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Interactive comment on "When is water withdrawal data enough?" *by* Benjamin L. Ruddell

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Overview

In his opinion paper *"When is water withdrawal data enough?"*, Benjamin L. Ruddell elaborates why, in many cases, census-based withdrawal data should be preferred as a *"policy and management index"* over consumptive water use estimates, which he consideres as inherently uncertain.

In section 1, the author outlines the fundamental lack of reliable and representative observations/measurements/censuses on consumptive water use in the US. In section 2, he discusses the difficulties in obtaining such data, mainly caused by the complexity of return flows and losses in different water use cases. He correctly notes that the consumptive use coefficient *U*, i.e. the ratio between consumptive use *C* and withdrawal

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W, depends on scale, being e.g. close to 0 at the global scale (due to the nature of the global water cycle), and having high values around 0.9 for intense usage chains in water-scarce basins, involving particularly irrigated agriculture. On that basis, the author establishes his main case in section 3, where he argues that we *"may be well served by water withdrawal data"* instead of insisting on the explicit consideration of consumptive water use. It is that section (only followed by a summarizing conclusions section 4) on which I would like to focus in the following.

Is withdrawal data enough...to do what, exactly?!

In general, I agree that it is sometimes (maybe even often) preferable to transparently admit ignorance instead of expert-guessing numbers. I am wondering, though, whether and how that thought is helpful to those people on the ground who are confronted with the need to come up with management decisions based on imperfect information. I take it from the paper – or is it just my hope? – that it is those managers (planners, stakeholders) who are dear to the author's heart – although he mentions quite a lot the "natural human system researchers"... whose role and science questions remain, in the context of this manuscript, mostly unclear to me.

What is missing, though, is the specification and analysis of management contexts for which the author shows that his claim is valid. As to that end, section 3 comes as close as it gets - and it is the most interesting part of the manuscript in terms of potential for controversy (I was told that the review should stimulate debate...). The author raises five points which are to support his opinion that *"the water withdrawal numbers we already possess provide a substantial portion of the information we need to assess the human water economy and its effects on the natural environment."* In the following, I will reiterate and discuss these points.

1. *"Withdrawal conservatively bounds consumption"*, or, in other words, water managers can typically assume *C* to not exceed *W*. While it is hard to disagree on

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that statement, I am wondering under which circumstances that information is actually helpful to support management decisions, and I would hope to see some evidence or corresponding best-practice cases. I am afraid, however, that in water scarce regions - where the issue of consumption matters most - the assumption of withdrawal being equal to consumption can make decisions about water allocation to different users or sectors fairly impossible. The rest of the paragraph about point no1 should also be supported by evidence: it is true, by definition, that *C* and *W* will have the same order of magnitude when *U > 0.1* - but will water managers find *"same order of magnitude"* a sufficient criterion for decision making? Are there surveys on stakeholder information requirements to support that notion? And, yes, the thermoelectric and industrial sectors may have values of *U > 0.1*, but what about irrigated agriculture – which is the most important water consuming sector due to both large withdrawals *and* low values of *U*. Apart from those concerns, I feel like the line of arguments is flawed by an intrinsic contradiction: The author states that for *U > 0.1*, *W* and *C* may be assumed to have the same order of magnitude, which might be sufficient given the fact that the uncertainty of *U "is also order-of-magnitude"* itself. So, based on that statement, how can a manager be sure that *W* and *C* are in the same order of magnitude? Depending on the water using sectors in the region, they can't - which is why they usually need to come up with their own estimates.

2. *"We have some spatially and temporally explicit W data, but not C data"*: This point is closely related to the first one. It says that spatially and temporally explicit withdrawal data is of comparatively high quality and its resolution in space and time is by no means matched by data on consumption. Computing spatiotemporal patterns of *C* from *W*, based on unrepresentative estimates of *U*, can thus be misleading, assuming that *U* is varying non-uniformly over magnitudes. And I agree that computing spatial patterns of *C*, based on unknown *U*, pretends a level of knowledge that does not exist. However, the reverse conclusion is not

valid, either! You cannot conclude that the spatial pattern of *W* per se is a helpful management proxy. In general, I am curious how large scale patterns of either *W* or *C* may exactly be helpful to water resources planners. Again, local decision makers will typically know better than to use uncertain off-the-shelve estimates of *U*.

3. *"Withdrawal is a good index of water use impact and risk"*: The author only provides one example to support that claim which is that *"fish mortality [caused by water intake of whatever water-related infrastructure or facility] is directly proportionate to withdrawal"*. That is a *very* specific impact of water use the generalisation of which is certainly unwarranted! The following statement of water supply risk being proportionate to withdrawals is, in this context, a text-book example of circular reasoning (withdrawal data is enough because withdrawal data is enough), and will probably not hold for basin-scale water resources planning – for the exact reason that consumption may be the decisive control for downstream water availability. Again, it would be helpful to see some evidence to support this point.

4. *"Withdrawal rates drives infrastructure capacity and fixed cost"*: That statement is true, but reflects only a very limited scope of water resources management. Management of a scarce resource is not only a matter of infrastructure. Furthermore, external (or environmental costs) are typically related to consumption (in contrast to the authors statements pertaining to point 3).

