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Interactive comment on "Stable isotopes reveal evaporation dynamics at the soil-plant-atmosphere interface of the critical zone" *by* Matthias Sprenger et al.

Anonymous Referee #1

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Review of "Stable isotopes reveal evaporation dynamics at the soil-plan-atmosphere of the critical zone" by Sprenger and colleagues.

The manuscript describes evaporation dynamics of soil water in podzolic soils in the Scottish Highlands that are then related to surface vegetation and aspect. The authors sampled soils monthly and analzyed the δ18O and δD stable isotope values of soil water from four different depths for one year using an isotopic equilibrium method. The authors inferred evaporative losses from the soil water profile as well as explored relationships between isotope fractionation and deuterium excess (normalized to the local meteoric water line) with soil organic matter characteristics and corresponding vegetation (forest and heather). They hypothesized that their isotopic patterns were

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related to precipitation inputs and mixing processes in the soil. They found the unique characteristics of the research site to dominate the isotopic patterns they detected, especially at the near surface. In particular, the high organic matter content of the soil served as an important storage pool that potentially dampened an evaporative signal in the water isotopic patterns. During summer, when evaporative potential is high, the evaporative signal was strongest in the upper 15 cm.

The experimental design is tailored for testing vegetation influence on site hydrology, the methods are appropriate and the analyses were exhaustive. However, a justification or more information is needed regarding the use of hydrocalculator. The issue at hand is that at the core of hydrocalculator is the Craig-Gordon model, which is designed for open water surfaces ÂňÂň– which the soil clearly is not. There are other models that consider diffusion in the unsaturated zone (i.e., Barnes and Allison, Journal of Hydrology, 1988) and they are able to model the profile while estimating an evaporative flux. In the study under consideration, estimating evaporation is not the focus per se, so removing these potentially erroneous estimates wouldn't be a problem. The figures are in general useful, however, I am afraid figure 8 is too busy to extract the point being addressed without investing a significant amount of time.

Despite the issue of the evaporation estimates, it is clear that evaporation occurs within these soils and the role of vegetation is highly relevant. And while this is interesting, it is not entirely unexpected. Emphasis is also placed on the high frequency of the sampling (11 campaigns), but with advent of portable CRDS lasers even monthly samples are considered a rather low frequency sampling strategy (for example, see Volkmann et al., New Phytologist, 2016). Modelling is mentioned in the discussion (L198-213; pg-22-23) although no modelling is performed and the results from the study are not really brought into the modelling discussion directly. There is a long history of investigating and modelling the soil water isotopic profile, and again while the results are interesting for this particular site, the title seems to promise more than the study can deliver. The merit in the study lies in the site-specific nuances such as the role of organic matter in

effecting soil water capacity and the subsequent isotopic mixing that occurs.

Specific comments: Title: It is not exactly clear what is meant by “the soil-plant-atmosphere interface of the critical zone”. In this study soil water is measured, why not just state this?

Abstract: Page1, L13: Because this paper is not a test of the method, it is necessary to report it here.

Page1, L22-25: I would argue that this sentence can be deleted.

Introduction:

Page1, L30: remove “well” from well understood so that it reads simply “insufficiently understood”

P2 L2: I think it is the age distribution of the water that is used in evapotranspiration that is meant here, and not the age of the flux.

P2 L10: perhaps introduce the term isotopic fractionation here, not all readers will understand the relevance of this process.

P2 L32: This sentence needs to be restructured.

Page 3: Research question 1: Instead of “critical zone”, I suggest soil profile.

Research question 2: This has already been done before. Better to restate your question/goal as to estimate evaporation of the site based on the water stable isotope values within the soil profile. This section might need to be removed if the hydrocalculator approach cannot be justified.

Research question 3: This is very vague. What is meant by feedbacks? And, how do you expect them to vary spatially and temporally?

P3 L20: catchment “is” covered

Page 5, lines 21-25: Please briefly explain how the ‘equilibrium method’ works.

P5 L26: Please explain how the standard water was “sampled”?

P7 L3: Please discuss the “effect of antecedent conditions”. What might we expect that occurs during the time window you suggest?

Page 7, Lines 4-6: Could you briefly explain why exactly you used these number of days (7 and 30)? Furthermore, how were the isotopic signals weighted? This whole approach is rather unclear.

Page 7, Line 13: Did you mix the isotopic results from 5 and 10 cm for achieving f?

Page 7, Lines 15-17: I have some concerns about this approach. What is the purpose of applying this correction to δS? This is not fully clear. Secondly, did you correct the δP for the net precipitation (mm), which was different between from month to month? This would affect the average δ18O and δ2H of precipitation.

P8 L16-17: What is the reason for testing for significance along such a small range? Isn’t 0 and 1‰ already in the measurement precision limits? What value is relevant to determine significant evaporation?

P8 L21: Pearson not Parson

Figure 3. I really like this figure, especially with the temporal patterns along the secondary plots.

Figure 5 b. The pattern in lc-excess over the season is interesting but what qualifies this as hysteresis and not just seasonal changes in PET? This plot needs error bars along both axes.

Page 14, Lines 22-23: What is ‘organic content’? Maybe “organic matter content” is more appropriate.

Figure 7. Is there a reason why LOI should change in such a short time period? The x label of this figure is really confusing.

P16 L74: I think this is in reference to 0‰ lc-excess

Page 17 L85: Should it be “depths” instead of “sites”?

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P21 L159: Be careful, technically even precipitation has undergone fractionation!

P21 L170: I assume the dynamics at 5cm are being described here. Is the replacement of autumn water the only possibility here? Is it not also possible that mixing (which the authors advocate elsewhere) is responsible? Are they mutually exclusive?

P21 L171: “further special” is a bit awkward

P22 L172-174: Time reference is lost here. Is the rest of the year for any time that does not occur in January? It looks like this pattern was also emerging in November of 2015 (probably also established in December).

P22 L190-197: I don’t think this part of the discussion warrants a whole paragraph.

Page 22, Lines 192-195: This statement is not fully clear. What is the importance of the accuracy? Please remember that you are only talking about the top 5 cm. So root water uptake will depend on the plant species and rooting depth.

P23 L217-218: “kinetic fractionation dynamics” is a little cumbersome; it may be easier to the reader if you simply refer to evaporation when referencing the isotopic effect.

P24 L246: citation

P24 L255: Do you mean isotopically depleted infiltration water? I don’t immediately see how this study shows that the “the legacy of evaporation losses” allows for separating pools of different water mobility in the SPA interface. Can you make this more apparent?

P25 L279: Where is the number 3 coming from? I think a few citations or reviews might help back this up. I think it is also important to keep the context of the research

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question in mind when assessing another study's design.

P25 L288: Isn't the storage capacity referred to earlier relevant here as well?

P25 L302-303: citation

P26 L345: I don't think the word "exceptionally" is warranted here, although the study is data rich. This sentence is also structured in a strange manner (i.e., "but also" when there isn't a contrast to begin with). I don't think the final statement is justified. Are modelers calling for higher spatial and temporal isotope data? Is this listed as a research priority in the literature? Do the data presented here help realize a realistic representation of "soil-vegetation" interactions? Statements like these are beyond the scope of this study.

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