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Interactive comment on "Simulation of groundwater and surface water over the continental US using a hyperresolution, integrated hydrologic model" *by* R. M. Maxwell et al.

Anonymous Referee #2

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The paper is very well written and I appreciate the immense effort to set up and run the model. However, for a number of reasons the paper is not publishable. There are number of issues, both obvious and hidden ones. The general claims based on model outcome are neither substantiated nor discussed in detail and/or are mainly trivial. A critical discussion on how the extreme uncertainties (e.g. the underlying geology) might challenge the bold claims on scalability is conspicuously absent. What is most troubling though is the presentation of the results. The paper suggests a reasonable to good model performance. Unfortunately, critical data that would allow the reader to evaluate how well the model performs in reproducing hydraulic heads are missing. We see scatterplots over scales that simply cannot be wrong. Over large spatial scales

groundwater always roughly follows topography, and a plot with altitudes ranging from 0-4000m will always look good, no matter how bad the model is. What would be really interesting instead is a scatter plot showing simulated and observed depth to groundwater. This would allow more transparent insights into model performance, but we are only shown histograms. Why? The flow model could be completely wrong in predicting spatial patterns yet the match of the histograms might still be excellent. I find this presentation very misleading and not an appropriate way to demonstrate model performance. Also, the comparison between simulated and observed river discharge looks terrible, even on a log-scale. The model should simply not be used. There is a lack of novelty. Instead, there are several claims on novelty that in fact are not at all new at all (see below). The conceptual setup of the model is somewhat incomplete and undermines the philosophy of a fully integrated approach by externally calculating infiltration rates (P-ET) . Fractured and karstic systems are not modelled, important processes are missing yet little is said about this. For all these reasons, the paper should be released. Below I provide some discussion concerning the points I made above.

Novelty:

âĂć The numerical model is not new. A proof of concept on high resolution modelling has already been published by Kollet. That the spatial scale can be increased with increasing computational power is in itself not a novelty. Going to much higher resolutions across this spatial scale (e.g. 20*20m horizontal and centimeter scale vertically) would however merit a publication.

âĂć Page 7326, line 27: The authors claim here that it is a novelty that streams form without predefining their presence in advance. This is simply not true. The models discussed in the introduction are all capable of this. Just by increasing the spatial scale of a feature does not make it new in any way. Also, the authors claim that their approach will capture the complete steam network. This is in theory true, but to what extent the model captures the network is dependent of the spatial resolution and quality of the DEM. On a 1 km scale as used here one is very far from capturing the complete network.

âĂć There have already been previous attempts to model the entire US. That a fully integrated model was used here is not the great step ahead, especially given that integrated refers only to the interaction between surface water and groundwater as mentioned above, not to the calculation of the important dynamics between infiltration, ET and recharge.

âĂć The paper claims to provide one means to bridge point measurements of hydrologic states and fluxes to continental scales (page 7319, line 5). This is an extreme overselling of the model capabilities, especially given its poor performance even in steady state, undiscussed uncertainties and its incomplete process conceptualization.

âĂć The general hydrologic behavior in steady state is well established for the US. There is nothing added with this model.

Conceptual model

âĂć Why was only a subsection of the US modelled? The authors had to make an award choice of a no-flow boundary condition.

âĂć It is a pitty that ET is not simulated, in my opinion the greatest advantage of fully integrated models. I am aware that to adequately simulate ET much finer vertical resolutions are required. But how good are the P-ET maps used? By not including these important dynamics the authors give away one of the greatest advantages of fully integrated models. The only advantage they retain is that the location of the rivers must not be predefined. However, I see no disadvantage to predefine the location of all medium to major rivers on this spatial scale, given that fact that most of the rivers are too small to nicely flow downhill in a 1km DEM anyway.

âĂć Nothing is said about the correction of the DEMâĂŤIn such fully integrated models, water will follow topography. Using a DEM with a 1 km resolution without any correction is not advised, as topography along rivers and drainage networks will go up and down

in the model, preventing the generation of small to medium streams.

âĂć The applied permeability map by Gleeson is not fit for purpose. In the Gleeson paper a number of warnings and constraints to use the map are highlighted that are relevant to this paper. The fact the permeability changes significantly across state borders hints to major unresolved issues on the subsurface conceptualization.

âĂć The vertical resolution is very rough to adequately model Richards equation.

âĂć A range of processes are missing that could be captured with easier models. For example interception. How is this considered with the P-ET maps? Moreover, snow processes are not implemented, and as mentioned above the model cannot simulate Karstic or fractured systems. This is important, as Karst aquifers are frequently found in the US. I suggest consulting the nation-wide karst-maps provided by the USGS. Nothing is mentioned on these missing aspects of the model.

âĂć It is a major disappointment to see how bad the comparison between simulated and calculated stream discharge is, even though the model is in steady state and the results are presented on a log-scale scatter plot. The presentation of the somewhat arbitrarily chosen histograms (e.g. median and 75 percentile vs. a steady state simulation) cannot coat the fact that the model is far from capable to adequately simulate hydrological processes on this scale.

âĂć Important information is missing, e.g. roughness in the overland flow domain or details on numerical performance and convergence.

Given that I have very little confidence in the model I do not think it should be used to develop any general conclusions. Nevertheless, some conclusions have been made, most of them trivial. For example, the equal importance of hydraulic conductivity and recharge (page 7330 line 14) in reproducing hydraulic heads is a lesson learnt in the early if not first modelling lectures. That there is a relation between drainage area, aridity and surface flow (page 7331, line 14) is well known and not very surprising.

The upper limits shown in the graphs are not further discussed. Given the major yet undiscussed uncertainties in the model there is no basis to make general conclusions.

Other comments

âĂć The title is misleading. 1km of spatial resolution is far from "Hyper“ as suggested by the title, nor is it "high“ as suggested in the abstract. The authors might refer to their own publication (http://onlinelibrary.wiley.com/doi/10.1002/hyp.10391/pdf, section WG3) where hyperresolution is claimed to be <1km. Clearly, the term “Hyper” is always relative to the scale considered. However, the comparison should be oriented towards the scale of the relevant processes, not the scale of the model domain. With the exception of the largest streams, the majority of runoff-processes will be dominated by terrain features and processes that have to be resolved on spatial scales significantly below 1 km2. Finally, not the entire continental US is modeled as suggested by the title, it is a rectangular sub-area with no-flow boundary conditions.

âĂć The fonts on some graphs are very hard to read.

âĂć The authors confused all the first and second names in the reference to Nir.

Interactive comment on Geosci. Model Dev. Discuss., 7, 7317, 2014.