Hydrol. Earth Syst. Sci. Discuss., 12, C404–C409, 2015
www.hydrol-earth-syst-sci-discuss.net/12/C404/2015/

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "GlobWat – a global water balance model to assess water use in irrigated agriculture" *by* J. Hoogeveen et al.

Anonymous Referee #2

Received and published: 4 March 2015

GlobWat - a global water balance model to assess water use in irrigated agriculture

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General impression

GlobWat describes the monthly water balance with 5 arcminutes with a specific attention to irrigated agriculture and the distinction between green and blue water, also for open water and wetlands. This is paramount for better describing the worlds' major water withdrawals, and GlobWat seems to be designed for that. Most hydrological models focus on river discharges, and GlobWat has a deliberately different objective. It is therefore worth publishing this new global scale model. Several recommendations are provided to develop the concepts further and refine the model formulation. It would

be good if the authors provide an outlook on the further development of GlobWat.

The model will become stronger if even more agricultural details will be included, also for rainfed crops. This will make the model more versatile to predict for instance impacts of droughts on crop ET. A simple crop production function could be added as well, for investigating the world food situation in relation to water inputs (rainfall and irrigation). The work will get more international attention if being referred to as GlobAgWat instead of GlobWat. The strong point is that the model relies on country statistics that have been carefully screened as part of Aquastat. The weak part is the calibration procedure of the model by adjusting the maximum water holding capacity and maximum groundwater recharge up to a factor 2.5. This is probably a consequence of some of the strongly simplified expressions, something the authors are at least clear about. Considering the character of the model, validation should focus more on rainfall, evaporation and soil moisture. While the calibration ensures measured and modelled discharges to agree (and hence Figure 2 does not come as a surprise), it is more interesting to validate and calibrate the vertical water balance, rather than the horizontal water balance. Another flaw is the statement on page 810 "it is assumed that there is always enough water available to assure that crops under irrigation never suffer water stress". If there is one global institute that knows that this is untrue, it will be FAO. Crop water stress canbe computed from irrigated/irrigable area from Aquastat or from available upstream horizontal water inflow (Qin). If Qin is lower than the net irrigation requirements, then water stress is obvious. I do not understand why this crucial process in describing water consumption is irrigated agriculture is overlooked.

CropWat model of FAO is well known and accepted in the international community. More comparisons and remarks on model similarities could ease the acceptance of GlobWat. The open character of the model is appreciated. If the authors consider the remarks explained below, and emphasize the agricultural character of the model, with more tabular agricultural water management data, then I find GlobWat an attractive alternative solution for existing global hydrological models.

Major remarks

Abstract line 15: "the comparison with other global water balance models" has hardly worked out, and only relates to incremental evapotranspiration values (which appeared to be remarkable close). This statement can be removed from the abstract. The introduction should from the onset be more clear on why there is a need for another global water balance model, using the full exploration of AquaStat, the 5 arcminute data and the agricultural focus as main arguments. The statement of page 804 (line5) comes too late, and is in my opinion not convincing. Be more specific and convincing ! Page 802, line 22: "agriculture accounts for about 70% of the freshwater withdrawals" is a statement that is often copied in the international literature without clear fundament on where this golden number is coming from. In a paper like this, this should be better specified with references, and I would even challenge the authors to come up with a table in the end showing this fraction for major river basins or for the 25 major irrigation countries. Is it really 70% ? It is in my view just a number that scientists do not like to argue. Page 805: Eq. (1) contains an Eincr-irr term at both sides of the equal sign ? The most consistent solution is to add an irrigation efficiency term Eincr-irr/Effirr at the inflow side. Page 805: Eq. (2) is a very welcome presentation of the horizontal water balance. This is appreciated, and especially if more discussion is dedicated to Eincr. Flood plains, riparian corridors, river valleys, groundwater seepage zones, oases, forests and phreatophytes for instance also consume large amounts of renewable water resources. An elegant way to include those processes is by taking a wider definition of wetlands. This needs more attention in the next version of the manuscript, because these classes occur frequently and increase the total Eincr considerably. The Eincr-irr/Eincr-wetl ratio will be surprisingly close to 1 ! Page 806: section 3 can be removed. All that is presented has been mentioned before (?). I associate Thornwaite and Mather not with such general equation, so for me this section is confusing Page 808: Eq. (8) and others are based on sharp boundary conditions that introduce rather bold step changes in the water balance. Try to model functions of soil water storage more gradual, or make at least some remarks that it does not affect the soil water bal-

