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Hydrology and Earth System Sciences Discussions

Interactive comment on "Catchment classification: hydrological analysis of catchment behavior through process-based modeling along a climate gradient" *by* G. Carrillo et al.

Anonymous Referee #2

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General Comments

This manuscript summarizes research into identifying the relationships between key landscape parameters and hydrologic signatures using a process based modeling methodology. Finding these relationships is very important for identifying similar catchments for the purpose of prediction. The authors argue that because the values of many of these parameters are unobtainable from field observations at the catchment scale, one possible means may be to deduce them using process based models calibrated to streamflow. The authors find some good results, especially relating some specific dimensionless numbers reflective of catchment function to hydrologic signatures. However, there is much information to be gained in where this methodology was less successful (see comments 5 and 13 below). I would encourage the authors to embrace these poor results. The manuscript could be even more intriguing if the authors interpret poor results as well as the good ones. This would provide valuable insight into the methodology that is currently lacking in this version of the manuscript. My specific comments follow.

Specific Comments

1) Abstract: Perhaps the third sentence could be better phrased as: "Where there are important subsurface properties that cannot be readily measured, the skill of classification may reflect the amount of cross correlation between observable landscape features and unobservable subsurface features."

2) Fourth sentence of the abstract; Isn't the power of generalization always a function of the sample dataset? This is not a unique problem to empirical methods, but the authors seem to imply that empirical approaches are particularly prone to this constraint. It might be my interpretation of the sentence, but maybe a more general statement would be in order.

3) Introduction: Second paragraph; the authors imply that subsurface parameters are "unobservable" and that surface parameters are not. To be frank, I do not understand why some continue to perpetuate the myth that the subsurface is impossible to parameterize. Rather, it's the myth that it's not impossible to parameterize canopy conductance because someone can see the forest that is perpetuated here. I would argue that it is just as difficult to parameterize the canopy as it is to parameterize the subsurface, and for similar scaling reasons. The authors could extend their argument to all catchment scale observations without rendering their basic thesis statements incorrect.

4) Introduction, second paragraph: The authors make some subtle mistakes in logic throughout the manuscript that must be addressed. For instance, while vegetation

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type and rooting depth are landscape features the latter is a model parameter while the former is used to deduce model parameters. This muddies the argument a little bit, as it is canopy conductance or light use efficiency that needs to be parameterized, neither of which is observed. Could I suggest the authors either compare vegetation type to soil type (which is very observable) or light use efficiency to rooting depth?

5) Introduction, end of second last paragraph: Several years ago researchers in Canada used parameters from a hydrological model calibrated to streamflow within an atmospheric model land surface scheme, with mixed results. The limited degree of success was exactly because of the reasons the authors allude to with their reference to Wagener et al. The parameter set became unique to the hydrological model. This is the key disadvantage to the authors' approach if anyone were to use the correlations discussed later in the manuscript to predict flow in an ungauged basin in the southeast USA. While the authors are fully aware of this limitation, as I alluded to in the general comments, I do not believe they discuss it enough, especially in regards to framing the value and applicability of results, or the impacts of the possibility of completely different results if another model were selected.

6) Introduction, beginning of last paragraph: There are a few typos scattered across the manuscript. This one is: ". . .. to analyze hydrologic response across many catchment(s) in the USA." A detailed proof reading is in order.

7) How the model solves the land surface energy balance is not clear. Both turbulent fluxes can be estimated without closing the budget following eqs. 14 and 16, and evapotranspiration is divided using eqs. 17 and 19. Are these equations (and eqs. 15 and 18) solved iteratively with eq. 13 until the net ground heat flux is zero? If this is so and the forcing data only includes downward radiation and albedo, how is emissivity derived?

8) Modeling results: Figure 4 does not show that the model has captured the average annual water balance, but only the annual runoff coefficient. Figure 6 details that the

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model efficiency to produce observed hydrographs is moderate to poor. Some NSE values are in the 0.3 – 0.4 range.

9) Just something to think about: if the model cannot reproduce individual hydrographs well, but can only reproduce well different modes of response (i.e., FDC slope, R/P), is it really a parsimonious model structure when applied for the present purpose?

10) The authors refer to data several times in section 4.3 that are not shown. I do not know if HESS has ancillary online data locations where it could be shown, but that could be helpful to future readers.

11) Section 5.2: Could I suggest that a future line of research may be to apply principal components or canonical correlation analysis to regionalization exercises?

12) I understand why the authors used the nomenclature they did for the dimensionless numbers, but it is still very difficult for the reader to flip between different tables and figures to discern which terms are being discussed. The text on Figure 12 was very helpful. Perhaps including this type information in the captions of Figures 10 and 11 would help.

13) The authors need to explicitly highlight which six are the snow dominated catchments. The model's ability to deal with snow dominated catchments appears somewhat limited perhaps because of the degree day approach to melt, the inability to express frozen ground, and the assumption of zero ground heat flux. The result seems to be some of the poorest NSE values from basins that are likely to be among the snow dominated ones (Rappahannock, Bluestone, Potomac, East Fork White), but the reader cannot know for sure. The lack of significant correlations among model parameters and hydrologic signatures from cold catchments may be a signal that the model performs less well in these conditions. Something that the authors' do not seem to have considered is that this result may imply the importance of model structure to successfully identifying correlations between signatures and catchment properties using this type of modeling methodology. These ideas and more critical discussion of the ap-

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plicability of the results of this type of research methodology beyond the realm of the sample set would help the manuscript. The authors do mention some ideas in the first paragraph of Section 5.4, but more insight should be provided and would improve the final article.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 4583, 2011.

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