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Interactive comment on "PCR-GLOBWB 2: a 5 arc-minute global hydrological and water resources model" *by* Edwin H. Sutanudjaja et al.

Anonymous Referee #2

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Major comments

The authors compiled their earlier modeling efforts and upgraded the PCR-GLOBWB global hydrological model. This paper consists of two parts, model description and validation. The latter part particularly focused on the comparison between two global simulations of 30 arc min and 5 arc min spatial resolutions. The authors claim that the simulation of finer resolution generally outperform the other.

I found the former part well written except for some technical issues listed below. I am concerned by the validity of discussion of the latter part. The authors mainly compared the histogram of several hydrological indicators for two spatial resolutions. This straightforward approach is sometimes misleading because the performance improvement in some specific conditions can be exaggerated. For instance, because elevation

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correction is applied for air temperature, the performance of finer resolution is expected better than the other in snow-dominant mountainous regions. Because the river gauging stations are concentrated in northern mid and high latitudes, the effect tends to contrast the performance of two resolutions. Performance improvement must be evaluated with more careful investigations. Second, the performance of water use estimation is questionable. The results indicate that estimated national water use differs from AQUASTAT by one or two orders of magnitude. Since little discussion is provided to these considerable discrepancies, I'm puzzled how I should take these results. Further clarification and reasonable discussion should be added to the water use section.

Specific comments

Line 52 "H08 (Hanasaki et al 2008a)": H08 seems recently updated (Hanasaki et al. 2018). The paper may be of interest of the authors because some of the model functions are overlapping with PCR-GLOBWB 2.

Line 229 "resulting crop specific potential evaporation": Do the authors estimate potential evaporation of trees as well? If this is the case, "vegetation specific" may look better.

Line 239 "tall natural vegetation, short natural vegetation, irrigated crops and paddy-irrigation": How are rainfed crops treated in this model?

Line 245 "using a monthly climatology of phenology and crop calendars": If the crop calendars are monthly, crops are always planted at the first day of month and harvested at the last day. Is this the case of this model?

Line 256 "All fluxes are computed per land cover type and balanced with the available storage to arrive. . .": Are storage terms computed independently for each land cover type? For instance, is the soil moisture of natural vegetation different from that of irrigated cropland? If not, how water is balanced with the available storage?

Line 284 "Alternatively, an initial estimate of a fossil, i.e. a non-actively replenished,

groundwater store can be imposed that provides a similar functionality": Hard to read. Rephrase.

Line 365 "the crop composition (which changes per month and includes multi-cropping)": Same question as above. If the crop calendar used is monthly, does it mean all the crops are planted and harvested at the first and the last day of month globally?

Line 378 "the irrigation water demand is increased by 40% to obtain gross irrigation water demand": Is there any rationale for this coefficient? In Section 3.4.3, the authors simply attributed the underestimated irrigation water withdrawal to this coefficient.

Line 383 "the gross demand and net demand are prescribed to the model and calculated using separate script": Confusing. Are gross and net demand prescribed or calculated?

Line 445 "2.4 Differences between PRC-GLBWB 1 and 2": This section seems better to be placed after "2.5 Model code".

Line 469 "tailor-made built-in hydrological function": Hard to read. What does it mean?

Line 470 "its syntax that reads like pseudo-code, generally results in short and readable model codes. . .": Sounds a bit subjective. Describe objective characteristic of code.

Line 505 "Note that parameterizations were derived directly following their source data sets using hydrological concepts described in Van Beek and Bierkens (2009)": "The way of setting hydrological parameters are unchanged from Van Beek and Bierkens (2009)"? Is this what the authors meant here? Anyway, it includes little useful information. On what parameterizations do the authors discussing here?

Line 515 "We used ERA40 and ERA-I results that had been resampled by ECMWFs resampling scheme from their original resolutions to 30 arc-minutes": What do you mean by resampling? Is this different from spatial interpolation? Elaborate methodology and some reasons for adopting the technique.

Line 522 “Equally monthly reference potential evaporation, computed with Penman-Monteith from the CRU data set was. . .downscaled to daily data proportional to Hamon evaporation. . .”: It would be better to state the background or key reasons for this procedure. Why didn’t you solve the Penman-Monteith equation and directly derive daily potential evaporation by using ERA40 or ERA-Interim?

