

Interactive comment on “Determinants of thermal regime influence of small dams” by André Chandesris et al.

Anonymous Referee #1

Received and published: 2 May 2019

General Comments: The topic of this paper is of high importance, and the research is critically needed as the authors correctly point out that our knowledge base of the impact of small dams on water temperature (and stream ecosystem function in general) is woefully lacking. The data set the authors have collected appears robust, and with a sufficient number of sites to make a useful contribution to the literature. I find the analysis of the data to severely lacking, and the presentation of the results to be mainly using individual sites as examples that are difficult to judge if they are representative. The discussion is quite thorough and insightful, but without adequate data analysis, I felt that the conclusions were not well supported.

Specific Comments: 1. Figure 2 – why present years in reverse chronological order? Also, why this stream and these years? If possible, it would be preferable to compare

C1

2014 (cold wet year) with 2015 (warmest, dry year in data set). 2. General – figures don't do a very good job of illustrating points made in text in results. I question whether all the figures are needed (e.g., Figure 3). Figure 5 – presenting time series does not show correlation between two variables –one would need to plot air temp vs. water temp to show directly. Figure 4 – never covered in results section. 3. The authors mention differences in mean temperature, but never provide this information in a table. Further, they report median differences without justifying why this metric instead of means. I feel medians can be a useful indicator of central tendency, but the mean is also useful, and needs to be presented if it is discussed. 4. Section 3.4 – authors state that air and water temperatures do not correlate, but did not perform a correlation analysis. 5. Section 3.5 – how were these groups distinguished (meaning, what formal method was used). My impression is that the investigators did this “by eye”, which is not acceptable in my view. A formal cluster analysis would be much more appropriate. Moreover, I think it is hard to defend splitting out groups with such a small number of sites. 6. Section 3.6 – in the methods, the authors state that they used mean temperatures in the PCA analysis, but this doesn't show up in the results. Further, the reporting of the PCA results is very incomplete. Loadings of the various variables is needed, as is some criterion for determining what are the significant correlations. I can't say I understand fully how to interpret the circle correlation plot. 7. Section 3.7 – this section does not provide a synthetic view of any of the data, and the intent of this section is unclear. Suggest removing it entirely. 8. Section 3.8 – the arbitrary nature of this analysis provides little insight or direct ecological interpretation. In the discussion the authors correctly indicate that the choice of a 22 degree is actually not arbitrary, but has a basis in that temperatures above this point are generally deleterious to salmonids. Although I think this section could be a valuable contribution by the research, the fragmented presentation leads me to suggest removing it entirely. 9. In the discussion, the authors talk about different years (hot vs. cool, or wet vs. dry), but none of the analysis really looks into this. I think it is an important point, so would like the authors to explore and quantify this in a reasonable way. 10. In the introduction and

C2

discussion, the authors talk about the importance of dam and reservoir size, but don't do any formal analysis. At a basic level, it would seem that correlation or regression of reservoir area, and another analysis with residence time, on the response variables of mean temperature difference, mean difference in maximum temperature, and mean difference in minimum temperature would be an important starting point. 11. The discussion of biological effects was quite thorough.

Technical Comments: 1. Many grammatical errors – far more than is appropriate for a scientific reviewer to make edits on, but these need to be addressed before publication. 2. The citation for Dunham et al. is incomplete, but I applaud investigators for addressing instrument calibration issues, which are often ignored!

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2019-136>, 2019.