

Response to RC2: Comment of eguspere-2023-1980. Referee Grey Nearing. 13 Nov 2023

We want to thank the referee for the detailed evaluation of our paper. In this document we answer the questions, comments and suggestions given. We will address those comments individually. For clarity, the original comments posted by the referee are written in italic, while our answers are written in bold.

Summary of Paper

The paper addresses the following two questions:

1. *Do conceptual models serve as an effective regularization mechanism for the dynamic parameterization of LSTMs?*
2. *Does the data-driven dynamic parameterization compromise the physical interpretability of the conceptual model?*

Neither question is answered directly in the paper, but as far as I can tell, the authors intend to convey that the answer to both questions is “No”.

Additionally, the authors conclude that while it does not help increase model skill to add a bucket model to an LSTM, doing so does allow the model to predict variables other than the training target (which here is streamflow).

Summary of Review

In general I think these are interesting and informative experiments. Also, I want to express how refreshing it is to review an ML-based hydrology paper that uses best practices. Specifically, the authors are careful to train the models in a way that matches prior publications, and they build on existing community benchmarks (although some improvement on using the benchmarks more carefully would help).

We thank the referee for the well-structured summary of our paper, and for the detailed revision he performed. His suggestions and comments were helpful to clarify concepts shown in the article.

I have two questions/concerns with the conclusions:

1. *The lesson that I take from the quantitative results in this paper is that there is no value in adding bucket models to LSTMs. In other words, there appears to be a clear and direct answer to the question in the title. Bucket models do not increase skill, and there are no examples in this paper of something that the hybrid modeling structure can predict that the LSTM alone cannot. It appears that the bucket model (unless intentionally configured to be nonsense) is a transparent “head layer” in the deep learning model stack that contributes no information. I suspect that the authors might respond to my interpretation of their results by saying that the bucket models allow for estimating non-target states and fluxes. However, what is conspicuously missing from this paper is a quantitative benchmark against the results from Lees et al (2022), who show that the LSTM can estimate (unobserved) soil moisture and snowpack (the two non-target variables explored here). I’m searching for anything in this paper that provides evidence that there is any value to using bucket models. Otherwise, I suggest that the authors answer the titular question directly.*

The idea of seeing the conceptual model as a head layer is quite accurate and is an alternative way of interpreting the hybrid model architecture we are using. We want to thank the referee for pointing this out. We will add the following table to the article to illustrate his idea and update our model description and discussion accordingly.

Model		Head	
LSTM	Input ->LSTM ->	Dense	-> Q
LSTM+Bucket	Input ->LSTM ->	Dense -> Bucket	-> Q
LSTM+SHM	Input ->LSTM ->	Dense -> SHM	-> Q
LSTM+NonSense	Input ->LSTM ->	Dense -> NonSense	-> Q

In the experiments we conducted, and as stated in the paper, we were not able to increase the performance by adding the conceptual part. In future work we want to test if hybrid models present other advantages for different conditions (extrapolation), but until this point the remaining advantage that we see is the access to non-target states. We will explicitly include these statements in a revised version of the manuscript.

The main difference with Lees et al (2022) is that they need to train an additional model (which they call probe) to extract the non-target states, while in the method we propose no extra training needs to be done. In other words, the SHM is acting both as a head layer and a probe. The fact that in Less et al (2022) the probe can be as simple as a linear model, which requires few points to train, is not being argued, and in many cases, this will reduce the advantage given by our hybrid approach. However, we argue that there is a methodological difference between the approaches, as in their case another model needs to be trained to extract non-target states while in our case the architecture of the conceptual model takes care of this.

To make a more transparent comparison between these two methods, in a revised version of the manuscript, we will include the previous discussion about the difference between them. We will also include a comparison between our predicted soil moisture and the predicted soil moisture presented by Lees et al (2022).

2. *I'm not 100% sure that I understand the reasoning behind concluding that the bucket models cannot regularize the LSTM. The 0.71 median NSE from the LSTM+NonSense model seems to indicate that this regularization is possible, in principle. Interpreting the bucket models as head layers on the LSTM, this experiment seems to indicate that a really bad head layer can result in information loss (which makes sense to me). I am not 100% sure what is going on in these experiments, but here is one hypothesis. Bucket models (I would prefer to think of them as head layers, because the lesson generalizes beyond bucket models) can eat information (presumably by poorly regularizing the loss response), but "reasonable" bucket models used as head layers don't add any information. Whether or not the "reasonable" bucket models are regularizing the loss response surface is unknown, since it appears that the LSTM is finding a similar solution either way. However, since the LSTM is finding similar solutions either way – there is no useful regularization from these head layers, and also no information loss. I would suggest that there are probably ways that we could look more closely at the functionals that the LSTM and hybrid models discover. Are they really finding similar relationships? Sensitivity analyses, input/output response surfaces, integrated gradients, etc. I'm not sure what the best approach would be (one of many*

would probably work), but I think that if you wanted, you could be more rigorous about understanding how these head layers are interacting with the LSTM component of the model to create functional mappings. I don't really care whether the authors do that for this paper or not.

