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Hydrology and Earth System Sciences
Discussions

Interactive comment on "On inclusion of water resource management in Earth System models – Part 2: Representation of water supply and allocation and opportunities for improved modeling" *by* A. Nazemi and H. S. Wheater

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We greatly appreciate Anonymous Reviewer #1 for their positive, constructive and thoughtful comments, which led to substantial improvements in the revised version of our manuscript. In the following, the issues raised are addressed point-by-point in the order they are asked. The reviewer's comments are numbered and our reply is immediately below each comment. Please note that our reply accompanied with a draft revision, which is attached to this discussion as a supplementary material.

1- Although well surveyed, I found some errors in text which misrepresent some formulations or concepts of models (please see below for detail). It is quite challenging for non-developers, if not impossible, to describe models perfectly by only literature review. Here I would like to suggest the authors to make a simple survey of models: contact the main developers of major models and ask to check whether the descriptions on their models are correct.

Many thanks for your suggestion and heads-up on some of the misrepresentations in the submitted manuscript. We corrected all points you highlighted and contacted the main developers to take all reasonable steps to ensure accurate representation of the scheme. In particular, we contacted Drs. Hanasaki, Oki, Haddeland and Voisin. Some of them have already came back to us and we hope to have other responses before we submit the final revised manuscript to HESS.

2- I found considerable overlaps in contents within this article. It could be attributed to its structure. Actually, the titles of chapters 2-4 read "Available representations of water sources in large-scale models", "Available representation of water allocation in large-scale models", and "Current large-scale modeling applications". In each section, both the reservoir operation and groundwater models are discussed citing the same papers repeatedly. I wonder the overlaps might be drastically reduced if the authors reorganize them into two sections, the reservoir operation and groundwater models.

Many thanks for your comment. We thought a lot on this and tried different variations to organize the content. This was not as easy task and took us a long time, considering the fact that we needed to somehow link the content of Part 1 and Part 2 papers together and keep the same narration style in both papers. The main objective in both papers is to breakdown different aspects of water resource management and describes briefly how various elements are included in large-scale models. This is mainly to inform non-familiar readers about how the big problem of representing water resource management can be divided in to rather stand-alone pieces, which can be then represented through a specific suite of algorithms. In the Part 1 paper, we defined water

resource management as an integration of water demand with water supply and allocation, in which water allocation links water demand and supply together. Therefore in this paper, we first breakdown water sources and then focused on algorithms available for representing water allocation. As the main papers cited in this paper include both water supply and allocation (and some also water demand, which are cited in Part 1 paper), such overlaps as you indicated become somehow unavoidable, even if we consider a section solely on surface water and another one only to groundwater supply and allocation. This is due to the fact that most of the cited papers include discussion on both groundwater and surface water. We believe that organizing our discussion in the present form can help non-familiar readers to understand how a specific paper deals with various issues around the representation of water resource management. Indeed, further consultation with original papers is required for further understanding of the algorithms reviewed in this review.

3- The objective of review is a bit unclear. As the title of this paper says, the authors may intend to make use of this review to develop an ESM including human activities based on an atmospheric model and conduct online simulations to study land-atmosphere interactions. If this is the case, the paper in the current form might pay too much attention to the application of offline simulations and too less to the problems inherent to ESMs and online simulation (low spatial resolution, biases in atmospheric and hydrological variables, small signal to noise ratio due to large internal variation, etc).

Many thanks for your comment. We noticed that both offline and online effects of water resource management are important and relevant to ESMs, in particular GHMs and LSMs. We do agree that both papers have more discussion related to offline applications; however, this is wholly a reflection of the current literature. In fact, most of the available studies are mainly offline (with exception of irrigation that discussed in Part 1 paper) and we are not aware of any study that incorporates ESMs with full consideration of water resource management for online simulations at the regional or global scale. Nonetheless, we suggest that the ultimate need is to move towards

online simulations in a way described in Figure 1. According to your comment, now we have added a substantial discussion on issues around online modelling in Section 5, in addition to incorporating several new references. Please see the supplementary draft revisions (lines 580 to 696).

4- The authors partly included discussion on water security in this paper, which confused me. Water security is largely a matter of socio-economic change, policy, institution, and governance, which is out of the scope of this paper. Above all, the review on this topic is insufficient.

