Review of hess-2017-537
October 12, 2017

This paper is on the interconnectedness of water withdrawals and water scarcity in transboundary basins. A method is presented to formally analyze this interconnectedness. This method is subsequently applied to a global hydrological model. The results show that (in my interpretation), interconnectedness is generally low. The implication is that water scarcity is mostly a local problem, which is new to me. My overall assessment is that this paper is a solid piece of work with a new result that has changed my perspective on the management of transboundary rivers, and I thank the authors for this contribution.

I do have some comments. Most of them relate to a lack of precision in the use and application of definitions and terminology. My comments may have implications for (the presentation of) both analysis and results.

Major comments:

1. The definitions of dependency in Table 1 are not mutually exclusive, although they should be. A visual representation of the authors' definitions and my proposal to adjust them are displayed in Figure 1 below. Adjustment would probably have some consequences for the analysis, which I hope/expect are easy to incorporate. If not, one simplification would be to merge the 'dep' and 'oops' categories. Perhaps the resulting categorization is the one intended by the authors. An even simpler, and perhaps more relevant categorization is to not only merge 'dep' and 'oops', but also merge 'no dep' and 'still no dep'. Results and insights will stay the same but the presentation will be easier.

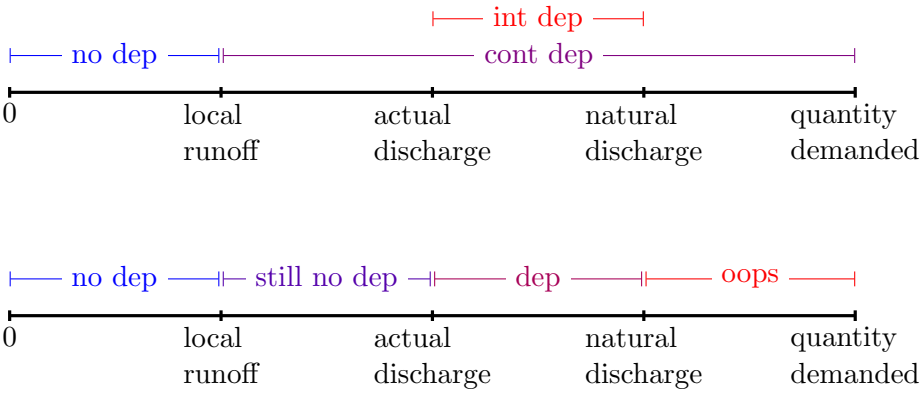

Figure 1: Dependency. Top plot according to Table 1. Bottom plot proposed by reviewer.

2. The terms used in Table 1 are not used consistently in the text.
   - The authors use the terms runoff and discharge interchangeably (even in Table 1), which is confusing.
   - The terms 'water withdrawals', 'need' and 'demand' are introduced as different concepts (L129) but they lack proper defitions. Perhaps 'demand' should be replaced by 'quantity demanded', which is something different, or 'use'.
   - In L164 the term 'discharge after upstream WW' is used where authors probably refer to 'actual discharge' from Table 1. In the same paragraph, variable '*avail.afterup*' is

introduced to reflect the same term, so that we now have three terms for the same concept. More variables are then introduced that face the same problem. This is really confusing and obscures the line of argumentation in the main text.

3. Another comment on Table 1. The order of presentation is illogical and should be reversed. Start with water stress/shortage, which you need to understand scarcity, then runoff/discharge, both of which you need to understand dependency. A more bold suggestion is the following. Since you assign variable names to some of the terms in Table 1, it would perhaps be transparent to introduce a formula for dependency (with shorter variable names), which would make it much easier to understand the definitions. For example, if q_i denotes water use in sub-basin i, e_i denotes local runoff, and P_i denotes the set of i's predecessors (i.e. sub-basins strictly upstream of i) we can write:

   - $\hat{e}_i = e_i + \sum_{j \in P_i}(e_j)$ as the total water available after upstream withdrawal;
   - $\bar{e}_i = e_i + \sum_{j \in P_i}(e_j - q_j)$ as the total water available after upstream withdrawal.

   Subsequently, when we denote x_i as the measure of water needed to avoid water scarcity (be it from stress or shortage), we have:

   - $x_i \leq e_i \quad \rightarrow$ no dependency;
   - $e_i < x_i \leq \hat{e}_i \quad \rightarrow$ still no dependency (see bottom plot of Figure 1);
   - $\hat{e}_i < x_i \leq \bar{e}_i \quad \rightarrow$ dependency.

   These formalizations of the definitions may also assist in discussing e.g. the typology of dependence categories in Section 2.2.4. I realize that I might be pushing this point too far. If this is the case then at least sharpen and streamline the definitions and terms used in the paper in a consistent way.

4. While you mention treaties on transboundary river water in the discussion, they seem to be ignored in the analysis. Dependencies may not be as severe when they are mitigated by treaties that provide security of continuous upstream inflow. Such treaties may even feature well-designed (flexible) sharing rules able to mitigate the impacts of e.g. climate change. We could even have reversed dependency when a treaty stipulates that local runoff should be shared with downstream riparians. In this case, even if local runoff would be sufficient to satisfy demand, the upstream country would be dependent on the downstream country(/-ies). An example would be Ethiopia's position in the Blue Nile basin.

Minor comments:

1. L53: Please define 'sub-basin' upon first use.
2. L53: 'experiences' → 'may experience'.
3. L55: 'Parts of basins' do not 'realise' much.
4. Figure 1 duplicates Table 1 and can be removed.
5. L131: The 30yr period is introduced here without any explanation. Why? And how?
6. L155–159: Are return flows accounted for?
7. L165–166: What if an SBA has multiple downstream SBAs? Possibility of double-counting.
8. L198–199: What happened to 'persistent' and 'occasional' from Table 1?

9. Figure 4: the color code categorizes SSS as featuring 'no dependency' which seems incorrect. In general, I would say that any setting where there is scarcity under actual discharge (i.e. after upstream water use) should be coded as 'intervened dependency', since the upstream water use exacerbates the downstream scarcity. I realize that the authors would probably say that this is a case of 'no dependency' because there would also be scarcity withouth upstream water use, but that is a semantic argument since scarcity is coded here as a binary variable.

10. The term 'ordering' and the arrows used in Figures 6 and 8 suggest that sub-basins can only develop in one direction, namely from good (NNN) to bad (SSS). You may want to present a more nuanced story, explaining under what circumstances this tendency may be reversed.

11. Figure 6 is presenting too much at the same time. From the text I understand that there is a natural ordering, but I do not see the added value of presenting all possible pathways through these orders. Same of course for Figure 8. Can you somehow summarize this in an easier way?

12. The numbers in Table 3 surprise me. To me, the category 'intervened dependency' is the most relevant since in both other categories there is not really a scarcity problem, right? Less than 2% are in this category. Oh wait, you include SSS in the 'no dependency' categoy, see my comment 9. If I include this, the number becomes 11%. This is still a low percentage in light of (my interpretation) of the literature on water scarcity. It implies that water scarcity is mostly a local problem so that not much can be expected from transboundary cooperation.

13. I find that Section 3.2 is very speculative and could perhaps be shortened.