when compared to the obtained one (~ 1 Ma).

Line 202: Why the absence of sediment in Rocas implies an age younger than 1 Ma?
This model is in our opinion the best one that can explain the data without the need of many ad-hoc assumptions or physically unsounded hypothesis. Given the fact that the sedimentary infilling dated at 1Ma are incised, at least one erosional phase is needed after the deposition. Because there is no quartz infilings to be found in the lower part of the Rocas, we assume that this lower network did not exist when the 1 Ma old sediments settled in the upper part. Indeed, if the lower part existed at that time, it is reasonable to think that quartz could be found somewhere (e.g. hydrological shadow areas).
It is indeed possible to imagine other models with the lower network being present prior to 1 Ma (or else), as for example assuming a total infilling of the network followed by a total removal of all the quartz in the lower parts only, etc. but such models quickly tends to be irrefutable and physically complex (where did all the quartz, in terms of volume, came from and where did it goes, etc.). Therefore we prefer the simpler model with formation post 1 Ma.

Line 208: Scorpions and Bergougnous sites seem to be affected by the Vis River. Why do you compare the Rocas sediments (from alluvial deposits on top of the plateau) with these two sites?
We compare the Scorpions/Bergougnous and the Rocas to highlight the inadequacy of the ESP relatively to the age-elevation relationship. At first, and along the ESP model, we assumed that the Rocas (or Fonctionnaire, Leicasse) would have shown older ages than the caves located lower (Scorpions, Escoutet, etc.).

Why the same age of 1 Ma cannot be related to the activity of the entire network from Sc/Be to Rocas?
See answer to the "Line 202" question.

As you proposed a new formation model it is worth better explaining this last part synthetized by fig. 6 and show how you construct the chronology from 1 Ma to present.
Thanks for saying that we propose a new formation model, but this not true, it was already observed in near real time by Yves Quinif and collaborators as well as by Joël Rodet based on field observations. We just observed it at a larger scale using TCN, which wasn't our goal since we were aiming at constraining incision rates in the area. To make it more clear that it is related to the downcutting of the canyon in the limestone plateau and hopefully make it more clear for the readers we have modified figure 6.