

## **Review of “Hydrological Impacts of Global Land Cover Change and Human Water use” by Bosmans et al.**

Generally the paper is interesting and well written. But for the moment it does not contribute originally to the literature on the impact of land use on the continental water cycle. Indeed this topic has been studied with many land surface models. But these are all numerical experiments which trust blindly that the parameters for the various vegetation types (Which have been tuned for the current climate and vegetation distribution.) apply to the original vegetation which existed before the human started to change landscapes massively in the mid 19<sup>th</sup> century. The authors acknowledge only partly this fact in the discussion section of the paper.

Coming from the global hydrological models community, the authors have a trump they should use. In contrast to classical LSMs, PCR-GLOWB is designed to simulate today's water usage and thus should simulate quite realistic river discharges in current conditions. Thus, the simulation HUM2000 should be much more realistic than the simulations on which the other land use studies are based. In other words, I would expect this study to present the realism of this simulation to argue for the quality of his study and its added value. Furthermore the use of the deviation from observed discharges could serve as an estimate of uncertainty and qualify the global averages changes in actET and discharge presented in section 4.1.

Thus, and before proposing a list of minor comments, I would suggest a major revision of this paper so as to present hydrological arguments as to why we should trust your numbers more than those of the cited papers. Else this paper will be just more noise on a topic where for the moment we are just guessing some numbers and anybody can propose “alternative facts”.

Below you will find some minor comments which will hopefully help improve the paper. These comments also illustrate the major changes I would deem necessary to raise the level of this paper above the previous studies on this topic.

- Page 1, Line 21 : It is not true that few studies focus on including land use. Most land surface models used in the CMIP5 simulations apply a land use scenario. It could be true that the work is not very visible in the literature. I would attribute that to the fact that this is only a set of guestimates as the vegetation parameters are highly tuned and cannot claim to have any generality.
- Page 2, Lines 1-15 : In your review you do not mention that the picture is further muddled by the fact that in parallel to the land & water use change climate and aerosol loadings have evolved. Thus potET has a significant trend through modification of incident long-wave and solar radiation, atmospheric turbulence, water vapour pressure deficit and amplitude of the diurnal cycle. I can understand that this is outside of the scope of your study but these caveats need to be mentioned in the introduction. The literature is plentiful on this topic !
- Page 3, lines 25-30 : Please state clearly that you assume that the potET estimated for the period 1979-2010 is valid in 1870. To me this casts a big shadow over all land use studies but intellectual honesty requires that this is stated as a working hypothesis !
- Pages 18, line 18 : Can this impact on the Nile really be trusted ? The observed discharge in Aswan for the period 1871-1900 (i.e. before the first dam) is 112km<sup>3</sup>/y or about 3500 m<sup>3</sup>/s (What does PCR-GLOWB say ?) . Your combined change (HUM2000-LC1850) seems to be above 100m<sup>3</sup>/s, thus the amount of water in the Nile at Aswan should have increased ! Observations indicate that the inflow into the great dam has not changed significantly since the end of the 19<sup>th</sup> century. On the other hand the amount arriving at the sea has dramatically been reduced. As you see, the value of your hydrological model is that you can check the reality of the predicted changes with the observations which date back to the 19<sup>th</sup> century. Based on my own experience the land use change proposed by LUH for the upper Nile is unrealistic, but you could quantify it !
- Page 9, line 33 : PCR-GLOWB has rounding errors ? That is strange and would point to numerical problems.

- Page 11, lines 15-21 : These numbers are strange. The equation in this paragraph is not balanced. Where have the missing 2km<sup>3</sup>/y gone ? Has the ground water increased or is the model not stabilized and shows different trends on the 1979-2010 period for the three configurations ? This requires some explanation.
- Section 3.2 : This section should include a discussion of the ground water recharge changes between LC1850 and LC2000 or HUM2000. This is another point where we have data to support a constructive discussions. There are many wells with over a 100 year long water table records where at least the sign of the observed recharge changes can be compared to the simulations. See for instance the study by MacDonald et al. 2016 for the Ganges.
- Page 14, line 3 : “leading to a strong increase in discharge” acknowledges better the existing relation.
- Page 14, line 24 : what supports the assertion that “crops lead to the largest reduction in evapotranspiration”. Models have shown it but what data is there to support this in all generality ? Does it not depend on the crop variety, the type of agriculture (in small units or large scale) and cropping practices (number of harvests and rotations) ?
- Page 14, line 32 : How can we believe a sensitivity analysis of a model if we do not know if the model is a trustworthy reproduction of the current situation ? As I have pointed above, not only would your study be more credible by using the available observations but you could nicely qualify the simulated sensitivity.
- Figures 7 & 8 : These are really complex figures which would benefit from a more didactic presentation. Take one case to walk through the graphical representation so that your interpretation is easier to follow.
- Figure 9 : Only 1 basin seems to have significant ground water pumping as the arrow points above the actET=P line. Which basin is this and are there observations to give some credibility to this result ?
- Page 18, lines 17-21 : I think that it is important to stress that we have no way of verifying that the parameters used for pre-land-use vegetation are correct. Vegetation parameters which are used to compute evapotranspiration have been calibrated to current vegetation covers in order to obtain correct fluxes and they have no fundamental physical or biological foundation. Today’s pristine forests can have functioning different from their ancestors because they are exposed to milder winters, air pollution, increased CO<sub>2</sub> levels and other environmental stresses.
- Page 18, line 24 : Is it really meaningful to distinguish between the 1850 estimated land cover and a potential cover ? I would contend that the uncertainty in the LUH data and vegetation parameters is larger than the difference to a potential cover. Could you give more substance to your hypothesis ?
- Page 19 : The discussion would be greatly helped with a table which provides the estimates of the previous studies and their main characteristics.
- Section 4.2 : I would like to restate that you have the unique opportunity to estimate the uncertainties by comparing your simulations with the observations available for most of your 100 basins. It just occurs to me that in figure 9 the century long records which exist for a number of basins could allow you to estimate the resulting arrow LC1850 → HUM2000 !