5. *"Marginal withdrawal pricing influences water user behavior"*: Again, that statement is true, but at the same time it is both incomplete *and* irrelevant. There is a substantial body of literature showing that water use behavior is influenced by many more factors than withdrawal pricing (see e.g. Bosworth et al. 2002). Apart from that, I do not really see how this point is relevant to the topic: even if pricing *were* the only determinant of withdrawal, how does that help us to represent the

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effect of consumptive use? How does the idea of “water withdrawal depends on pricing” pertain to the notion of “water withdrawal is a good management proxy”? The line of argument appears to end in the middle of nowhere...

Altogether, I feel that I cannot agree on any of the five points raised by the author. Some are mere speculation, some represent a very narrow or even misleading perspective, and some are just off topic. The loss of water at scales between plot and basin, through consumptive use, is a reality particularly in water scarce regions! Ignoring that reality is just as little helpful as pretending a comprehensive knowledge of consumptive use coefficients at any scale, as criticised by the author.

Hydrological and hydrometeorological information will, in the foreseeable future, remain uncertain and limited - irrespective of the fact that scientific progress may improve the overall situation by advances in hydrological and hydrometeorological monitoring, modelling and assimilation techniques. And as the underlying data will never be immaculate, water resources planners will never be released from the need to critically appraise their quality and helpfulness. The key to this appraisal is a clear definition of the management scale. That management scale is typically neither global nor continental nor national, and it depends on various factors such as the specific composition of water using sectors, the dominant hydrological processes, and the administrative and governance structures. While national censuses may supply data, its evaluation and procedural usage has to be taken care of by water managers and stakeholders. In that context, the role of simulation models, in their most abstract sense representing a combination of available data and our notion (expert knowledge) of a system, is widely ignored by the author. As always, George Box’ aphorism that *“all models are wrong, but some are useful”* is right at the heart of the problem: I cannot think of many management contexts in which global or continental scale models of water consumption and consumptive use coefficients could be substantially useful. For local and regional contexts, however, a model may be *essential* - in the sense that it represents the essence

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of our understanding of the managed system (in terms of observations and dominant processes) and its relevance for solving the management challenge. Let me take the opportunity to warmly recommend, in that regard, Chris Perry's paper on the ABCDE and F of water resources management (Perry, 2013).

Other remarks beyond section 3

"Simple net consumptive water use" and other terminology

With the *"simple net consumptive water use"*, the author introduces new terminology (new at least to me) without necessity. I don't see any need to discard the idea of a well defined control volume for any kind of water balance calculations. The notion of a control volume can be applied to any (management) scale. And the attribute "simple" is just as misleading as it is unnecessary. Likewise, the author repeatedly emphasizes the term "Coupled Natural-Human System" without doing much more than stating the obvious: that water resources management is of course at the interface of natural and socio-economic systems (besides, the original reference of Liu et al. (2007) in Science was about properties of specific coupled natural-human systems, not about coining a new term of *the* "Coupled Natural-Human System" in general).

Situation in the US

While it is of course justified to focus on the situation in the US - with its very unique level of water census collections - the author should put this situation into perspective with other countries that face dramatic water scarcity issues, but which aren't anywhere near the data standards currently prevailing in the US.

"Consumptive water use becomes less important, and more overestimated, at macro scales"

That statement (p. 4, l. 27) is bold and fuzzy at the same time. What is meant by "less important"? "Importance" (in terms of "relevance") of consumptive water use is a

matter of water availability, a fact that is often ignored also by water footprint community (Heistermann, 2018, section 4, although I admit that's another debate), and I'd like to see evidence to support that statement. The fact that *U* converges to *0* at the global scale does not imply that it continuously decreases with increasing scale. In specific (semi-arid) climates with intense and widespread irrigation, *U* might reach its maximum value at the basin scale due to cumulative effects - just take the Aral Sea basin as an example.

Future water use censuses

In section 4 (p. 7, ll. 4 ff.), the author elaborates briefly on the requirements to future water use censuses. He expands a wish list that includes *"water quality at withdrawal and return, seasonal timing, specific stream segment and aquifer sources, multiple and specific stocks, accurate attribution of use to legally responsible human agents, multiple uses of a withdrawal, multiple processes, and return flow in addition to simple withdrawals"*. In the following sentence, the author claims that his *"paper [...] provides guidance on what measurements would be needed to nationally survey consumptive uses of water at census scales"*. However, I cannot really find that kind of guidance in the manuscript, at least not at a level that actually adds new insight. I also think that such a wish list and requirements to future census are not really the subject of the paper (and shouldn't be, either).

Summary and conclusions

While I understand that this is not a review in a strict sense, I have to admit that I do not find the present manuscript very convincing. Given my above reservations on large parts of the manuscript, I am wondering what actually remains. Right now, there is too much speculation, not enough evidence, and a discussion that is only weakly linked to specific management challenges. Altogether, I am not at all convinced of the validity of the author's main claim - that water withdrawal should be preferred as a "policy and management index". That notion can be as misleading and harmful as the application

of invalid or uncertain estimates of consumption. So what remains is the fuzzy notion that we lack data and understanding and should address that lack in the future... - is *that* really enough?

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