Line 562 “main river in PCR-GLOBWB”: What is the main river?

Line 563 “This yielded 5363 stations for the 5 arc-minute simulation, 3910 stations for the 30 arc-minute simulation”: I’m interested in the distribution of catchment area of these stations. For instance, the number of station for 30 arc-minute is smaller than 5 arc-minute one. Is this mainly because the stations below ∼2500 km2 of catchment area (an approximate area of a single grid cell) cannot be represented by 30 arc-minute? To answer such questions, why don’t you show the maximum, minimum, mean, and median of catchment area for each spatial resolution?

Line 602 “regionalization”: What is this? Do you mean optimization (or tuning) of hydrological parameters to reproduce historical records?

Line 603 “scale-consistent flux-preserving”: Hard to know what is meant here. Rephrase

Line 604 “parameterization is possible”: What kind of parameterization is mentioned here? What does possible mean?

Line 623 Table 1: It is interesting to compare with earlier works (e.g. Table 2 and 5 of Hanasaki et al. 2018).

Line 635 “cross-correlation”: Which did you use cross-correlation (a technique frequently used in signal processing) or Pearson’s correlation? I’m asking this because the results indicate the authors used Pearson’s correlation, but they always wrote cross-correlation throughout the text.

Line 647 “Figure 3”: It is hard to see the differences between a (30min) and b (5min).

Why don't you show the difference between two?

Line 648 "Figure 4": As Figure 3 clearly indicates, the selected GRDC stations are concentrated in Europe. Figure 4 might be a bit misleading if the majority of stations are concentrated in some specific regions (e.g. snow dominated stations in Europe). My suggestion is to make Figure 4 for major climatic zones. It would be useful to identify in which climatic conditions the results are improved. As mentioned above, it would be also interesting to separate Figure 4 by catchment area. I suspect the improvements are concentrated in relatively small basins. Another point is that total frequency apparently far exceeds the number of stations (3597). Elaborate how to see these panels (same for Figure 5).

Line 694 "in case of the Niger River, not representing the inner delta. . .": Or simply something wrong with input or validation data.

Line 653 "a better delineation of the outline of the basins. . .": You mentioned that the error in catchment area is less than 15% for all basins for either spatial resolutions. Further elaborate what do you mean by "better delineation of the outline" here.

Line656 "better snow dynamics due to the downscaling of temperature to 5 arc-minute resolution": Similar comment to above. This argument must be easily supported by showing the performance of snow dominated regions for two simulations (i.e. excluding snow free regions from Figure 4).

Line 670 "Although results are generally better, the spatial distribution of results is similar to those found by Van Beek et al. (2011) for PCR-GLOBWB1". This conveys hardly any information. What does "generally better" mean? What are similar and what are not?

Line 688 "indicating a higher skill with regard to capturing extremes and anomalies": I'm not convinced at all. As mentioned above, the performance must be different by catchment area, climate, topography and other factors. Show concrete evidences for

this claim.

Line 763 “Also, Figure 10 shows that agricultural water withdrawal is underestimated . . .”: I’m quite puzzled by the right panel of Figure 10a. First, majority of plots are located far below the y=x line indicating most countries are underestimated one (or two) order of magnitude. Next, although majority of countries are strongly underestimated, the correlation slope is larger than 1 which indicates the overall results are overestimated due to some outliers’ behavior. These points should be more highlighted to call readers’ attention. Finally, my honest interpretation of Figures 9 and 10 is that this model fails to reproduce the historical dynamics of country-specific water withdrawal. At best, the simulation outputs are considerably different from AQUASTAT. Further clarify the authors’ intention to show Figures 9 and 10 together with discussion on the capability and limitation of the water use module of PCR-GLOBWB 2.

Line 786 “Simulated water withdrawal, by source and sector, matches reasonably well with reported water withdrawal from AQUASTAT”: I’m not able to agree with this statement. The authors reported that the regression slope was as low as 0.54 for some cases (line 761).

Technical comments

Line 67 “Schewe et al. 2013; Haddeland et al. 2013”: Check publication year of these articles. It must be 2014.

Line 89 “collectively over 2100 references”: do you mean citations?

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