HESS-2017-537

How downstream sub-basins depend on upstream inflows to avoid scarcity: typology and global analysis of transboundary rivers

We sincerely thank the reviewers and editor for their valuable comments on this manuscript. We greatly appreciate the reviewers' input in helping to point out areas of improvement. We agree with their concerns about definitions of dependency, which have encouraged us to provide further clarity. The definitions are now updated in the revised manuscript to hopefully clarify our key conceptual innovation. To address reviewers' concern regarding the complexity of the study, we have now simplified the transition map by considering only persistent scarcity and not occasional scarcity, which gives rise to four system regimes instead of 10, connected by a simple map of transitions: NNN-SNN-SNS-SSS. This simplification of the analysis will make the typology easier to understand without compromising the key novelty of the research or the main conclusions resulting from the study. The reviewers have highlighted very useful points that helped us improve our originality by sharpening our conceptualisation, as well as description of why this approach to analysing upstream dependency is useful – tying especially to resilience literature about understanding system regime shifts. We have now substantially revised the discussion and split out a new sub-section of the discussion – 'Limitations and future work' – to address shortcomings and possible future work in more detail.

Below we first reply to the Editor's main concerns, followed by point by point response to specific questions by the editor.

Response to Editor's comments

Comment 1: The paper is of some interest to better understand the dependency of downstream subbasin areas on upstream sub-basins. It is also quite cumbersome to read, in particular because some symbols are prone to confusion (e.g. S for stress or for scarcity or for shortage?). I find the two reviews illuminating, critical and very constructive. I expect the authors to benefit from these comments and to significantly improve the manuscript. In so doing, some choices have to be made. The authors must clarify the added value of their typology (Fig 6) for better understanding basin trajectories.

Response 1: We are glad to hear that editor sees the interest in this topic and we are grateful for assistance in making it easier to read.

We have substantially revised the methodology, definitions used, as well as terminology. For the specific case of the symbol 'S', confusion arises from both 'stress' and 'shortage' being specific examples of scarcity, such that the symbol indeed stands for all three. In our revised manuscript, we have now simplified the method considerably by dropping occasional scarcity and using only scarcity (S) and no scarcity (N) to identify different scarcity categories, which has now reduced the number of variables used in the analysis.

We have now reworked the argument underlying the transition map previously shown in Figure 6 to clarify its origins and motivation. The original concept for the analysis came from the literature on resilience of socio-ecological systems, which was not sufficiently acknowledged in the previous manuscript. We regret the confusion that this omission has caused. Specifically, we use the definition from Walker et al (2004) that resilience is "*the capacity of a system to absorb disturbance and reorganize while undergoing change so as to still retain essentially the same function, structure, identity, and feedbacks*". The state of the system is defined in terms of "*state variables*", such that thresholds in those state variables are then used to define the points at which change occurs in the system function, structure, identity and feedbacks. As a short hand, we talk about a transition in system regime. Literature also talks about moving between basins of attraction and regime shifts. The former emphasizes stability and the latter tends to be associated with irreversible catastrophic failures. Our focus on transitions in system regime emphasizes simply that the system operates differently, in particular that structure, identity and feedbacks have changed (even if function may be preserved). In our revised manuscript, we are going to add this description to clearly connect the study to the resilience literature.

We agree that the original typology used in the analysis was complex and gave rise to many new terms and definitions, which were difficult to follow. The complexity arose naturally when taking into account i) both persistent and occasional scarcity and, ii) min and max of local runoff, natural discharge and actual discharge. These conditions resulted in altogether 10 system regimes (in Figure 4 of the original submission), connected by a complex map of transitions.

To reply to the reviewers' comments, we have now simplified the approach and we consider only persistent scarcity and no scarcity, leaving out occasional scarcity and using average discharge instead of min and max (as suggested by Reviewer 1). This simplification now gives rise to four system regimes (updated Figure 4), connected by a simple map of transitions: NNN-SNN-SNS-SSS (updated Figure 6) as shown below:

