egusphere-2022-1092 (SOIL) - Answers to reviewers comments (iteration: minor revision)

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Title The QuantiSlakeTest, measuring soil structural stability by dynamic underwater weighing

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MS type Original research article

Iteration Minor revision

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Dear Reviewer, Dear Editors,

We would like to thank you agin about the time spent for this second revision of our manuscript. Most comments appear to be relevant and will help to strengthen the manuscript.

Revision 2 - Reviewer comment

Suggestions for revision or reasons for rejection

- Overall, the articles has been much improved in terms of clarity and the authors should be commended for the revisions. There are nevertheless a few minor issues left to address.

  – –> **Thanks a lot for this comment.**

Tittle

- "The QuantiSlakeTest, measuring soil structural stability by dynamic underwater weighing" : strictly speaking, the weighing is not done underwater. The sample is underwater. What about "The QuantiSlakeTest, measuring soil structural stability by dynamic weighing of undisturbed samples immersed in water"?

  - **–> We agree with the comment and adopt the proposed title for the article : The QuantiSlakeTest, measuring soil structural stability by dynamic weighing of undisturbed samples immersed in water**

Abstract

OK

- **–> Thank you**

Introduction

I greatly appreciated the changes brought by the authors. The reading is now more fluid, and the text more focused. Nevertheless, there are some mostly minor issues that need to be addressed. See also the annotated manuscript for typos and corrections of English language.

- **–> Thanks a lot for the general comment. We include the correction of English language suggested and other remarks done in the pdf**

- L24 : please add a reference

  - **–> We added (Lal, 1991)**

- L25-26 : remove 'of Belgium', because this is equally true across all of the western European loess belt; add a reference supporting the fact that structure is particularly relevant for Luvisols (I don't think it is true, but what the authors probably mean is that structure of Luvisols is particularly sensitive to management (due to their texture) and therefore even more attention has to be paid to structure for these soils)

  - **–> We removed Belgium**

- L34 'structural stability' would probably be more relevant, as 'aggregate stability' does not relate to compaction.
  - –> **We changed.**
- L44 : I didn't check the publication of Meersmans et al. 2011, but I don't think these authors are actually at the source of this threshold value. It is good practice to cite original papers rather than citing authors which cited other authors. Though I've also read about the 1.2% SOC threshold for luvisols, a recent paper has reported that this threshold may be closer to 2% SOC (Shi et al., 2020; `https://www.sciencedirect.com/science/article/pii/S0016706119310298`)
  - –> **Thank you for your advise on avoiding bypass when citing scientific articles. The Shi et al paper relies only on the fast wetting test of Le Bissonnais which explains why the limit is higher. The 1.2 or 1.15 value generally considered as a suitable threshold in Belgium comes from a technical report of Van Camp et al. (2004) that is generally considered for loessic Luvisols of Belgium, so we will stick to it.**
- L51-53 : rephrase sentence, because climate and soils are all part of environmental factors (not just topography), while 'soil cover' is more of a management factor (and not an environmental factor).
  - –> **The rephrased sentence is: "Soil erosion is governed by rainfall erosivity, topographic factors, soil cover and intrinsic soil erodibility, depending on several soil properties such as hydraulic conductivity and aggregate stability"**
- L59 significant presence of Al only in soils with pH < 5 (strongly acidic). Even though base saturation will progressively decrease with decreasing pH, Ca and Mg will thus remain important in weakly acidic soils
  - –> **Precision is now given : Al in strongly acidic soils and Ca and Mg in slightly acidic to slightly basic soils.**
- L75 : sounds intuitive, but has this been studied ? A reference would be nice. Compaction most strongly affects macroporosity, which is typically not the porosity found inside small aggregates. Macroporosity

corresponds more to pores in-between aggregates + macropores resulting from biological activity (roots, worms, etc.)

  - **–> As this input had been suggested by the reviewer himself in the first round of review, we were supposing that it was a known cause to effect relationship. We didn't identify literature making this link in our bibliography so we propose to rephrase: "The resistance of soil to mechanical breakdown possibly also improves resistance to soil compaction due to traffic on the field"**

- L75 "differential swelling occurs under wet conditions" : actually, it happens during the process of wetting. In the presence of swelling clays, as soon as free water is added, swelling will occur as a result of water moving in-between the clay platelets.

  - **–> Rephrased sentence: "... occurs during soil wetting"**

- I'm surprised by the statement that differential swelling mainly plays a role at macroscopic scales. I could not find this in the paper of Le Bissonnais 1996 (which is cited here by the authors). On the contrary, it says in that paper To my understanding, differential swelling is a microscopic mechanism (separation of clay platelet) inside clay domains. Because the orientation of the platelets of different domains is more or less random, the swelling generates mechanical stresses inside the macroscopic aggregates.

  - **–> Rephrased sentence: "Differential swelling plays a role at both macroscopic and microscopic scale and may split the soil into macro- or micro-aggregates."**

- L82-89 : I would have expected a few words about the 'scale' of the units being subjected to aggregate stability measurements : from aggregates of a few mm to ... (cores) ?

