

General comments

This manuscript explores the important questions of short-term SIC dynamics which have been overlooked for many years because SIC was associated with long-term geological cycle. It focuses on the impact of changes in precipitation regimes on SIC concentrations/stocks/features with a very nice experimental design of precipitation exclusion/addition. Although links between mean annual precipitation, i.e., stable climate, and SIC dynamics have already been reported, the response of SIC dynamics to short-term climate changes remains poorly understood and is addressed in this manuscript. Overall, the results presented in this manuscript are of interest to improve understanding of short-term SIC dynamics under a changing climate. However, this manuscript would have needed more cautious review by the co-authors given the errors it contains and the reorganization it needs. That is unfortunate because it likely partly explains why it took so long for this manuscript to be reviewed and it could have led to its rejection. Consequently, this review is quite long and requires more work, but I truly think these comments would help the authors improve the scope and readability of their nice results. I would like to point out that I am not a native English speaker, contrary to the authors I presume, so I will not grammatically correct this manuscript, and I apologize if this review contains grammatical errors or weird wordings.

Specific comments

I will summary here the major changes I suggest and detail them afterward.

- Title of the article: I agree with the first reviewer that this title does not reflect the entire scope of the study. “Drought” and “SIC stocks” are restrictive compared to the experimental design and the analyses performed. The “sub-decadal” term was a bit fuzzy to me at first. I suggest mentioning in the abstract and the introduction “(≤ 5 years)” after its first apparition.
- Shorten the sentences: several sentences are unnecessarily long. Wherever possible, splitting the sentences could improve readability.
- Reorganization:
 - M&M: The Materials and methods (M&M) could be more detailed. Several elements from Results and discussion (R&D) section should be transferred to M&M section. Moreover, I suggest first describing the materials/samples then the different methods and on which samples they have been applied, for instance:
 1. KAEFS study site: localization, climate etc. and the soil profile: sampled with hand auger, depth, number of samples, textural analysis, description (color, structure, texture) etc.
 2. Drought-Net experimental plots: treatments, date of sampling/treatment duration, depth, sampling method (hand auger?), replication, number of samples, sampling for bulk density, VWC analysis etc.
 3. Aggregate-size fractionation: protocol description, reference, checking of aggregate persistence etc.
 4. Aeolian deposition collection and XRD analysis: aeolian deposition sampling, bedrock sampling?, XRD device etc.

5. Elemental analysis: drying/milled, pretreatment for SOC/SIC, device used, calculations, sample analyzed etc.
 6. Pedogenic carbonates micromorphology: device, mounting, sample analyzed etc.
 7. Soil carbon stock calculation: surface estimation, bulk density estimation, consideration of rock fragment content, formula, units etc.
 8. Data analysis: software used, statistical test performed, function used, interpretation of the tests etc.
 - ➔ Maybe an illustration (map of the sampling sites, summary figure, table of samples/analyses?) could help the reader understand the experimental design, which analyses have been performed on which samples etc. This is only a suggestion, maybe reorganizing the M&M section will be enough.
- R&D: The second part of this section is very long compared to the others. Consider reorganizing this section. Moreover, the titles of the sub-sections look like the ones of M&M section. I suggest modifying the titles of section to help the reader understand what results are presented but also for what goal. For instance: 1. Information on SIC dynamics at KAEFS site given by the soil profile study, 2. Influence of precipitation treatments on SIC concentrations, 3. Influence of precipitation treatments on SIC stocks. Or splitting the R&D section into 3. Results and 4. Discussion.
 - Interpretation of experimental plots results: I strongly believe linear regression is not the adequate statistical test to perform on these results. First, simple pairwise comparison between the treatments would be more significant and intuitive. Then, the linear regressions can be mentioned but should not be presented as the major results. In my opinion, the results on the 0-15 cm depth should be at least reported and eventually compared to those of the 30-45 cm depth.
 - Discussion of results: the links between “result sentences” and “discussion sentences” could be improved to help the reader understand the reasoning. For instance, if I understood well, the soil profile has been studied to have a larger picture of SIC dynamics in the KAEFS site. Thus, the conclusions useful for next interpretations should be more highlighted in the first sub-section. In the second sub-section, the transition from SIC concentrations results to the PC formation discussion should be justified as SIC includes geogenic and pedogenic carbonates. Here, I guess the assumption is that the difference in SIC concentrations between extreme drought treatment and the control plot (ambient) is due to new PC precipitation. This should be clearly explained at the beginning of result interpretations.
 - Titles of Figures: Figure titles should only describe what the figure shows and the elements to interpret it. Figure titles should not include new interpretations that are not mentioned in the text.

