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Hydrology and Earth System Sciences
Discussions

Interactive comment on "The yearly amount and characteristics of deep-buried phreatic evaporation in hyper-arid areas" *by* H. Li et al.

Anonymous Referee #1

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General comments

This paper describes and discusses the results of a long-term experiment carried out in a hyper-arid region of the Gobi Desert, with the aim of quantifying soil evaporation and identifying the main drivers of the process. Soil evaporation was assessed through a greenhouse equipped with an air conditioner, while soil moisture, temperature and air humidity were recorded both inside and outside the greenhouse. The main results are quite obvious and show that evaporation is, as expected, very small (4.52 mm) and that the main drivers are temperature and solar radiation. The only original contribution of the study is the claim by the Authors that the water table, located on average at 200 m depth, is able to provide moisture to sustain evaporation.

In my opinion, the paper is not suitable for publication in HESS, as i) the main results

are not new, ii) the fact that the deep aquifer contributes to soil evaporation is not demonstrated by the experimental observations, and iii) the methodology appears to be flawed.

Specific comments

Page 13125, lines 4-11: I wonder what is the connection between deep groundwater availability and this study. Evaporation in hyper-arid zones is probably negligible, but this does not mean that deep aquifers cannot be exploited. In summary, I cannot understand what is the relevance of this research.

Page 13125, lines 20-26: these statements are reported as results, still they are in the introduction and refer to papers already published. Rewrite the last part of the introduction to better explain what has been done in this study compared to the previous ones.

Pages13126-13127: soil water content must be either 1-1.5% or ranging from 2 to 9%. Which is correct?

Sections 2.2.2 and 2.2.3: I wonder if the sensors used in this study are suitable for measurements of temperature and humidity of the air phase in soils. I am not familiar with the humidity, but soil temperature typically requires the use of thermocouples or thermistors, while, as far as I understand, the device used in the study is suitable only for outdoor applications.

Section 3.2: it is not possible to assess daily fluctuations of soil moisture from Figure 3. I strongly recommend that the complete and continuous time series of soil moisture at different depths be reported.

Section 3.3: Comparison between the temperature and humidity data inside and outside the greenhouse shows that temperature, but most importantly humidity, is constantly lower inside than outside. This, combined with the fact that when it rains there is no infiltration under the greenhouse, make me suspect that the air conditioner in the

greenhouse is extracting soil moisture laterally, as Figure 4 would suggest. Water vapor could be even recalled from the outside atmosphere through the shallowest layers of soil (but deeper than 30 cm), being the latter basically dry and thus prone to conduct gases. Therefore, the evaporation is likely to be overestimated and possibly related to the annual amount of rainfall in the area. As a matter of fact, I would like to see whether or not there is a significant correlation between annual rainfall and annual evaporation. Why is yearly rainfall from 2010 to 2015 not shown in the paper?

Page 13132 line 15 to page 13133 line 6: this paragraph is rather obscure. First of all, it is impossible to distinguish what has been done in the present paper from the work by Li et al. (2010a, 2013, 2014b). Second, the series of six soil moisture snapshots over 6 years is quite limited and show a distinctive profile, typical of an infiltration front. I wonder whether this makes any sense, given the very small and sparse rainfall events that characterize the area.

Section 3.4: this entire discussion is difficult to follow and it is not very clear to what extent the data shown in Figure 5 support these statements and conclusions. In order to confirm or confute the hypotheses, I suggest the use of a numerical model that can be calibrated and validated against the data. If not a simple 2D Richards equation coupled to heat transport (e.g., Hydrus 2D), a two-phase model of air and water flow would help to better understand the main driving mechanisms of water movement in this arid region. Other statements, such as the ones related to movement of film water, seem just speculations, as they are currently not supported by neither cited literature nor observed data.

Section 4: the Discussion section should be rewritten according to the previous suggestions. At present, daily fluctuations of temperature and soil moisture are not shown; therefore it is difficult to follow discussions such as the one reported in Section 4.1. Similarly for Section 4.2, I am not able to see any connection between the data reported in Figure 3 and soil moisture hysteresis.

Page 13139, lines 9-11: I agree that deep PW can be an important resource (not new though) in hyper-arid areas, but this is true regardless of the results and conclusions of this study. In fact, the main message seems to be that soil evaporation in such areas is extremely small; not only that, but given that it is probably overestimated, I expect it is practically negligible.

Technical corrections

Although the paper is mostly well-written, English language sometimes sounds awkward and does not help the comprehension of the discussed topics. Perhaps help from a native English speaker is required.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 13123, 2015.