By regularization, we are referring to adding extra information to our model that constrain the space of solutions. For example, an L1 regularization includes information that the solution should be sparse. Moreover, a specific loss function might include information that the solution should be smooth, etc.

The statement that the bucket models cannot regularize the LSTM responded to our initial expectations when we were creating the experiments. As we can see in the table added in the previous point, and as stated by the reviewer, the difference in the models can be seen in the head-layer. Our initial expectation was that if our head layer: a) restricts the flexibility of the LSTM because the output of the LSTM (after our dense layer) is further passed through a one-process (single bucket) layer, and b) the one-process layer encodes almost no hydrological process understanding, then the performance of the model would drop. However, this was not the case for the LSTM+Bucket case. In the LSTM+NonSense model, we observed a performance drop of 0.1. However, it is important to note that to be consistent with the other cases, the range in which the LSTM was able to vary the parameters of the NonSense – Conceptual Model was constrained, and the performance drop was caused by hitting those limits. Therefore, our statement that the bucket models cannot regularize the LSTM for the experiments that we propose, answer to the fact that besides allowing extracting untrained variables (if the conceptual model is consistent), the conceptual models are transparent layers that do not change the performance of our model. The LSTM is learning the signal required to predict discharge, and we're not helping (or harming) by adding a local physical operation (the bucket model).

Moreover, the fact the models are transparent layers, also indicate that this hybrid approach is unfeasible to select a certain model architecture or a process description over another one, as the LSTM reaches very similar performances with very different even unfeasible head-layers. Therefore, we cannot use this approach in a classical process based manner in which we see different models as hypothesis and select the one with the highest performance as the most realistic one.

Specific Comments

Line155: "...selected the one that performed the best for each basin. "Was this selection done with train-period, evaluation-period, or test-period data?

The selection was done for the test period.

Line215: I'm not sure that I understand (or agree with) the distinction being made in this paragraph. The LSTM has a state vector, just like the SHM – there is no difference here. The LSTM requires consecutive predictions in exactly the same way as SHM – again, there is no difference. We use a sequence-to-one training procedure when training the LSTM because this provides more diversity in the mini batch and accelerates training. You could do exactly the same thing for training SHM and/or the hybrid model. You are using a 2-year period for the hybrid model, and you could accomplish the same thing by using a 2-year sequence length for the hybrid model while still training sequence-to-one. You can think about the LSTM as having a 180-day spin up while the hybrid model has a 365 day spin up, and the LSTM is trained seq2one while the hybrid model is trained seq2seq. It's fine if you've found that training the hybrid model using a

sequence-to-sequence approach is better, but the way that is motivated (and the way this distinction is framed) in this paragraph is not correct.

You are completely right. This paragraph was intended to clarify how we trained the hybrid models, but it is not correct to indicate that the training processes are conceptually different, as both approaches can be trained as seq2one or seq2seq. We will correct this in a revised version of the manuscript.

Line230: Do these median statistics come from ensembles (as is used by Kratzert et al), or from single LSTM/Hybrid models?

They come from single models.

Line235: I think it is too strong to claim that the “consistency and proximity ” with Lees “validate the reliability of findings. ”This isn’t a rigorous way to build on a community benchmark. A better approach is to actually recreate the existing benchmark exactly, which demonstrates that the models are built and trained correctly, then transition the experiment to the basins/ time periods that you want to use for this study. This is, for example, what we did in the Frame et al. papers that required different training/test sets than what were used by previous community benchmarks.

To be more rigorous about this we propose the following.

In his benchmark, Lees tested his model for 518 basins between 1998-2008. For our case, we tested the model for 60 basins between 2006-2012. Of our 60 basins, 48 are also included in the benchmark, sharing two years of data (2006-2008). Therefore, in a revised version of the manuscript we will compare our results with the ones from the benchmark for the 48 basins and 2 years of data.

Line 240: “The LSTM network has the capability to account for biases in the forcing variables (e.g. precipitation or evapotranspiration) because mass conservation is not enforced.” We have a paper that demonstrates this explicitly:

Frame, Jonathan M., et al. “On strictly enforced mass conservation constraints for modelling the Rainfall-Runoff process.” Hydrological Processes 37.3 (2023): e14847.

