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Interactive comment on “Development and calibration of a global hydrological model for integrated assessment modeling” *by* Tingju Zhu et al.

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Comment 1: This is a well-written paper describing the global hydrological component of the integrated assessment model IMPACT and its calibration. Generally, I have no major issues with this paper, except to say that certain integrated assessment models are actually moving to higher resolutions with all of their process descriptions and that they employ the original versions of global hydrological models (e.g. LPJml in IMAGE). Therefore, it would be informative to compare the run times of WGHM and the simplified model IGHM (for instance per year integration) to see how much is gained by the use of the IGHM model. I think this is needed to underpin the rationale of this work.

Response: First of all, we would like to thank the referee for these constructive and helpful comments that helped us to improve the quality of this paper. We have prepared responses to all comments as follows. Revisions made are highlighted in the text of the paper.

The run times of WGHM and IGHM differ substantially. For a 30-year simulation, with PET data pre-processed with the PET module of IGHM, the run time of IGHM runoff module is 1 minute 28 seconds on a Lenovo ThinkPad with Intel i7-5600U processor (base frequency 2.60 GHz), whereas the WGHM needs around 90 minutes for a 30-year simulation on a Delta server with i Xeon E5-1620 v4 processor (base frequency 3.50 GHz).

Changes in the manuscript: On page 6, Section 1.2, in the third paragraph, we added: “The reduction in computational time is significant when comparing runtimes of IGHM and WGHM. For a 30-year simulation, the run time of IGHM is 1 minute 28 seconds on a portable computer with Intel i7-5600U processor which has a base frequency of 2.60 GHz, whereas that of the WGHM is around 90 minutes on a Unix server with Intel xeon E5-1620 v4 (base frequency of 3.5 GHz).”

Minor comments

Comment 2: Line 10, page 2: securtiy -> security

Response: This has been corrected in the manuscript. We thank the reviewer for pointing this out.

Comment 3: Line 20, page 2: uses -> use

Response: This has been corrected. Thanks.

Comment 4: Lines 4-5 and 10, page 6: first you speak of 7 challenges, and then of 2. This is confusing.

Response: Thank you for pointing out this source of potential misunderstanding. The seven challenges were raised in Döll et al (2016) in their paper about “challenges and prospects of global hydrological modelling”. The seven challenges are listed in lines 6 to 9, page 6. Two of those seven challenges are of importance for the IGHM model. For example, the IGHM global hydrological model simulates natural runoff, without considering human water uses, so it is not affected by data scarcity for quantifying human water use.

Changes in the manuscript: Line 10 to 13 now reads as: “Out of the seven aforementioned challenges of global hydrological models, we identified two major challenges that hold true also for IGHM: data constraints and limited understanding of macroscale hydrological processes. These two challenges are. . . ”

Comment 5: Line 20-25, page 6: The statement: “Second, hydrological models are traditionally developed based on measurements and understanding of “micro” scale processes. As such, observed data and hydrological processes are often not compatible or representative at larger scales relevant for macroscale processes (Singh and Woolhiser 2002)” does not lead to the conclusion that: ”Therefore, sophisticated data-intensive watershed hydrological models may not be suitable for macroscale hydrological modeling, due to their large data requirements (Chen et al. 2007), the relatively highly detailed specifications of hydrological processes with a sophisticated model structure, and the large number of parameters that are tailored for a specific watershed at the cost of broader model applicability” In fact, the conclusion from the first argument is that macro-scale hydrological models should be underpinned by correct upscaling procedures of parameters and processes to find a link with the scale of the project description (macro-scale) and that of the observations and process understanding (smaller scale). This upscaling may lead to a less complex model structure, but it does not have to be (if small-scale processes do not average out). Moreover, if the larger-scale model is simpler, it still does not have to be more parsimonious, because the data at the larger scale may be lacking to constraint the macro-scale parameters. This (sometimes) false argument that simpler is necessarily more parsimonious keeps

on popping up in the hydrological literature. A distributed model of a basin that is not calibrated but whose parameters are determined from auxiliary information that is available at that scale from DEM, soil map and remote sensing information, is more parsimonious then a lumped conceptual model that has 7 free parameters that all have to be calibrated on a single hydrograph only.

