Es fehlt / wird kritisiert:

-Zu wenig aktuelle Referenzen außerhalb unserer Forschungsgruppe (gibt es da noch welche?)

-zu wenig Replikation Datengrundlage.

-inwiefern wirkt sich heterogenität des Bodens auf die Unsicherheit der Methode aus

-braucht man die referenz Probe überhaupt? testen

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| --- | --- | --- | --- |
| Option | Pro | Contra |  |
| Technical Note draus machen | Wenig aufwand | Kein richtiges Paper |  |
| Neue Messungen an anderem Standort | Verbesserung der Datengrundlage durch Replikation, bessere Abschätzung für unterschiedliche Böden. | Zeitintensiv  Nicht wirklich etwas neues |  |
| Laborversuche mit künstlicher Produktion | Evtl. Beweis das Produktionsprofil abgeleitet werden kann | Nicht wirklich zielführend für das Projekt |  |
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23-Jan-2021

Dear Dr. Osterholt,

Manuscript ID jpln.202000493 entitled "A differential CO<sub>2</sub> profile probe approach for field measurements of soil gas diffusivity and soil respiration" which you submitted to Journal of Plant Nutrition and Soil Science has been reviewed. The comments of the referee(s) are included at the bottom of this letter.

Unfortunately, the referee(s) raise a number of points of concern. Therefore and based on the recommendation of the Associate Editor Dr. Gerke, your manuscript has been denied acceptance for publication. However, a revised version of your manuscript that takes into account the comments of the referee(s) may be resubmitted. In order to speed-up the review process of the re-submitted manuscript, please, in your cover letter refer to the number of the rejected paper. Furthermore, please provide us with an additional document in which you explain in detail which changes have been made to meet the reviewers' critical comments.

Please note that resubmitting your revised manuscript will be handled as a new submission and may not be reviewed by the same referee(s) as during the first submission.

You can upload your revised manuscript and submit it using this direct link:

Once again, thank you for submitting your manuscript to Journal of Plant Nutrition and Soil Science and I look forward to receiving your resubmission.

Sincerely,

Prof. Hermann Jungkunst

Editor in Chief, Journal of Plant Nutrition and Soil Science

jungkunst\_eic@uni-landau.de

Associate Editor's comments to the author:

Associate Editor: Gerke, Horst

Comments to the Author:

Dear Authors, we received two excellent reviews (one major and the other complete revision) with two different perspectives. The review#1 focussed more on critical the missing details on e.g., the soil and the material, and other aspects that can be resolved in a Major Revision. Rev#2 gives more general criticism on the scientific approach e.g., limited referencing and replications and aspects of dealing with soil heterogeneity, which cannot easily be considered during revision.

I think your manuscript covers an important topic and could become a valueable contribution.

Also I think that both reviews have expressed valid concerns: It is not really clear whether you are presenting a technical note on an improved method or a scientific paper.

For a technical note, an example showing the technique would be sufficient, but it is loo long. For a scientific paper, the objective does not really fit: "develop and test a mobile system that allows a fast and easy installation", and scientific hypotheses are missing so that I can understand the concern of rev#2 that the testing is not sufficient, explaining that sensors and installions are working and the gas flux simulations can be used.

By the way, the COMSOL modeling and parameter fitting is not explained in detail.

I tend to recommend Complete Revision in order to give you the most flexibility with respect to changes and that you are not limited in time with the revision.

I hope to receive a nicely revised version in which all comments are adequately considered.

I found the discussion too ong and not substantiated enough, mostly without references to data.

Last note: one of my basic principles is that I should not make the judgement for myself, as in your conclusion: "We presented a highly innovative Differential CO2 Profile Probe Approach..." Please let the reader decide if the approach is highly innovative but present all the arguments that enable the reader to do such evaluation.

Referee(s)' Score Sheets and Referee(s) Comments to Author:

Reviewer: 1

Recommendation: Accept paper with major revision

Comments:

This is a methodical study on a new developed measuring device for determination of the soil gas diffusion coefficient and monitoring of gas diffusion. This is a sound and very complete study, testing and calibrating the system in comparison with different approaches. Results are presented in one table and six figures. The manuscript is well organized and largely well written. It can be accepted after revision.

Major deficits are: A description of the field soil profile, its horizons and properties are missing.

The sand and grit used in the laboratory experiments should be further characterized. Was it pure quartz sand or sand from natural deposits or even from soil? What was the particle size (particle size distribution) of sand and grit? Grit was derived from what type of parent rock material? Respective information could be added to Table 1.

The used CO2 is termed as ‘inert gas’. However, it is highly soluble in water and adsorbs to soil surfaces including meso- and micropores. The authors should clarify the possible bias of gas adsorption and dissolution in water (especially with the soil moisture varying in field soil).

Detailed comments (line)

33: Replace ‘observe’ by ‘monitor’.

35-36: Change to ‘installed in soil …’.

50: Gas exchange will be important for soil biota rather than for the scientific discipline of soil biology.

64: Change to ‘understanding of the …’.

66-67: It is suggested to write ‘the diffusion coefficient of the specific soil (DS) …‘.

