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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Harnessing Big Data to Rethink Land Heterogeneity in Earth System Models" *by* Nathaniel W. Chaney et al.

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I do not want to be too critical because I fully understand why this work is being done and so much effort has gone into it, and I was arguing for the inclusion of more heterogeneity into land surface parameterisations more than 20 years ago. But it is also an excellent reminder to me as to why I have chosen not to work in this area - the underlying "science" is really not very satisfying. There is an implicit assumption throughout the paper that the fine scale model, based on the various databases available, is correct. Further results are essentially expressed relative to this fine scale model and show that it is indeed important to account for heterogeneity (to the extent of the order of 300 deterministic tiles per grid square). But how can we still ignore the difficulties of parameterising the relevant processes and the resulting uncertainties in effective pa-

rameter values and how that might impact on the appropriate complexity of models to be considered. There are much simpler ways of incorporating heterogeneity into such predictions – is the use of 300 deterministic tiles really the best strategy to match actual landscape scale fluxes and integral measurements such as stream discharge? And to ensure that such heterogeneities are reflected in longer term future predictions?

So I would suggest that this is a paper that is acceptable with some minor modifications, but it is quite the wrong paper for what is needed.

Some specific comments.

P2 L5 Beven and Kirkby might be a relevant reference but not in this context - it did not deal in any way with large scale hydrology.

But the next sentence is also wrong – TOPMODEL was designed with a view to handling sub-grid variability in hydrology that would then form the basis for other predictions (including land management effects) – we were both interested in predicting sediment transport at the time. I also later extended it to allow for variable infiltration and conductivity characteristics and more explicit surface energy balance calculations. Also same issue on P12 L7

P2 L30 Beven and Kirkby did not use a mosaic approach – though there were later forms of TOPMODEL that did so, including TOPLATS (developed at Princeton!!!) and later Dynamic TOPMODEL based on multiple overlays and which forms the basis for HydroBlocks.

P9 L1 Why is this expected? Upslope elements surely contribute "baseflow" to those tiles adjacent to channels? Or do you need to explain what you mean by baseflow (there is an interaction here with your definition of channels – there would be many downslope fluxes in small, possibly ephemeral streams at scales much less than 100000kmˆ2)?

P9 L18 patterns in ET. This is also evident from remote sensing derived estimates –

but there are two aspects to this – one Is the uncertainty associated with both derived and modelled estimates (see papers by Franks et al. e.g. WRR 1997, 1999), the other is whether the type of model being used here can really be supported by the data (see Bashford et al. HP2002). Both issues are worthy of mention given that this paper considers only a comparison of model runs from a single structure without uncertainty.

P12 L23. This is a claim too far. There has been a lot of previous work linking topography, hydrology and energy balance/evapotranspiration and vegetation interactions even if not such detail (e.g. Franks et al.; Quinn et al HESS, 1998; Blazkova et al. WRR 2002). Then there are all the Teague and Band RHESSYS papers, and the Topmodel based land surface scheme used by MeteoFrance (ISBA-TOPMODEL, Vincendon et al JH2010). . .. And more!!

P12 L28 It is clear that these heterogeneities make a difference, and that there are more that we can think of, including the biophysical feedbacks, that would undoubtedly result in more complex parameterisations. BUT, in complex terrain with heterogeneous cover that also introduces all sort of local boundary layer effects that have proven too difficult to deal with at the micrometeorological scale. And yet, at the landscape scale the actual heterogeneous latent heat and gas fluxes are additive, effectively filtering out much of the variability (the REA idea that the distribution might be important but the pattern may not be) – so is there not an issue of appropriate complexity of model constructs that needs to be addressed here (e.g. Bashford et al HP2002) – especially given the uncertainty with which the parameters of your model structure can be defined.

P14 L17. But that only considers fitting one model to another. How can you simply assume that your fine scale model is correct. It is not (no small scale channels, preferential flows, lack of knowledge of role of regolith and geology, parameterisations of biogeochemical processes etc) or at best involves significant uncertainty in effective parameter values – it does not consider whether the underlying model structure and parameters are adequate to reproduce observations, and what those observations might mean in this context (discharges, FLUXNET, surface temperature images,. . ...).

Section 5.3 is really addressing the wrong problem.

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