We will add this reference in a revised version of the manuscript.

Line 270: What does the word “overwrite” mean in this sentence? I think that being very specific here about what you want to test is important because how the bucket model influences the LSTM is the central question. There is no such thing as “overwriting”. I suggest being crystal clear and precise about what you are envisioning when you ask this question.

We agree that overwriting was not a good word choice. As stated in previous responses, our initial idea is that a head layer built without hydrological knowledge would much strongly affect the performance of the hybrid model. Therefore, in this question we wanted to evaluate if the LSTM can operate the head layer such that it overcomes (rather than overwrites) these constraints. We will modify the phrasing in a revised version of the manuscript.

*Line 300: I agree that the results from section 3.2 don’t indicate that a conceptual model can’t be used, but again, I want *some* type of evidence that this has value. I don’t care about subjective arguments or what members of “the community” might think (e.g., line 35), I want some type of real, quantitative, scientific*

evidence that this is a useful thing to do. Showing strong correlation with soil moisture is interesting, but isn't anything new for these ML-based rainfall-runoff models.

With the experiments provided here, besides having access to non-target variables we do not have evidence that suggests other benefits, like an increased global performance or a way to distinguish hydrological meaningful head layers from less meaningful ones. We will explicitly include these statements in a revised version of the manuscript. However, we will conduct future studies in the subject to evaluate if under other conditions the hybrid approaches can provide additional benefits. As discussed in detail in question 1, even though other methods (Lees et al, 2022) have been used to extract non-target information, such approaches require additional training, which makes them different from the methodology proposed here.

Line 315: "...suggesting that our model effectively utilizes the well-structured conceptual part to get better predictions of the untrained variables." I disagree — this is one possible explanation for these results, but there is another possibility (that SHM is simply doing nothing), and these experiments can't differentiate between these two possibilities.. These results show that the poorly-structured head layers cause the LSTM to lose information about soil moisture, but there is no indication that the "well-structured" model is being used in any way. The LSTM could be (and I suspect is) providing all of the information about soil moisture here, and the SHM model is doing nothing. A way to test this would be to use the Lees methodology and see what the LSTM can do alone to predict soil moisture. You probably will need to check both the LSTM standalone and the LSTM component of the hybrid model, since the method by which SHM would add value would be through regularization during training, and it is likely (although not guaranteed by the data processing theorem, due to the meteorological inputs to SHM) that the LSTM in the hybrid model will contain all or most of the information about soil moisture that is present in that whole model.

Our statement is that the well-structured conceptual model allows us to better predict untrained variables, which we argue is correct, and it is backed up to the fact that the highest correlation of soil moisture is reported for the LSTM+SHM case.

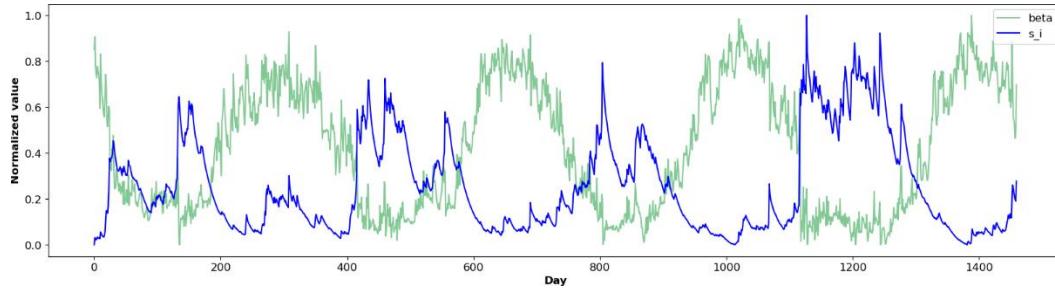
As discussed before, the addition of a conceptual model in a head-layer is not improving the performance with respect to the stand-alone LSTM, so in that specific case it can be seen as "the SHM is simply doing nothing". However, the well-structured conceptual model is allowing us to have (without any further training) access to non-target variables with more accuracy than in the other cases, therefore it is doing something. We wanted to extract soil moisture, so we explicitly chose a conceptual model structure that contained a consistent representation of this component, and we were able to reproduce our non-target variable of interest. We are not implying that the soil moisture information is not present in the stand-alone LSTM or even the LSTM part of the conceptual model. We are also not implying that the addition of a well-structured conceptual model boosts the ability of the LSTM to encode soil moisture information coming from the input data. Our only argument is that a well-established conceptual architecture allows us to extract untrained variables without any additional training or model. Whether this is meaningful or not is surely open for discussion.

