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Author Response on RC4

Danielle S. Grogan et al.

Author comment on "Water balance model (WBM) v.1.0.0: a scalable gridded global hydrologic model with water-tracking functionality" by Danielle S. Grogan et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2022-59-AC5>, 2022

Reviewer #4

General comments

The paper "WBA: A scalable gridded global hydrologic model with water tracking functionality", by Grogan et al. provides a description of the first open source version of the University of New Hampshire Water Balance Model. The authors chose an approach that combines parts of a "classical" model description – i.e. description of the functionalities and fundamental equations, validation and a selection of case studies – with a literature review of the history of the model, previous studies with WBA and validation of previous model versions. Overall, this structure works really well and the paper is well written, thus, I only have a few minor suggestions.

We thank the reviewer for taking the time to carefully read and review this paper.

Minor comments

1) With respect to the model description, the authors did a very good job at providing a general overview over the basic equations and dependencies in the model without overloading the manuscript with technical details (which is perfectly reasonable given that most WBA components have been used in previous studies and have been well documented). However, it would be extremely helpful if the structure of the section 2.2 could be related to what is shown in figure 1, i.e. that all elements that are shown in the figure are discussed in the model description, preferably even in a way that each element in the figure has a subheading in the text.

We will work on harmonizing the figure 1 labels and the section titles to improve clarity.

2) I find the use of the term "unsustainable ground water" somewhat problematic, since it is not the groundwater itself that is unsustainable but its use e.g. for irrigation. A term that clearly states either which real world pool is represented – e.g. fossil water – or what it constitutes in the model – namely an unlimited water supply to balance demand-supply mismatches – would be more appropriate. Maybe the authors could also add some discussion to section 2.2.2, detailing how the use of this pool affects simulations

(especially projections) in regions where fossil ground water is being depleted.

From the model perspective the water users are tapping into a pool of water that is not explicitly recharged and therefore it is unsustainable, even though this pool is not finite. The concept and the term “unsustainable” has been used in our published papers (e.g. Grogan et al., 2017; Liu et al., 2017).

Several terms for this concept are used in the literature depending on the application, and we make particular reference to the useful table of definitions in Bierkens and Wada (2019). These different terms have been acknowledged in the text (starting on line 289). We selected our terminology as an adaptation to the “physically non-sustainable groundwater use or groundwater depletion” defined by Bierkens and Wada (2019), and our conceptualization is consistent with that use. Because we define a flux of water that satisfies demand unmet by local resources (i.e. non-sustainable groundwater use), this implies a pool of water from which this water is drawn, which we do not represent explicitly, that is best identified in our use as a tracking descriptor as “non-sustainable groundwater”. We adopt “unsustainable” as equivalent to “non-sustainable” because the word “unsustainable” is defined in both the Merriam-Webster and Cambridge dictionaries and “non-sustainable” is not. While we do agree with the reviewer that it is the flux or the use of groundwater that is unsustainable, it is illogical to describe subsequent fates or flow paths abstracted unsustainably as “use”. For instance, in describing the fractions of primary water components that drain to the ocean, it is awkward to describe a fraction of this discharge as “unsustainable groundwater use”, when we are clearly treating it as representing a fraction of flow within the river system.

We are also unsatisfied with the alternative terminology posed (fossil groundwater) by the reviewer. Again, following definitions from Bierkens and Wada (2019), some terms imply knowledge of the age of recharge to the aquifer that we do not characterize in typical WBM simulations (e.g. recharging 12,000 years before present (Jasechko et al. 2017) in the case of fossil groundwater). Therefore, we prefer terminology that implies no assumptions of the era of recharge.

We acknowledge that the terminology that we have been using in this and our prior papers is not ideal, though it has been deliberated extensively. However, we view it as satisfactory for the time-being and for the purposes we are using it. We plan to clarify our definition of unsustainable groundwater in any revision of the manuscript.

3) I think it is a good idea to discuss existing model validation in this paper, rather than repeating the respective simulations with the present model version. However, it would be helpful if the authors could detail if and how the present model version differs to the model versions used in the previous studies and how these differences affect the results. Furthermore, with respect to the FrAMES model (component) I find the model validation a bit out of place here, as it is not merely a different version of WBA but a completely different model. I would expect the performance of the implemented functionalities to depend on many other aspects of the model and the forcing data, hence different simulated nitrogen concentrations with WBA. I think it would be sufficient to state, in the model description sec. 2.2.6, that WBA now includes these functionalities based on the parametrizations of the FrAMES model and reference the studies in which FrAMES was validated. However, I think it would be even better if the authors could actually perform the analysis and validate N and temperature in WBA simulations.