  - **–> The sentence was completed accordingly : "Traditional methods are destructive and rely on the resistance of soil aggregates to soil undisturbed cores to fragmentation under wet or, less often, dry conditions."**

- L98 : what do you mean by 'delay' ? 'labour requirement' ?

  - **–> Yes, we adopted the proposed term**

- L100 : throughout the introduction, 'aggregate stability' has been used (almost) exclusively. Here, the authors switch to 'structural stability'. As mentioned in the previous review, I believe this is an adequate use of terms, but for the reader this switch happens insidiously. It would be good to explicitly express this conscious change in terminology. This is also related to the 'scale' issue raised in relation to L82-89.

  - –> **We propose one complementary sentence to clarify this point: "The test works on soil cores of a large volume (100 cm^3), therefore we consider that QST provides measurements of soil "structural stability" rather than aggregate stability (related to the properties of isolated aggregates)."**

- L109-113 : these lines should be moved to the beginning of the M&M section.

  - –> **We moved them**

Materials and methods

- L127 I believe plots must always be aligned in rows in a Latin square design

  - –> **We removed this information**

- L141 : please specify whether the means are followed by standard deviation or standard error

  - –> **we added this info : standard deviation**

- L158 'repeated three times in ... two blocks' ? 'three blocks', I suppose? Please check or explain (I assume 1 block = 1 rep)

  - –> **In fact, rach of the two blocks contains 2 repetition. We will be more clear, writing « repeated six times (i.e. three repetitions in each block) ». The total is well 54 plots (=3*2*9) is already stated in the manuscript.**

- L177- L178 : It was initially not clear to me that these oven dried samples were used ONLY for bulk density determination.

  - –> **OK, FYI we already tested these samples, but the results is beyond the scope of our proposed manuscript.**

- L212 change to 'namely slaking, mechanical breakdown and differential swelling – clay dispersion . . . '

  - –> **We adapted accordingly**

- L216-219 : this should be moved to the introduction (see earlier comment regarding aggregate / structural stability)

  - –> **The paragraph can be removed since the information is now given previously in the introduction**

- L223 'timestep' (units = time), and not 'frequency' (units = 1/time)

  - –> **OK**

- L245-246 : the nota bene is unclear to me. Please rephrase

  - –> **This is of secondary importance, we removed the nota bene**

- L249 : 900 sec. sounds arbitrary, but probably based on the authors experience of when the mass loss becomes negligible for a majority of samples. Please justify briefly the origin of these '900 sec'.

  - –> **Yes, we added this precision : « These two indicators were calculated for a reference time of (900 s), for which the loss of soil from the basket was negligible for a majority of samples »**

- L251 how were roots separated from soil ? by sieving ? on what mesh size?

  - –> **We simply removed roots retained the « qst basket ». So it is just a global information. We added : « root biomass retained in the 8 mm-mesh metallic basket »**

- L259-260 and L265-266 : the authors explain on L259-260 that they used a linear mixed model approach to evaluate whether soil management affects QST indicators, and on L265-266, they say they used ANOVA for the same purpose. Please clarify.

  - –> **Our text was indeed confusing, sorry for that. We used mixed models (with restricted maximum likelihood as the estimation method), not classical ANOVA (with**

least squares as the estimation method). The results of our F-wald tests are presented in what is still often called an "Anova table", and the function of the package we used is called "Anova", which can indeed cause some confusion. We have removed references to the "Anova" table to avoid ambiguity. The proposed text is now :

In order to test whether soil management practices affect QST indicators, Linear Mixed-Effects Models were fitted. For each model, the QST indicator was used as the outcome variable and the treatments of the trials were used as a fixed explanatory variable, whereas the blocks were defined as a random effect. As several samples were related to one single plot (157 QST in total from 35 plots), the plot identifier was added as a random effect of the model to take into account the dependence between field replicates from one plot.

The normality and the homoscedasticity of the residuals of the models were verified using respectively Shapiro-Wilk and Bartlett tests. For all models, the significance of differences in QST indicators between soil management practices were tested using Type II Wald F tests with Kenward-Roger estimation of degree of freedom (Rcar). When the F-test was significant ($p<0.05$), post-hoc comparisons were performed: treatments of the trial were compared pairwise at (0.05) probability level of significance using estimated marginal means (Remmeans).

All statistical analyses were performed using R version 4.3.0 (2023-04-21) software (R2023). The linear mixed-effect models were performed with the lme4 package (Rlme4), the Wald F tests with the car package (Rcar) and contrast analyses with the emmeans package (Remmeans).

Results and discusion

- L291: besides slope 0-max, slope 60-300 and slope 300-600 are also NOT positively correlated to MWD1 according to the table.
  - **–> We corrected : « Except for Slope~0-max~, Slope~60-300~ and Slope~300-600~, a positive correlation was found between »**
- L324 : how could slope affect structural stability (at plot scale) ?

