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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 #2

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General comments: The article "The yearly amount and characteristics of deep-buried phreatic evaporation in hyper-arid areas" by Li et al. submitted for review addresses an important aspect of bare-soil evaporation in which very little work has been done. In hyper-arid regions where annual rainfall is low and the water table is at depths exceeding 150 m, phreatic water and fossilized water held at shallower depths can offer the liquid water source for much of the total evaporation. In the aforementioned article, the authors attempt to characterize and quantify phreatic evaporation using a novel experimental set-up located at the Dunhuang Mogao Grottoes. Despite the interesting approach taken by authors, and the importance of the research to water stressed regions, I would recommend that article be rejected or returned to the authors for significant revisions. The authors consistently demonstrate that they do not have a strong grasp or

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understanding of the physics of evaporation and the literature available on the subject through many false comments and interpretation of the data (well established explanations of the observed phenomena can be found on bare-soil evaporation). Many times they make conclusions on their datasets that are not clear by the data that they actually present in their figures and tables. I feel that the authors do not fully validate and demonstrate that their novel experimental technique provides good estimates of evaporation and control of all the boundary conditions in their test domain. In the introduction, the authors even say that the original experimental setup had a large number of problems which is unclear by the end of the manuscript whether these problems have been solved or at least addressed. This is all further compounded by the manuscript does not have a clear purpose; the authors change subject and focus of their discussion throughout in a disjointed manner, making it very difficult to follow and read. Please find specific scientific issues, comments, and concerns, below.

Specific Comments: Lines 35-36: Water resources are the most important thing for what? You mention its key for ecological recovery, but it is important for so many other different things as well. Line 37: What do you mean by changing the arid climate? Are you talking about importing water from other regions for irrigation, or directly trying to manipulate the atmospheric conditions through things like seeding techniques? Lines 37-49: Given a large part of the manuscript is dedicated to phreatic evaporation, more information should be provided on past studies investing evaporation from phreatic water sources, or more importantly past studies from in arid and semi-arid regions. Work by researchers like Shmuel Assouline, Uri Nachshon, and Noam Weisbrod in Israel could be good starting places to help define the focus of the paper and provide a much better foundation for the work that has been done to date on the subject. Lines 48-49: It appears that this may be the focus of the paper, but it is never made clear. If anything it becomes murkier by the end of the paper. Line 49: You can not form evaporation. Evaporation is a process, specifically it is defined as the flux of water vapor. Line 58-65: Red flags are immediately raised with respect to the entire paper when the major issues of the experimental setup are raised in the introduction

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of the paper. You raise all of these concerns with the experimental setup (it's not clear if this is regarding the original apparatus or the current one). The authors should have spent more time on demonstrating that their experimental set-up actually works and provides accurate estimates. More details regarding concerns of the experimental design will be discussed later. Line 62: The authors use the abbreviation PE to represent phreatic evaporation. Classically, PE is defined as potential evaporation which in many contexts throughout the text, the authors seem to be referring to. Line 77: Are the climate conditions when the original site characterization was made representative of the conditions today? Line 84: Much of the paper depends on the assumption that all precipitation evaporates before the water has a chance to infiltrate. Sands and gravels which have high permeability can potentially allow water to infiltrate to great depths, providing sources of shallow liquid water. Figure 1: Why is there no mention of the Daquan River that appears to be less than 1 km from the site? Depending on the characteristics of the river system, it can have a major impact on year-round or seasonal evaporation by providing an important/primary source of water. Lines 93-96: In these types of studies, it is always nice to provide the aridity index of the site when information such as precipitation and potential evaporation rates are known. Lines 97-100: This information may be easier to present in graphical form. Lines 100-103: I feel that the authors do not fully justify that there were no significant sources of water present in the soil when the greenhouse was constructed. More information needs to be provided regarding how the "calculations" were performed and the experiment that they refer to (Li et al., 201a). Line 109: How did the authors handle the heat generated by the air conditioner? Was it discharged outside of the greenhouse given it looks like the model used is portable. Did the authors assume that the temperature within the greenhouse was uniform throughout? Air conditioners typically do not work effectively during the winter when temperature gradients are difficult to establish. How did the authors take this into account? This is an important factor given the fact that it means that evaporation rates measured during winter may not accurate. Line 109: Was the temperature adjusted throughout the day to account for natural diurnal fluctuations of temperature

