We provide here a detailed answer to the questions raised by the reviewers (same answers were posted in the public discussion).

REVIEWER 1

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 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. We will remove this part from the main text, put it in appendix with a note showing how the two functions relate to the TF and TM formulas..

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 did remove it elsewhere, and replace it by “puzzling” in the title

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.

The 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 formulation, 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 dependency 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 will put this short section in appendix

REVIEWER 2 (Maik Renner)

The manuscript by Andréassian and Sari explains the historical background of two well known formulations which describe the partitioning of water and energy balances under climatological average conditions. They also clarify the naming of these formulas and I believe that this note can help to achieve a more consistent usage of the two formulas in the literature. The appendix on the genealogy of the two formulations is quite a treasure and I have a small concern that it might be overseen. I think that this appendix could be a section in the main text. Only the subsection on Yang's system is a bit long, but indeed very interesting. The paper is very well written and thereby provides a clear and easy to follow discussion of the hydrological interpretation and the mathematical derivation. Hence this paper will be a valuable source for hydrologists which need orientation in the vast literature on that topic. Minor remark: Figures: the limits of the y-axis could be decreased to better see the differences. In the moment there is too much unused space.

We hesitated to introduce the historical part in the main text, but we did not find a way to do it that would not turn the paper too complex to read. We left it in appendix but added a sentence to encourage readers to go and read this part.

REVIEWER 3 (Laurène Bouaziz)

1. The authors provide a comprehensive and well-written comparison of two independently derived water balance formulas: Turc-Mezentsev versus Tixeront-Fu. The authors show that the two formulas are numerically equivalent (also in their partial differentials), and even though the Tixeront-Fu formula can be characterized as slightly more general, hydrologists can feel free to choose either one of them. An interesting analogy is made between the mathematical characteristics of the shape of the formulas and their hydrological meaning. Additionally, the Appendix provides an overview of the history and derivation of the formulas. I enjoyed reading this comprehensive comparison of the two water balance formulas with a clear final message and I therefore recommend the publication of this manuscript after only a few minor corrections.

Comments:

2. Line 24: Apostrophe s is missing in: "Turc's work" : done
3. Line 86: 'than' instead of 'that'? done
4. Line 97: It is mentioned that both formulas are equivalent except for very low values of the humidity index and I wonder if there is an explanation to this observation. We could not

think of any mathematical explanation (and because these hyper-arid catchments are anyway extremely difficult to model, we stopped looking for it)

5. Section 4.3 (line 163-180): This section makes an interesting mathematical analysis of the hydrological formulas, but it would make it easier for the reader to explicitly refer to Eq. 2 and Eq. 4 to explain the analogy with Eq. 15 and Eq. 16. Thank you, however we are not sure that we will keep this section, reviewer 1 found it extremely difficult to understand. We found that interpreting the two formulas as an approximation of the classical Min and Max functions would help the reader "visualize" what the formula was doing… but it seems that it remained to abstract?
6. - Line 255: I believe a typo was introduced in this formula and that the authors meant E/E0 ~ P/E0 instead of P/Ex - done, thank you
7. - Line 259: here also I think a typo was introduced and that the formula should read x = P/E0 instead of x = P/E - done, thank you