Anonymous Reviewer #1:

Review report

Title: Riverine dissolved organic matter responds to alterations differently in two distinct hydrological regimes from Northern Spain

This study examines the effects of anthropogenic flow alterations, primarily caused by dams, on DOM concentration and composition in Spanish rivers of the Atlantic and Mediterranean region. This research compares rivers with natural and altered flow regimes and looks at how different flow components impact the DOM regime, such that altered Atlantic rivers generally show lower DOM composition shifts compared to natural ones, while Mediterranean rivers appear more resistant to flow alterations, maintaining relatively consistent DOM characteristics.

The study is overall well conducted, relies on a sound empirical basis and uses advanced statistics to identify patterns. The authors introduce the topical background excellently. In that sense I think this is definitely publishable and interesting to the EGU readership. However, there are several issues that I think need some close attention to increase the accessibility and clarity of the study. There are, in my opinion, terminology and reasoning aspects that needs improvement. I hope my suggestions in this regard are helpful.

REPLY: We thank the reviewer for their positive evaluation of our work.

General comments

Regarding the study concept and abstract, and even for someone who works with DOC, the goals and findings of the study are not easy to grasp. I think this has partly to do with the comprehensive aspiration: the authors do not only want to look into DOM “regime” shifts after flow alterations, but also compare these shifts in two different river system types, and seek for the system properties that are statistically connected to response. This is tough to comprehend, and it does not help that the terminology is at times imprecise and self-defined: DOM “Turnover” is used here differently than in most other contexts (where it essentially means transformation and/or mineralization) – is “compositional shifts” not clearer? I also have problems to understand was is meant by “annual DOM composition” (L12), “temporal turnover indicators” (L256) and several other derivatives of the DOM-related language. I suggest to revisit the part of the work that introduces the terminology use in general, and specifically the analysis goals, concepts and expectations, and harmonize the language related to these. One headline in the results “Linking DOM regimes to flow regimes” could for example be used more often.

REPLY: Thank you for pointing this out. We agree that some of the terminology used in the manuscript, particularly the term “turnover,” may be confusing. While we originally borrowed “turnover” from community ecology, where it precisely means “shifts in composition”, we acknowledge that it carries an established meaning in other contexts, which can lead to misinterpretation. To improve clarity, we will replace this term with less ambiguous alternatives, such as “compositional shifts,” as suggested.

Similarly, we recognize that expressions like “annual DOM composition” are imprecise. We propose revising this term to “annual average DOM composition” to better convey our intended meaning. Additionally, we will carefully review the manuscript to harmonize the terminology with the study’s goals and concepts.

To further enhance clarity, we will provide detailed explanations of key terms in concert with an explanation of the study concept in a conceptual graph presented early in the paper. We believe these measures will improve the manuscript’s readability and address your concerns regarding the difficulty in grasping the study’s goals and findings.

Specific comments

L30-32: “This highly reactive fraction…” a reference is needed.

REPLY: Here, we will add the necessary citations.

L34 but also temporally (Catalán et al., 2016)… not an adequate citation in that context, because that work really looks at spatial differences of a time-reated property

REPLY: Thank you for noticing. We will add the necessary citations.

L53 This rather general model of a DOM regime´s reaction to damming needs fine-tuning… quite a colloquial language for the central part of the study motivation

REPLY: We will change the language here.

L55 inflowing DOM concentration: not really the concentration but the amount

REPLY: We will correct this.

L56 I don’t agree that “all” these biotic factors are “associated” with the natural flow regime

REPLY: True. We will update this sentence.

L 58, the term “compositional turnover of DOM” needs to be clearly defined, see above general comment.

REPLY: We will change this wording (see our answer above) and clearly define what we mean by it here where it first appears.

L63 two naturally defined hydrological classes,.. this is the first appearance outside of the Abstract and the relevance of this concept demands appropriate introduction on first appearance

REPLY: Thank you for noticing, we will add a definition before this point.

L65 We expect the effect of flow regime alterations on the DOM regime to depend on certain characteristics of the natural flow regime. … this is an unintuitive research goal, what “characteristics" could this be?

REPLY: We apologise for the confusion, this was not well formulated. In fact, we here merely wanted to suggest that effects of flow regime alterations on the DOM regime will depend on the initially unaltered flow regime. Natural flow regimes (and likely also associated DOM regimes) are quite diverse. And even if flow alterations are also diverse, any alteration of the DOM regime still happens from a certain baseline dictated by the natural flow regime. We will reformulate this part of our research objectives and support our reasoning with a conceptual figure laying out our hypothesis framework. In this, we will also point to our analysis in the last section of the study where we try to identify hydrological drivers of DOM regime characteristics among a large number of hydrological indices.

L 161-163, the authors state that the sampling dates to the centroid of a river serves as a measure of temporal turnover of DOM and it is computed as a dispersion. There is not a clear explanation of what this dispersion precicely means and how it is derived. More explanation would be useful.

REPLY: We will replace the phrase “temporal turnover” by the less ambiguous phrase “temporal shifts in composition” and will explain better how it can be captured by a multivariate measure of variation (i.e. “dispersion”).

L259, what are the “temporal turnover indicators”? These indicators are not explicitly defined.

REPLY: These will become “indicators of temporal shifts of DOM composition”. We will add a clear description in the methods to explain the coefficient of variation calculations and how they serve as such indicators.

L309 blurry but more encompassing… not sure I understand what you mean here

REPLY: Our intention is to express that a multivariate measure of temporal compositional change of DOM (“turnover”) like “dispersion”, which is influenced by all included DOM indicators, is a very integrative measure, yet this comes at the cost of less precise meaning. We will update this sentence and use less colloquial language.

L 361, “hydropeaking” may need definition or referencing

REPLY: We will add a reference here.

L477-479, sentence quite long, consider dividing

REPLY: We will divide this into 2 sentences.

Fig 2 add aM irrigation

REPLY: Thank you for noticing, we will add this to the legend.

Table 4 sample n and frequency may be a useful information here

REPLY: We will add sample sizes and the number of sampling occasions to the legend of the table.

Figure 7 flow properties: these should be introduced at one point earlier in the text. Maybe revise the image altogether because it is hard to read and unexpectedly complex for this stage of the manuscript. Why not for example instead of grouping by category, sort by influence, or withdraw from showing *all* influences and select the most significant/important ones. I believe this would increase the interaction with the information massively.

REPLY: Indeed this figure is very complex and we have tried many alternatives for the sake of saving space and increased clarity. We believe dividing it even more would create more confusion than provide clearance at this point. The figure already shows only a subset of all the indices used in the model, selected according to VIP values. Also, we wish to keep the aspect of comparability among the 3 models built for the three response variables, as this makes the plot both informative as well as space-efficient. The current plot version also nicely shows that there is a trend throughout the year in flow magnitudes (positive correlation in winter, negative in summer) as well as in other indices such as minimum-maximum daily to monthly events and other contrasting indices of high water vs low. We will think of some additional graphical ways to improve the interaction with the information.

494 subscript CO2

REPLY: We will correct this.

Citations of the Xenopoulos review show up several times. Maybe it is useful to cite original study in some cases?

REPLY: We will update these references to the original study where applicable.

We thank the reviewer for their insightful and constructive comments.