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Discussions

Interactive comment on "Toward seamless hydrologic predictions across scales" *by* Luis Samaniego et al.

Anonymous Referee #2

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The authors make a nice case for the value of their multiscale parameter regionalization (MPR) method, analysing several aspects (and advantages) of the method. This is in principle a laudable thing to do. The manuscript itself, however, is quite frustrating to read. One the one hand it remains completely unclear what the novelty is. Large parts of the manuscript essentially repeat what has already been published earlier (as also acknowledged in the references provided). On the other hand, the argument remains in places quite imprecise with a lot of quite sweeping (and not necessarily well substantiated) generalizations. In addition, other approaches to parameter selection are quite outrightly dismissed while essentially no critical discussion on potential drawbacks or limitations of MPR are provided. In an exaggerated way, the authors present their MPR method, which I think has formidable potential, like in a product promotion folder. I think the manuscript would strongly benefit from (1) considerably reducing the redundancies

with previous work (sections 1-3 can be *substantially* shortened) and (2) taking on a more critical perspective towards MPR. I think that many in the community will agree that it is a great tool. Instead of highlighting this over and over again, it would be more instructive to learn were its limitations are to allow further improvement.

In general, I think it may be more interesting for a wider audience if the MPR technique was scrutinized and compared to other parameter selection and regionalization approaches *independent* of the model it is used for. In this manuscript it is applied exclusively with mhm if I understand correctly. In my understanding, it is a stand-alone method that should be applicable to any model. Would it not be fairer to be more consistent in the comparisons here, i.e. compare mhm with/without mpr and/or other models with/without mpr?

The bottom-line is that I have the feeling that two quite independent things are not clearly separated here: the regionalization technique (MPR) and the models (mhm, etc). Here the text needs to become much more precise. Right now it seems to the reader that MPR is compared to e.g. the HBV model. This is not a valid comparison as these are completely different things. In contrast, it would be excellent to make the fact that MPR is a standalone tool clearer, as this may result in more modellers actually picking up the idea for their very own models (which they may not do at the moment due to its perceived exclusive association with mhm).

Specific comments:

p.2,l.5: why only over time and not also over space?

p.2,l.10-12: please avoid subjective terms as "elaborate" or "sophisticated"

p.2,l.28-29: is this actually true? Why would process dynamics that emerge at larger scales and that integrate several processes necessarily reduce "realism"? It is surely possible, but I do not think that it is a physical necessity. In any case, what is the meaning of "realism" in a situation where most of the system is de facto unobservable?

How do we know if something is “realistic”?

p.2,l.33-34: this is a sweeping generalization. What is actually meant by that? Why should an observed quantity, such as for example the stream flow recession constant have no physical meaning? Of course it has, albeit on the scale of the observation.

p.5,l.15ff and elsewhere: many things are mixed together here and the logic is not convincing. For a meaningful argument they need to be carefully disentangled. Is this about models? About parameter selection/calibration procedures? Parameter regionalization? It reads as if MPR does not rely on calibration, which is not correct. and why should lumped and/or semi-distributed models not be run with MPR-derived parameters? Would this for a, say 100km2 catchment, not be the same as if running a distributed model with a 10x10km2 grid in mhm?

p.5,l.19 and elsewhere in the manuscript: much is made of “discontinuities”. However, the authors do not provide a clear definition of what they mean. Nature is, in places, discontinuous (e.g. forest vs. grassland, north vs. south aspect, sharp transitions in geology, breaks topography, etc). thus it is not clear why models should not represent these discontinuities. I suppose that the authors want to say that between individually calibrated catchments discontinuities can occur, where there are in reality no discontinuities. But this needs to be made clearer.

p.5,l.21-23: sure, but is this not also the case for distributed models and dependent on the calibration/parameter selection method?

p.6,l.29 and elsewhere: “CONUS”: not necessarily every reader will be exposed to large scale studies employing these terms. Thus please avoid the use of fashionable abbreviations without first defining them.

p.8,l.7: a question cannot be postulated. Please rephrase.

p.8,l.10-11: what is meant by “poor”. How do you define it?

p.9,l.3: over-parameterization is only addressed in MPR if simultaneously calibrated to

a high number of catchments and/or objective functions. Thus, it depends on how MPR is implemented and applied. Please rephrase.

p.10,l.17-18: how do you know that the parameters are “realistic”? See also comment above. Does this not also strongly depend on the assumptions in the upscaling relationships? It is always a question of how MPR (or other parameter selection techniques) are implemented and not a defining proprietary feature of MPR.

p.13, section 4: in many parts of the section it is unclear what is meant: the individual models or rather the parameter selection/regionalization techniques in the different model applications? These are different pairs of shoes and need to be carefully separated.

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