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Hydrology and Earth System Sciences
Discussions



# *Interactive comment on* "Technical Note: On the confounding similarity of two water balance formulas – Turc-Mezentsev *vs* Tixeront-Fu" *by* Vazken Andréassian and Tewfik Sari

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Thank you very much for taking the time to review our technical note. Please find below answers (as well as one question concerning one of your comments that we did not understand):

1. The main point that the Turc-Mezentsev and the Tixeront-Fu are near equivalent has been established previously by Yang et al. (2008). Why is it worth repeating this point? What is really the novel addition of this work?

We completely agree that Yang et al. (2008) established the equivalence, and we do



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give them proper credit for it in our note. However, we do consider that their paper was not clear on a few points, and this is why we saw a need for a "clarifying" technical note. We find the Yang et al. paper unclear/incomplete on the following points:

* Equivalence between the two equations: Yang et al. write that the TM and TF equations are "approximately equivalent", we find the expression much too weak and this is why we wished to use the much stronger "confounding" ;

* Literature review: Yang et al. make no reference to the original work of Turc (1954) and Tixeront (1964). They likely were not aware of it ;

* Uniqueness of solution: Yang et al. wrote in their conclusion (p.8) that "this paper mathematically derived the general solution to the mean annual water-energy balance equation, and proved its uniqueness". This is obviously wrong (and to tell the truth this is extremely surprising because Yang et al. are comparing the TM and TF formulas, they know perfectly that the solution is not unique) and this is why we added table 6 to show that the TF formula respects both hypotheses. Last, in our note we tried to treat as much as possible the two forms of the formulas in parallel (streamflow & actual evaporation) to provide a reference for those who wish to use one or the other.

2. What is the point of section 4.3: I read this section several times, but the description is not clear enough (for me) to understand what the value is of this paragraph (and I suspect other readers may suffer from the same problem as me).

Section 4.3 was an attempt to explain with a lot of words and little formulas what the TM and TF represented. This was not easy and we know that the result is not perfect. If you did not understand it, it very likely means that we failed to explain clearly what we had on our minds. She should probably remove this part.

Detailed suggestions

3. Line 1: I am unsure that "confounding" is really useful here. Would removing this word not make the title simpler, more accurate, and more objective? The same applies

for every time the word "confounding" is used throughout the manuscript.

We added "confounding" precisely because we thought that Yang et al. had not been affirmative enough when stating that both formulas were "approximately equivalent". But we take your point on the fact that this word is perhaps useful in the title, but not anymore is the rest of the paper (we will remove it elsewhere).

4. Line 13: "identified" seems redundant?

Yes indeed, removing it does simplify the sentence.

5. L36: why "maximum evaporation", rather than "potential evaporation"? The latter term seems more consistent with commonly used hydrological terminology.

Hydrologists usually use only "potential evaporation" while the agronomists distinguish theoretical potential evaporation/potential evaporation/actual evapotranspiration/maximal actual evapotranspiration/potential (grass) evapotranspiration, etc. You are right that potential evaporation is more common in hydrology. Because the TM and TF formulas are considered as "Budyko-type" formulas, we wanted to utilize Budyko's expression (i.e. maximum evaporation) to avoid any debate with our colleagues agronomists.

6. L65-66: Explain why.

We could rewrite L 65-66 as follows: "In our interpretation of the TM and TF formulas, we will also use their partial derivatives, which we present in Table 2 and Table 3 (they are sometimes used to predict the hydrological impact of climatic change)."

7. L88: Is this a result from this paper, or is this sourced from literature?

It is in fact in Yang et al. paper (which as cited a few lines above). We will add a reference.

8. Table 4, property7: this statement is true for "absolute streamflow changes", not for "relative streamflow changes (i.e. streamflow elasticity)". Be explicit about this

difference.

We are not sure to understand this remark, because we would define the relative elasticity as the linear relationship between (Qn/Qmean -1) and (Pn/Pmean -1), with n an index for the year. Could you be more explicit?

9. L138-140: explain in simple terms what is different.

The detailed mathematical explanation comes a few lines later (LL 144-151) so for this sentence we could simply complement the sentence: "What can be concluded from the analysis presented in the appendix is that both formulations are based on very similar but nonetheless slightly different hypotheses" into "What can be concluded from the analysis presented in the appendix is that both formulations are based on very similar but nonetheless slightly different hypotheses, which set the "ependency of the partial differences of streamflow to the partial differences of climatic variables"

10. Section 4.3: I don't understand the point of this section.

We tried to explain the behavior of the generalized harmonic mean with plain language, in a less mathematical way, but if you did not understand, this probably mean that it did not help to make think clearer, so we could remover this short section.

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