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Geoscientific Model Development Discussions

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

Anonymous Referee #1

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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.

Minor comments

Line 10, page 2: securtiy -> security

Line 20, page 2: uses -> use

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

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.

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.

Line 7, page 7: priori -> a priori

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

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.

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.

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

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

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.

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

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.

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.ÂăIs that possible? This would allow the reader to see which parameter has the largest effect on the calibration results.

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