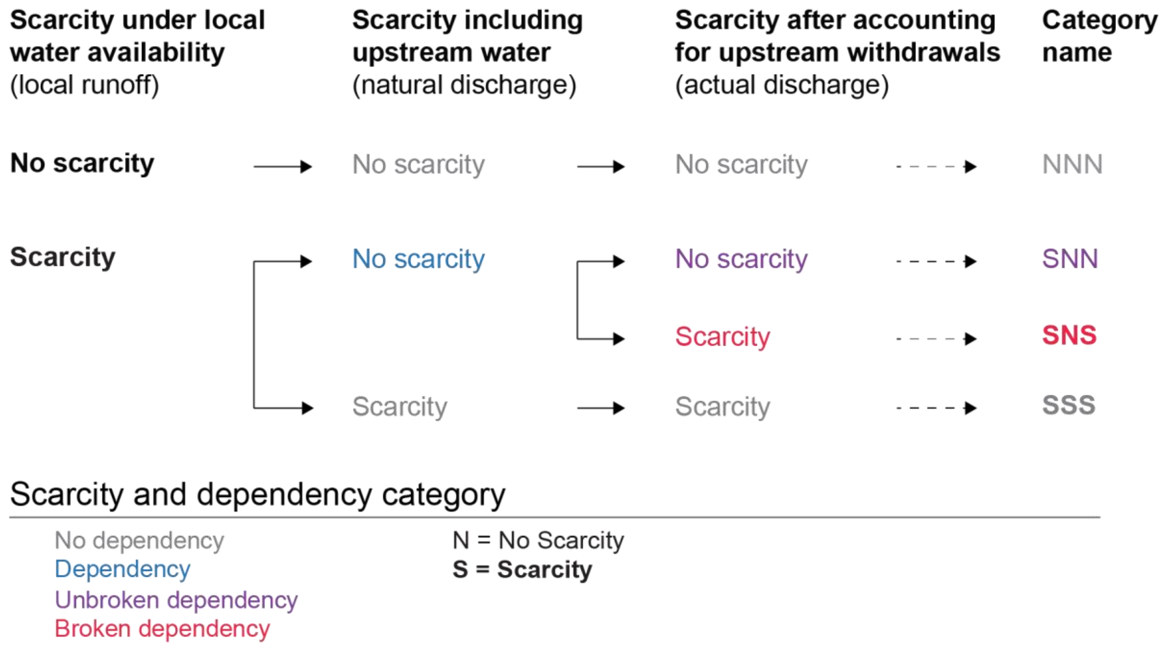

Fig 4. Definition of potential upstream water dependency categories. Dependency categories are obtained by summarizing three letter codes representing the scarcity category using runoff, natural discharge and actual discharge respectively.

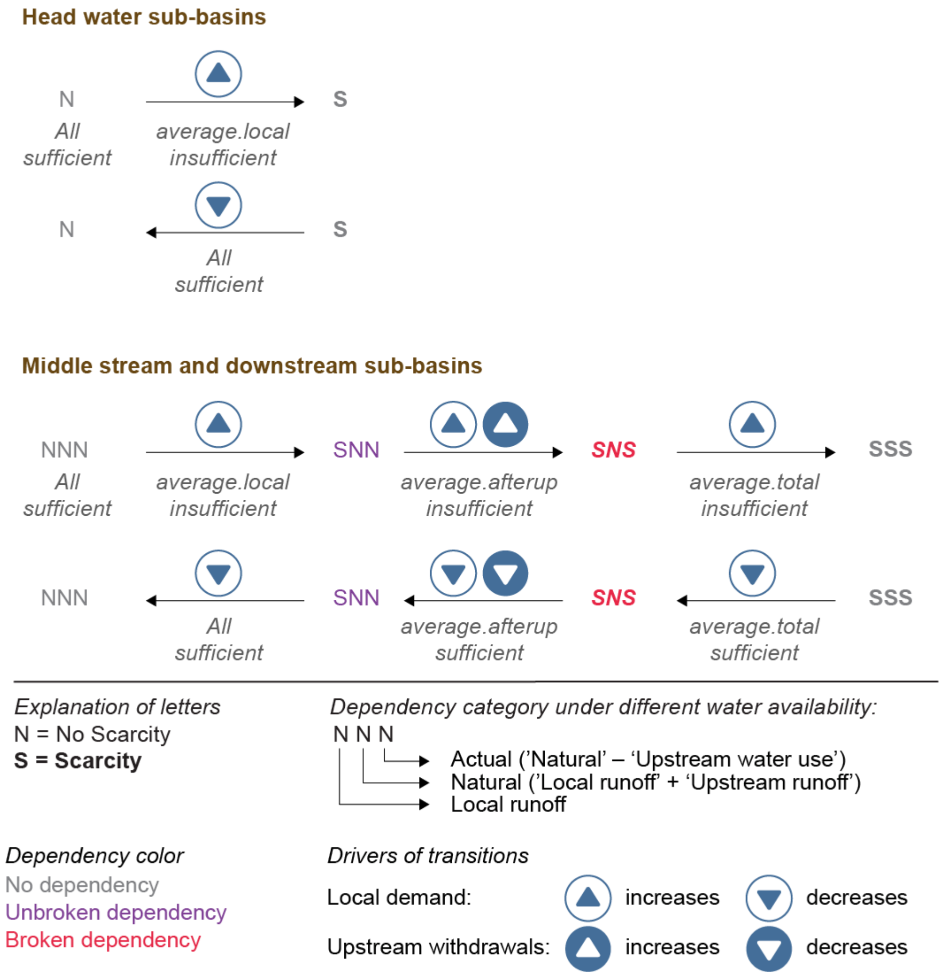

Figure 6. Typology of groups and orders of possible transitions in dependency category, as local water demand or upstream water withdrawals (WW) increase/decrease. Upstream WWs decrease the downstream water availability, while local water demand increases the pressure on available resources given subbasin area (SBA).

