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Earth System Dynamics Discussions

Interactive comment on "Comparing projections of future changes in runoff and water resources from hydrological and ecosystem models in ISI-MIP" *by* J. C. S. Davie et al.

Anonymous Referee #1

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GENERAL COMMENTS

In this paper, Davie et al. compare different projections of climate change effects on runoff for different global simulation models. They explicitly distinguish between hydrological models (7) and ecosystem models (4). The main difference between those 2 types of models is that the ecosystems models include vegetation dynamics and CO2 responses, whereas the hydrological models do not (line 25-27). The authors hypothesis is that the two types of models will show different runoff responses, due to the effect of vegetation processes that are considered in the vegetation models, but not in the hydrological models. Specifically, they want to test the effect of vegetation dynamics and

of plant CO2 responses on simulated runoff changes In order to focus on the differences between the impact models, and not the whole range of uncertainty in climate change impacts, the authors choose to use only one RCP, and one GCM to force their models with. The authors conclude that ecosystem models generally tend to project larger increases and smaller decreases in runoff than the hydrological models. This means that the processes that are unique to the ecosystem models tend to decrease evaporation. They plea for a wider range of impact models to be used in impact studies and planning water management than the hydrological models used usually.

The paper shows an important issue, namely that there is even more uncertainty about the effects of climate change on future runoff than previously considered, because the indirect effect of CO2 concentration which is usually not considered in hydrological models. However, I have some difficulties with the general objective of the paper, the way the results are presented and the conclusions drawn from the analysis, which I would like to see improved. Also, in many cases, the model results are presented and described as they are, without a further explanation of the observations. All in all I find the paper not yet readable and therefore not yet suitable for direct publication. While reading the paper, I got very much the impression that the paper was written quite quickly. I found it difficult to read and follow the line of thought, and it looks messy in places. For example, there are author notes still included in the text where references need to be inserted (see technical corrections), there are terms introduced without explanation (e.g. the "minimal" settings simulations, the JULES model (r 23 p 284), some more information on ISIMIP programme). The main objective of the paper remains somewhat unclear. If the main objective is to draw general conclusions on the runoff response of ecosystem models vs hydrological models? Or to discuss each model individually? The figures might be changed so that the conclusions that the authors draw from them are more convincing.

SPECIFIC COMMENTS

Some illustrations that illustrate the general comments: - Title. I suggest to remove the

water resources from the title, since it is not part of the paper. The paper only focusses on runoff. - Abstract. The main conclusion of the abstract seems to be that there is more uncertainty in runoff response to elevated CO2 than previously considered. However from the conclusion part of the paper I understand that the main message is to include ecosystem models in water resource management and impact studies, because they give different results. This is somewhat unclear and should be consistent. - Definition of ecosystem models is that they inlcude vegetation dynamics (line 26, p 282). In table 1 it is shown that VISIT model does not include vegetation dynamics... - It would be very good to have a short description of the used models in the text. Now the models are presented without any description on main differences - Line 21, p 283. Whether the model shows sensitivity for CO2 depends very much on the model. Before it was stated that most hydrological models do not consider CO2 concentration at all, therefore they are not sensitive to CO2. I agree that the difference between ecosystem models and hydrological models is not always very clear (in other studies LPJmL and JULES are regarded as hydrological models), but that should be explained. Now it is rather confusing what are the differerences between the two. - Line 23, p 284 introduces JULES without any references or description - Line 21, p 284. It would be good to know what the 'minimal settings' are. - Line 2, p 286. Why not use basins? - Line 4, p 286. Which regional differences? Unclear sentence. - p 287 first paragraph is already described in the intro. - Line 10-19 p 287 start with a discussion of this study's results, before the results are even presented. I think it should start with fig 1 and after that the discussion on what is presented in fig 1 etc. This would make the - Line 21 p 287. What is the definition of 'consensus' here? Does it mean that 50% of the models show the same signal? OR also in the same magnitude class of change? This needs some explanation. - Line 25 and further p 287. It is not very strange that other patterns of change are found than in other studies, because they might have forced with different GCMS which show very different patterns of precipitation change regionally. Also Hagemann et al (2012) used three different GCMs, therefore, runoff changes between this study and Hagemann et al. cannot be directly compared. Tang

and Lettenmaier foudn that spatial patterns are stable across emission scenarios, but NOT across GCMs!! - Line 8, p 288. What is actually said here is that the ecosystem system models are generally the upper points in the graph. I find the figure too messy to draw this conclusion, because all the experiments (including sensitivity) are included in the graph. I think the figure would be stronger when just the 'standard' runs are presented, with changing CO2 and dynamic vegetation. It would become much clearer and convincing in this way. - Line 18 p 288. Why does the fact that the points are not on a 1:1 line indicate that there are regional variations? I don't understand this conclusion. I would say that it means that not all additional precipitation is added to runoff, but also partly enhancing evapotranspiration. - Line 21 p 288. For readibility, I suggest to add the abbreviations of the regions as used in the figure here. - Figures (3 and 5) have too many lines to be readable. Since the individual models can not be distinguished, it is also possible to show the ranges for each type of model. - Line 23, line 290 Over Wester Africa.... hydrological models. This conclusion cannot be drawn based on fig 5, because hydrological models are not shown in there. - Line 16-20 p 291. Isn't this very logical? In some locations there won't be a vegetation shift, in some locations from high to low vegetation and in other lcoations from low to high vegetation. - Line 22, p 292. Changes in vegetation had more effect on what? - Line 11, p 295. What is the definition of runoff deficit? - Line 15, p 296. It would be helpfull to start with a summary of the main differences.

TECHNICAL CORRECTIONS

- Reference Falloon et al, 2012a and 2012b are the same. - Pag 285, line 14, text between brackets should be removed. - Page 286, line 10, include a reference here - Fig 3 caption. Add reference here. - P 295, line 11. Insert 'constant' before CO2.

Interactive comment on Earth Syst. Dynam. Discuss., 4, 279, 2013.