Review of a manuscript “Revisiting global hydrological cycle: Is it intensifying?” by Demetris Koutsoyiannis

Reviewer: Zbigniew W. Kundzewicz

Overall recommendation: moderate modification

The paper under review is a result of a massive, independent, work, driven by author´s curiosity rather than a funded research project. The author has analyzed various data sets and substantial (yet selective) literature. The paper is long and contains 23 figures and six tables.

The author touches upon a broad range of issues rendering the paper somewhat unfocussed. There are several loose ends and dead-end streets and the overall coherence is deficient. The breadth of the material covered renders the paper difficult to review in its entirety by a single referee who is unlikely to be very competent in all the aspects tackled in the paper.

In some journals, there is an explicit request to provide highlights and a graphical abstract. It would be useful here. The title of the paper reads “Revisiting global hydrological cycle: Is it intensifying?”, hence probably the principal take-home message (main highlight) is as follows - Intensification of hydrological cycles is problematic. Changes are weak, amidst large noise. I would suggest that the author restricts his paper to this very issue. But then, some discussion of findings of papers devoted specifically to intensification of the hydrological cycle, abundant since the first decade of 21st century, should be included. I suggest a sample of relevant references, for possible consideration: Ziegler *et al.* (2003); Huntington (2006); Wild *et al.* (2008); Déry *et al.* (2009); Gloor *et al.* (2013), Creed *et al.* (2015), Madakumbura *et al.* (2019).

However, in addition to expressing justified doubts about the general, flat-rate, statement on intensification of hydrological cycle, the author delves with other topics, that are also very important but not directly related to the mainstream of the paper under review. For instance, he provides an interesting claim that overexploitation of groundwater and groundwater inflow to the oceans are meaningful (yet typically overlooked by the disciplinary experts) sources of sea level rise. Also an updated review of global water balance and water resources assessment could be of broad interest. In my opinion, both these topics deserve separate papers, where they could be discussed in more detail.

The part of the paper devoted to sea level rise seems to overlook important recent publications (available in open access) that offer disaggregation of mechanisms: thermal expansion, melting mountain glaciers, Greenland and Antarctica (solid ice discharge, surface mass balance), with quantification. I suggest that the author takes a recourse to the following four recent source items: IMBIE team (2018, 2020); Mengel et al. (2016); and WCRP (2018). Explicit discussion with those papers, co-authored by recognized experts on sea level rise, could be considerable value. If the author can convincingly demonstrate that indeed the effects of glacier melt and overexploitation of groundwater on sea level rise are of comparable size (lines 16 and 651), this would be a very important, high-impact, finding (but not necessarily in the particular paper under review).

I suggest prioritization of the material contained in the paper under review, with more focus and less breadth of material. In addition, I feel that the material could be divided between the body of the paper and an appendix (or a separate supplementary information). The body of the paper should contain the essential, high-impact, text and a smaller set of persuading figures and tables, while some material could be shifted to an appendix (or a separate supplementary information) for those readers who want to find additional details.

The author attempts to "sell" many things in one article, rather than developing two or even three more focussed papers. I have a (perhaps subjective) feeling that the author´s tone is somewhat defensive, like if he was expecting attack. Why not assuming an objective, open, and constructive reviewer, without prejudices, whose motto is – to search for co-benefits and multiple wins – for the audience, for the journal, for the discipline and for the author.

The overall sentiment expressed by the author is that even if we have ample global data sources, available in open access, the entirety of the data do not show a clear pattern. Fluctuations, noise, and chaos are dominating and overshadowing weak trends, if any. So, we know better that we know little, and thus the saying Οἶδα οὐδὲν εἰδώς, attributed to Socrates, holds in this case as well. We should be careful (constructively suspicious and critical) with raw data that are problematic. We should be careful not to issue general, flat-rate, but unfounded, statements. Even if the available data bases have increased dramatically, we can still identify ample problems and weaknesses related to the data and to the lack of homogeneity, in particular.

The author´s excursion into water resources assessments and a recourse to old literature by Zekster (1973, 1993), Nace (1964-1970), and Shiklomanov is interesting. Yet, older estimates contained in these works were based on a far smaller data base in comparison to the present. In the old days, people were guessing rather than assessing global water resources. Now, we have big data available in public domain, stemming from many observing stations, re-analyses, remote sensing, GRACE, and

UNESCO´s world water assessment programme and world water development reports.