The reviewer requested that we provide details of how the current version of the

model differs from prior versions of the model. We provide a table describing the evolution of the WBM family of models as Table 7 and discuss this evolution in greater detail. We can add a comment in this section to make the reader aware that there is a section devoted to describing the evolution of the model; however, we decided that bringing such a discussion earlier in the manuscript before the validation section detracted from the flow of the document. The reviewer also asked us to comment on how differences in the current model and prior implementations of the model could affect results; however, we view this as an impossible request. Each study referenced in this section had specific motivations, input data, and simulated processes. We do not see what utility would be gained by comparing results from different applications of the model, as it would be impossible to attribute differences to model structure, input data, or parameterization without a rigorous and controlled series of experiments. We consider such an experiment beyond the scope of this manuscript, and that this manuscript be an important first series of steps to such an effort.

The older versions of the model, WBMplus and FrAMES, are closely related to the model version described in this paper. Despite the different name, FrAMES is not a completely different model, but it was built from WBMplus with the ability to simulate water quality (including temperature) incorporated into the code. The current WBM version used both as a guide with extensive internal model coding to better allow for human use of water, an improved ease of coding, and increased run time speeds. Verification of equivalent results was paramount when developing the current version of the code. Therefore, the older validation analyses are very much applicable to the current model. However, we will incorporate a statement in the paper to clarify this information to better justify the use of these prior validation exercises.

We would also like to respond to the reviewer's comment or request to repeat simulations of temperature or dissolved inorganic nitrogen (DIN) with WBM v1.0. We debated this extensively in the development of this manuscript and decided that DIN functionality within WBM is limited to regions with ample observational data. We are planning to provide a brief description of the methodology used to parameterize the model following Wollheim et al. (2008) in any revision of the model documentation, and more clearly describe the limitations of this functionality. We find it beyond the scope of this manuscript to apply this functionality to the two simulation domains presented and consider the addition of a third simulation domain more likely to add more confusion than clarity to the manuscript.

Specific comments

Line 22: "... as well as perform model experiments in new ways". Please, clarify which are these new ways.

Yes, this statement is ambiguous. We will be more specific in the abstract as to what these are: tracking throughout the water system, including tracking attributes such as state of origin.

Lines 55 ff: Evapotranspiration will eventually lead to precipitation and a large fraction of the respective water is even recycled locally. Thus, the statement that only 50% of water is returned to "the system" is somewhat misleading. In contrast, when talking about specific pools in the system a 50% return rate is also often questionable, e.g. in case of fossil water, at least on a centennial timescale.

Without an atmospheric model WBM is not able to assess the degree of local

recycling of evaporate. However, we will improve the statement in the text by clarifying that we are referring specifically to direct returns of liquid water to shallow flow paths in local watersheds.

Fig. 1: Would it be possible to make the elements of the figure consistent with subheadings in section 2.2.? For example infiltration is not specifically discussed in the text.

We will work on harmonizing the figure 1 labels and the section titles to improve clarity.

Line 166: E_{ow} is not defined.

Thank you for catching that oversight. E_{ow} is open water evaporation expressed in mm/d and we will add this to the text.

Line 170: "Storm runoff" is this the same as "stormwater runoff"?

The term "storm runoff" provides a useful semantic distinction from "stormwater runoff". We view the collective understanding of stormwater runoff to refer to immediate runoff from impervious surfaces connected directly to water bodies via built infrastructure. While we include such a flux in WBM, we add two other fluxes to this variable in our internal calculations: 1) precipitation incident directly to open water within each pixel, and 2) water that overfills the surface flow pool (R_{excr} Equation 13).

Line 179: What does WBA do in these grid cells e.g. in case of endorheic basins?

The endorheic basins accumulate water at their "outlets" in a dedicated endorheic lake storage pool and we will update the text to explain these details.

Line 183 f: Is this the only limit on infiltration? Is the state of the soil not taken into account?

