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Discussions

Interactive comment on "Recasting catchment water balance for water allocation between human and environmental purposes" *by* S. Zhou et al.

Anonymous Referee #2

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Summary of the paper: The authors argue that while previous studies have focused on partitioning catchment water balances between ET and runoff – few studies have tried to further partition ET between the component that is beneficial to humans and that which is purely for natural ecosystems. They want to do this for the MDB over a 100 year period. The authors have three pieces of data:

1. Convention catchment balances from 1900-2010 which give them P, ET, and Runoff. Since non-agricultural water uses are small – they only need to concentrate on Crop ET.
2. They have estimates at GPP (in gC/m2/year) at a coarser scale from 2000-2010 from satellite data

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3. They have area ratios of cropland, grassland and land under native vegetation from 1900-2010.

Since historical ET by land use type is not directly observable, but GPP is observable for recent periods, the authors make some assumptions about ET/unit area (I think) for each land use type and then use the relationship between GPP and ET to check if the modelled GPP estimates match those obtained from satellite imagery.

Concerns over framing: I have concerns over the framing of this paper, specifically the "so-what" aspect of this work.

1. The objective of this paper seems to be to partition ET into human and ecological ET. The authors conclude that humans have been capturing a greater and greater share of ET over time until recently, when this trend has begun to reverse.

   Questions about whether humans are capturing too much have been raised by many previous scholars – particularly ecologists who asked what fraction of primary production was being captured by humans (see work by Stuart Pimm, Peter Vitousek in the 1990s). In the ecologists' case they were concerned by how much was left for other species and the implications for the earth's carrying capacity.

   In this paper, my concern is that it's not clear to me why this analysis is interesting and what the analysis contributes beyond what we already know from the land ratios. Initially, I thought that perhaps the ET ratios would turn out to be wildly different from the land ratios (which would be interesting). But Figure 3 and Figure 4 suggest that both the ET and GPP more or less follow the land area ratios.

2. The authors argue in several places that there is some kind of an "ideal" ET ratio. I find this unconvincing. Most decisions about how much water to leave for environmental uses are made with the objective of sustaining either biodiversity or ecosystem services. To justify this analysis – the authors would need to make

HESSD
12, C136–C140, 2015

the case that the ET ratio is a magical number that is not perfectly correlated with simpler, easily available indices like land area ratio AND yet it is also correlated to something societies care about.

The paper does not make the case for an ET-ratio being relevant well.

3. In the particular case of MDB, as I understand it, the ecological concerns that drove the buybacks of irrigation water rights, were over freshwater ecological flows (the blue water or runoff component which the paper does not address) and not diminishing area under native vegetation (the green water component this paper does discuss).

   So an interesting insight that could come out of this analysis – for MDB at least – is the opposite of what the authors are arguing. It seems to me from the analysis presented, converting the land from cropland to native vegetation may not generate much additional stream flow at all because natural vegetation transpires up as much as much as cropland.

   This contradicts what I understood from other papers on the MDB, that stream flow has in fact slightly increased a bit after restoration efforts. So native vegetation must be transpiring less water than irrigated cropland. However, this doesn't emerge from this analysis because the ET ratios track the land ratios so closely. But perhaps the authors are finding that land conversions to natural vegetation result in less streamflow than expected because their ET has not been accounted for?

   This cannot be discerned from the paper as such - but this seems worth exploring further. Would have been good to see just the three ET values for cropland, grassland and native vegetation - but this was never clearly presented anywhere. It cannot be read off Figure 2, as effects of the restoration phase comes at the very end of the time-series. Figure 3 only displays the ratios and not the absolute numbers.

HESSD

12, C136–C140, 2015

Concerns over structure: I also had a lot of difficulty understanding the paper. The data used come in after the methods – so its not easy to infer that the methods are driven by the particular data sets available. As I understand it, the whole GPP thing was only brought in because that GPP is the only variable for which data are available to cross-check ET estimates. But unfortunately – this is never made clear in the paper and it took me a while to understand that the "optimization" was just a calibration of some sort. I am not familiar with this GPP dataset – so I did not delve into the technicalities and uncertainties of this analysis and cannot comment if the results are driven by the peculiarities of that dataset and the GPP-ET function chosen.

Specific Comments:

1. The authors seem to hint at an ideal ratio of ET for humans versus ET for ecosystems – e.g. Line 17 of Section 1 "there are no clearly defined theoretical guidelines for water allocation between humans and the environment". As I said earlier, I am not sure why there *should* be theoretical guidelines at the basin scale. E.g. It may be perfectly OK to have millions of acres of corn fields in Iowa, if biodiversity rich hotspots are protected in the Amazon – why should every basin aim for a specific mix of natural vegetation and cropland?

2. There seem to be inconsistencies between the text and the equations. The whole introduction of WUE into the equations seems strange to me, but perhaps it is just not explained well.

   On line 12 in Section 2.3, the authors say there is a "linear relationship between GPP and ET at a regional scale". However, eq. 8 ultimately implies a non-linear relationship between GPP and ET.

3. I am not sure why the Area Ratio suddenly appears in the denominator in eq. 10.

4. There were a few minor typos - Line 29 in Section 4 - persuit should be pursuit.

HESSD
12, C136–C140, 2015

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