

# ***Interactive comment on “The potential uses of tracer cycles for groundwater dating in heterogeneous aquifers” by Julien Farlin and Piotr Małoszewski***

## **Anonymous Referee #1**

Received and published: 28 September 2016

Reviewer's comments for HESS-2016-393

Farlin and Maloszewski, 2016, The potential uses of tracer cycles for groundwater dating in heterogeneous aquifer, Hydrol. Earth Syst. Sci. Discuss.

Tracer test techniques are widely used for groundwater dating applications. The authors provided three points of arguments and suggestions on the hydrogeology tracer tests, based on some previous studies. Generally speaking, most statements in this manuscript have been discussed in many previous literatures, in other words, not novel. The manuscript failed to explain the statements clearly with enough information. This paper is still far away from the quality to be published, especially the figure. I would suggest a major revision, but recommend the editor to take an additional look since the

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required revision will be large. Please see my detail comments as follow.

1) Title: The authors use so many words and pages in the manuscript to discuss the groundwater temperature tracer applications, however, not mention it on the title. The third statement of groundwater temperature model is obviously the most important part in this paper. It is necessary and worthy to stress on the title.

2) Ln 15-16: Why exponential model? Can you provide any mathematic and/or statistic analysis on different fitting model? I would also suggest to take a look/cite more study results in previous literatures.

3) Ln 18-19: The authors discussed the degree of heterogeneity is small enough or not for tracer cycles. How to define the degree of heterogeneity? Is there any quantity definition?

4) Ln 20-22: Groundwater temperature tracer has been widely used in so many previous studies. It's not a new technique at all. I'm not quite sure about the authors' major contribution and breakthrough in modeling development of temperature tracer application, which should be highlighted in the abstract.

5) Ln 41: Groundwater system is not always "slowly-responsive". The authors probably need to be specified their study area as "sandstone aquifer".

6) Ln 66-80: Detail explanations are needed for the modification of Kirchner's methodology. For example, what are the benefits of increasing number of sub-catchments? Why do you use a random number as MTT? Is the result sensitive to the number?

7) Ln 123-125: A more detailed introduction of field site is necessary. You can't only cite previous paper without enough information.

8) Ln 130-131: What are the time spans of summer/winter periods?

9) Ln 146-147: The authors claimed that results are insensitive to the number of sub-catchments. Why did you still need to divide sub-catchment?

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10) Ln 170-171: How to determine the method is “sufficiently robust”? Any quantity? A statistic analysis will be important here.

11) Ln 184-185: Is it redundant to state the total TDD exhibits the same properties with individual TDDs? I think this is obvious. Some explanation is necessary if the authors believe there is any scientific contribution here.

12) Ln 196: The word “density” is misleading here and easy to confuse readers.

13) Ln 214-216: The first conclusion is obviously not new found.

14) Figure 1: The y axis and label missing. The MTT range in the third row (Kircher method) is actually between I1 and I2, so it's not right to call it large MTT range. This figure quality is not acceptable.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-393, 2016.

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