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Discussions

Interactive comment on "Investigation of variable threshold level approaches for hydrological drought identification" *by* B. S. Beyene et al.

Anonymous Referee #2

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1) General Comments:

This paper investigates different threshold level methods and their suitability in characterising droughts from a range of catchments in Europe. The work is well-referenced and finds that the choice of threshold level method may not be as important as has been previously considered.

However, there are some significant issues in the interpretation of the information presented in Table 1. The number and scale of the misinterpretations are such that I urge the authors to thoroughly review the data and interpretation presented in this paper before re-submission. In addition, the content of the discussion section needs to be addressed, and there is an over-reliance on one specific reference. Considerable work

is required on major revisions to address these issues before the paper is ready for potential publication.

2) Specific Comments:

A) Interpretation of data

There are a number of statements in Section 4.1 that are not supported by the results in Table 1. These include:

∼∼ "the M_MA threshold method has given the least number of droughts in most catchments (Table 1)" (Page 12776, Lines 2-3)

The only rows in Table 1 for which M_MA produces the least number of droughts (n) of all of the four threshold methods is for GW in Nedožery and GW and Q for Guadiana (there are some other rows for which M_MA is tied for the lowest, but not enough to validate the statement above). In the case of the Narsjø, for P and GW the M_MA method actually identifies more droughts than any other method.

∼∼ "the method [presumably still referring to M_MA] generates longer mean drought duration" (Page 12776, Line 10)

The section preceding this line is a little difficult to follow, but if I have understood correctly, this sentence refers to M_MA producing longer mean drought duration. If this is the case, Table 1 does not support this claim. D for M_MA is less than D for D_MA in 13/20 rows, and is the same for another 4 rows. In addition, D for M_MA is less than D for 30D in 11/20 rows, and is the same for another 2 rows. For three quarters of the rows, D_MA or 30D (or both) actually produce longer mean durations than M_MA.

∼∼ "the computed mean drought duration for these hydrometeorological variables is much longer than those computed with other methods for the rest of the catchments" (Page 12776, Lines 23-25)

I assume that the sentence refers to D_FF as it appears in the section on D_FF. There

HESSD

11, C6533–C6537, 2015

are only 4 rows (out of 20) in Table 1 for which D for D_FF is larger than for any other threshold method (another 2 rows are tied largest with another method). In 7 out of 20 rows, D for D_FF is actually the lowest D value compared to the other three methods. There is also inconsistency between this statement and the one quoted above, with one claiming D_FF computes longer mean duration and the other claiming M_MA does the same.

∼∼ "Mean calculated deficit volume is often higher when using the D_FF and D_MA threshold methods than using the M_MA and D30 threshold methods" (Page 12777, Lines 1-3)

The values for V or H are generally very similar across all four threshold methods; where they are different, there is only 1-5 mm difference between the highest and lowest of the four. There is so little variation, it might not even be worth mentioning (except to say that there is so little variation). With reference to the phrase above, I can only count 4 rows out of 20 where V or H for both D_FF and D_MA are higher than V or H for both M_MA and 30D.

B) Content of discussion section

More than half of the discussion section (Page 12778, Line 22 to Page 12780, Line 8) focuses on different options for threshold level approaches and how these have been applied in other research. Although well-referenced, this part reads like a literature review of threshold level approaches, and could probably be moved earlier in the paper into the Introduction. The first detailed discussion specifically on this paper starts at Page 12780, Line 9 and runs until Page 12781, Line 2 (less than a full page). Perhaps moving the more literature-based first half of the Discussion into the Introduction will allow more thorough interpretation of the results in this paper.

C) Over-reliance on specific reference

The work of Van Loon & Van Lanen (2012) is referenced twelve times in this paper.

HESSD

11, C6533–C6537, 2015

Figures 1 and 3 of this paper are reproduced directly, and the modelling work underpinning the research in this paper was also conducted for the earlier work. Given the common model outputs underpinning both studies, it is surprising to note that the data in the left-hand columns of Table 1 are marginally different (but almost identical) to those given in Table 3 of Van Loon & Van Lanen (2012), if I have understood correctly the methodologies of the two papers (perhaps the authors could clarify this). I think Equations 8, 9 and 10 in this paper also appear in the earlier work, although Equation 8 may be incorrectly written (Di+1 has been written twice). Whilst the analysis of different thresholds is novel in this paper, there is perhaps an over-reliance on Van Loon & Van Lanen (2012) in general.

3) Technical Corrections:

Page 12766, Lines 25-26: Floods can have a "very complex development pattern" too

Page 12767, Lines 15-16: "Holling et al., 1978" is given as "Holling (1978)" in references

Page 12768, Line 1: It might not be clear what "SD" means to the reader without writing out in full

Page 12772, Lines 15-16: What happens to data / thresholds for 29th February?

Page 12774, Lines 2-4: It would be worth stating the cutoff frequency used in this study

Page 12774, Line 19: "magnitude" is a drought characteristic itself; this phrase could confuse readers

Page 12775, Line 22: Can you justify the hypothesis "drought severity should decrease"?

Page 12776, Line 13: Text says "130 days" but Table 1 seems to suggest this value is 756 days. If the correct value is 130 days, this would make it considerably smaller than for the other three methods (unless those data are incorrect as well). Are these

HESSD

11, C6533–C6537, 2015

differences between tabulated data and data quoted in text the cause of the issues (mentioned above) surrounding the interpretation of Table 1?

Page 12776, Lines 25-29: It is not clear which catchment this sentence refers to

Page 12777, Line 3: It should be “30D” rather than “D30” written here

Page 12777, Line 22: The artefact drought is only for a small part of “December 1984 to June 1985”

Page 12778, Lines 15-17: Looking at Figure 8, there seems to be a break in the drought across June-July 1976 for discharge in the Narsjø catchment (as well as other smaller breaks later in 1976). This seems to contradict the phrase: “the discharge anomaly persisted for 308 days since 7 March 1976”

Page 12781, Line 1: Abrupt changes in discharge can also occur in temperate climates

Page 12781, Line 2: I am not sure that analyses on five European catchments means that conclusions can be drawn on the most suitable threshold level method for “global scale drought analysis”

Page 12788, Table 1: Vertical lines could be used to separate the threshold methods into four sections (otherwise the headers “V (mm)” and “n” are too close together, and the “/” links them together)

Page 12789, Table 2: “SD” should be written out in full in a figure caption

Page 12791, Figure 2: It should state in the figure caption which catchment these curves represent

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