Technical corrections

1.9 (and where it applies): I suggest to cautiously use the terms “sequestered” or “sequestration” which convey a different idea than “stored” or “stockage” (see for instance, Don et al., 2023, *Carbon sequestration in soils and climate change mitigation—Definitions and pitfalls*). To my knowledge, these considerations are still ongoing for SIC and, as explained later in Introduction,

determining if PC precipitation/dissolution result in net atmospheric CO₂ removal/emission is not straightforward

1.10: please add “(≤ 5 years)” after sub-decadal

1.12 (and where it applies): I agree with the first reviewer that the terms “persistence” or “stability” should be used with caution/justified given the sampling design i.e., one sampling after 5 years of treatment

1.21: add “(MAP)” after mean annual precipitation

1.26: add a point at the end of this sentence

1.27-28: this definition of PC is unusual and restrictive from my point of view. I suggest defining geogenic/lithogenic (please chose one terminology) carbonate (coming from bedrock alteration) at the same time as pedogenic carbonates (precipitated within the soil). As mentioned later (1.37-38), PC can also form from geogenic carbonates dissolution, not only from “respired carbon dioxide (CO₂)”

1.29: I suggest also referring to the bibliometric analysis of Raza et al, 2024, *Inorganic carbon is overlooked in global soil carbon research: A bibliometric analysis* and specifying at what depth these stock values refer to

1.37: the “PC” abbreviation is already detailed earlier

1.41-44: please split this sentence, for instance: “To this point, semi-arid and arid environments are generally considered best for the formation and stability of PC. However subhumid and humid soils can also host significant stocks of PC. Development of PC in humid environments has been hypothesized to result from either a carbonate-rich parent material or summer rainfall followed by a distinct dry season (Railsback, 2021; Srivastava, 2001).”

1.51-52: The significance of PC dynamics to soil CO₂ efflux is not explained before 1.55-61. The order of the Introduction ideas could be improved.

1.52: CO₂

1.58: The C abbreviation could be defined.

1.59-60: As mentioned in the previous sentence, the links PC dissolution/CO₂ sequestration and PC precipitation/CO₂ emissions depend on the source/fate of reagents/products of these equations at the global scale. These considerations are greatly explained and summarized in Sanderman, 2012, *Can management induced changes in the carbonate system drive soil carbon sequestration? A review with particular focus on Australia*. I suggest cautiously addressing these questions by using modal verbs as “could” or “might” and/or detailing with more references.

1.64: add a space between the end of the sentence and the references.

1.68: “the relationship between” what? Please check this sentence, it seems unclear to me.

1.70-73: Summarizing the results and perspectives at the end of the Introduction seems unusual to me. I think these two sentences belong in Results or Conclusion/perspectives sections

1.75: Materials and methods

1.84: please give more details on the parent material (calcareous?, calcite/dolomite?) as it has been sampled if I understood well.

1.89-91: I suggest specifying the dates of treatments implementation and soil sampling i.e., specifying the 5-year duration of the treatments, earlier in the Drought-Net plots presentation. It could be specified that -100% = no precipitation at all. What is the difference between “ambient precipitation (0% change)” and the “unmanipulated control”? I guess there is none as 7 precipitation treatments are counted and 8 are described.

1.95-97: I suggest splitting this sentence and starting with the shelters overhang aspect then the overall effect of the shelters.

1.98: As the three-times replication is explained at the beginning of the sentence, I am not sure that the “(n=3)” mention is necessary. However, having the total number of soil samples could help to understand the sampling design: 7 treatments x 3 replications x 2 sampling depths so a total of 42 soil samples if I understood well.

1.99: I am not sure that the reference “(Projets, n.d.)” is useful.

1.101: 1.164-165 belongs in the profile sampling description. “the soil profile excavated” has never been mentioned before. The “hand auger” method must be clearly mentioned as a “soil profile” is usually described through a pit. A picture of the samples in the gutter (I presume that how the horizons have been delineated) could be useful in Appendix if one exists.

1.103-105: The difficulties in quantifying SOC and SIC in calcareous soils could also be one of the reasons why this type of soil has been less studied until now (see for instance, Shamrikova et al, 2024, *Methods for Measuring Organic Carbon Content in Carbonate-Containing Soils: A Review*; Apesteguia et al, 2018, *Methods assessment for organic and inorganic carbon quantification in calcareous soils of the Mediterranean region*). Thus, please be cautious with the wording and devote a sub-section of M&M to specify the method (which is the same for all samples I guess) and help further meta-analysis. EA does not directly measure SOC and SIC, it measures TC only. For SOC and/or SIC measurement, pretreatments are necessary before the EA to remove one of the C forms. Consider correcting this paragraph accordingly, for instance: “Concentrations of TC, N and S were measured using dry combustion EA. SOC concentrations were measured by pretreating the samples with in-situ HCl acid treatment [removing the SIC] before the EA. [...] Then SIC has been calculated as TC-SOC.” In addition to the references, please report at least the HCl concentration and the duration of the acid pretreatment.