Many thanks for your comment. As you truly noted in your comments, our main aim in this paper is to discuss how to represent water supply and water allocation in large-scale models. We highlighted three main practical reasons for this, including quantifying the effect of human-water interactions on terrestrial water cycle and climate as well as addressing the water security concerns at regional and global scales. We did not intend to provide a comprehensive review on water security assessment. Rather, we would like to justify the need of representing water resource management in large-scale models based on emerging issues around water security assessment. From a broader perspective, water resource management and water security are rather interconnected as both are influenced by of socio-economic change, policy, institution, and governance. However as you noted these issues are beyond the scope of this paper. As a result, we modified the discussion related to water security assessment according to your comment to avoid further confusion. Please see the supplementary draft revisions attached (lines 87 to 108 and 484 to 557).

5- P8303, L2, "focus mainly on measuring the annual difference between natural water availability and projected demand as an indicator of water scarcity": I'm wondering this part is of relevance to this review article. This paper basically focuses on the representation of human activities in numerical models rather than its application to water resources assessments. Indeed, dozens of high quality papers have been published on global water scarcity and security, which is largely missing in this article.

Many thanks for your comment. This comment is closely related to the previous comment and comment #14. As we mentioned above we revised the discussion related to water security assessment to keep the focus on the main objective of this paper, which as you mentioned is representation of human water management in large-scale models. Please see the supplementary draft revisions attached (lines 87 to 108)

6- P8304, L19, "10% of the annual runoff": The number may be too small. 8000km3 of storage volume must be accounts for 20% of global annual runoff (approximately 40000 km3/yr).

Many thanks for the heads-up on this. We referred to the original articles as well as Gleick (2000) and you are absolutely right. We highly appreciate your careful reading of our paper. We revised the paper accordingly. Please see the supplementary draft revisions attached (lines 147 to 150).

7- P 8304, L16, "Available representations of water sources in large-scale models": The section includes a subsection "groundwater", while it excludes "surface water". I understand that river and lakes are "natural" processes and do not include explicit human activity, but these are the most fundamental water sources.

Many thanks for the comment. We included a brief discussion about the natural lakes in Section 2.1. Accordingly, Section 2.1 is now titled as "Lakes and reservoirs". Please see the supplementary draft revisions attached (lines 142 to 186). Please note that we already have a brief discussion in Section 2.2 related to river flow abstraction. Please see the supplementary draft revisions attached (lines 193 to 215).

8- P8307, L24, "often groundwater availability is assumed as unlimited local source": Please carefully revisit the original article. For example, Rost et al. (2008) devised a technical term "Nonlocal and nonrenewable blue water (NNBW)" and avoided assuming groundwater is unlimited source.

Many thanks for your comment. You are absolutely right as she did advise that in

the 2008 paper. We revised this in the paper. However, from a numerical modelling perspective, these are rather similar. Please see our discussion related to comment #13. Please see the supplementary draft revisions attached (lines 235 to 239 and Table 1 in page 55).

9- P8308, L1, "Wada et al. (2014)": Döll et al. (2014) should be mentioned here as well.

Many thanks for introducing this paper to us. We were not aware of this paper and read it with a lot of interest. Accordingly, we included a brief discussion on this paper. Please see the supplementary draft revisions attached (lines 246 to 249). We also used the reference in the draft revisions in some other place where applicable.

10- P8310, L14, "Hanasaki et al. (2006) assumed that large reservoirs can supply all downstream demands within 1100km and with lower elevation": When the model of Hanasaki et al. (2006) estimates the monthly release of individual reservoirs, it only uses the information of water demand in downstream. Released water is not always sufficient to "supply the all downstream demands". This kind of details might be difficult to learn from literature review. Voluntary checking by model developers would substantially improve the accuracy.

Many thanks for your comment. We exactly meant what you indicated but we poorly wrote it. What we meant was large reservoirs consider supplying demands that are located within 1100 km and lower elevation. This does not mean that they can fully supply them. Please see the supplementary draft revisions attached (lines 309 to 310 and Table 1 in page 55).

11- P8310, L28, "Irrigation has often been given the highest priority": At least, Hanasaki et al. (2008a) gave priority to domestic and industrial water over irrigation in abstraction of water from river.