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and humidity? Line 109: Did the authors measure the wind speed of the air that was blown from the air conditioner. Many studies have shown that wind speed is an important forcing variable for evaporation. Not reproducing airflow like that outside of the wind tunnel can lead to significant underestimations of evaporation. Lines 125-128: How did the authors actually do the destructive sampling and were they concerned with it changing the overall soil properties? Was a single trench dug and sampled from throughout the duration of the experiment, or a was a single hole or series of holes dug and backfilled? A single open trench would create conditions that are not representative of the entire soil sample. Using a single hole that was repeatedly backfilled would have very different soil characteristics than the surrounding soil. Digging several holes throughout the experiment would create local heterogeneities and hence preferential flow paths which is important to take into account for the relatively small footprint of the greenhouse and the duration of the experiments (several years). Line 126: Do the authors mean that the sample size, taken at the various depths, were 1 cm thick. Most studies usually report destructive sampling in volumes, not thicknesses. Lines 128-129. The authors appear to often confusedly switch between soil moisture and humidity. These are two distinct phases of water. Humidity is the gaseous vapor form of water whereas soil moisture represents the liquid phase. The authors need to choose one in their analysis. The authors also do not seem to realize that correlating soil moisture to pore space humidity is extremely difficult given its reliance on so many other variables. Lines 129-131: Soil moisture may not decline at these depths for other reasons such as the condensation of water vapor that had migrated from greater depths. It may not be directly indicative of phreatic water only. Line 132: The authors again show their confusion with distinguishing soil moisture and humidity. Why do the authors not sample soil moisture outside of the greenhouse given their concerns of precipitation events? Line 139: Why were the humidity sensors not installed in the soil of the greenhouse? Line 146: Is the evaporation assumed to be uniform? How did the air conditioner affect conditions locally? Were the evaporation estimates affected by a lack of airflow. Why was the novel method of using an air conditioner to measure

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evaporation never compared with more traditional approaches? Such as comparison would allow a researcher reading this manuscript to put more faith in the experimental setup. Lines 155-156: How can the authors say that the daily evaporation was sinusoidal (which it should be) if atmospheric conditions (with the exception of radiation) were held constant within the greenhouse and evaporation rates were only measured once a day? This is a problem that consistently comes up with this manuscript. The authors often claim things or interpret results based on unavailable data or data constrained by their sampling frequency. Lines 159-160: Evaporation occurs year-round but to varying degrees. How can the authors justify that no evaporation occurs between November and March. Again, this may be a demonstration of the limitation of the experimental setup. Figure 3: Why is the data shown at different times throughout any given year. The same approximate day within a given month should be shown rather than what appears to be randomly selected times? Why do the authors choose the times that they do? Figure 3: Why is there no discussion of the difference in the soil moisture profiles inside and outside of the greenhouse? How do the authors get soil moisture profiles outside of the greenhouse when they never measure this variable according to their methods section? Lines 181-182: I still am not sure I agree with their attribution of evaporation solely to phreatic water sources. Lines 184-189: How do the authors make conclusions regarding diurnal fluctuations in soil moisture content when it was measured only once per day? Lines 202-208: It is not clear what the authors are saying. Lines 201-204: The authors claim that no soil moisture or vapor can migrate from the soil outside the greenhouse to the soil below the greenhouse. They contradict themselves here with this statement. Figure 4: See comment regarding Lines 201-204. There must be an influx of soil moisture and water vapor from the surrounding soil as they show it. Figure 4: How can the authors say anything about humidity within the soil below the greenhouse if they did not have any sensors installed there? Line 233: Rainfall does not reduce the hydrophobicity of soil. The authors should review imbibition and infiltration research to understand how water will initially pond before infiltrating. Line 233: How does elevated soil moisture increase phreatic evaporation? The ele-