1.105: please specify somewhere how many soil samples have been collected in this profile. “each horizon” is mentioned but 5 horizons are described in Table 1 and 6 points are displayed on Figure 1.

1.108: maybe “aeolian deposition” or “atmospheric deposition” would be more appropriate than “dust”.

1.110: Figure 2 shows the diffractogram of “whole rock”, referred to as “parent material” in the text. How, when, where has this sample been obtained? Please specify this in this section or the KAEFS study site presentation section or wherever it is relevant.

1.115: I do not understand the title of this section. This section focuses on soil fractionation. “SIC response to drought manipulation treatment” sounds like a title of Results section. I do

not understand the link between this soil fractionation and the SIC response to drought. Why drought only? Please consider modifying this title.

1.117-118: Sodium hexametaphosphate (HMP) is usually used for particle-size fractionation. It is unusual to use HMP for aggregate-size fractionation because it is a dispersant which could damage the aggregates. Please add a reference using this protocol and specify the HMP concentration, the duration of the dispersion and how the presence of remaining aggregate has been checked.

1.123-124: I do not understand the last sentence of this section. It should be linked to the sentence 1.118-119 I guess. Why SIC:TC ratio? Why not only SIC? Is this ratio will be compared to the one of bulk soil?

1.134-145: as SOC stocks are also mentioned in R&D section, please replace “SIC” by “C” in the 2.6 section

1.139: it is unclear why C stocks have been estimated at 30-45 cm only, why not at 0-15 cm? As mentioned earlier, I believe the results of 0-15 cm should also be reported.

1.139-140: I suggest clarifying this sentence, for instance: “by averaging SIC concentrations of the three replicated plots for the 0%, -100%, and +50% precipitation treatments...”. Otherwise, it gives the impression (at least for non-native English readers) that the SIC concentrations of 0%, -100%, and +50% have been averaged.

1.140: Specify how the bulk density has been estimated (1.166) and how the rock fragment content has been considered. These are important considerations for calculating stocks (see for instance: Poeplau et al., 2017, *Soil organic carbon stocks are systematically overestimated by misuse of the parameters bulk density and rock fragment content*).

1.141: Please add “C stock =” before the formula and “in [unit]” after the formula.

1.143: C_{SIC}. [SIC] is also a conventional way to write concentration in formula.

The unit are usually written as g.m⁻³ rather than g/m³.

Usually, the C (or other elements) concentrations are expressed in mg g⁻¹ or g kg⁻¹, % is not an SI unit. Unfortunately, I think it would be better if the concentrations could be converted to mg g⁻¹. The unit % is already used for drought/wet treatments. Moreover, the dimensional analysis of the equation is consequently weird. By using mg g⁻¹ for concentration, it becomes clearer:

$$m^2 \times m \times \frac{g}{m^3} \times \frac{mg}{g}$$

1.143-145: I do not understand this last sentence, why would this stock calculation reflect micro-pedogenic carbonates only?

1.148-149: Please specify what statistical tests have been conducted, their hypothesis to know how to interpret the p-value reported below and, preferably, the function used (e.g. *lm()* for the linear regression).

1.150: “All analyses were conducted using the Python and R programming languages”, I guess this part of the sentence must be removed.

1.150-151: what is SSURGO? This acronym has not been mentioned before. Grant soil series have not been mentioned before either. As the use of ArcGIS Pro has been already mentioned in the previous section and these considerations concern only the Appendix, I suggest moving this explanation with the Appendix, in the Figure caption or below the Figure. Also, I suggest clarifying the difference between Nash-Lucien and Grant or using the same terminology if there is none.

1.154-160: These sentences do not belong here. This summary of the experimental design could be moved at the very end of Introduction or dispersed into M&M section.

1.160-161: This sentence belongs to Conclusions

1.164-170: these sentences belong to M&M section except the second sentence about Grant series soil and the bulk density values.

1.167: g cm^{-1} is not a density unit. In section 2.6, the density unit was g m^{-3} .

