Response letter concerning the manuscript hess-2020-274.

hess-2020-274-referee-report-1
Dear Markus Merk and co-authors,

the manuscript has partially improved, but as already mentioned in the first revision of the draft, there is nearly no discussion, broader interpretation and comparison to other studies of the results in the section 4. In addition, drainage data that is now shown are poorly evaluate in the context of the main message of the manuscript. I think thes data can nicely show/help to quantify the impact of SWC depletion on recharge from this study. Many other study can not do this, because the don't have the data from lysimeter. So I want to encourage the authors to add this analysis in a sub-section of Results and Discussion section, which would be an important information for the water sector, especially in the context of climate change.
I recommend again that the manuscript section 4 needs a broader discussion, interpretation and comparison of the results before publication in HESS.
We would like to express out thanks to the reviewer for valuable suggestions! We would be happy to share the data for further use in additional studies after publication. However, further analysis of the drainage data and discharge behavior is beyond the scope of this study.

General

1) It was difficult to follow up the response in text on the review, as authors did not adapt lines in the response from the revised version of the manuscript.
We would like to thank the reviewer for his time and effort to diligently consider our responses.

2) I recommend adding a section where the authors link the soil moisture depletion with the drainage data. Many other studies lack on the this information and the authors should use the available data from the lysimeter to link impacts of changing soil moisture on drainage/ recharge which is of high importance for the water sector
We agree that this information is valuable and even more could be learned from our data. However, further analysis of the drainage data and discharge behavior is beyond the scope of this study.

Specific comments:

L99: Missing units in the following sentences: "For the year 2002, the porosity of the RL is 0.4, usable field capacity 0.25 and the wilting point at 0.14. The 100 permeability was estimated as kf = 1:0 10 6. Values were observed to be variable over time."
added the appropriate units: porosity [-], usable field capacity [-], wilting point [-], permeability [ms^{-1}]. (L.99-100)

L100: Please explain the statement that values were observed to be variable over time.
The determined parameters were used to model discharge from the lysimeter using weather data. In subsequent years, these parameters were changed (taking the range of determined uFC, wilting point, etc. into account) to calibrate the model. We changed the statement to read "Formation of preferential flow paths in the lysimeter lead to changes in hydraulic properties over time (Gerlach, 2007)." (L.100-101)

L128: Again, please provide units for the given wilting point and field capacity
Added appropriate units: [-]. (L.129-130)

First response Review round 1 L102-107: Totally unclear why the authors want to use the model uFC? This should be explained in the section.

As noted by the reviewer in a previous comment, any soil moisture time series could be used. We used external data by the DWD to compare with measured soil moisture and validate our findings. Added this explanation to the manuscript.

Response: Validating measurements with external simulation data?

The changepoint in the year 2003 has been observed before, but it was thought to be the result of an initial draining of the lysimeter after construction. The discovery of similar behavior in modeled soil moisture (e.g. uFC) led us to investigate this further. Some groundwater level time series also indicate a change that happened in 2003 with a subsequent decline groundwater levels.

L171: Change Zhao et al. 2019b to Zhao et al. 2019a, and change vice versa in line 176 Zhao et al. 2019a to Zhao et al. 2019b

Quotations are in accordance with the manuscript preparation guidelines.l

L240: Figure caption of Fig. 6. “real evapotranspiration”. Please use in this context actual evapotranspiration, because there is “no unreal” evapotranspiration. It is also not clear to me where this data is coming from and if it is an outcome of the simulation from the AMBAV model (than please refer to simulated actual evapotranspiration) or if it is indeed measured actual evapotranspiration EC-tower, lysimeter? Please clarify this please

The data are from the cited source. The term “real evapotranspiration” is used in the official documentation by the DWD:
ftp://opendata.dwd.de/climate_environment/CDC/derived_germany/soil/daily/historical/
We changed the figure caption to read “Simulated monthly averages of usable field capacity (loamy sand), simulated potential evapotranspiration (red) and simulated actual evapotranspiration (black) (DWD Climate Data Center, 2020)...” (Fig. 6 caption)

L199: reformulate this sentence, as low precipitation does not lead to a drying out. It is the large atmospheric demand for ET paired with low P [….].

