

### Overall comments

Sutanudjaja et al. combine a distributed land surface model with a groundwater model for two large basins in Europe as a test case for a new methodology of using existing models and available data. The idea is timely and innovative and the manuscript is clearly written and well illustrated with nice figures. I think the authors nicely and clearly state the purpose and limitations of the study both in the intro and discussion.

I do not have any significant problems with the science the authors present but the paper is quite unbalanced in that 25 of the 35 pages (71% by length) is the methods section called “Model concepts, parameterization and forcing data”. I realize that the purpose of this paper is primarily to test a new methodology so it is important to clearly describe the methodology but I think the authors are much too detailed throughout most of the methods section. If the method has been described by previous paper, do not reproduce the description, just refer reference the previous paper and say how you changed it. The authors should aim for maximum 30-40% of the paper length being methodology which will naturally mean the results and discussion/conclusion section has to be longer and more full as well. One idea to condense the methods section significantly is to create a table like this (which make work really well for some sections and parameters and may not work for others). Wmin shown as example...

Parameter	Parameter name	PCR-GLOBWB-ORI	PCR-GLOBWB-MOD	Reason/explanation for change
Miniumum local sub-grid water storage capacity	Wmin		0	Wmin less important at small scale

My two scientific concerns about the paper are

- 1) The authors ignore lithology maps [Dürr, et al., 2005] or recent permeability maps [Gleeson, et al., 2011] and instead make up (a somewhat fictitious) spatial permeability distribution based on the assumption (low areas = sediments = high K) and (high area = mountains = low K). At the very least it is important for the authors to show that they tried to use the previous maps which are much more based on actual geology (and report on the results). Ideally, the authors should use this lithology/permeability data in this paper or future papers. Since the authors do not use the Durr map, this should be removed from the intro (Pg. 3, line 27). I also question the use of 100 m<sup>2</sup>/day as a base case transmissivity value. Gleeson, et al, [2011] calculate a global k mean of ~5e-14 m<sup>2</sup> which is ~4 m<sup>2</sup>/d for a 100 m thick aquifer. I suggest the authors justify their base case permeability value using this new permeability compilation.
- 2) I think the authors should make the potential for non-uniqueness in their model results more clear. It is relatively easy to match (although the authors nicely make it clear they are not calibrating) flow observations but it is more difficult to model transport, as [Konikow, 2011] makes clear in a recent paper.

1. Does the paper address relevant scientific questions within the scope of HESS?  
yes
2. Does the paper present novel concepts, ideas, tools, or data? yes
3. Are substantial conclusions reached? yes
4. Are the scientific methods and assumptions valid and clearly outlined? yes
5. Are the results sufficient to support the interpretations and conclusions? yes
6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?  
yes
7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? yes
8. Does the title clearly reflect the contents of the paper? yes
9. Does the abstract provide a concise and complete summary? yes
10. Is the overall presentation well structured and clear? Yes although too detailed in methods as described above.
11. Is the language fluent and precise? yes
12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes, I like Table 2.
13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? See above
14. Are the number and quality of references appropriate? Yes
15. Is the amount and quality of supplementary material appropriate? N/A

**Detailed comments:**

- Should make it more clear why the models were run uncoupled.
- A few sentences about coupled vs. uncoupled computer run times would be useful.
- Page2, Line 1: I am not sure that it is just lack of hydrogeological data that limits large models – it is also model platforms
- Hydraulic conductivity should be capital K and Sy should be called specific yield.
- Section 2.3.2 is strange – Van Beek should not be listed as personal communication if he is a co-author.
- Pg. 23, Line 13: dgw should be described as depth to water table
- Pg. 25 “Big Lake” is colloquial – change to large lake.
- Add a map of Europe with basins shown on it to Figure 1.

**References:**

- Dürr, H. H., M. Meybeck, and S. H. Dürr (2005), Lithologic composition of the Earth's continental surfaces derived from a new digital map emphasizing riverine material transfer, *Global Biogeochem. Cycles*, 19.
- Gleeson, T., L. Smith, N. Moosdorf, J. Hartmann, H. H. Dürr, A. H. Manning, L. P. H. van Beek, and A. M. Jellinek (2011), Mapping permeability over the surface of the Earth, *Geophys. Res. Lett.*, 38, L02401.
- Konikow, L. F. (2011), The Secret to Successful Solute-Transport Modeling, *Ground Water*, 49, 144-159.