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Hydrology and Earth System Sciences
Discussions



# *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 #1**

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Review of 'Field-based groundwater recharge and leakage estimations in a semi-arid Eastern Mediterranean karst catchment, Wadi Natuf, West Bank' by C. Messerschmid et al.'

General comments

Overall, the paper is remarkably chaotic. Points are being made based on material that is presented later, part of the mathematical relationships are presented verbally in the text instead of in equations, and the notation of units is at times inconsistent (see



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the detailed comments in the text to find many of these). Furthermore, text fragments appear in the wrong sections throughout the paper. Introductory material is introduced in the conclusions, elements of the discussion appear in a figure caption, and methodological aspects are scattered throughout the paper. The only section of the paper that is well written is the geological description of the research area.

Many sections of the text are carelessly written:

- paragraphs are not well organized, evidenced by repetitions and discussions of related observations occurring at different locations within a paragraph

- terminology is used in way that make sense neither grammatically nor semantically, rendering the text incomprehensible

- the tables are not numbered in order of reference

- table captions contain much too little information to allow the tables to be read independently of the text (or even after consulting the text)

- entire table columns are left unexplained

The consequences of the poor structure of the paper are severe. The authors claim to have developed a new method to estimate groundwater recharge but even a very patient reader willing to go through the manuscript multiple times would be unlikely to be able to reproduce the approach. I suspect key elements were simply not reported because many technical details were omitted from the methodology section and could not be found elsewhere even though almost every section of the paper contains methodological elements.

This lack of completeness is aggravated by the liberal use of strong assumptions that are never made explicit, critically discussed, or tested (see the comments in the text for examples). This hits at the heart of the science in this paper and cannot be remedied by a thorough revision. In setting up the project and the modelling strategy, here seems to have been a lack of critical thinking.

Statistical jargon is used copiously but incorrectly. Any statistical analysis is missing, yet bold claims are being made about correlations, representativeness of a seven-year data set, and the inclusion of spatial variation within the research area even though only 8 monitoring sites were available. The contrast between boldness of the claims and the flimsiness of the evidence that supposedly backs them up casts serious doubt on the credibility of the science.

The HESS formatting guidelines are frequently ignored, for example by including footnotes, underlining parts of the text, placing equation numbers in front of the equations, etc.

HESS does not wish to publish regional studies. The paper mentions that the method developed here is applicable to other areas, but does not indicate how, nor does it devote any section of the Methodology or the Discussion to the generalization of the results. The Conclusions repeat the claim of generalizability, but limit it to the Mediterranean region. This is followed by a list of conditions that need to be met before the method can be used. That list is so demanding that it serves as proof that the method can hardly be applied anywhere else.

Figure 4 seems to indicate measurement issues. When it rains after a long, dry period it stands to reason that the observed soil moisture storage in equivalent water layer cannot rise above the accumulated rainfall. If we look at Nov 2006, we see several showers before storage peaks at about 108 mm. It is not easy to see but I estimate the total amount of rain that generated that peak to be less than 50 mm. May 2008 is even more pronounced. This can only happen if recharge is captured from a large area and concentrated in the soil over a much smaller area. This would require

1) considerable lateral flow before infiltration (overland flow)

or

2) rapid infiltration through cracks to the groundwater, followed by lateral flow in the





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groundwater. For this lateral flow to converge towards a much smaller area there must be some kind of depression in the impermeable layer below the aquifer. The water moving toward that depression can only wet up the soil from below if the groundwater table is shallow enough. The authors do not mention any of the factors supporting mechanism 2, which is improbable anyway. But overland flows supporting hypothesis 1) were not mentioned either although the simple presence of the wadi indicates that overland flows do occur.

In the unlikely case that flow according to mechanism 1 or 2 wetted up the soil in a fraction of the area only, the normalization to mm should not have been applied, since the various variables presented in the graph and the overall analysis represent different areas. The dimension of choice should then be volume.

In case that neither mechanism applies, the authors need to explain how the difference between soil storage change and rainfall can be positive. If they cannot, this must be a measurement error. The size of this error throughout the observation period and its effect on the recharge estimation error need to be examined then.

The caption of Figure 5 mentions the cleaning of one spring and the effect on its flow rate. The condition of the springs apparently affects their discharge. So, evidently this also affects groundwater recharge (see Eq. 5). Therefore, spring maintenance is an important factor for groundwater recharge, yet it is brought up only in a figure caption.

The caption calls the effect temporary but is appears to double the well production for at least two years and the effect was still very strong at the end of the observation period so there is no clear picture of the persistence of the effect. It seems obvious that the net recharge to the aquifer feeding that particular well changed in 2007 with effects that last well over three years. I have the impression that the authors did not consider any of this in their calculations, or carried out a scenario study to examine the effect of different well maintenance practices.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2018-329/hess-2018-329-RC1-supplement.pdf

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