Sentence reformulated “...below average, and paired with a large atmospheric demand for ET, once again drying out the lower soil...” (L.201)

L201: not clear from which figure I can actual see this?

Added reference to Fig. A1. (L.204)

L180-227: I could not find any discussion of their results in this section, also newly added discharge in the Figure is not presented in the text nor its effect from depleting soil water storage/ decline in soil moisture profile on DL is given. In addition, the potential and actual evapotranspiration is not present in the text and discussed.

Further analysis is beyond the scope of this study.

L242: thanks to add the other SWC time series in the corresponding subplot (gray lines), but the authors should mention this in the legend of the figure and also in the text figure caption.

We added a legend to this figure and amended the figure caption accordingly. (Fig.7 and Fig.7 caption)

First response Review round 1: L271: I could not find any data in the manuscript that actual shows that hysteresis plays are role at this site. So please clarify the following sentence: “There are clearly hysteresis effects during drying and re-wetting of the soil”.

Section 4.1 is dedicated to the asymmetry of drying and re-wetting. This asymmetry could also be called a hysteresis, as drying and re-wetting do not follow the same temporal paths. Additionally, we

added a citation to Augenstein et al. (2015). They investigated and found evidence for the hysteresis between soil moisture and discharge.

Response: there is no hint on hysteresis in this section. Observed changes in SWC might be related to hysteresis, change in soil surface cover (large ET, which reduce SWC), or actually to a change of soil properties itself. Dear authors there is no discussion of the presented results.

Changed sentence to make the citation even more clear: “Augenstein et al. (2015) found, that there are hysteresis effects during drying and re-wetting of the soil at this site.”
A description of this hysteresis is given in section 4.5. of their manuscript and the hysteresis between discharge and soil moisture shown in their Fig. 7. (L.331-332)

L274: the authors should have a closer look at this data, and should show recharge data from both lysimeters in a better way to connect depleted soil moisture to changes in the recharge behavior of the soils over time.
This has been done in depth by Augenstein et al. (2015) and is beyond the scope of this study.

L276: this statement is not clear to me? Did the authors mean characteristic length of evaporation i.e. this means the depth until which a wet soil dries out during stage 1 evaporation? But it’s an vegetated soil so E and T occurs (see next commentary). I suggest rather that it is an impact of many different factors, e.g. high ET that reduce soil moisture and also seasonal low P, but also to other factors like changes in the vegetation cover. As this is a main outcome the results should be discussed in a much broader context and should be compared with other studies.
It is the depth at which a clear seasonal pattern is visible. We changed it to evapotranspiration as per the next comment. (L.272)

L276: Also here it is rather related to ET and not only to E.
Changed to evapotranspiration. (L.272)

L336-354: this is actually the first and only part of the Results and Discussion section, where the authors discuss their results in the light of previous studies. This need to be done in a proper way in each of the previous section, if not the autos should change in L180 Results and Discussion to Results and add after this a separate Discussion section.
Separated Results and Discussion as suggested. (L.181, L.328)

L367-368: so this indicates that changes in the vegetation cover might be the large driver of the observed depletion of SWC
As suggested we added this to the manuscript: “This indicates that changes in the vegetation cover might be the large driver of the observed depletion of soil water.” (L.369)

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The authors have revised the paper according to the comments of the first review. Below are the remarks that I have on this revised version. In general, I agree that the long time series of soil moisture measurements is valuable and worth publishing. However, my concern remains that a) the interpretation and in-depth analysis of the data is restricted by the fact that information on soil properties is lacking and b) the study represents a very specific case and the interpretation of results is limited to these specific conditions at the landfill. I suggest to underline this in the manuscript.

a) We agree. But further sampling to increase knowledge of soil properties is not possible.

b) We added "The study represents a very specific case and the interpretation of results is limited to these specific conditions at the landfill." at the beginning of Results and Discussion section. (L.182)

Fig. 6: mark individual figures from a) to g). Is it monthly precipitation? Pleas add this info in the figure caption. Same with the blue line (mean monthly precipitation?) Explain the discussion of Fig. 6 why uFC is partially >100%?