Figures require re-consideration. Busy, overloaded, diagrams (e.g. figures 1, 2, 4), with seasonal oscillations are hard to follow. How about aggregates, e.g. simple annual plots, for better visibility. This concern about legibility and easy interpretation also holds for many other figures. I am not persuaded by Figure 13 and the caution stamp (to me this is a dead-end street, in the context of this paper). I have seen figures like Figure 14 many times before and I do not clearly see the rationale for it in the paper under review. Figure 21 is clearly out of scope, as it refers to several types of non-water catastrophes. Many figures illustrate the problems with data, e.g. high differences in left panel of Fig. 22. I wonder what was the purpose for including this particular figure.

In lines 35-36, the bit "without involving extreme floods and droughts, future climate threats may not be frightening enough" cannot stand. Even more frightening are sea level rise (especially in longer time scale) and heat waves.

The sentence in line 632 is obvious and unnecessary. Nobody objects this.

The sentence in lines 655-658 looks pathetic, even if somewhat vague and in need of support. It is by no means trivial and self-explanatory. A broader readership would miss a proof or at least some explanation. Moreover, the link of this sentence to the rest of the paper is unclear.

The rich lists of references and of data sources are useful. However, I would suggest adding Archfield *et al.* (2015) where many important data sources were reviewed.

Discussion of 6%–7% increase per °C of warming shows up in several places (lines 32, 257, 310).

A few typos have been spotted in the paper under review, such as: 477 procced / proceed; 632 been / be; 651 it / in ("it this case it")

Additional references:

Archfield, S.A. *et al.* (2015) Accelerating advances in continental domain hydrologic modeling. *Water Resour. Res.* 51(12), 10078-10091.

Creed I. F. *et al.* (2015) Climate warming causes intensification of the hydrological cycle, resulting in changes to the vernal and autumnal windows in a northern temperate forest. *Hydrol. Process.* DOI: 10.1002/hyp.10450

Déry, S. J. *et al.* (2009) Observational evidence of an intensifying hydrological cycle in northern Canada. *Geophysical Research Letters* 36, L13402, doi:10.1029/2009GL038852

Gloor, M *et al.* (2013) Intensification of the Amazon hydrological cycle over the last two decades. *Geophysical Research Letters* 40, 1729–1733, doi:10.1002/grl.50377

Huntington, T. G. (2006) Evidence for intensification of the global water cycle: Review and synthesis. *Journal of Hydrology* 319(1–4), 83-95 https://doi.org/10.1016/j.jhydrol.2005.07.003

IMBIE team (2018) Mass balance of the Antarctic Ice Sheet from 1992 to 2017. *Nature* 558 (7709): 219–222. doi:10.1038/s41586-018-0179-y.

IMBIE team (2020) Mass balance of the Greenland Ice Sheet from 1992 to 2018. *Nature* 579(7798): 233–239. doi:10.1038/s41586-019-1855-2.

Madakumbura, G. D. *et al.* (2019) Event-to-event intensification of the hydrologic cycle from 1.5 °C to a 2 °C warmer world. *Scientific Reports* 9, Article number: 3483

Mengel, M; Levermann, A; Frieler, K; Robinson, A; Marzeion, B; Winkelmann, R (2016) Future sea level rise constrained by observations and long-term commitment. *Proceedings of the National Academy of Sciences* 113(10): 2597–2602. doi:10.1073/pnas.1500515113.

WCRP (World Climate Research Programme) Global Sea Level Budget Group (2018) Global sea-level budget 1993–present. *Earth System Science Data* 10(3): 1551–1590. doi:10.5194/essd-10-1551-2018.

Wild, M. *et al.* (2008) Combined surface solar brightening and increasing greenhouse effect support recent intensification of the global land-based hydrological cycle *Geophysical Research Letters* 35, L17706, doi:10.1029/2008GL034842

Ziegler, A. D. *et al.* (2003) Detection of Intensification in Global- and Continental-Scale Hydrological Cycles: Temporal Scale of Evaluation. *J Clim* 16, 535-547.