95: Write ‘helium’ (lower case letter).

97: Suggested to write ‘’requires using sensitive techniques such as …’. Actually, I wonder whether it is needed to mention gas chromatography. Today it is quite widespread available in numerous laboratories.

120: Check for the correctness of the automatic figure references.

123: Here and throughout the manuscript: Write out numbers <10, when they come without a unit. Here it should be ‘three pipes’.

125: It is unusual to use negative numbers for soil depths. This is also inconsistently done in this manuscript.

155: The authors mention that dissolution of CO2 in soil water is negligible in acidic soil. However, numerous agricultural soils have a pH of 6 and higher.

176: In the Materials and Methods section it should be clarified how the gas transport modelling is linked to the laboratory experiments.

In general: The different depths used in laboratory experiments, field experiments, gas transport modelling, and for monitoring of soil moisture are very confusing. There are doubts that gas diffusion in 3 cm depth can be sufficiently linked to soil moisture in 20 cm depth (line 244-245).

204: Write ‘nitrogen’ (lower case letter).

205 and 211: Delete ‘please’.

208: Write ‘Scots pine’ (upper case letter).

212: What is ‘relatively stable weather conditions‘? This requires some more detailed information, maybe added in a Supporting Information section.

221: Soil moisture was measured 20 to 50 m away from the devices for gas diffusion monitoring? I truly doubt that such data can be linked to each other, or should it read ‘cm’?

244-245: Write ‘soil water content’ and ‘soil temperature’ (lower case letter).

251: Write ‘with R² of around …’.

253: Suggested to write ‘Replications … standard deviation of around …’.

276: See comment 251.

277: Write ‘probe during the injection ..’.

291: Write ‘to 0.21, and in the bottom …’.

295: Write ‘concentrations were reached …’.

307: Write ‘porosity’ (lower case letter).

310: It sounds a bit awkward that fluxes ‘behave’.

339: Write ‘helium’ (lower case letter).

369-375: It seems rather typical that soil microbial activity is unevenly distributed within a soil area. Especially when it rains after prevailing dry conditions, resulting in hydrophobic soil properties, a very uneven distribution and moistening of soil and even more of forest floor layers (!) must be expected that is reflected by unevenly distributed hot spots of microbial activity. Consequently, previous results of the authors are not able to cover this scenario.

387: It has not been mentioned before that also litter layers were investigated. The field soil must be properly described (see general comments).

399-400: Write ‘Although we did not use …’. However, in total, this statement appears to be contradicting and it is rather confusing. Rewrite it. What do you want to tell the reader?

402-409: This section is a summary but not a conclusion. It should be rewritten and not simply repeat results.

419: I assume that the authors mean ‘Meteorology’?

421: Add the funding number of this project.

422 ff: All the references must be carefully checked and corrected, following the style of the journal.

Figure 2 a: Not the COMSOL model is visualized but the modelled data for gas diffusion.

Subject and contents are suitable for publication in 'J. Plant Nutr. Soil Sc.'?: Yes

Novelty of the results is appropriate?: Yes

Conclusions are well proven?: See Report

Title is suitable?: Yes

Number and quality of references is appropriate?: Yes

The linguistic quality of the paper is good?: Yes

Where data are reported, SI units are used correctly?: Yes

With respect to scientific content the size of the paper is: Adequate

The number of tables is: Adequate

The number of illustrations is: Adequate

The English summary is: Adequate

Reviewer: 2

Recommendation: Complete Revision

Comments:

The manuscript presented a CO2 profile probe with build-in sensors that can easily be installed into soils. The authors develop an approach to take into account the depth distribution of soil CO2 production, as this affects the soil CO2 gradients, in the determination of Ds by using a reference probe. The authors call this “new approach”, but from my point of view, one should consistently speak of an evolved method. The more recent developments of other groups in this field are not mentioned, the cited more recent literature (after 2010) is almost exclusively from the own group.

The authors mention the heterogeneity of soils, but they do not elaborate on what this means for the uncertainty of their approach. However, this would be absolutely necessary for a good scientific paper. This is also not possible because work was done without repetition. In my view, this is no longer the state of the art.

A possible way would be to test the approach in the laboratory under variable, but exactly defined flows and production rates. In this form, the publication is not acceptable from my point of view, even if the ideas represent a good approach. As a hint I would like to add that one should also find a way to consider the uncertainties adequately, the authors should compare what it brings to work with reference measurements, versus using the measurements of the probe just before injecting the gas and the decay behavior after switching off, if one uses only short intervals (one to a few days) for the additional gas flow to be able to assume the moisture conditions in the soil as constant as possible, or with constant trend.

Subject and contents are suitable for publication in 'J. Plant Nutr. Soil Sc.'?: Yes

Novelty of the results is appropriate?: See Report

Conclusions are well proven?: No

Title is suitable?: Yes

Number and quality of references is appropriate?: No

The linguistic quality of the paper is good?: Yes

Where data are reported, SI units are used correctly?: See Report

With respect to scientific content the size of the paper is: Too short

The number of tables is: Adequate

The number of illustrations is: Adequate

The English summary is: Below standard