I will reiterate my concerns about this paper again, since they have not been addressed - even superficially - by the previous replies. However, I will not respond to further comments - there is nothing that can improve this paper without a complete re-write beginning with at least a cursory literature review of the subject that is being discussed and then a rethinking of the primary message in the context of a more rigorous attempt to derive concepts. Any further dialogue along the lines of the comments below is fruitless. This is exactly the type of article that the peer-review process is supposed to catch, although this one is so obviously flawed (i.e., does not cite any literature from the domains that it speaks about), that I am a little surprised that the editor requested a review.

In detailed response to responses by the authors (author's responses in purple):

CC3 says: "*You have chosen to focus on one (small) aspect of my comment and ignore the rest.*" In fact, we focussed on a claim that our model has absolutely no lasting value whatsoever to anyone, and a claim to the effect that the subject of the title of the paper is a redundant conception. These claims are central to two of the three general concerns you highlighted in your review. The claims are not well founded (they lack insight) and some readers may read the review but not the paper. We therefore responded to the claims as soon as we could.

As explained in my original comments, the point of the comment about the skill of your model is that your proposed philosophy and writing suggestions lack *empirical* (in addition to theoretical and philosophical) support. As an empirical demonstration of the proposed philosophy and technical writing suggestions, the empirical results *degraded* relative models developed using current standard practice.

A sense of proportion and fairness is needed in discussing the third highlighted concern (philosophy).

You are writing on an academic subject (model realism) that is centuries old and one of the most widely studied problems in academic philosophy, and perhaps in academic history (e.g., Quine, Hemple, Cartwright, among hundreds or thousands of others). It is ok to have opinions about this subject, but unless those opinions contribute to the academic discussion in a rigorous way, they do not belong in peer-reviewed journals.

In CC4, in the name of philosophy, you try and shout down (cancel) hydrologists who use intuition creatively in hydrology.

The point of peer review is to keep non-academic papers from being published, and this current paper represents the epitome of the need for peer review. You are not being "cancelled" - you simply have not done what is necessary to participate in the academic discussion that you are trying to write about. You do not have a right to publish your meandering, uninformed, non-academic opinions in a peer review journal -- this is what blog posts are for, and having a paper rejected because you failed to follow basic protocol for writing an academic paper (e.g., citing even a single source from the primary topic you are attempting to publish on) is not an injustice.

Cannot intuition, and the insight it brings, not simply be appreciated and be adapted for use for the general good.

Intuition is not a substitute for rigorous epistemology, which is one of the oldest and most mature fields of study. Intuition is not a substitute for all of the components of an academic article that are missing from this paper: (i) a relevant literature review, (ii) formal logical or theoretical foundations, (iii) logical derivation of the proposed philosophy, (iv) supporting empirical results, etc. Intuition is sometimes useful, but it is not a sufficient basis for formal academic contributions, especially on a subject that is as mature and well-studied as this one.

It never crossed our minds that a reader or reviewer would persist in the notion that we are somehow trying to reinvent technical writing or are engaged with what you describe as "*changing how we write scientific papers*". Neither did it cross our minds that a reader or reviewer would persist in assuming we propose the use of everyday English other than in scientific exploration, and then only when it is useful and practical (our background is physically-based, distributed RR modelling, where the documentation runs to hundreds of pages of text, equations and diagrams).

Your (only) actionable suggestion in the paper is to use knowledge tables written in "everyday english" (this is a direct quote from your paper). I have a hard time imagining that you failed to anticipate that people would read the exact words you wrote in the paper.

Additionally, this paper does not make a formal (either theoretical or empirical) case for moving away from the standard methods for technical writing that you alluded to here, which are extremely effective. Personally, I would rather have a 100-page model documentation that gives rigorous, reproducible methods and statements of theory and assumptions than tables with sentences in "plain english". However, I'm not going to argue with you about the validity of your technical writing suggestion because - frankly - neither of us are qualified to discuss the subject of technical writing or philosophy of communication at a level sufficient for peer-reviewed journal. And this is the entire point of my criticism - you substituted uneducated intuition for academic rigor, and this kind of writing does not belong in peer-review.

The paper gives a science-based solution to a real-world problem: benchmark links between hydrologic knowledge and performance are needed as a basis for measurements related to engineering decisions.

There is no science in this paper. No hypothesis was tested (in my original review, I attempted to - generously - treat the new model development as a hypothesis test of the new philosophy, but the reviewers did not even recognize that this is what I was doing in their replies).

There is irony in that a serious attempt to be clear about what is assumed known in reaching the solution is attacked on philosophical grounds, especially given L128-131. Also, any discussion of the solution, or how it was arrived at, must take into account that in L180-181 we explicitly allow for permanent review.

There have been many (thousands) of serious attempts at reconciling the model realism problem both in philosophy journals and domain science journals. The problem with this paper is precisely that it is *not* a serious attempt to do that - it does not even recognize, let alone build on any of the existing work on the subject. It is just the intuition-based musings of people who have not even made a cursory attempt to do a literature review on the topic they are attempting to publish on.

We have been thinking about what might be covered if a discussion section is to be added to the paper. The predictions are for the numbers in runoff records, so in the context of the paper the records are reality. Say there are three regions in a space: physical reality (i.e. the river catchments), hydrologic knowledge and performance. The paper is about a single mapping from hydrologic knowledge to performance. Other mappings are not discussed, such as mappings to or from physical reality, or back from performance. One-to-many, many-to-one and many-to-many mappings are not discussed. To the extent that it can be helpful, such mappings could be described in a discussion section in terms of common philosophical concepts which interest RR modellers.

This would just be more non-rigorous, intuition-based musings that would not improve the paper.

The 2nd paragraph in CC4 is grossly unfair. It seems to be a reaction to this text from AC1: "*One of the points made in the blind validation work is that models and modellers must be seen as a package (Ewen and Parkin, 1996). Our experience is that hydrologists running an RR model sometimes forget the nature of the model. Sometimes it is treated as a statistical black box. The worst case is when the model is treated as if it is reality, and it is implicitly assumed that there are no constraints on what can be concluded from the resulting simulations.*" The term "black box" seems to have been lifted from this text and its meaning adjusted to fit your case. The reality is that RR models are often run as a general resource, well outside the control of model developers (you seemed to have assumed that the text is about model developers running their own models). Some models run as a general resource have considerable complexity, and this can lead to belief in simulated detail (including spatial variations in response) or in all the available energy being spent on the sheer effort of parameter calibration against one or a few statistics (i.e. treating the model as if it is a black box).

I disagree that there are model users out there who do not understand that models are approximations and have limited domains of applicability. But again, I will not engage in this discussion beyond stating this disagreement because it is irrelevant to the question of whether this article is publishable. My dis/agreements with the authors on matters that are not quantifiable and/or not derivable (e.g., how humans interpret or how they apply models) should not (and does not) factor into my review of this paper. The paper fails (abjectly) due to lack of academic rigor.