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vated soil moisture would simply provide a local source of water vapor, meaning its contribution to total evaporation (not phreatic evaporation) will change. Lines 233-235: It is not clear how this shows all the water evaporates. Lines 236-238: The authors are now discussing data that occurs in years that have yet to occur (2016-2017). Line 237: I don't understand what the authors are calculating, and where the data is coming from. Lines 241-242: The author's still have not fully proven that what they are discussing is phreatic evaporation. They need a stronger argument showing that they can differentiate between the different sources of water. Line 243: The authors never mention that they are interested in the physics of evaporation in the heterothermozone before this. It should have been first introduced in the introduction and identified as one of the purposes of the paper. Line 243: What the authors call the heterothermozone is actually normally referred to as the heterothermic zone of the annual skin depth. It is important to be consistent with the rest of the literature. The authors should actually define what this zone actually is. Line 252: Liquid water does not decompose, releasing water vapor. Liquid water vaporizes, releasing water molecules in the form of water vapor. Lines 250-300: The authors clearly demonstrate that they do not understand how phase change works and the role that it plays in evaporation/condensation in the heterogthermic zone. Line 253: When temperature drops, soils do not absorb moisture (again a mix up of soil moisture and humidity). A drop in temperature actually lowers the equilibrium pressure of the air, meaning less water vapor can be held. As a result, water vapor must condense, forming soil moisture. Just think about a plot of the temperature dependency of dew point. The soil grains have an affinity for absorbing the soil moisture via electrical bonds. Lines 243-260: I have never heard of the vapor migration rule – are the authors referring to Fick's law of diffusion? Even if this is a different rule, the authors need to distinguish between soil moisture and humidity. If anything, it is the water vapor that migrates. It is fallacious to say that vapor migrates from high humidity and high temperature to low temperature and low humidity. Vapor migration is primarily controlled by the concentration gradient and the phase change is predominantly affected by the temperature. How else could one explain the fact

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that evaporation will occur under stable and unstable atmospheric forcing conditions? Lines 261-300: The authors provide a very rudimentary explanation of vapor migration and phase change without providing any information on the physics occurring. If they did, they would realize that their interpretation is incorrect and likely identify the many different problems with their experimental design. Lines 261-264: It is highly unlikely that there is 0 evaporation. It may be outside the measurement capability or may in fact be impeded by their experimental setup. Lines 265-272: The authors should be referring to early work on temperature decay and signal delay with depth. It can be explained in terms of the thermal properties of the soil and various heat and mass transport processes occurring. Lines 276-279: This makes no sense. Lines 301-304: I still don't understand why the authors think there is a downward flux of vapor in the soil. The macroscopic gradient says that the vapor concentration in the atmosphere is always less than that in the soil, making their entire interpretation wrong. Line 312: The authors demonstrate that they don't understand film flow and soil moisture held in strong bonds with soil grains. Gravity absolutely affects water films. The only place that gravity is not an important force would be in space. Lines 314-320. The authors spent the last several pages describing the evaporation. Now they turn to film flow without any good reason. Why should we be interested in salt and mineral precipitation when the paper is focused evaporation. Lines 314-320: The discussion of water films providing a source of water again is outside of what I believe is the actual focus of the paper. Lines 326-334: The authors should seriously consider reading more of the established papers on diurnal cycling on evaporation. It is accepted that vapor continuously migrates from high concentration to low concentration. It changes phase at isolated temporary shallow evaporation fronts as a result of changes in temperature. Lines 344-345. Annual and diurnal cycling changes several important variables – wind, temperature, humidity, and solar radiation. Lines 345-346: The authors again seem to forget that evaporation is a flux. Lines 355-383: Again, this entire discussion seems to be outside of the focus of the paper. Much of what is said here, also goes against or at least misinterprets established literature. Lines 379-380: It is unclear what the

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authors are saying. Their interpretation of evaporation behavior and physics soils with high to intermediate saturations again shows that they are not familiar with the bulk of the work on bare-soil evaporation. Line 385: The authors are absolutely incorrect. By definition, any water held in pores above a shallow water table that is still connected with the water table is water held in capillary action. Statements like this cause one to question the authors understanding of much of unsaturated flow and unsaturated soil mechanics.

Technical Corrections: I provide only a few technical corrections based on the introduction. Given the need for extensive careful revision and time constraints of the reviewer, an in depth series of corrections are not provided despite their need to improve the readability of the total text which in many places is disjointed and confusing. Lines 27-42: Several sentences should be combined because many are disjointed and fragmentary. Lines 44-45: Disjointed transition between sentences Lines 49-69: There is no clear purpose in the paper, the paper ends abruptly.

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