We will include this discussion in a revised version of the manuscript.

*Line 345: The su_max and beta parameters being higher *might* result in lower water availability for down-stream buckets, but really what they are doing is reducing the outflow *rates*. More total water could compensate for this. Is all that is happening here is that the model is pulling a lever to reduce the*

total output, or is it more complicated - does the rate need to reduce for some reason. What do total inputs (PET) look like over these seasonal cycles? If P-ET is lower for the low-flow seasons, why is it necessary that the flow rates from the unsaturated zone also must be lower? Anyway, it's a little simplistic to just say that higher parameter values result in lower water availability in the downstream buckets.

We agree that the effect that the variation of the parameters have in the lower buckets can be compensated by more total water. However, the figure below shows that this was not the case.



In this figure we plotted, for basin 10003, the normalized time variation of the beta parameter (green line), and the normalized time variation of the interflow storage (blue line). Here we see a clear pattern that increases in beta are associated with lower water content in the interflow, and that decreases in beta have the opposite effect. Therefore, even though our justification was simplistic, we can see that the parameters are moving water closer to the output in situations where more water is required and reducing the total water content in low flow situations.

Line 350: Is there any reason to believe that "noisy" parameter values indicate parameter interaction? Could it just be that there is no low-frequency signal that is needed in this basin to compensate for lack of information in seasonal precipitation signal? Also, what does "noisy" mean? Why do we think this high-frequency variability is noise?

This whole set of experiments is leading me to hypothesize that the problem with the bucket models is that there aren't enough buckets. More buckets allow for more flexibility in the mixing of residence times. We could use hundreds of buckets wired in parallel and series, with skip connections (to account for mass transfers between buckets on timescales shorter than the number of timesteps represented by the distance between two buckets in series). We could train this much bigger bucket model and look at residence time mixing ratios over seasons. And really, this would not be very much different than using a set of fully connected layers with linear coefficients, which - surprise, surprise - is exactly what the head layer on the standalone LSTM is.

Yes, it can be that the basins that do not present "seasonal patterns" are because no low frequency signal is required. In accordance with what was suggested by the reviewer, in a revised version of the manuscript, we will replace "noisy" with "high-frequency variability".

About the second paragraph, we also agree that hundreds of buckets connected in parallel, and series will be similar to an LSTM. Each cell state of the LSTM is modified by a forget gate, input gate and (indirectly) output gate. This is equivalent to having a bucket in which its internal state is updated considering some mass loss (e.g. evapotranspiration), an input (e.g. precipitation) and an outflow (e.g. discharge). The main difference is that the gates in the LSTM depend on the input and the previous states, and in conceptual models, for simplicity, we usually take this as constants. However, this can also

by modified by making the gates of the buckets context dependent, and then both models would be equivalent.

However, if we have hundreds of buckets, and we want to extract information about non-target variables, similar to the LSTM case, we need an additional model (probe).

Line 415: Referring back to my main criticism, I would like to take issue with this sentence from the conclusion section: "This test addressed one of the main benefits of hybrid models over purely data-driven ones, which is their ability to predict untrained variables." I do not believe that it has been shown that this is a benefit of the hybrid modeling approach. LSTMs do this by themselves, and the authors even cited papers in their introduction that demonstrated this. I can imagine thinking that this might be a benefit, but that was not demonstrated in this paper (or any other that is currently published).

We argue that the head layer does allow to extract information about untrained variables. The difference between our method and Less et al. 2022 was discussed above.

One question I have after reading all this is about why the parameters should change over time (i.e., why dynamic parameters work and static ones don't). Of course, the answer appears to be simply that the bucket model isn't providing any information and since the LSTM is doing the predicting, of course that prediction needs to be dynamic. But I wonder whether the dynamics in the conductivity parameters in particular might vary with moisture content in a some way that could be interpreted as a characteristic curve?

In a hypothetical case in which we have a 'perfect' conceptual model, that considers all the processes happening in the basin, the predicted parameters will be constant. However, due to structural limitations of the conceptual architecture, the LSTM does take part in the predicting. Because of how the LSTM and the conceptual model are connected, the only way for the former to pass 'predicting information' to the latter is through the parameters. We argue this is the reason why the parameters change over time. And, yes, it is very well possible that from a detailed analysis of parameter co-variation (with input and with other parameters), a lot could be learned about the underlying hydrological system (but beyond scope here).

We will add a related sentence to a revised version of the manuscript.

We thank Grey Nearing for the detailed evaluation of our manuscript. We believe that the changes proposed here will increase the quality of the manuscript and hope we addressed the questions raised in a satisfactory manner.