Review of "HESS Opinions: The complementary merits of top-down and bottom-up modelling philosophies in hydrology"

By Markus Hrachowitz and Martyn Clark

Summary, recommendation and general comments:

In this manuscript the authors share their opinion about the ongoing discussion on different competing modelling philosophies in hydrology. Besides describing the different modelling approaches in detail, they also discuss what they call "various modelling myths". In the end they propose a way forward and give some recommendations for hydrological modelers.

I do agree with the authors that the discussion about the different modelling philosophies is sometimes rather driven by emotions than by facts. I also think that an opinion on this issue and proposals for a way forward could be of interest for publication in HESS. However, I believe that before this paper can be accepted for publication substantial revisions are needed.

First of all, both authors have a separate opinion paper or comment with a closely related content in HESSD at the moment (Clark et al., 2017; Savenije and Hrachowitz, 2016). Especially the discussion and the review of the opinion paper by Savenije and Hrachowitz (2016) cover a lot of similar points and arguments as this paper. But also the comment by Clark et al. (2017) has several overlaping arguments, especially related to the propsal about how to progress in hydrological modelling. With three papers in HESSD covering similar topics I think it is especially important that the authors clearly show what this opinion paper differentiates it from the other two manuscripts.

My second concern is that a substantial part of this paper reads like a text book. While the language is clear and easy to follow, I was wondering if the potential audience really needs a two page long introduction to "conceptual" and "physically-based" models? Similarly, other sections seem to be redundant as they have already been covered in great detail in several opinion, comment and review papers (e.g. Bahremand, 2015; Clark et al., 2015; Gupta et al., 2012).

This brings me to a more general comment aimed at all opinion papers which is that careful reading is required to identify where facts end and the opinion of the authors starts. One example for this paper is when the authors write that top-down models have "a parsimonious representation of the energy balance". Is this a fact and has it been shown somewhere or is this an opinion? As far as I know, most hydrological models do not close the energy balance or even keep track of the energy in the system. How can you know if you close the energy balance, when you only try to close the mass balance?

Another example is the unclear separation of the macro- and microscale in this paper. For instance the authors argue that macroscale models are important and physically-based with e.g. Sivapalan's (2005) search for a general law at the macroscale or with a comparison with Gay Lussac's law. However, the papers they mention to support this argument use often macroscale models to define various states of the microscale, for example the root zone storage. While using macroscale models to estimate states at the microscale is a perfectly valid approach, it is very important to make clear to the reader that this can only be an estimate and is rather difficult because of the high degrees of freedom we have in hydrology. A precise definition of the macro- and microscale and a clear structure of the manuscript in this context might help to improve this paper and would ensure that not even more “modelling myths” are generated.

As I have the highest respect for both authors I am sure they have great and positive ideas for the future of both modelling philosophies. However, I believe that we do not need another paper where we discuss how physically-based or not the different modelling philosophies are. I recommend that you focus on the complementary merits of both approaches. Furthermore, I suggest giving clear examples and sharing your ideas how we could for instance combine top-down and bottom-up models in practice. This could make the manuscript much more unique and meaningful. As I believe that the discussion of this topic is of relevance for the hydrological community I hope my comments, questions and opinions are constructive and can help to improve this manuscript.

Comments, corrections and questions:

Section 1 What is the issue:

Page 2 Line 28: Maybe add some references where the authors showed that their model failed after the calibration period, both from the bottom up and top down community.

Page 3 Line 6: Could you define catchment scale?

Page 3 Line 10-11: What do you mean here with “respect to bottom up models”.

Page 3 Line 11 – 12: I couldn’t find the part where you provide a perspective of how to take advantage of different modelling philosophies.

Section 2 Modelling philosophy:

This section is mostly written clearly and precisely. Nevertheless, I think the potential reader of this opinion paper is already familiar with the different modelling approaches and reading this section is very akin to reading a text book. I would consider shortening this section with references to other studies or textbooks.

Page 4 Line 4 – 5: From my point of view the scenario in which you end up in a catchment where you only have reliable runoff and rainfall data but nothing more available is rather unrealistic: In which catchment in the world do you have reliable streamflow, evapotranspiration and rainfall measurement but no other information of the catchment? At least in Europe and the US you have land cover and geological maps. Furthermore, if there is a gauging station and rainfall measurements, most likely a person is doing maintenance on the respective instruments on a regular basis. This person will most likely accumulate a lot of qualitative information about the hydrological functioning of the catchment and could possibly also complement this picture with low-effort additional measurements or soil sampling. For instance Jackisch et al. (2014) showed how fast one can characterize a remote meso-scale catchment based on a brief measurement campaign. If land cover is managed forest or agriculture, frequently nationwide reports on productivity and for example drought risks are available. We have digital elevation models for the whole earth in decent resolution, monthly estimates of precipitation and soil moisture from satellites and so on. In my opinion the problem is often very different from the projected scenario: We do not know how to use the data in our hydrological models or if it is of relevance. But I admit that this may be a different story.

