Dear Editor and Authors,

General comments

The authors combined an energy-economic model GCAM and a group of models on hydrology and water use including a water balance model GWAM and global water demands models for six sectors. In this paper, the authors mainly introduced the formulation of GWAM and the results of water scarcity assessment using a well known index devised by Raskin et al. (1997). They reported that in some parts of the world, water scarcity considerably increases in the middle and the end of 21st century under a SRES A1Fi compatible scenario. They also briefly discussed the water use projection under the scenario but the details are described elsewhere (Hejazi et al., 2013, Technol. Forecast. Soc., Submitted).

Integration of energy-economic models and hydrological models with details on water use and management is crucial to analyze the impact and policy of global climate change. The authors have devoted themselves to this challenging task and achieved the integration to a certain extent. The manuscript is basically well prepared. Nevertheless their success, I have to take a critical position to this work as a research article of HESS, one of most important journals in the field of hydrology, because of the reasons shown below. In short, this paper should be a supplemental material of the accompanying paper by the same authors.

Specific comments

The manuscript consists of six sections.

Introduction: The authors reviewed earlier works on land surface models and global water balance models, but it is too superficial and unfocused in the current form. A lot of important papers are missing, particularly the development of land surface models (page 3330, line 6-21). If I understand correctly, the key strength and uniqueness of the authors work is integration of energy-economic models and hydrological models. I'm wondering why the authors didn't focus on the history of this effort, for example Hayashi et al. (2013) and many others.

GCAM: No specific comments on this section, because it only briefly overviews the GCAM model.

GWAM: The authors introduced their water balance model here, which is one of the main content of this paper. However, the author's water balance model is too simple to be discussed in detail in main text. The model is founded on the most basic water balance equation (dS/dT=P-E-R) and well established classical formulations. I think the authors can move a large part of this section to supplemental material because the most of the techniques are devised elsewhere. In the following part, the results of model evaluation is shown but it is less informative. For example, Figure 7 shows that in some of the basins, the error reaches 1000% (there is a plot showing that the observation is around 80 km^3/yr while simulation is around 800 km^3/yr). I understand it very well that the current global hydrological models subject to produce large errors in arid basins, but at least the authors should carefully discuss in which regions the model are more/less reliable, because this is crucially important to interpret the results.

GWDM: The authors introduce their global water demand model (GWDM) and some of the results of water demand projection, but again I need to be critical here. First, GWDM consists of six major sub-models (irrigation, livestock, domestic, electricity production, primary energy production, and manufacturing), but details are described in elsewhere. The only original content seems a description on how they spatially interpolated their projection from regions to grid cells. But it is a quite simplistic technique (i.e. weighting by population density, etc.) which has been widely used for a decade (Vorosmarty et al., 2000, Alcamo et al., 2003). Because the most of the contents are shown in elsewhere, I got an impression that the whole section is less scientifically important.

Water scarcity: The authors conducted a global water scarcity assessment using a conventional water scarcity index of Raskin (1997) under a global scenario of SRES A1Fi. A grid-based calculation of the index first appeared in Vorosmarty et al. (2000) and hundreds of similar reports have been published since then. I hardly believe the necessity to repeat such an exercise here in HESS. The authors may claim that the results are consistent with energy-economic factors. Then, the authors should emphasize the difference and advantage of their results compared to those of conventional stand alone hydrological models.

Discussion and conclusions: As mentioned above, the key contents are less novel and original, the discussion and conclusions are basically conventional.

In summary, although it is well written as a scientific report, the manuscript contains not enough novel and original contents as an independent research article. I believe the accompanying paper includes many new challenges, I recommend that two papers should be merged. A substantial amount of the contents of this manuscript should be placed in supplemental material because they are originally published elsewhere as discussed above.

Technical comments

Page 3330, line 24, "WaterGAP/WEHY": What is WEHY?

Page 3330, line 26, "Gertena et al." reads "Gerten et al."

Page 3330, line 27: "Wide'n-Nilsson" reads " Widén Nilsson".

Page 3330, line 28: "H07/H08 (Hanasaki et al., 2006, 2007)" reads "H08 (Hanasaki et al., 2008)", since H08 is short for "the model described in Hanasaki et al. (2008)".

Page 3330, line 28: "Weiland et al." reads "Sperna Weiland et al."

Page 3341, line 3, "Figure 9 shows…": According to the GCAM, irrigation water demands grows throughout the 21st century. However, some of earlier works showed pessimistic view in increase of irrigation water (e.g. Alcamo et al., 2003; Rosegrant et al. 2009). Because the projection of irrigation water demand critically affects the results of the study (including the accompanying paper), the authors need to explain here in detail how irrigation demand is modeled in GCAM.

Page 3344, line 23: "cumulative probability density function" reads "cumulative distribution function".

Page 3346, line 3 "Thus, both water demand and supply are driven from the same set of assumptions about population and income growth, technological change, and emission scenario": In my understanding, all earlier assessments under SRES and CMIP3 meet this condition. I'm not convinced that it is the advantage of this study.

Page 3346, line 26 "Indi's" read "India's"

Page 3347, line 3 "These high water scarcity values indicate that the scenario is likely infeasible from water perspective, since such high water stress would typically lead to the adoption of water conservation technologies with implications for other human choices": Does GCAM include this feedback mechanism? If so emphasize it more in text.

Page 3348, line 13, "Wada et al.": Add the year of publication.

Page 3348, Appendix: The description of Hargreaves method is quite easily found in the textbook of hydrology, and the whole section can be omitted.

Figure 3: Because all information is in text, this figure can be omitted.

Figure 10-14: These figures are not very informative, because neither the models nor the results are discussed in detail.

References

Hayashi, A., Akimoto, K., Tomoda, T., and Kii, M.: Global evaluation of the effects of agriculture and water management adaptations on the water-stressed population, Mitig Adapt Strateg Glob Change, 18, 591-618, 10.1007/s11027-012-9377-3, 2013.

Rosegrant, M., Fernandez, M., and Shinha, A.: Looking into the future for agriculture and AKST, in: Agriculture at a crossroads, edited by: McIntyre, B. D., Herren, H. R., Wakhungu, J., and Watson, R. T., Island Press, Washington, D.C., 2009.