1.172: table captions are usually above the Tables while figure captions are below the Figures. Moreover, the low pH (<7) and substantial [SIC] ($> 1\%$) of the upper horizons should be discussed somewhere.

1.173-174: It should be mentioned somewhere that the correlation between TC and SIC could also be due to incomplete SIC removal before [SOC] estimation.

1.185-187: Captions of the Figures should only describe the figure: “Total C, SOC, SIC... concentrations ([unit]) according to depth of the soil profile representative of ... characterized at ... and identified as ...”. The last sentence on visible pedogenic carbonate can be mentioned in the caption but should also be written somewhere else in the text. From my point of view, it is an important observation which should be highlighted because these pedogenic carbonates have been observed at a greater depth (75-95 cm) than the depths considered afterwards (max 45 cm).

1.188-199: This paragraph is interesting but unclear. First deposition of gypsum-rich dust is mentioned then unvalidated at the very end by the absence of gypsum in the XRD diffractogram of the aeolian deposition. I suggest first addressing this inconsistency by explaining that [S] in the profile suggests gypsum deposition, however XRD analyses do not corroborate this hypothesis because... etc. Then addressing the eventual disturbance of aeolian deposition by the shelters and finally concluding on how these observations are useful for the interpretations of the next results.

1.203-204: “XRD diffractograms of parent material (dotted line) and aeolian deposition (solid line) samples collected... Major peaks ...”.

Parent material sample should be mentioned in M&M. I suggest using “parent material” or “bedrock” rather than “whole”.

No calcite peaks have been identified? If yes, I suggest mentioning it somewhere in the text, otherwise, please label them on the Figure.

The two last sentences should be removed or reworded. General/new interpretations cannot be in figure captions only. “The higher dolomite peaks in aeolian deposition samples than in bedrock samples suggested an external source of dolomite.” is acceptable in the caption if

discussed in the text. However, according to the figure caption, the dolomite peaks seem to be mainly present for the parent material rather than in dust. Please check this inconsistency between the Figure and the interpretation.

1.206-209: These two first sentences are unclear and maybe not necessary. I think this aspect of “soil-forming factor” should be first mentioned and explained in Introduction or M&M sections then eventually remembered here. Given the treatment duration (5 years), I am unsure considerations at the time scale of soil formation can be made.

1.212-213/Figure 3:

I feel that linear regression is not the adequate statistical test to perform here, I think pairwise comparison will be more adequate. A p-value of 0.031 is indeed significant because inferior to 0.05 but is still very close to 0.05... I presume this p-value is the one given at the end of the summary of *lm* model on R (F-statistic) indicating the overall significance of the regression by checking if at least one of the model coefficients (slopes or intercept) is not zero. It is usually way closer to zero for significant regression (expressed in 10^{-x}). Another usual parameter to evaluate a linear regression is the R^2 which reflect the proportion of variance explained by the regression/the dispersion of the points around the line. I guess the R^2 is not reported here because it is quite far from 1. I suggest starting the interpretation of the results by simple pairwise comparisons between chosen precipitation treatments. For instance, I guess the p-value of a Student test (t-test, as I presume the variable is “normal” as it is only 3 values) between -100% and 0% or +50% would be way more significative and could be reported as the major results. And eventually, indicating afterwards that a global linear regression exists but global p-values of linear regressions at 0.03 should not be, in my opinion, the major results presented as they are quite weak. Moreover, I suggest also displaying [SIC] at 0-15 cm on Figure 3 (for instance, with another color). No significant difference or relationship is also a result, which is interesting to discuss or at least report for further comparison with other studies (and citation!). If error bars overlapped, the points can be spaced out horizontally (R *ggplot* function: *position = position_dodge()*) to improve readability.

If you choose to keep this linear regression, please specify, in M&M section, (1) if the regression has been performed on the mean values of the three replicates or if each measurement have been considered as independent values (I presume each measurement have been considered otherwise the p-value would not have been significant), (2) how you transformed the categorical variable of precipitation treatments, (3) to what corresponds the p-value: overall p-value of the regression, p-value of the slope or p-value of the intercept? and, in R&D section, (4) the R^2 .

1.215-219: “Mean SIC concentrations ([unit]) for each treatment Error bars represent The dotted line indicates the linear regression (dotted line, $p = \dots$, $R^2 = \dots$) between ... and”. The indirect measurement of SIC is already explained in the M&M section. Please choose one way to talk about the different treatments “precipitation treatments”, “rainfall treatments” or “drought treatments” and do not change afterwards. I think “precipitation treatments” as mentioned in the M&M section is clear enough.

