Referee report by Maik Heistermann, 2nd iteration

Introduction

I would like to thank Dr. Rudell for his extensive responses to the referee comments, and for the effort he has spent on revising the manuscript.

Yet, I am not convinced by the presented line of arguments, maybe even less than before. I find the entire article hard to follow, and I am concerned that it will leave the HESS audience puzzled. But maybe I was just the wrong guy for this review, lacking insight into the issues relevant in the US? Be that as it may, I will present my concerns, and I am more than glad the editor is the one who will have to come up with a decision.

The author raises two main questions (p. 3, ll. 3 ff.): *"What is a proper census data model for consumptive water use?"* and *"when can census water withdrawal data replace consumptive use data?"* Both questions can be (and are), in principle, addressed independently, namely in sections 2 and 3 of the revised manuscript.

What is a proper census data model for consumptive water use?

I find it difficult to find the actual message in this section. The author spends a much effort to establish the concept of *"simple net consumptive water use"* (SNCU) which is, in plain words, an idealised case in which water withdrawal, consumption, and return flow of one user take all place at a well defined point in space and time. The author then spends even more effort to establish the fact that most use cases are more complicated than SNCU. From the perspective of hydrology, but also from the perspective of common sense, these cases are self-evident. The author admits that himself on p. 5, l. 24: *"The complications [...] should not surprise us"*. Indeed, they are in the very nature of water use, in particular for those cases in which consumption matters. I just do not see how the introduction of the SNCU concept helps us to conceptualize water consumption in a census context. Instead, the idea of a water balance for a well-defined control volume (let this be a reservoir, a watershed, or a county) is perfectly suited - across scales - in order to represent different types of problems - and it is intuitive. It might be desirable to resolve all the different flows inside a control volume in order to deduce an overall balance. But from section 2, I do not see a feasible concept on how that could be achieved in a national census. Instead, p. 5 ll. 24-37 leaves us with the diffuse notion of a data model that should represent a *"network by which water is moved, stored, used, transferred between users, transformed in quality, and (sometimes) returned to the original water source- but just as often returned to a different source"* and which should thus "*explicitly treat spatiotemporal scale, production of water, transfer of water, pass-through of water to other users, transformation of water quality, return flows to water stocks other than the source (i.e. negative consumption), storage, and delayed flow and use."* Without further foundation, this concept will literally remain a *pipedream*. It is far more important to understand the key processes that dominate the water balance at a specific management scale. And *maybe* a census is just not the perfect tool to capture some of the key processes, specifically when it comes to the systems where consumption matters (what about monitoring, modelling, and remote sensing?). Maybe a census should in fact continue to focus on what it can do and has done best: focussing on water withdrawal!

When can census water withdrawal data replace consumptive use data?

I have been very critical about that section in my first review, and I do not see much to change my opinion. The author starts by arguing that *"unless U is very small (U < 0.1), C and W are guaranteed to be on the same order of magnitude"*. In the original version of the manuscript, the author had stated that *"similar orders of magnitude is decent data quality when you consider that our current uncertainty regarding U for most water users is also order-of-magnitude."* That sentence has been dropped in the revised version, but the implication remains the same. And if the uncertainty of U is order-of-magnitude, how can you be sure that W and C will be the same order-of-magnitude?

The author then continues to list cases in which withdrawal data is informative, e.g. to compute the impact on fish mortality related to water withdrawal (intake), to compute the withdrawal-to-availability ratio, the costs of infrastructure for water withdrawal, and the influence of water withdrawal on the use rates as a result from pricing. I had already extensively commented on that section. But in the previous sentences, I have tried to put this more clearly: of course, water withdrawal data is enough if you investigate issues that are exclusively related to water withdrawal! No one ever claimed that fish mortality due to water intake was related to water consumption, or that we needed consumption data for dimensioning intake infrastructure! In my opinion, no argument in section 3 really holds, except the points in which the author actually emphasizes the importance of water consumption, e.g. p. 6, ll. 33-34: *"Consumption is a water supply risk factor at aggregated scales, and it contributes indirectly to the availability of water to support withdrawal."* Yes, exactly. I find it particularly strange that while the author acknowledges the relevance of regional (i.e. basin scale) analysis, he implies that *„classical hydrologists and water resource engineers tend to work at fine spatiotemporal scales and on problems that require highly precise but localized water balance data"* (p. 2, ll. 24ff.). That is, in my opinion, a gross misconception of hydrological science.

So is the level of disagreement just a sign that a discussion is needed, and that, consequently, this opinion paper is needed?

My answer is "no". Honestly, I don't see a real subject for discussion: neither the SNCU concept, nor the proposal for a future consumption census, nor the notion that we should replace consumption data by withdrawal data - in cases for which no one ever called for consumption data. To me, the problem with this manuscript becomes apparent in the conclusions, when the author states that *„in some of the simplest special cases withdrawal based numbers are approximately sufficient [...]"* (p. 7, l. 34). Four sentences later, he states that *"surveys of water withdrawal are feasible and they approximately address many of the most important economic, socio-hydrological, and CNH problems […]"* (p. 7, ll. 39 ff.). So which one is it? *"Some of the simplest"* or *"many of the most important"* ones?

In my opinion, this is a phantom-discussion, and I do not see merit for the community - be it hydrologists or other users of water-related information - to lead it. Please understand that I really do not want to sound cynical or condescending. As I said above, I may have just failed to understand the actual problem at hand. Maybe a reviewer from the US can capture the issue better?

The only message I agree with (and which I have already agreed with in my first review) is that simply guessing values for U pretends a level of knowledge that does not exist. That is a problem for many large scale hydrological studies. It is important to address that knowledge gap instead of keeping on pretending. But is that really the point of these ten pages of the manuscript? I don't think so.

Some other issues

I have to say that I have a series of other issues that I disagree with, for example the alleged contradiction between different defintions of a water balance as pointed out in the first paragraph (p. 1, ll. 17-27).

I also disagree with the implication that *"classical hydrologists"* have different requirements to data quality than other users interested in water-related information, or in other words, that the latter were happy with less reliable estimates at larger scales. The role of hydrologists is to understand the water balance and the role of different components of the water cycle at different scales, and to convey that understanding to those concerned with making decisions. I cannot imagine a reason why an "application" of hydrological knowledge should be happy with anything less than the state of the art. In the same way, I disagree that *"classical hydrologists and water resource engineers tend to work at fine spatiotemporal scales [...]"* (p. 2, l. 24) while *"economists, policymakers, sociologists, industrial engineers, and researchers of broader Coupled Natural-Human systems problems [...]"* are supposed to be interested in *"meso scales and regional socio-political boundaries"* (p. 2, ll. 26-28). If we agree that "water resources management" implies activities to match different demands with water availability, it should be obvious that neither part of that management process is strictly local, but always integrates over a geographical region with regard to both availability and demand. In that context, I am still missing the specific case studies which I had asked for in order to clarify what kind of problems the author actually wants to address.

I should not elaborate further on points I disagree with, in order to not distract from the main points I made above. So I will stop right here.

Conclusions

I do not get the point of this paper, and I disagree with a lot of the statements made. I think that the paper is more confusing than helpful to the audience of HESS. Having said that, I am happy with whatever decision the editor will make. Thanks for involving me in this discussion, although I am increasingly under the impression that I am not fully qualified.