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Hydrology and Earth System Sciences Discussions

Interactive comment on "Applying PUB to the real world: rapid data assessment" *by* C. Jackisch et al.

Anonymous Referee #3

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General comments

This paper should be rejected. It is so chaotic and uninformative that I find it impossible to distil any scientifically relevant information out of it.

There is no real hypothesis, the data collection seems to be largely random, or based on ad-hoc decisions and practical constraints, and the discussion is hardly more than a largely qualitative description of the collected data.

I do agree that regionalisation is an important topic in hydrological science, and that many currently available methods rely on the availability of a large dataset (e.g., the plethora of studies using the US MOPEX dataset, the UK dataset of Wagener and

Wheater (2007) or the Austrian dataset of Parajka et al. (2007)). These methods are indeed difficult to apply to other regions of the world because of the lack of high-quality data, the very different processes that occur, or the nature of local management questions.

In this light, a method or framework to analyse the value of data as they become available would be very useful. The abovementioned studies have indeed highlighted that the methods that deploy the largest dataset are not necessarily more accurate than simpler methods. Other studies (e.g., Seibert and Beven, 2009) have highlighted that a limited amount of data has a strong capacity to constrain the parameter space of a model, while subsequently collected data are far less informative (at least, when one is not too interested in the extremes).

In many regions in the world and especially developing countries, it is very relevant to include factors such as collection time, financial resources and even human effort are valid considerations when making decisions on which type of data to collect. After all, resources are limited and decisions need to be taken urgently. I agree that such factors are often overlooked when abundant data are available.

All this supports the author's cause to investigate rapid data assimilation strategies. However, the study is extremely poorly worked out. The abstract is poor and uninformative. The introduction does not properly cover the relevant literature but is more a random selection of vaguely relevant articles. There is no rationale of how this study advances beyond existing knowledge. The materials and methods do not properly describe the type of data that were collected. Part of this may have to do with the setup of the field work in the Mod basin. Often study sites are indeed selected on an ad-hoc basis of available contacts, or opportunities, but if it turns out that the data are of such low quality or just plainly available such that hypotheses cannot be tested, or very little information can be obtained, than one should reconsider whether this is a useful case for scientific research. After all, would it not be much more relevant to select a basin with more data available, and treat it as data-scarce? At least then it is possible to re-

ally "assess" whether a limited subset of data is useful for making predictions. The fact that (virtually) no discharge data are available makes it a priori impossible to really assess the usefulness of data for hydrological prediction. Even in the type of study region (intensively altered, rural meso-scale basins in densely populated (sub)tropics), more intensively gauged basins are really not as scarce as the authors seem to believe.

Finally, the discussion and conclusions cannot be described in another way then simply chaotic (see specific comments). These sections, as well as the rest, suffer from inadequate and hugely obfuscated language (some examples are given in the comments section).

Therefore, I can only conclude that this manuscript was submitted prematurely. It should be rejected, and the authors should carefully think about the message they want to convey, formulate some hypotheses they want to test, design their field work around these hypotheses, and consider how to communicate in a scientifically rigorous manner.

Specific comments

7501/3: ad hoc assumptions: please clarify

7501/8: a theoretical discourse: despite the abovementioned issues of PUB often being analysed in a data-rich environment, I think this is a very strong claim that should be further substantiated.

1.3 study setup: it is unclear what kind of decisions are targeted. Nor is it clear why 10 weeks and one person where chosen. Did this just happen to be the budget and manpower the authors had available? If so, this surely is not a very “scientific” setup. Do they think this is representative for a “real-world” problem? If so, why?

1.4 scale: not large enough for remote sensing: yet, remotely sensed data are being used?!?

largely integrated system behaviour: how is this defined?

the regarded processes and entities: what processes and entities?

1.5. uncertainty

is uncertainty somehow quantified?

1.6. overall aims:

What is an optimal land use strategy for the Mod basin?: To be honest, I do not see this question answered in the manuscript. At best, the aims could be described as “assessing whether the available data are of any use in informing land use management decisions in the Mod basin”.

methodological questions: I do not think any of these questions are answered in the manuscript either

a targeted approach: I see a very much ad-hoc approach with very little structure

2.1 Scarce data basis

- remote sensing: but was not argued earlier that remote sensing is of little use at this scale?

- synopsis of many disciplines and methods: it is extremely surprising that the authors seem to think that interdisciplinary and diversity can be achieved by one single person. This would contradict a very large body of social science!

2.2. Mesoscale

is a catchment in Chile really the only data-scarce catchment described in literature that the authors could find? Only in India, there is a vast hydrological literature describing basins of many sizes, land-use, data-availability and other characteristics. Basins in similar conditions have been described in Africa and South America. Just as an example, the CGIAR Challenge Programme for Water and Food (ref) deals with much larger and much more complex basins. The authors surely have no monopoly on meso-scale

basins!

2.3 More than a bio-physical problem

Nowhere do the authors convince that they go beyond a simple description of the data.

4 Balanced assessment strategy

The question is not so much about an infinite demand for data, but about which data to collect to inform adequately management strategy decisions.

4.1.3 Practical implications

what is the decision of 10 weeks based upon? It does seem very much ad-hoc.

hierarchically seek for representative samples: what is the hierarchical part? Fig.3 shows a flow diagram, but not much hierarchy

4.1.4 Data synopsis

Still, it is unclear what the DSS is supposed to do. It seems that a major issue is that the authors have little clue about what exactly they want to demonstrate. What kind of model do they want to implement, what should it predict, with what accuracy?

4.2.2 Sampling objectives

what sampling strategy was used? How were sampling locations chosen? What considerations are the number of samples based upon?

infiltrability -> infiltration capacity

4.3.2 Spectral top-soil properties

eminent impact on moisture dynamics: what kind of impacts are expected/observed?

4.4 Extension beyond bio-physics

Again, it can be very useful to collect socio-economic data and local expert knowledge,

but it is unclear how it fits in the rest of the study.

5 Results and discussion

this is very mcuh a listing of available data, but hardly any of the obtained results are reported. What about all the soil data that were obtained in the laboratory (texture, organic carbon content etc.)?

6 Conclusions

If under “data assessment”, a simple listing of available data is understood (which the manuscript currently is), then this would clearly fall below the standards for this journal. I do perceive an intention to assess these data from a management perspective, which would be relevant for the PUB questions, however I fail to see any such assessment in the current manuscript.

References

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Wagener, T., and H. S. Wheater. Parameter estimation and regionalization for continuous rainfall-runoff models including uncertainty. Journal of Hydrology, 320:132–154, 2006.

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