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.

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We thank the reviewer for his/her careful review of our manuscript. Below we provide a detailed reply to the comments.

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".

We thank the reviewer for his/her assessment of our article.

"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".

We agree that one can learn as much (or even more) from poor results, and we will modify the article according to the specific comments below.

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."

The original sentence tries to convey the message that not all catchment characteristics can be observed at the appropriate scale. If surface characteristics that are readily observable would provide us with enough information about subsurface features, a catchment classification system aimed at explaining hydrologic response from climate and catchment characteristics would be much more efficient. However, any attempt to link climate and catchment characteristics to hydrologic response is limited in explaining the observed spatial and temporal variability of catchment response (see e.g.

Sawicz et al., this issue). This is a reflection of the (lack of) amount of cross-correlation between surface and subsurface features, and therefore other methods (such as the one discussed in this paper) are needed to compliment the catchment classification system. We feel that the original sentence expresses this more clearly than the proposed edits.

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.

We agree with the reviewer and will remove that sentence.

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.

There is a difference between "observing" and "parameterizing". We agree with the reviewer that the subsurface can be parameterized, as we clearly illustrate in our work. There is valuable information about the subsurface (geologic maps, soil data bases, below-ground vegetation characteristics, etc) that can be used by the hydrologic modeler, but certain subsurface characteristics (effect of heterogeneity on 'effective' hydraulic properties, to name one) will never be available at large spatial scales (unless some revolution in geophysical instrumentation takes place). We agree that it is equally difficult to obtain for instance canopy parameters, such as stomatal conductance, from

C2321

vegetation and land use maps, and therefore these need to be parameterized as well (as done in our study). We will modify this paragraph and discuss the issue of parameterization in more general terms, including the surface properties that define hydrologic response.

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

This is a good point and we will edit our manuscript to remove such mistakes.

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.

We completely agree with the reviewer. In fact, in Section 5.4 we make exactly this point: ". . . model construction is to some degree subjective and different hydrologists will develop different generic catchment models with the same purpose of capturing hydrologic response. Therefore, model time scales derived from individual model com-

C2322

ponents are not universal and will depend on the model construction. Model inter-comparison is needed to check to what degree different model formulations will lead to different conclusions about the cause of hydrologic similarity". We will expand this paragraph to further reflect this reviewer's comment.

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.

Thanks for catching this typo, we will screen the text carefully when submitting a revised version of the manuscript.

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?

The reviewer is correct that we solve the energy balance iteratively for surface temperature. We will add an equation that defines the net radiation in function of incoming and outgoing radiation, and specify that the surface emissivity is constant and assumed independent of surface temperature. This is a reasonable assumption.

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

Figure 4 present the average runoff coefficient. Assuming that the 10-year average water balance allows assuming storage changes to be negligible, the average runoff coefficient uniquely defines the average water balance. The second term, the evap-

otranspiration index (E/P), is simply 1-Q/P. We agree that some of our NSE values are moderate to poor (as stated in our manuscript). However, the good fit between observed and modeled flow duration curves suggest that our model reproduces accurately all modes (high and low, fast and slow) of response, which is important for our application of the model (explaining how catchments filter incoming climate forcing). The moderate to poor NSE values are mainly due to timing issues of storm hydrographs, a feature that we care less about in this study. The model can be calibrated to do a better job, at the expense of preserving some of the functional roles of model parameters, as mentioned in the Introduction.

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?

We agree that more parsimonious models could have been constructed using a top-down approach. We opted for a process-based bottom-up approach, however, to complement the work done by Sawicz et al. (this issue) which is exactly doing that (top-down modeling to explain hydrologic similarity).

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.

We can provide the information and will check with HESS whether there is an option to link Supplemental Documents.

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?

Sure, that's a great idea. Perhaps in future work we can use these techniques to gain more insight in correlation among catchment characteristics and model time scales.

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.

This is a good suggestion. We struggled with this issue ourselves, and we will add more information in the captions of Figures 10 and 11.

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 applicability 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.

We will clearly indicate in Figure 3 which six catchments are snow-dominated. We agree that the simple snow accumulation and snowmelt model is likely the cause of the poor performance. The issue is that without good simulation of snowmelt it is impossible to know when the catchment receives liquid water. This may affect the model parameters of the root zone (and other stores) and this may very well be the reason for lack of correlation between model time scales and catchment characteristics. We are

C2325

currently investigating these issues and hope to report about our findings in a subsequent article. We will extend the discussion to refer to this ongoing work.

Peter Troch June 21, 2011

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