

# ***Interactive comment on “A soil moisture monitoring network to characterize karstic recharge and evapotranspiration at five representative sites across the globe” by Romane Berthelin et al.***

**Romane Berthelin et al.**

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Referee #1

Reply to comments by an anonymous #1 on the manuscript "A soil moisture monitoring network to characterize karstic recharge and evapotranspiration at five representative sites across the globe" by Berthelin et al.

The paper introduces a major international field campaign and observational research program which is based on the networking of instruments for enhancing the high tem-

poral and spatial resolution of soil moisture observations in karstic areas in different climates around the globe. This network will present novel data that could help to develop physical and conceptual models in the karstic regions. The strategy for selecting different climates, land-uses, scientific methods, and assumptions are valid and clearly outlined. The description of experiments and calculations is sufficiently complete and precise to allow their reproduction by fellow scientists. The authors give proper credit to related work and clearly indicate their own new/original contribution. The language is fluent and precise; and symbols, abbreviations, and units are correctly defined and used. The amount and quality of supplementary material and references are appropriate. The title clearly reflects the contents of the paper. The overall presentation is well structured and clear.

Reply: We thank the reviewer for her/his positive and constructive comments that contributed to improve the manuscript. According to her/his comments, we performed the following changes.

The results are not sufficient to support the interpretations and conclusions, however, they are useful for giving positive indications about the correctness of authors' assumptions.

Reply: We specified in the discussion that the work is on progress and that the interpretations made from the reaction examples are only the first suggestions. They should be explored in more detail in future work. The focus of this paper is to present the experimental design. The data that we showed are used as examples to illustrate the future possibilities that the data will give in the exploration of the different flow processes happening during karst recharge. See page 13 line 6 of the revised manuscript.

The abstract provides a complete summary but is not concise and lacks the keywords.

Reply: We shortened the abstract. The following keywords were added below the revised abstract: soil moisture, karst characterization, groundwater recharge, evapo-transpiration. âĂČ Referee #2

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Reply to comments by an anonymous referee #2 on the manuscript "A soil moisture monitoring network to characterize karstic recharge and evapotranspiration at five representative sites across the globe" by Berthelin et al.

This paper propose an interesting network for monitoring soil humidity in contrasted karst area. This experiments have a high potential to understand subsurface flow in karst and I recommend the publication.

Reply: We thank the reviewer for his/her useful and valuable comments that helped to improve the manuscript. We applied the following suggestions as suggested in her/his comments.

With 5 places in the world, the ambition to discuss impact of climate on recharge seems unrealistic, but the device is well design to validate models with a dataset including a large range of variability/configurations.

Reply: Indeed, the quantitative comparison of the impact of climate on recharge will be questionable as too many different parameters can have an influence apart from the climate. Instead of focussing on differences among the sites caused by climate, the network allows analyzing differences that are caused by the integrated influence of different parameters. However, the repetition of the measurement settings at each site will at least allow a qualitative comparison. We clarified the respective formulations in the manuscript accordingly (see page 3 line 24 and 32 and page 13 line 25 of the revised manuscript).

Introduction: At the system scale, rainfall/discharge models are the one of the most popular method to quantify the karst recharge. This should be mentioned with examples.

Reply: We mentioned rainfall/discharge models used to quantify the karst recharge and provided the respective references (see the introduction of the revised manuscript: page 2 line 14).

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The aims of this paper is to propose a soil moisture monitoring network in karst area. This is not the first time that the soil moisture is monitored in karst area, I know some examples in China, in Houillon et al. 2016 as part of the SNO KARST in France with interesting results. This should be mentioned in your introduction.

Reply: Houillon et al. 2017 do not seem to use soil moisture in their study. However, we used this reference to discuss the use of hydrochemical methods and physicochemical parameters to characterize the flow conditions in the vadose zone. We also added the reference of a study conducted in China that investigates soil moisture in a karst area to compare the impact of vegetation on soil water storage. See page 2 line 40.

L11 Hartmann et al. 2014 are not the first to use tracer in karst system, if it is the most popular approach, older references should exist.

Reply: We apologize for this confusion, Hartmann et al. 2014 is a review paper including diverse references using tracer in karst system. We clarified this in the text. Other references of studies using tracers are cited in the following text (see page 2 line 6 of the revised manuscript).

I wonder if all the selected sites are included in areas where precipitation and ET0 are known? Otherwise Future models of soil moisture partitioning will have low constraint. To fully valorize a 15 min sampling rate, the rain gauge should be close to the monitoring device and have the same sampling rate.

Reply: We thank the reviewer for pointing this out. Indeed, climate stations are installed close to all our sites. Since the stations are operated by our collaboration partners, their temporal resolution differs due to energy supply and storage limitations. We provided information about the precipitation measurements on page 10 line 30 of the revised manuscript.

In the same way concerning valorization of this dataset, are this site located in catchment where regional karst spring is also monitored? This should open nice confronta-

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tions between entire aquifer approaches and the proposed one. This could be mentioned in the manuscript.

Reply: This valuable suggestion was already given to us by other colleagues in the past. For that reason, CTD diver probes measuring water levels are currently being installed at the sites drained by a spring. No data has yet been downloaded. We added this information on page 10 line 33 and page 13 line 18 in the revised manuscript.

Slope is one of the main driver to describe infiltration in soil, as mentioned P4L2. The value of the slope are not given. Are all the site located in the same range of slope value? Photo suggest that site are all in flat areas? In the same way, the geomorphologic location can be a main driver: upper part of a plateau or depression areas where preferential recharge take place? This could be mentioned.

Reply: Almost all sites are at locations with a similar slope. Except for the German site, which is located in a mountainous area with steeper slopes, which are typical for mountain karst regions. We added the slope values in the description table 1.

Soil description is mainly qualitative, what about bulk density, porosity, conductivity.

Reply: Soil texture analyses were conducted in the laboratory for at least one soil sample per site. We added the results to the revised version of the manuscript. Due to the difficulty to transport the soil samples from the sites back to Freiburg, bulk density, porosity, or conductivity measurements were not possible.

The observed (non)sequential reactions can be explained by properties heterogeneity with deep. At this scale, the decrease of properties with depth suggested in introduction is not obvious and should be validated site by site.

Reply: Preferential flow (non-sequential reactions) might take place at different places or not. It is a hypothesis based on literature that we will use for future analysis. As answered to reviewer #1, we specified in the discussion that the work is still in progress and that the interpretations made from the reaction examples are only the first sugges-

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tions to be explored in more detail in the future. See our response to the 2nd remark of review #1 and the respective changes from page 13 line 6.

If you choose to present monitoring network into a GI paper, without waiting for more results, this suggest that this network is design for a long time. In conclusion, you speak about 3 years, it is surprising.

Reply: The 3 years represent the temporal frame of the PhD project that will be the first to work on the collected data. We hope that the monitoring network will continue to be maintained in the future depending on the collaborators' possibilities and interests. We clarified this point in the manuscript to avoid confusion. See page 14 line 15 in the revised manuscript.

Please also note the supplement to this comment:

<https://www.geosci-instrum-method-data-syst-discuss.net/gi-2019-22/gi-2019-22-AC1-supplement.pdf>

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