Page 4 Line 5-8: Is the "system integrated response pattern" really the "starting point" of top-down models? Isn't the starting point the delineation of a catchment based on the surface topography assuming a closed water balance? Since most top-down models are calibrated on the streamflow, do you mean streamflow by the term "system integrated response pattern"? Consider clarifying what you mean with the terms, maybe some examples beyond stream flow, and what you mean with "starting point" here.

Page 5 Line 12: Could you please explain in more detail what you mean with a parsimonious representation of the energy balance?

Section 3 Modelling myths

(C1) "Top-down models have a poor physical and theoretical basis"

Comparison with Gay-Lussac's law:

I think that the comparison with Gay-Lussac's law and the top-down modelling approach is a little misleading. I am not saying top-down models are not physically based. Like most hydrologists I believe that this entire discussion is based on an ill-posed definition and classification of hydrological models into the dichotomy of physically-based and conceptual models. However, with Gay-Lussac's law you can describe the macroscopic state of a system. But you can't say anything about the microscopic state of the system, for example where the molecules really are. Following your arguments and speaking of top-down models now this would mean that you can't say anything about the microscale of a catchment, for example where the water is in your catchment. However, later you argue that you can identify the root zone storage with a top-down model. Is this not part of the microscale? With a macroscopic model you can only infer about the microscale if you constrain the possibilities of the microscale using either additional measurements or process-based reasoning with the help of statistics. However, this is really difficult in hydrology due to the large number of degrees of freedom. For example, if your model is calibrated to mimic the runoff generation and if we assume for a second that the two water worlds proposed by McDonnell (2014) are real, there is no information about the root zone storage in the rainfall-runoff data and it is really difficult to know if what you learn from your models is true.

Overall, it is not clear where you want to go here. A top down model is based on 1.) the conservation of mass and 2.) on the delineation of the landscape into some kind of control volumes mostly in form of a catchment. With a top down model you can hence make assumptions about the macrostate of a catchment or of a similar control volume. With the help of statistics, process-based understanding or additional measurements you might be able to get a grasp of the microscale. So why are you comparing it with a natural law which is constrained by the energy and mass conservation when the model you defend is not? I believe most hydrologists know how a conceptual model works so is this whole comparison necessary at all? Maybe a rigorous definition of macroscale and microscale might help to improve and clarify differences, similarities and linkages between top-down and bottom-up models?

Page 6 Line 3: The molecular dynamics approach might be untestable and unfeasible but certainly not unnecessary. It is the theoretical basis of the movement of gas particles and hence necessary if you want to understand a system.

Page 6 Line 23: Can you please explain in more detail what you mean with parsimonious representation of the energy balance, again?

Page 8 Line 1: Holistic empiricism and on *Page 7 Line 6* assign physical meaning to them a priori? Please explain why the two statements are not in contradiction.

(C2) "Top down models are too simplistic…" and (C3) "Top-down models are ad-hoc formulations…"

Both sections are written clearly and well but I think this has all been said and written down several times. You might consider to shorten this section.

Subsubsection 3.2.2 and 3.2.1: What do you mean with process and spatial complexity. Could you please define complexity and how it relates to the respective models?

Page 10 Line 30-31: Is it really multivariate observed response dynamic? At least in one of the cited examples the authors only use streamflow and derivations of it.

Section 4 Implications, potential ways forward and concluding remarks

Page 12 Line 19 - 20: "Competing approaches" Despite the title of the manuscript I had the feeling that the main focus was on defending top-down models. Why do you stress the dichotomy although I understood the overall aim of your opinion paper to be exactly the opposite?

Page 13/14 Line 34 / 1-2: I think this sentence is a little misleading. Obviously you can use a Darcy-Richards based model on the macroscale. However, you need to use a rather fine discretization of the model elements.

Page 14 Line 16 - 17: Why are you so pessimistic here? Maybe you could add some references so the reader can better understand your pessimism.

Page 15 Line 9: If this is about hydrological modelling at the catchment scale you might consider adding catchment scale modelling to the title. Unfortunately it is not clear what the catchment scale is. It would be nice if you could add a definition.

References

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