We added a) to g) to individual subplots. Added information of mean monthly precipitation to the figure caption. Added a legend to subplot g). (Fig.6 and Fig.6 caption)

L185. "...because it is thought to reflect best the processes and moisture dynamics found in natural soils". From the site description you give, it seems that it's not close to a 'natural soil'

It is of course not a natural soil but an artificially built system. However, that does not necessarily mean that it behaves in a way that is different to natural soils. These can have very diverse properties. What is originally meant by this statement is, that the soil properties of the drainage layer and MCL are more different from natural soils compared to the RL and natural soils. So, out of all layers in the lysimeter, the RL is a best representation of natural soil. Sentence was changed to: "*...because it is thought to be the layer in the lysimeter that reflects best the processes and moisture dynamics found in natural soils.*" (L.188)

L. 189ff: what is meant by discharge here? Discharge measured at the bottom of the soil profile, or movement of water through the soil profile?

We added: " *and is measured as discharge from the DL.*". (L.192)

L. 192ff: NP5 instead of NP3?

We changed NP3 to NP5 because NP5 data is shown in Fig. 6. (L.195)

L.201ff: which Figure are you referring to? NP9, NP12 are not shown in Fig. 6; same with MCL of Field 1

This refers to supplemental Fig. A1. We added the appropriate reference. (L.204)

L. 209f: "Porosity and hydraulic conductivity is therefore not uniformly distributed over the complete depth of the lysimeter". I think that holds true for most soil profiles. The consistent and very distinct break of soil moisture over the entire measurement period rather suggests that there is a distinct change in porosity and hydraulic conductivity between the two layers (i.e. lower porosity at the top of the lower soil layer

We added: "*The consistent and very distinct break of soil moisture over the entire measurement period suggests that there is a distinct change in porosity and hydraulic conductivity between these two layers.*" to the manuscript. (L.213-214)

L. 212f: "Settling down of the soil cover in the years after construction may additionally change soil properties over time." The soil moisture break remains consistent over the measurement period. Soil properties may have changes over time, but this is not reflected in the data you present. What about the consistently lower soil moisture values between approx. 100 – 150 cm, and the higher soil

moisture at the bottom of the RL? Please discuss this also in the text.
See previous comment.

Fig. 7: explain the different lines in upper and lower graph (gree-blue line and grey line). What is the data unit at the polar coordinate graph? [%] (as from Eq. 1 / 2)? What do the negative values stand for?
We added a legend to upper and lower graphs. The gray line was added as visual reference by reviewer request. The data unit (%) was added to the polar graphs and the sign of negative values removed. The bottom “axis” in the polar graphs is more like a scale-bar than an actual axis. Although it is uncommon to move this axis in a way that it no longer intersects the origin point, we think in this case it is less obstructive to the shown data. (Fig.7)

L. 221-225: This new section is more appropriate in the Discussion section. Last sentence is not clear, please rewrite
Paragraph moved to the Discussion section. And last sentence changed for clarity. (L.217-223, L.341-348)

L. 233: Is there a time lag in the measurements or a lag in the propagation of soil moisture in the profile? Please restructure/clarify this sentence.
It is indeed the latter. We changed the sentence accordingly. (L.229-230)

L. 251: coefficient~~s~~
Deleted the “s”. (L.247)

L. 260: add “Resulting slopes with $p > 0.05$ (i.e. soil moisture change is not significant) are indicated...”
Added according to suggestion. (L255-256)

Fig. 9: add info that upper graph is for Field 2 and lower for Field 1 in caption
Added “Upper graphs: Field 2, lower graphs: Field 1. “ to the figure caption. (Fig.9 caption)

L. 281: month**s**
changed to months. (L.276)

L. 312: Sentence can be skipped because it was mentioned before: “Measurements at Field 2 (NP 3, NP5, NP6, NP7) have started later compared to Field 1.”
Sentence skipped and changed to: Measurements at Field 2 (NP 3, NP5, NP6, NP7) ~~have started later compared to Field 1. They~~ also show higher initial soil moisture contents. (L.304)

L. 377: when ~~a~~ applied
a deleted. (L.378)