The literature on resilience and complex adaptive systems emphasizes that it is difficult to predict what will happen in future, but we can identify what are the transitions that might occur to prepare ourselves such that the system either avoids or manages those transitions. A transition map shows the system regimes and transitions for which a sub-basin may want to prepare. The new simplified version of the analysis specifically emphasizes 1) the importance of a hidden dependency, in which a subbasin may not be aware that they are avoiding scarcity because of upstream inflows, 2) the idea that the dependency may be interrupted not just due to upstream withdrawals, but also because of increases in local demand. These are fundamental ideas that are not widely recognized in existing literature.

Comment 2: I have one significant problem with the paper, namely that the approach is completely blue water biased and green water blind – there is no mention of green water and its importance, nor is the capacity of green water to partially substitute for blue water needs ignored. At least in the discussion section this limitation must be discussed, and the possible implications for the findings.

Response 2: We agree that the paper is completely blue water biased, and it was indeed a shortcoming not to mention green water at all. Specifically, we can consider the effect of green water availability on three crucial variables in our analysis: *avail.local, avail.total, avail.afterup*. Green water availability increases the amount of locally available water by including soil water in addition to runoff. This affects scarcity, as the need for blue water should vary in response to changing green water availability, e.g. when there is less green water available, more blue water is needed. Decreases in availability of blue water (e.g. due to upstream withdrawals) may also push a region to use more green water. This is, however, a rather complex issue and not easy to quantify.

It is, however, important to note that green water is an important part of the local water availability, but by definition, it does not affect inflows from upstream. Water is called "green water" when evapotranspiration occurs directly from rain or soil water, without runoff occurring. There is no additional effect on *avail.total*, other than that on *avail.local.* Incorporating green water into our analysis will not affect our *avail.afterup* data either, as upstream withdrawals are in principle already accounted for in the water use model (including the effects of green water availability).

The thresholds for both water shortage and stress are highly uncertain, so the effect of green water on our results is difficult to anticipate. We now explicitly mention the importance of green water in the introduction and discussion, including these points.

Comment 3: Related to this I have problems with the use of Falkenmark's per capita water availability as a measure of water scarcity (which the paper distinguishes from water stress). This is an old (1970s!) and very crude measure (with highly arbitrary thresholds of 1,700m3/cap/year for stress, and 1,000 m3/cap/year for water scarcity). It was precisely Prof. Falkenmark who later introduced the very important concept of green water, which taught us that it matters a lot whether one lives in a humid (with a lot of green water) or an arid (little green water) climate, how much blue water one needs. So fixed global threshold values do make little sense. Perhaps the paper does not need to use this flawed concept at all – omitting it may not alter the results nor the conclusions.

Response 3: We entirely agree that per capita water availability has limitations as an indicator. But we still think that both stress and shortage are useful indicators of the more general concept of scarcity. Shortage, measured by per capita water availability, captures an important intuition that sufficiency of water availability depends on population. Leaving out shortage would mean that only the stress indicator is used. This would give the impression that it is only high water use that should be avoided, not deficiency in human needs. Even though, the thresholds are arbitrary, it provides a useful balance to understand the development of water scarcity (Kummu et al. 2016), as well as illustrating the generality of our analysis framework.

We already explicitly acknowledge that these are simplistic indicators, and highlight options for future work. In our new sub-section in discussion- 'Limitations and future work', we now address this issue more explicitly.

Comment 4: A second concern that was not raised is the concept of environmental water requirements / environmental flow requirements (EFRs), which are water flows that literally run through all the SBAs and that are untouched by the riparians to safeguard the survival of aquatic ecosystems and the like. How would these feature in the typology? Atleast in the discussion section I would expect a reflection of the proposed method and how, if at all, EFRs could be included.

Response 4: We agree that EFRs are important in transboundary water management. In addition, we agree that the paper should also have explicitly mentioned environmental flow requirements. The original manuscript did mention in passing "*sustainability of water withdrawals*" but we did not elaborate the issue further. Moreover, the stress indicator includes environmental flow requirements, assuming 30% of the water is needed to satisfy the EFRs (e.g. Falkenmark et al. 2007). It is true that we do not account for EFR in a spatially disaggregated way, but global scale EFR methods could in turn be criticized for not adequately capturing on the ground conditions – our treatment of environmental flows is fit for purpose given that our focus is on the resilience-based analytical framework. In the revised manuscript, we now explicitly mention this. Further, in the new sub-section in discussion- 'Limitations and future work', we also raise this issue and suggest that the EFRs should be addressed in more detail, spatially explicitly, in possible future work on the issue.

References

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