Response: We thank the reviewer for these thoughts about model parsimony and do agree. We have revised the paragraph.

Changes in the manuscript: We revised the use of the term “parsimonious” and changed the paragraph to: “Out of the seven aforementioned challenges of global hydrological models, we identified two major challenges that hold true also for IGHM: data constraints and limited understanding of macroscale hydrological processes. These two challenges are interlinked. First, although new data sets and updated versions of existing data sets of climate, soil, and water bodies are being made available frequently, the representativeness and quality of these data sets are fundamentally limited by available in situ observations (Harris et al. 2014; Lehner et al. 2011). Second, hydrological models are traditionally developed based on measurements and understanding of “micro” scale processes, rather than macroscale processes (Singh and Woolhiser 2002).”

Comment 7: Line 30-32, page 6: This argument is against physical logic. Usually, the more generally applicable a model or theory is, the more involved it is in terms of equations etc.

Response: We agree with the reviewer and have deleted the sentence.

Comment 8: Line 7, page 7: priori -> a priori

Response: Corrected. Thanks.

Comment 9: Lines 1-2, page 8: natural runoff: are the reservoirs themselves taken out?

Response: Yes, in the WGHM model the flow regulation effects of dams and regulated lakes have been switched off, thus handled as natural lakes. In the IGHM model, the lakes are simulated with water balance equation for open water body, as described by Eq. (16) in the paper.

Comment 10: Line 7, page 8: “probabilistic distribution”. It is better to speak of a spatial frequency distribution, because it represents spatial variation without actual reference to a specific location. It does not represent the outcome of some probabilistic process.

Response: Thanks for pointing this out. We have replaced the word “probabilistic distribution” with “spatial frequency distribution.”

Changes in the manuscript: On page 8, the original sentence has been revised to “... and determines total saturation excess runoff with a spatial frequency distribution of soil water holding capacity in a grid cell.”

Comment 11: Line 8-10, page 10: modeling. “Weiß and Menzel (2008) compares four PET methods using gridded global climate data and concludes that the Priestley–Taylor equation proved to be mostly suitable for a global application” This is not a strong argument. the main reason for using simpler PET relationships is the lack of data to parameterize e.g. Penman-Monteith (PM). However, we are 10 years down the road and much more datasets have become available since then. Also, PM has indeed problems in dry climates where the ventilation term may be too high because of lack of correct observations of RH in heterogeneous landscapes (feedback effects between land and atmosphere). However, Priestley-Taylor may underestimate evaporation and sublimation in colder areas during days with strong winds and little radiation.

Response: We agree with the reviewer’s note regarding the possible applicability of Penman-Monteith (PM) and limitations of both approaches. In addition to data requirements of PM (such as wind speed which is rarely available in reasonable quality at the model’s resolution and global scale), we assume that the robustness of Priestley-Taylor with net radiation as main determine still has benefits compared to more complex approaches like PM (see also the discussion of Milly and Dunne 2017). However, the

main practical reason of using the Priestley–Taylor equation in IGHM is to enable consistency with the WGHM model which uses this approach.

Changes in the manuscript: This sentence/argument was deleted in the manuscript. The remaining sentences for justifying the use of the Priestley–Taylor equation read as (on page 9) “The Priestley–Taylor equation is used in the WGHM model (Döll et al., 2003; Müller Schmied et al., 2014), which generates gridded runoff used to calibrate IGHM. Therefore, in IGHM we also use the Priestley–Taylor equation to calculate monthly PET, as follows.”

Comment 12: Section 3.1: I understand that the main purpose of the model is to emulate WGHM.

Response: Yes. The main purpose of IGHM is to emulate runoff values simulated by WGHM under the same climate forcing.

