Dear Editor and Reviewers:

We appreciate the constructive comments and suggestions which help improve the manuscript. We provide the point-by-point response to all the comments, each comment starting with "***Response***". We look forward to hearing from you at your earliest convenience.

For Editor:

I agree with the comments of the reviewer that the model should be called semi-distributed rather than distributed. Please correct this in the title and through the manuscript, and address the other reviewer concerns.

Response: Thanks for your recognition and suggestions. We have revised the manuscript according to all comments. Our models are called semi-distributed models in the revised manuscript.

For Reviewer #1

The authors indicate they have a distributed model, but they are not using any routing method, which is especially important considering the size of the basins they are analyzing. In the response from the previous review, and in the revised manuscript they indicated that "The technical requirements of differential programming framework limit the consideration of routing methods in our hybrid hydrological models." I do see why this would be the case. In differentiable programming, you can include routing methods, and it has been done multiple times. Feng et al., (2022) (https://doi.org/10.1029/2022WR032404) included a routing routine and the end of the pipeline using a unit hydrograph, Bindas et al., (2024) (https://doi.org/10.1029/2023WR035337) did the routing using Muskingum-Cunge, and Yu et al., (2024) also showed a similar approach (https://doi.org/10.5194/hess-28-2107-2024). Differentiable programming is a flexible approach that allows for routing. Considering that the authors are acknowledging the limitation and indicating that: "Future research will focus on developing hybrid distributed including routing processes and extending the

evaluation of the hybrid distributed model to encompass a broader range of basins." I would suggest that they rename their current approach to semi-distributed. It would be consistent in the sense that they are giving more flexibility to the model by considering subbasins, with the clear limitation that no routing is being done. I would also suggest indicating in the Abstract that it is semi-distributed because of the routing problem.

Response: Thanks for your suggestions. Our models are called semi-distributed models in the revised manuscript.

Finally, about the argument that they indicated in the response to the previous review: "To compensate for the lack of consideration of the routing process, we calculate the river length from each sub-basin to the basin outlet and employ this static attribute as the inputs of ENNs to implicitly characterize the routing process within the basin" This is not fully correct. The river length of each subbasin could indeed give an idea to the model about the internal routing in each subbasin, but it does not create routing between subbasins, because it does not have information to do this.

Response: Thanks for your suggestions. We used the river length from each sub-basin to the basin outlet rather than the river length of each sub-basin. Employing the former as the inputs of ENNs can represent the flow distance of runoff from the river channel of different sub-basins to the outlet of the total basin and further can implicitly characterize the routing process within the basin.

Minor comments:

Line 137: Remove "in this study" from the end of the sentence, you already mention that in the beginning of the sentence.

Response: Thanks for your suggestions. "in this study" has been removed in the revised manuscript.

Line 247: In the previous review it was mentioned that the authors should be careful in reporting NSE differences of 0.01 and 0.02 as significant, because they can be associated with the stochastic optimization process. They corrected this, as

shown in line 240. However, in line 247 they also indicated that a difference between 0.06-0.09 is a small increase, which I think could be statistically significant, and therefore not a small increase.

Response: We have revised it as your suggestion in the revised manuscript. The revised sentence is "First, the results between $DM_{\theta\text{-}Q\text{-}T}$ and DM_{θ} models show the significant improvement in runoff modeling brought by the incorporation of ENN_Q. This enhancement is illustrated by an increase in *NSE* and *mNSE* values, ranging from 0.06 to 0.09 in Yellow and Yangtze."

Line 261: "This enhancement is evident through closed NSE and mNSE and lower PFAB values in all three basins." The first part is confusing. Why would an enhancement in performance be evident if you have similar NSE and mNSE metrics?

Response: We want to illustrate that the model is enhanced in peaking runoff modeling. This sentence has been revised as "This enhancement in peaking runoff modeling is evident through closed *NSE* and *mNSE* and lower *PFAB* values in all three basins."