  - –> **The sentence was adapted: "...soil conditions of the sampling sites (namely, microsite heterogeneity related e.g. to microrelief, the presence of crop residues, roots, earthworm galleries, ...)"**

- L330-333 : this sentence is not very clear (':' is used twice in the same sentence), and seems to repeat what has been said on L288-289. Repeating the info is useful, but the sentence can be simplified (see annotated manuscript)

  - –> **We remode the last part of the sentence, like proposed by the reviewer in the pdf.**

Fig. 5 : please indicate on the graphs the results of the pairwise comparisons between treatments (using for instance small cap letters) to better highlight which treatment is different from which other treatment.

- –> **We did it**

- L340 : 'discordant' doesn't sound right in English, but can't think of the proper word. 'Antagonistic results' ?

  - –> **We adopted the proposed word**

- L349 : please express root biomass in terms of root mass density (mg/cm^3 or g/dm^3 or ...) for easier comparison with the literature

  - –> **We expressed it in the new version of the manuscript**

- L350 name of the variable is misspelled

  - –> **We corrected**

Isn't Fig. 6b the same as Fig. 7d ? I suggest removing 6b.

- –> **Yes the figures are based on the same data, but they have different objectives : 7 is results. 6b is for the sake of pedagogy, illustrating how we come from curves to boxplot of the Wend indicator. We propose to leave the right part of Fig. 6 (6b)**

In Fig. 7, please indicate on the graphs the results of the pairwise comparisons between treatments

- –> **We did it**

- L372 'advocate' means 'publicly recommend'. This is probably not what the authors had in mind, since the sentence is not a 'recommendation'. Please check

  – –> **We change to « We argue »**

- L392 : what does a 'field gradient' mean ? 'field' is not a property (temperature gradient, concentration gradient, ...)

  – –> **We changed to « concentration gradient »**

- L402 : I don't understand the statement 'with an average complexation potential of 1 g of SOM for 10 g of clay', especially given the first part of the sentence. The next sentence seems to imply that 1 g SOM for 10 g clay is a threshold value. Please explain and rephrase this sentence.

  – –> **The sentence was rephrased: "... who found that, for a variety of soils from France and Poland, about 1 g of SOM was necessary to decrease the dispersive power of 10 g of clay by organo-mineral association." Hope this reads better now. If confusion persists please read Dexter et al. (2008)**

- L417-418 : not sure why the authors advocate the use of the 0.1 SOM/clay threshold value, even though their results do not support the existence of such a threshold, and the results of Johannes et al. (2017) and Prout et al. (2020) also do not support the existence of a "threshold" (but rather a linear relationship, as stated by the authors on L405). It may still be that the 0.1 threshold value coincides with a change in structural quality class according to VESS (class boundaries being often more or less arbitrary), but there is nothing in this paper to support the existence of a threshold.

  – –> **The sentence was rephrased to present the interest of the SOC:Clay ratio in a more general way : "To sum up, we suggest that the SOC:clay ratio, a proxy for soil intrinsic 'potential' structural stability, is a valuable indicator of the organic and structural status of agricultural soils (Dexter2008, Johannes2017, Prout2020)."**

- L455 replace 'advocate' by a more suitable word

  - –> **we changed by « higher root density and SOC contents in the topsoil also positively impact the biological activity »**

- L459-462 : did the authors check the composition of the exchange complex ? Is the proportion of K significantly (and substantially) higher in K2 than K0 treatment, for instance ? As stated by the authors, it may well be that the exchange complex has had time to reequilibrate since 2016, and looking at the relative importance of Ca, Mg and K should give a clue.

  - –> **Unfortunately we didn't measure plant available or exchangeable cations and CEC.**

- L473 'crumbled' ? (rather than crambled)

Yes we changed the term, it was already « crumbled » in other paragraphs of our manuscript. Here it was a typing mistake

- L475 : sampling stony soils with a kopecki ring is very tricky . . . !

  - –> **We completed the sentence: "... for which the adequacy of the sampling procedure and running of the test needs to be verified"**

- L488 '. . . but curve modelling is another perspective of curve interpretation' : replace by '. . . but curve modeling may offer further perspective for curve interpretation'

  - –> **We changed**

- L496-497 : summing up before a conclusion seems redundant

  - –> **We removed the sentence as suggested in the pdf alsoo**

Conclusions

Some sentences are almost exact copies of sentences used in the discussion, which should be avoided.

- L511 : 'is closely related to the SOM status of soil, well-captured by the SOC:clay ratio' : sounds a bit contradictory. So it's not just the 'SOM status', but the concentration of SOM relative to the clay content.

– –> **The following sentence was rephrased to avoid repetition with the discussion and clarify the point raised about the SOM status: L510 ff. "We found that soil resistance to disaggregation correlates positively with SOM content and negatively to clay content, making the SOC:clay ratio a key indicator of the potential structural stability for the soils of central Belgium."**