Comment 13: But it would also be good to have an idea about the "real" performance of the WGHM model used in this study, by showing some validation results of WGHM using GRDC data (or perhaps repeat some statistics from previous work and refer to this work).

Response: The performance evaluation of the WGHM model in a comparable (but slightly updated) version (see Müller Schmied et al. 2016a, 2016b, Müller Schmied 2017) has been done in the framework of the Inter-Sectoral Impact Model Intercomparison Project (ISIMIP), in which WGHM discharge values were compared against other global hydrological models and GRDC data globally. In summary, WGHM performs reasonably well in many regions and outperform other models in a majority of river basins, but have difficulties in e.g. cold climate zones (Zaherpour et al 2018). The work of Veldkamp et al. (2018) shows in particular that WaterGAP has no problems in capturing mean observed river discharge which is due to the calibration approach. The routing approach of WGHM is in some basins also comparable (or better) than a more physically based river routing approach (Masaki et al 2017). The model variant of

WGHM used in this study (without human impacts) does not allow meaningful comparisons to observed river discharge data as the large majority of river basins is affected by human impacts to some degree.

Changes in the manuscript: We added the following paragraph with regard to WGHM validation at the beginning of section 3.1: “Recent comparisons of a slightly different WGHM version (Müller Schmied et al, 2016a, b, Müller Schmied 2017) in the framework of the Inter-Sectoral Impact Model Intercomparison Project (ISIMIP 2a, https://isimip.org) showed a relatively good agreement with observed river discharge data for many basins; due to its calibration to mean annual river discharge, it outperforms other global hydrological models except in cold climate zones (Zaherpour et al 2018). The work of Veldkamp et al. (2018) shows in particular that WaterGAP has no problem in capturing mean observed river discharge, which is due to the calibration approach. Still, streamflow seasonality cannot be captured well in many river basins, in particular when streamflow is strongly affected by human water use and man-made reservoirs. The routing approach of WGHM is in some basins also comparable with (or better than) a more physically based river routing approach (Masaki et al 2017).WaterGAP monthly Nash-Sutcliffe efficiencies for streamflow at the 1319 gauging stations are larger than 0.7 for 372 stations, between 0.5 and 0.7 for 349 station and smaller than 0.5 for 598 stations (Döll et al, 2018).”

Comment 14: Line 2, page 5: “strong correlation”. Would be good to calculate its value and put it in the figure.

Response: We did not find the word “strong correlation” in line 2, page 5. We checked the rest of the manuscript and found the following sentence on page 14, lines 2-3: “A comparison of the KGE (validation) with the KGE (calibration) plot reveals that there is a strong linear correlation between the KGE of the calibration period and the KGE of the validation period.” This is the only sentence that mentions strong correlation throughout the paper.

Changes in the manuscript: Thus, we calculated the correlation coefficient and added it to the text that describes the figure (i.e. Figure 3).

Comment 15: Figure 5: Also mask out the areas not considered such as in Figure 4. This would also allow you to increase the resolution of te legend.

Response: Figure 5 was updated to mask out the areas not considered as in Figure 4.

Comment 16: Figure 6. The map for b in also has a magenta colour in it which is not in the legend.

Response: We double-checked Figure 6 and the way they (the four maps) were produced using ArcGIS. It appears that the maps do not include magenta color.

Comment 17: Figure 6. Some of these parameters, such as Smax I expect to be part of WGHM as well. Thus, I would like to see some maps of these parameters compared to the patterns of similar parameters in WGHM to check for consistency of the calibration results.

Response: We have added a map of Smax from the WGHM model.

Comment 18: Table 2. Apart from the correlation, it would be good to have a global sensitivity plot: global average KGE versus percentage change in each parameter.ÂËŸ aIs that possible? This would allow the reader to see which parameter has the largest effect on the calibration results.

Response: A sensitivity analysis figure was added to show how global average KGE values respond to perturbations in each calibrated parameter.

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