

***Interactive comment on* “Towards the response of water balance to sugarcane expansion in the Rio Grande Basin, Brazil” by F. F. Pereira et al.**

Anonymous Referee #2

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General comments

In their paper "Towards the response of water balance to sugarcane expansion", F. F. Pereira et al. assess the hypothetical changes in

the hydrologic cycle, by simulation of different sugarcane areas under the same climate. The paper is well structured and in the 1st

half of the MS well written and providing sufficient details to follow the calibration & validation procedure of their key-tool the

MGB-IPH model. However, there are some issues in the calibration procedure which need to be addressed and clarified by the authors. In

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the results section, the link between the biophysical conditions and the effects claimed to follow from the conditions is not always

clear (see details below). The discussion / conclusion section does not provide any comparative analysis with similar studies, nor as a

minimum, recycle the papers used in the introduction.

In the two decades covered by the study, many other conditions may have changed as well. I consider it as indispensable to inform the

reader about e.g. (dis-)intensification processes, in all relevant land-use types, i.e. sugarcane, pastures and cereals. It needs to

be shown, or at least discussed how de-/increasing plant growth and ET of ALL land uses and the forest were contributing to the effects

on runoff etc., currently exclusively attributed to sugarcane. Beyond, if crop yields (and thereby automatically ET and other

hydrological parameters) and land use have changed over time a different calibration & validation strategy is needed.

The results section is confusing in various sections, as the authors present a mix of soil, land-use, hydrological information, while

phenological information is lacking and in many instances the conclusions drawn from the facts they present seem not logical. This

problem could either be attributed to biophysical interpretations or grammatical problems, but as a result, I as a "professional

reader" in many parts either do not understand their logic or disagree. Thus, I suggest to restructure the text, more clearly addressing

causal links & effects separately and investing a couple of additional phrases in the

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results section to enable readers to understand

the simulation results. Alternatively, the authors could try to identify some generic effects, which they introduce in a section before

providing the sub-basin details. An additional figure showing typical annual ET pattern of the 4 major land use and landcover types may

help to understand also runoff and subsurface flow pattern of all 4.

The authors do not link their findings with similar field or simulation studies, making it almost impossible for the reader to

interpret the significance of the findings.

To summarize, the paper has a high potential, but considerable additional efforts are needed before this paper can be accepted.

Detailed comments

case study region: Please provide more general hydrological information characterizing the (sub-)basins and helping the reader to

interpret the changes caused by the simulated sugarcane expansion (rainfall, ET, runoff ratios or percentages e.g. in dry & rainy

season). Water users: Unfortunately, no information is provided about other water users in the basin and how the patterns and amount of water

use has changed over time between 1993 and 2007. As all water users influence the gauges used for calibrating the model, it is

absolutely essential to take them into account. Calibration & validation: To avoid e.g. land use related biases in the calibration period, the authors could run the calibration for

the 1993 conditions for instance from 1988 to 1997 (they claim to have all data since

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1970), and the validation with year 2000 land use

from 1995 to 2004 (or from 2002 to 2011 if you validate with 2007 land use). In general terms, the authors need to address the issues

of (i) land-use intensity and (ii) other water users potentially affecting runoff, as changes in (i) and (ii) may strongly influence

results and conclusions. In the current version of the MS it is unclear whether 1993 land use is used for validation, which would make

little sense given the partly large land-use change between 1993 and 2007.

p. 5566 | 12: please replace last word river by Rio p. 5567 | 9: Spatially distributed model?? p. 5574 | 16: I consider the historical land-use constellation NOT to be scenarios, as the term scenario is usually used for potential

(land use) constellations or plausible options of future developments. Thus, I suggest to use the term scenario only for the EMBRAPA

case. p. 5574 | 23: Please explain the "warming-up period". p. 5578 | 14 ff: In my understanding conditions and effects don't match. Please rephrase to explain the causal links much clearer. p. 5579 top: Why would "the lower soil moisture content at this basin", lead to "higher evapotranspiration rates"? To my knowledge, low

water availability in soils LIMITS ET, assuming that water is a limiting factor. Unclear. Please rephrase and explain better. p. 5579 Furnas sub-basin: The logic of the explanations seems wrong. For instance, why would humid soils keep ET down? see above. p. 5584 | 23: only Rembrapa presented, what about the others? Discussion: A serious discussion is lacking. The authors do not link their findings with similar field or simulation studies, making it

almost impossible for the reader to interpret the significance of the findings. conclusions: only here the authors provide a little bit of phenological information. This info is

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urgently needed in the results
section. Please invest more efforts to critically discuss your study.

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