Yes, we do not simulate explicit Hortonian runoff. In the case that the soil is already saturated, any throughfall will be split between the quickflow and recharge flow paths. This may present a localized low bias in runoff for extreme precipitation events, but we consider it a suitable simplification for a macro-scale model.

Line 190: It may be helpful to mention that WBA does not have soil layers and does not explicitly represent the vertical flux through the soil or a soil moisture profile.

WBM has a single soil layer. Section 2.2.1 Land surface fluxes discusses the soil layer (defined by rooting depth on line 187) and section 2.2.2 Groundwater discusses the below-soil storage pool (shallow groundwater storage pool). We will correct the labelling in Figure 1 to reflect these terms more accurately. Any revision of the manuscript will provide a brief description of soil water balance within the root zone adapted from previous publications and our technical documentation.

Line 257: How do you justify this default value of 1000 mm, i.e. that the model, in the default mode, has no real limit to the surface storage?

You are correct, the default behavior is to effectively turn the process off. This default keeps model behavior more in line with prior model usage (from early

Vorosmarty papers through Grogan et al. 2017), until at such time that we have experience with the parameter to provide better direction to users.

Line 280: I am a bit confused by the unit l/d is that per m^2 ?

We plan to clarify the text about calculation of baseflow drainage from the shallow groundwater pool. The shallow groundwater pool stores space-averaged groundwater in units of mm. The time-constant drains water from the pool with units of $1/d$, yielding a flux to the stream in units of mm/d. We will adjust the text to clarify this.

Line 490 ff & 503 ff: Is there a lag connected to the return flows?

There is no lag, and we can revise the text in any requested revisions to clarify.

Line 564: I am not familiar with the term "relict water", so I am not sure whether some definition is necessary.

Relict water is defined in lines 854-859 along with a citation to Zuidema et al. (2020) where we first used the term in reference to tracking return flow. In response to other reviewers the definition of relict water found on Line 856-857 comes too late in the manuscript and should be moved to the area around Table 4, and this may help alleviate reader's confusion at this point in the manuscript.

Fig. 2: What is the meaning of the colors? Also why is the down-stream cycle different (no subheadings in "sources" and "water")?

The diagram was designed to show that the same processes are applied to the water as it moves downstream. The second "Local Cycle" box used simplified labels to reduce complexity in the figure and the third cycle is represented by a "...". We will work with the figure to make it clearer and/or add in explanatory text to the caption.

Section 3.1: Could you maybe add a table for a quick overview?

While we acknowledge that there are numerous numeric values and citations presented in these paragraphs, we have reservations about presenting this information as a table. As a table, the citations and numeric values are likely to present a firmer or more concrete assessment of model performance than we are trying to communicate. Much of the nuance and critical aspects of the findings of these studies are presented in the text, and the values are properly contextualized with the text.

Line 611: What about the UDEL climate? In Fig. 3 The R2 looked very promising?

Thank you for pointing out that the UDEL climate was not mentioned. We will correct that oversight.

Fig. 4: Maybe use the same axis for subfigure b and c?

The panels in Figure 4 are from an existing publication, however we will recreate the figures with adjusted axis ranges.

Line 671: While I think it's a good way to use existing validation, I am not sure about the FrAMES model, as the respective formulations lead to a very different outcome in the WBA framework?

The similarities between FrAMES and WBM v1.0's river temperature and in-stream nitrogen modules are essentially identical making the evaluations from the earlier papers appropriate.

Line 725: Could you also include R2 to make it easier to compare the present simulations to those in section 3.1 ?

In response to this and other reviewers' comments, any revision of the manuscript will include additional efficiency metrics beyond the index of agreement.

Line 725: I would be very curious if you could also include an evaluation of the simulated evapotranspiration ... maybe against GLEAM data?

Evapotranspiration data is sparse and applies to scales much finer than resolved by the large grid cells used in this paper (0.5 minute cells). The GLEAM data uses a Priestley-Taylor approach and therefore any evaluation would be a comparison between the two PET functions. A comprehensive PET comparison in the WBM context can be found in Vorosmarty et al. (1998).

Fig. 5 & 6: Why is there no Index for the Nile/Indus/Ganges in subfigure c ? Also, would a relative measure make more sense than MBE.

We used Global Runoff Data Centre (GRDC) river flow stations for these maps and the GRDC relies on all nations to contribute their data. In some international basins countries are reluctant to share this data and therefore only a limited time series is available. This is also the case for many stations in Asia. For the Nile, the data range from GRDC covers 1973-1984 while the model runs presented here ranged from 2000 to 2009.