ance of crop seasons. Page 808: Eq. (11) these type of results depend very strongly on SCmax. There are newer soil maps of the world available, then the one used in GlobWat (line 22). A more critical discussion on the sensitivity of SCmax and the uncertainty of the estimate of the SCmax value is necessary. Page 808: the impact of salts is completely absent. This should be mentioned, and also it should be made more clear that actual / potential ET is accounted for when $S < 0.5$ Smax. While this applies for rainfall, crop water stress (and preferably also salt stress) should also arise under irrigation conditions. Page 809, line 10: The WHYMAP recharge values are a serious underestimation in my experience, and not very reliable. This also appears later in the calibration, where most recharge values had to be multiplied by a factor 2.5. Eqs. (12) and (13) are therefore not much meaningful and it is better to model a recharge on the basis of Ksat and S. Page 812, line 12: the outflow from open water is zero if the open water balance is negative. I think that Lake Victoria and Lake Tana have outflow during the dry season, so why outflow is prohibited ? Page 814, line 8: it is mentioned that the river has "sub-basin storages" without any further explanation. The size of these areas has significant impact on the computation of river flow balances. How is the discretization organized, and can you provide the arguments for that ? It is clear that GlobWat is not meant for simulation of river flows, but some kind of accounting is necessary to express water availability for withdrawals and incremental evaporation. A related question is how reservoirs are considered in the model ? Perhaps by a large response factor F to allow carry-over water, or not at all ? Would it not be nice to separate the response factors for runoff and base flow ? Now the base flow seems to be treated hydrologically similar to surface runoff, so why is this parameterized anyway ? Apparently all recharge goes to base flow, which is rather simplistic. It is highly recommended to provide more attention to recharge, groundwater abstractions and base flow. Or provide reasons why this is not done. Page 814, line 24: the irrigation requirement seems according to Eq. (33) to be merely an area-integrated Eincr, where Eincr describes the difference between Etotal and Erain (or effective precipitation). This is in usual terminology referred to as crop water requirements. Irrigation requirements are the crop water requirements corrected with an efficiency term. I have the strong feeling that the authors here deviate from the standard international (FAO) literature, and this is confusing (see also my comment on irrigation efficiency later). The irrigation efficiency describes ratio between crop water requirements (assuming no stress) and water withdrawals. The inclusion of the irrigation requirement (which already encapsulate an efficiency according to international literature) is in contradiction with many course materials. The calibration with CalSW and CalGW is rather arbitrary, and not well described. I interpret that it is achieved by using measured discharges in rivers. If so, then why a validation is needed ? Perhaps different data sets are used ? More elaboration is expected in the next version of the paper. Page 818, line 9. It is great to show the global map of evaporation. Several comparison studies with land surface models and remote sensing data are available, and it really would re-enforce the paper if a comparison is made for evaporation. The LandFlux-EVAL multi-data set analysis study by Mueller et al. (2013) published in HESS could be a good opportunity. Also the new evaporation data sets of USGS and CSIRO.

Minor remarks

Page 802, line 25: better define "human use", do you perhaps mean manmade withdrawals ? Page 803, line 9: the term "water use" is confusing when not being associated to a certain flow path. Can you be more specific ? Page 805, line 5: this is not true, as hydrological processes - and runoff in particular - has much faster time scales than one month. It is better to leave this out. There are very good arguments to select a monthly time scale (e.g rainfall and agricultural production), but the lag time between rainfall and runoff is definitely shorter. Page 805, line 12: a words are missing at the end of sentence Page 805, line 22: nice to insert a clarification on evaporation terminology, but this should be mentioned with the introduction of the word for the first time, hence earlier in the manuscript Page 806, line 19: introduce the type of rainfall data set here as one get curious. Fine to summarize later all datasets in a table, but timely introduce them when relevant for the understanding of the procedures Page 807, line

12: provide more background why CRU CL2.0 is chosen. Is it because it is based on rain gauge measurements and excludes satellite data ? But later the authors do use satellite data on irrigated areas and more. CHIRPS and CMORPH are well respected remotely sensed datasets being more robust than CRU CL2.0. More elaboration is required Page 810, line 15 to 25. The explanation onf Kc is great, but it comes rather late in the paper. Can this be moved forward ? Page 812, line 3: "the actual evaporation depends on the depth of the water bodies", this function is not described in the subsequent sections of the paper Page 812, line 9: the value for Kow provided is with 1.1 rather low, although not impossible if one considers heat storage in lakes and reservoirs. More background information on Kow needs to be provided Page 815: Eq. (34) does not describe the spatial mismatch between country and river basin withdrawals. Page 817, line 19: specific discharge is mentioned several times, but the time unit is not provided. I guess these are annual values ? Better to mention that throughout the paper. Page 818, line 6: the term "drainage fluxes" is suddenly introduced. Better stick to the nomenclature used before. Page 818, line 25: I could not find water use efficiency values in Table 4

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