1.220: I presume the 3 means of intermediate treatments are not statistically different because of the variability justified well afterwards. This should be clearly mentioned at the beginning of their interpretations as it is explained afterwards that “the influence of treatment on soil moisture” cannot be reflected by these intermediate treatments because there was no significant

difference in VWC. The “more gradual slope” is in the opposite direction of the trend. I think highlighting the lack of significant difference and explaining it by similar VWC fits well with the interpretation. As mentioned, the influence of precipitation treatments is reflected by the end-member treatments with, I hope, significant difference with the control. I suggest addressing more cautiously these indistinct [SIC] and not overinterpreting the data.

1.221-225: please mention this VWC data somewhere in M&M section as they are interpreted in the results.

1.231-233: The variability of all treatments is higher than the unmanipulated control. I suggest also discussing the treatment duration as this variability could also be due to difference in equilibrium recovery of the treated soils compared to the undisturbed soils.

1.233-237: these two sentences are interesting however I do not understand their link with the intra-treatment variability mentioned before. The fact that different precipitation manipulation could have impacted the depth of wetting front does not explain why this impact is different for the 3 plots under same treatment.

1.238: were the [SIC] at 0-15cm different from those of 30-45cm? How? No statistical difference between the mean [SIC] of the extreme treatments and the control ones neither?

1.239: Does “mesic” refer to +50% treatment? Please specify. More generally, it could improve the readability to homogenize the terminology for paraphrasing the precipitation treatments and, possibly, specify it in M&M section. For instance, 1.210-211: “Extreme drought treatments” refers to -100% and -80% I presume, then 1.223-224 “intermediate treatments” refers to -20%, -40%, -60%, -80% and 0% while few sentences before, “intermediate treatments” referred to -60%, -40% and -20% only.

1.240-245: please clarify the links between the “results sentences” (two first sentences) and the “discussion sentences” (rest of the paragraph). The link between [SIC] and PC should be clearly explained as mentioned earlier. The assumption that additional PC precipitation is likely not controlled by Ca^{2+} availability on the plots under -100% (I presume) could be discussed as the possible impact of shelters on aeolian deposition has been mentioned earlier and this additional PC formation has been reported on the 30-45 cm depth only. The link made between additional PC formation under drought treatment and supersaturation of soil solution (saturated with what?) and decrease of pCO_2 should be more detailed. What “dissolution of pre-existing soil CO_3 ” means? I think CO_3 does not exist in soil, it is a very unstable oxide. HCO_3^- , CaCO_3 , CO_2 ?

1.246-247/figure 4: same comment than 1.212-213/Figure 3. Please describe the effect reported in Figure 4 as it is done for Figure 3.

1.246-247: This identification of iron oxides in the soil fraction has not been mentioned before. Please specify how in M&M section.

1.251-254: why geogenic carbonates could not be found in the $< 53 \mu\text{m}$ fractions?

1.258-259: which variables? Which results? Only concentrations have been reported yet, why is whole soil storage mentioned? This sentence is unclear.

1.261-262: same comment than 1.215-219.

1.266-295: I am wondering if these observations, or part of them, should not be mentioned earlier to justify that greater [SIC] concentrations under drought treatment are likely due to additional PC formation.

1.270: I do not understand the mention “over the preceding 3 years”?

1.271: “regimes. The -100% treatment, representing...”

1.283: why “over 3 years of treatment” while the M&M section mentions that the treatments have been implemented in 2016 and the soils sampled in 2021?

1.292-295: Describe only what the figure shows, e.g. “Images of the energy dispersive X-ray analysis (EDXA) of the PC grain from extreme drought plot. Pink indicates ..., blue indicates ... (...), and black indicates....”. The rest goes in M&M.

1.301-311: The storing aspect also depends on bulk densities, not only [SIC]. These considerations should be addressed after stock values.

1.302-303: I suggest explaining the *per descendum* concept here or in the Introduction and making the link with the results on the soil profile, which corroborate this assumption.

1.314-315: same comment then 1.283

1.319: CO₂

1.325: why referring to Figure 5?

1.326: 6.01 10⁶ Mg. Please check these values according to the comment on the formula (conversion of [SIC] from % to mg g⁻¹).

1.326-327: I suggest also reporting the stock values for 0-15 cm depth.

1.328: SOC stocks? I suggest also reporting them.

1.332-333/1.334-335: “Recent research by Li et al. (2024) suggested that drought might exacerbate SIC losses”/“The authors suggest that under drought conditions, SIC will contribute the largest proportion of soil C losses (Li et al., 2024).”

1.342-350: the main goals and results could be summarized with more details in Conclusions section.