Relative bias metrics would provide an additional metric for performance evaluation, but we argue that it would be inappropriate to substitute the absolute measures for relative ones. The absolute values provide some indication of total errors in estimating global discharge, and where errors are located relative to global discharge. We acknowledge that WBM exhibits lower performance in more arid regions, where relative errors would be substantially larger due to both model error and substantially lower discharge. A plot of relative bias would be expected to identify substantial errors in areas that are not contributing significantly to global discharge. However, such a plot would also identify critical simulation errors in regions that are more sensitive to water scarcity. We expect to add a plot in addition to Figure 5 and 6 to illustrate a relative bias metric.

Fig. 8 & 9: Could you do such a figure also for evapotranspiration?

Unfortunately, our tracking system for evapotranspiration is not sufficiently complete to provide equivalent panels to Fig. 8 and 9. We do thank the reviewer for pointing this out, and we have added this to our model development to do list.

Fig. 10: I find the purple and blue colors are very similar, and I am not sure that its only an issue related to my printer.

We printed Figure 10 on an eleven year old HP Color Laserjet CP2025 and the colors are very distinguishable. We will try some other palettes to identify a better color scheme. Perhaps the journal could provide some guidance if they

perceive this as an issue.

Line 906 ff: "... published in (Vörösmarty et al., 1989)". I would not use the brackets here.

Yes, that is a typo and it will be corrected.

References

Bierkens, M. F., and Wada, Y. (2019). Non-renewable groundwater use and groundwater depletion: a review. *Environmental Research Letters*, 14(6), 063002.

Jasechko, S., Perrone, D., Befus, K. et al. Global aquifers dominated by fossil groundwaters but wells vulnerable to modern contamination. *Nature Geosci* 10, 425–429 (2017). <https://doi.org/10.1038/ngeo2943>

Grogan, D. S., Wisser, D., Prusevich, A., Lammers, R. B., and Frohking, S.: The use and re-use of unsustainable groundwater for irrigation: a global budget, *Environ. Res. Lett.*, 12, 034017, <https://doi.org/10.1088/1748-9326/aa5fb2>, 2017.

Liu, J., Hertel, T. W., Lammers, R. B., Prusevich, A., Baldos, U. L. C., Grogan, D. S., and Frohking, S.: Achieving sustainable irrigation water withdrawals: global impacts on food security and land use, *Environ. Res. Lett.*, 12, 104009, <https://doi.org/10.1088/1748-9326/aa88db>, 2017.

Vörösmarty, C. J. and Iii, B. M.: Modeling basin-scale hydrology in support of physical climate and global biogeochemical studies: An example using the Zambezi River, *Surv Geophys*, 12, 271–311, <https://doi.org/10.1007/BF01903422>, 1991.

Vörösmarty, C. J., Moore, B., Grace, A. L., Gildea, M. P., Melillo, J. M., Peterson, B. J., Rastetter, E. B., and Steudler, P. A.: Continental scale models of water balance and fluvial transport: An application to South America, *Global Biogeochem. Cycles*, 3, 241–265, <https://doi.org/10.1029/GB003i003p00241>, 1989.

Vörösmarty, C. J., Federer, C. A., and Schloss, A. L.: Potential evaporation functions compared on US watersheds: Possible implications for global-scale water balance and terrestrial ecosystem modeling, *Journal of Hydrology*, 207, 147–169, [https://doi.org/10.1016/S0022-1694\(98\)00109-7](https://doi.org/10.1016/S0022-1694(98)00109-7), 1998.

Zaveri, E., Grogan, D. S., Fisher-Vanden, K., Frohking, S., Lammers, R. B., Wrenn, D. H., Prusevich, A., and Nicholas, R. E.: Invisible water, visible impact: groundwater use and Indian agriculture under climate change, *Environ. Res. Lett.*, 11, 084005, <https://doi.org/10.1088/1748-9326/11/8/084005>, 2016.

Zuidema, S., Grogan, D., Prusevich, A., Lammers, R., Gilmore, S., and Williams, P.: Interplay of changing irrigation technologies and water reuse: Example from the Upper Snake River Basin, Idaho, USA, 24, 5231–5249, <https://doi.org/10.5194/hess-24-5231-2020>, 2020.