

## Response to Reviewers

Dear Editors and Reviewers,

Thank you very much for your time and effort for reviewing the article titled “**A socio-hydrological framework for understanding conflict and cooperation in transboundary rivers**”.

We really appreciate all of your insightful comments. To address them, please see a revised version of the manuscript (changes marked in red font). We have also provided point-by-point responses to each of your comments in blue font below:

### Referee #1:

*I believe the paper would benefit by bringing it all together a bit more. The synthesis table and how the case studies are similar or different in terms of various elements of the framework such as social motives, power, institutional capacity etc is useful but how such system components talk to each other is not clear from the narratives of the case studies.*

**Agreed.** We have revised the purpose of Section 4 in **Line 290-295**, and the whole section paragraph by paragraph to reflect similarities or differences in terms of various elements of the framework among case studies (**Line 300-415**). A summary paragraph has been added in **Line 415-425** to summarize the implications of the proposed framework to the three case studies.

*Description of slow and fast dynamics is not so clear in the case studies. While the authors argue that hydro-economic treatment of transboundary river sociohydrology has gaps, they do not convincingly demonstrate that these gaps are filled by bringing in additional components through the case studies. I also think that the authors are unclear about how to 'quantify' various variables and concepts corresponding to these system components (also the sets of these variables and components appear to be 'open' sets) - they do allude to somethings in the paper (Table 1) but it is not clear to me how it is educating the slow and/or fast dynamics. Perhaps a more tangible effort to quantify some slow/fast dynamic equations (such equations can be conceptual in nature) will help. Also, some more tangible evidence of how some of its corresponding variables can be observed/measured, e.g. through behavioural experiments or surveys in environmental psychology will help. Finally, what is it that neo-classical economics cannot explain that the proposed system components help explain in the narratives of the case studies? Almost all of the case studies can be explained by dynamic non-cooperative game theory under uncertainty (evolving benefits, power, institutions, capacity and their feedbacks under exogenous shocks). So, what exactly the framework is accomplishing remains unclear and should be clearly brought forward. What is endogenous, what is exogenous to the system, how behavioral experiments/ environmental psychology data collection and analysis methods are being deployed, is it very slowly evolving culture/institutions and its effect on norms, perception of risk and capacity (given the time horizon of the case studies discussed) etc that are not covered by the current hydroeconomic models and needed to fully make sense of the presented narratives of the three basins?*

**Agreed.** As the reviewer's comments are high level and quite comprehensive, we have fully rewritten Section 3.1 - 3.2 (**Line 185-290**).

*I also think the figures and language at places can be improved.*

**Our apologies for the grammatical errors and improper use of language. We have carefully revised the whole manuscript and updated both Figure 1 and 2.**

## Referee #2:

*With that said, revisions are needed to clarify the contribution of this paper and provide an evidence basis with which to evaluate the framework. My concerns include that (a) the review of existing literature on conflict and cooperation is overly general, (b) I don't see where the authors articulate their approach to developing the framework, and (c) the case studies (and possibly the framework) are built upon other manuscripts that have also been sent to the same special issue in HESS. It is therefore important that the authors more clearly delineate the objective and contribution of this manuscript (including differentiating it from similar works recently sent to HESS). I elaborate on these concerns below.*

*1. The abstract states: "This article aims to review the existing knowledge on conflict and cooperation in transboundary rivers from a multidisciplinary perspective..."*

*a. However, the literature review of other disciplines (Section 2.2) is cursory, and more specifics would provide a clearer picture of how the framework relates to other fields. Ideally the literature review would not only motivate the framework but also provide a theoretical foundation for the framework. It seems to me that the literature review focuses on quantitative models of conflict and cooperation in transboundary basins, which is quite different from a general literature review on conflict and cooperation in general. The issue with this is that the framework presented seems to be quasi-quantitative – it is packaged in such a way that it could be formulated into a quantitative model, but seeks to find a middle ground whereby simplifications needed for simulation. To give an example, the paragraph beginning on line 104 reads:*

*"Neoclassical economics has dominated the simulation and explanation of human cooperation behaviour. It explains cooperation in riparian countries from a purely economic perspective, focusing on the tangible outcomes received by these countries, assuming them as rational actors with perfect information about all potential choices and their consequences (Schill et al., 2019). Hydrological models have been integrated with neoclassical economic models to simulate cooperation in transboundary rivers by optimizing the incremental economic benefits under a set of specific societal constraints. Thus, the influences from the social dimension are only considered as residuals from explanations of rational economic behaviour. These models have been criticized for being overly simplistic, and unable to capture the diversity of human behaviour (Schlüter et al., 2017), and thus fail to reflect the reality of conflict and cooperation in transboundary rivers (Wei et al., 2021)."*