Many thanks for your very careful reading and comment. We checked the article and

you are completely right: “The anthropogenic water withdrawal module withdraws the amount of consumptive water use for domestic, industrial, and agricultural purposes from river channels in that order at each simulation grid cell”. However later on, it indicates that “Withdrawn irrigation water was added to the soil moisture in irrigated areas, and domestic and industrial waters were removed from the system”. We mistakenly took this statement as an indication of priority in the water allocation. Please see the supplementary draft revisions attached (lines 320 to 322 and Table 1 in page 55).

12- P8311, L3, "the deficit is typically shared proportionately to the demands": Because of the reason shown above, the proportion among water sectors is not shared at least in Hanasaki et al. (2008a, 2013a).

Many thanks for the heads-up on this. We corrected this in the paper.

13- P8312, L7, "If the groundwater is considered as an infinite local source (Rost et al. 2008; Hanasaki et al. 2010: : :)": This is not the case for Rost et al. (2008) and Hanasaki et al. (2010). What they assumed infinite was Nonlocal Nonrenewable Blue Water (NNBW) which indicates water sources that are not explicitly represented in their models, namely, water diversion, glacier melting, desalination, and others.

Yes, you are completely right. We corrected this in the revisions. However, from modelling perspective, assuming groundwater or NNBW sources as infinite are quite similar. In fact, (1) both do not consider water shortage; (2) both bring water from outside the modelling domain and (3) the estimation of water withdrawal from either sources wholly depends on how water demand and water supply are estimated at the grid scale. As a result, the errors from these estimations can wholly propagate in to estimation of groundwater or NNBW withdrawals. Please see the supplementary draft revisions attached (lines 353 to 359).

14- P8319, L10, "Impacts assessment and water security studies": It is not very clear what kind of impacts on what are discussed here. For example, the reservoirs influence not only the surface water/energy budget, but also sedimentation (e.g. Syvitsky et al.,

2005), ecosystem (Vörösmarty et al., 2010). These issues are not mentioned here.

Many thanks for your comment. In order to avoid further confusions and focus only on the main objective of the paper, we extensively revised this section according to you comment. Please see the supplementary draft revisions attached (lines 87 to 108 and 484 to 557).

15- P8322, L23, "Computational complexities": Personally, I am not very much convinced by this sub-section. It is quite subjective to discuss what is computationally "complex" or "expensive". I am wondering whether this subsection is necessary.

Many thanks for your comment. We removed this subsection; but we modified and incorporated some of the discussion in Section 5.2, which is now dedicated to problems related to online simulations and including groundwater. Please see the supplementary draft revisions attached (lines 671 to 681).

16- P8326, L23, "implement the operation at finer temporal resolution (sub-hourly to few hours rather than daily and monthly)": I am wondering why such finer temporal resolution is needed. The atmospheric processes and reservoirs are primarily connected by the water surface of reservoirs. More specifically, the area and temperature of surface water, if I understood correctly. In most cases, both of them vary slowly, hence the reservoir operation in online modeling might not request such a fine temporal resolution. What I think more important here is that the river inflow to reservoirs by online simulation includes substantial bias compared to offline ones, particularly when it is not assimilated. A fundamental problem here seems to be how to represent reservoirs in a robust manner while the inflow simulation is highly unreliable. An old saying goes "garbage in garbage out".

You are right. This was inaccurate in our discussion and we highly appreciate your careful review of our paper. We revised the discussion accordingly. Please see the supplementary draft revisions attached (lines 756 to 769).

17- Table 1, "Demand-supply dependency": upstream reads downstream.

Many thanks for your comment. We revised the column related to “supply-demand dependency”. Please see the supplementary draft revisions attached (Table 1, page 55).

18- Table 2, "Host model": H07 reads H08 (Hanasaki et al. 2008), and PCR-GLOBW reads PCR-GLOBWB (PCRaster Global Water Balance).

Many thanks for your comment. We corrected these typos. Please see the supplementary draft revisions attached (Table 2, page 56).

19- Table 2 "Discharge data": Does it show the validation data used in earlier studies? It is a bit confusing because many of studies simulated discharge by their models.

Many thanks for your comment. Yes they are mainly for validation except for Wu and Chen (2012) that we indicated that. We added “Validation” before discharge data to avoid confusion. Please see the supplementary draft revisions attached (Table 2, page 56).

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/11/C4904/2014/hessd-11-C4904-2014-supplement.pdf

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