Review HESS – Seibert-Bergström, A retrospective on hydrological modelling based on half a century with the HBV model (HESS-2021-542)

GENERAL COMMENTS

The manuscript describes the history of the HBV model development and applications. As documented through its widespread international use and the many scientific publications, it is unquestionable that HBV has made significant impacts on hydrological science and water resources practitioners. Hence, this is a success story that is definitely valuable to publish and learn from. And as such I believe that the topic and the story is relevant for the readers of HESS.

My main general comment relates to the writing style and the level of scientific discussion. The writing style is somewhat "easy going" with chapters 1 (Introduction) and 5 (Concluding dialogue) written as an informal dialogue between the two authors and a hypothetical PhD student. Furthermore, the manuscript is to a large extent "inward-looking" in the sense that it tells the HBV story without much critical self-reflection and without much scientific discussion of competing developments and discourses. While this probably makes the manuscript easier to read as a memoir/biography, it also reduces the scientific value.

Furthermore, as can be seen in my specific comments provided below, I find that some of the statements in the manuscript are conditioned on the relatively narrow scope (HBV story), which for instance makes some of the concluding dialogue appearing as general statements without proper reservation that the perspective deals with the HBV model (and similar rainfall-runoff models) and not hydrological modelling in general.

SPECIFIC COMMENTS

1. In my view HBV is a rainfall-runoff model and as such one of several types of hydrological models. To me hydrological modelling is not, as indicated in the first paragraph of the Introduction, confined to simulation of river discharge. Other types of hydrological models include plot/field scale models of the unsaturated zone, groundwater models, land surface models and coupled surface water/unsaturated zone/groundwater models. The authors talk about the HBV model as a hydrological model. I do not disagree to this. But it should be made clear that the statements in the manuscript on "hydrological modelling" are confined to "rainfall-runoff modelling". In line with this I suggest that the term hydrological model in the title be substituted with rainfall-runoff model.

2. The aim of the manuscript, most clearly stated in the abstract, "is to provide an understanding of the background of model development and a basis for addressing the balance between model complexity an data availability". I do not think that this is well elaborated in the discussion and in the concluding dialogue with respect to hydrological modelling in general. I could agree that this is

addressed in the narrow context of the HBV and similar model types, but the manuscript does not deal with this issue for other model types. This limitation must be stated explicitly in the manuscript.

3. Lines 30-32. The authors state that around 1995 the typical argument was "Use a physically-based model, not something as simplistic as this HBV model …". While I agree that such arguments existed at that time, many other scientists argued differently, namely that HBV type models were ideal for rainfall-runoff modelling, while physically-based models would mainly be useful for other applications – see e.g. Beven (1989), Refsgaard et al. (1992) and Refsgaard and Knudsen (1996). I am not arguing that this aspect should be mentioned here, but if the authors want to go into the scientific dispute between modelling schools that existed these days, it should somehow be considered.

4. Lines 86-88: This discussion is again confined to catchment scale, surface water hydrology. I can find many other examples on hydrological questions that cannot be addressed with HBV.

5. Chapter 2. Sections 2.1 (the early days of hydrological modelling) and 2.3 (the story of HBV in Sweden): Interesting and well written sections. I have some difficulties with section 2.2 as reflected in the next two comments.

6. Lines 171-179: The listing of "early models from the 1970s" does not reflect the heading "early types" – the models are not linked to different types.

7. Lines 184-205. The manuscript here introduces and discusses the typically used classification terms conceptual versus physically-based and lumped versus distributed. Without introducing rigorous definitions, the authors end up concluding that there is hardly any difference and that the HBV can be considered distributed and physically-based. Depending on the definitions one chooses this may be correct, but if you choose so broad definitions the classes become meaningless. I acknowledge that HBV can be used in a semi-distributed (HRU type) manner and that there is a physical basis behind most of the equations in HBV. But I shall also claim that there are fundamental differences between a model based on HBV and a very sophisticated spatially distributed, physically-based modelling system, for instance like the one described in Kollet et al. (2018).

8. Subsection 3.1.2 and lines 352-353: I completely agree and would consider the soil moisture accounting and the split between fast/surface runoff and infiltration to be the heart of a model of HBV type.
    - In fact, there are many similarities between all good performing models of this type with respect to how they handle this – the equations may appear different, but substantially the models act the same way.
    - Another reason for the success of the soil moisture accounting is probably that it is robust towards the temporal discretization of precipitation. The first versions of HBV operated with daily time steps – and you do not generate Hortonian overland flow from 24-hours rain but from high intensity rain over shorter time, e.g. minutes or a couple of hours. The

HBV (and the other similar models) have proven to perform well both when using daily and hourly rainfall – i.e. it is robust for temporal lumping of rainfall.

I am not suggesting text changes here, but if the authors want to discuss one of the reasons for the success of HBV and similar models, this aspect should probably be emphasized.

9. Lines 393 -403, Intercomparison project (Breuer et al., 2009). I have a couple of reservations about the way this study is referred to:
    - "SHE with hundreds of parameters to simple models such as HBV". It is correct that distributed models have many parameters, but parameterization is always done in such a manner that most parameters are not modified during calibration. In this study, 7 parameters were calibrated for SHE, while 10 parameters were calibrated for the HBV. So, I think the text is potentially a bit misleading here.
    - Although Breuer et al. (2009) concluded that "there was no superior model if several measures of model performance are considered", I agree that it is correct that HBV showed the best performance with respect to NSE. Based on experiences from other intercomparison studies, e.g. WMO (1988) and Karlsson et al. (2016) I would argue that the differences in performance might be explained by the skill of the hydrological modeller rather than the quality of the model code.
    - Lines 401-402: I agree that the results from Breuer et al. (2009) suggest that simple model can perform at least as well as complex models with respect to runoff simulations. But the qualifier "with respect to runoff simulations" is important, because the results from Breuer et al. (2009) do not go beyond this.

10. Lines 403-405: It is correct that such statement has been made, especially during the 1980s, but my impression is that the overwhelming part of the hydrological modelling community would argue that all hydrological models, including complex models, need some kind og calibration (Hrachowitz and Clark, 2017). So, I do not find that statement representative.

11. Section 3.3: The international use of HBV has been an indisputable success story. I would argue that several other models of the same type had similar qualities when they were originally developed, but were not maintained and kept alive for several decades as the HBV. It would be interesting to hear the authors' evaluation of why HBV succeeded in this respect.

12. Lines 545 – 554: I agree to this, when the statements are conditioned on HBV. However, this does not apply for more complex model types, where there are many examples of models calibrated against several target variables (e.g. field data on discharge, soil moisture, groundwater heads, land surface temperature, evapotranspiration) result in models with a more realistic representation of the hydrological processes, e.g. Stisen et al. (2018). I would also argue that the emergence of huge amounts of high-resolution good quality data in these years are likely to change the conclusions to this question.

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