*This paragraph would fit within a general discussion of sociohydrology, but I'm concerned it oversimplifies the contributions/relationship between neoclassical economics and transboundary studies. Further, it does not articulate how neoclassical economics informs the development of the transboundary framework. I find most of Section 2.2 to be similarly general and lacking details that would be expected from a general literature review of conflict and cooperation in transboundary watersheds. I would encourage the authors to more clearly describe which authors/manuscripts used which models (based theory from neoclassical economics), rather than use neoclassical economics as the subject (as in the first two sentences of the paragraph), which is confusing to me because the theory is applied by researchers to explain cooperation.*

**Agreed.** We have revised the abstract (Line 15 – 20) and the final paragraph of Section Introduction (Line 60 – 70) to clarify the aim of this paper.

We have also revised Section 2.2 with more detailed description on the specific models, for example, the hydrological-economic model (Line 105 – 115).

*2. The epistemological basis of the framework is not clear. Even though it is presented as a "proposed" framework, describing the origins of the framework (e.g., the methods / theoretical foundation) is critical because this will shape how the framework should be interpreted and applied. Line 190 states:*

*“We ... thus develop this framework by following the system theory in particular the complex system theory. The development of this framework are also built on the recent advances on understanding the coupled human relationships from social-environment ecological system (Folke et al, 2005), the Coupled Human and Nature Systems (CHANS) (Liu et al, 2007) and the general socialhydrological framework (Elshafei et al, 2014).” But these citations are particularly general and more evidence should be provided to support the framework. Related to this concern are the following points:*

- a. Is the framework meant to be general? What does it capture and what does it miss?*
- b. The notion that “Social motives,” “Institutional capacity,” and “Power status” affect international cooperation is uncontroversial and well established. These concepts are broadly defined, and therefore the relationship with “Willingness to cooperate” is likely context specific – with that said, Table 1 indicates that each of these can be computed via index. The rationale behind this choice should be more clearly explained along with a description of the relationship between these variables and “willingness to cooperation”. This rationale would also make it easier to evaluate the structure of the framework.*
- c. Watershed management is motivated strongly by interests within countries, but there is no arrow from “benefits” to “water management” in Fig 1.*
- d. The only interaction between countries is through the binary variable “Cooperation”, which is itself influenced by the “willingness to cooperate” of each individual country. Cooperation between countries is typically not binary — it can be continuous and it can be multi-dimensional, including many areas of cooperation beyond water or transboundary resources — the choice for a single binary variable is therefore confusing to me.*
- e. Additionally, what about the relational aspects that influence willingness to cooperate vis-a-vis specific countries. For instance, Sudan appears to have maintained a high willingness to cooperate, but this fact conceals an underlying shift in preference to cooperate from Egypt towards Ethiopia. Part of this shift was driven by changing power differentials across the three countries and the relational aspect of this differential must be considered, but does not appear to be reflected in the framework. Additionally, how do bilateral relations factor into the framework, and is this exogenous or endogenous?*

**Agreed.** As the reviewer’s comments are very comprehensive, we have fully rewritten Section 3.1 - 3.2 (Line 185-290).

3. *Some aspects of the framework are unclear. For instance:*

- a. Why are some variables slow or fast? Willingness to cooperate is marked as a slow variable but it could change rapidly with, e.g., a newly elected political leader.*
- b. Willingness to cooperate is driven by “Social motives,” “Institutional capacity,” and “Power status”. These variables can be represented by indices, but it’s unclear how these indices could be related to changes in willingness to cooperate. For instance, the social motives variable is represented by an index in the range 0-1. But how does this index relate to cooperation, and why?*

**Agreed.** Please see the rewritten Section 3.1 - 3.2 (Line 185-290).

4. *This paper presents three cases that build upon other manuscripts in the same special issue of HESS (p 281). These papers should all be cited on L281 and the authors should be clearer (up front, ie the abstract/introduction) about the relationship between this manuscript and the other case studies, including how this paper builds on those studies (e.g., was the framework developed based on those studies?) and what specifically this paper introduces that is a new contribution to the literature.*

**Agreed.** We have clarified in Line 420 – 425 the relationship between this manuscript and the case studies.

*I appreciate the value of using the framework to compare across case studies in Table 2. With that said, the case studies were described in such a way to fit within the framework, but it's unclear what value the framework added to understanding the individual case studies.*

We have clarified the purpose of applying the framework and how it can add values to the three case studies in **Line 290 – 295**, and **Line 410 – 425**.