

## ***Interactive comment on “Field-based groundwater recharge and leakage estimations in a semi-arid Eastern Mediterranean karst catchment, Wadi Natuf, West Bank” by Clemens Messerschmid et al.***

**Anonymous Referee #2**

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This paper deals with groundwater recharge estimations for a semi-arid environment. The author distinguishes between deep percolation into the major aquifer system and quick spring outflow. Especially for water scarce environments, those estimations are very important and the development of new ideas and methods would be very welcome. Unfortunately, the presented study just applies quite simple (and from my point of view, not really adequate) methods, which are lagging behind modern and creative techniques. Moreover, the paper is not very well structured and scientific terms are not properly used.

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Specific concerns:

Line 18-21: There are tons of studies applying spatially distributed hydrological model for groundwater recharge estimation. It is very common that they base on “a) soil type and soil condition, b) land forms such as relief, vegetation and land use, and c) lithology and hydrogeological characteristics of the subsurface rock formation”.

Line 19: Evapotranspiration records, really?

Line 30: What do you mean by “solid”?

Line 31-32: Model fitting also bases on quantitative field observations.

Line 37: Eight soil moisture measurement stations for six litho-facies groups (with different topology and soil characteristics) are not really much. I would not consider this as “high spatial resolution”.

Line 79: Indirect recharge originates not only from transmission loss in wadis. Also, the infiltration in cracks and shafts of outcropping karst is indirect recharge. As soon as karst is outcropping, this cannot be neglected. In principle, your idea is ok, but I would not phrase it like this.

Line 92: What about the studies you are referring to in the section line 133ff? Some of them apply spatially distributed models for the WAB.

Line 98: I disagree with the state about the controlling factors/processes. The listed factors are the relevant ones for a saturation excess situation. In semi-arid areas an infiltration excess situation seems to me more plausible (or better both in combination). This means, additionally, you should consider the infiltration capacity. (Consequently, models need to have a high temporal resolution.)

Line 114-118: Fitted conceptual hydrological also base on reliable field data (to define the parameter range) and not “indirect theoretical values” - whatever this means.

Nobody is able the determine 100% correct values for each point in space. Moreover,

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none of these models is 100% physical. They all have empirical / conceptual parts, i.e. parameter fitting makes sense.

Line 119-121: This statement requires further explanation. For me, it is too general and actually, just depends on the basis of the recharge coefficient determination.

Line 149: In line 74 you have classified this (correctly) as an indirect method.

Line 187: You are criticising annual recharge coefficients, but using annual runoff coefficients. For my taste, exactly the same problems with recharge coefficients are valid for runoff coefficients.

Line 250: What does extensive field mapping mean?

Line 283-284: What does "successfully" mean? The model was already applied in 1992 – lot of development has happened since then.

Footage 2 on page 7: Potential ET is only energy limited. Why you are referring to precipitation characteristics?

Line 297: You considering the minimum recorded soil moisture as the wilting point. What is the basis for that assumption?

Line 303: You are talking about "bypass flow through preferential pathways". Earlier you talked about directly exposed karst (line 183-184) and soils with high clay content and cracks during dry season (line 225). These are clear signs for indirect recharge processes and preferential flow. However, your model does not account for this.

Line 313: So far as I understand, this is a simple one bucket model. If the bucket is full, you get recharge. It seems to me that you not distinguish whether soil moisture is stored at the surface or in e.g. 50 cm. As long as there is enough water (it doesn't matter in which depth) it will be evaporated by potential evaporation. If so, this is far away from real hydrological processes and also not state-of-the-art.

Line 371-372: From my point of view, the temporal pattern, i.e. interannual variation,

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of precipitation is one of the very crucial factors for recharge processes especially in semi-arid environments. I would not separate it.

Line 372-373: There is a clear definition that min. 30 years are needed to represent climatic condition.

Line 380: What about an objective measure for the goodness of fit, like NSE, KGE etc.?

Line 392: Can you quantify this correlation? Btw, from my point of view, the correlation is not an ideal measure. Again, I would propose to use the common ways to compare data series in hydrology: RMSE, NSE, KGE etc...

Line 401: I am not so sure what you mean by "correlation". Your visual impression that two data series fit to each other?

Line 440-441: I not really understand this sentence. Earlier, you stated that you do not calibrate. However, "this was done in order to minimize the bias" sounds like fitting for me. And which bias do you mean?

Line 488-489: What does representative means? You should provide numbers that the reader can follow your ideas and conclusions.

Line 545: Why you consider the soil moisture measurements, FC and thickness as representative? What is the basis for this assumption?

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