Review

The authors propose an approach to account for transmission losses when estimating runoff in arid areas with ephemeral stream networks. They utilize high-resolution datasets and streamflow observations for model calibration and validation. While the methodology appears to improve performance in fewer than half of the analyzed basins, it does not significantly advance our understanding of the underlying processes or contribute to their accurate quantification. Furthermore, the assumptions and limitations of the approach are not sufficiently discussed, which raises concerns about its broader applicability. These critical gaps weaken the robustness of the approach, and I strongly recommend that the authors address these issues to improve the manuscript's quality. The manuscript would also benefit from a clearer structure to better articulate the methodology. Therefore, I recommend that the manuscript, in its current form, should not be considered for publication in the HESS journal.

Major comments

The authors need to clearly define transmission losses, including the specific processes encompassed by this term, and maintain consistency in its usage throughout the document. At present, the definition appears to shift depending on the context. To improve clarity, the methods section should be organized into distinct subsections addressing individual components, such as runoff estimation, datasets used, and evaluation. Each section should explicitly justify the choice of methods or datasets, which would also help eliminate the repetitive content scattered throughout the manuscript.

Moreover, the assumption that all basins contain ephemeral stream networks is critical to the application of the proposed methodology. However, no effort is made to evaluate this assumption. Factors such as water table depth, which could influence transmission losses through ephemeral streambeds, are not considered, and the limitations of this approach are left unaddressed.

The methodology also relies on the use of reduction factors (referred to as 'Transferal ratios'), yet no justification is provided for their application. Additionally, this approach depends on the assumed low uncertainty of Curve Number (CN) values, which are known to be highly uncertain. No attempt is made to assess the potential impact of this uncertainty on the model's performance.

It is unclear how the authors can assume that grid size does not significantly influence model performance. The spatial aggregation of topography alters flow paths and catchment areas, which is highly likely to affect the model’s accuracy. A multi-scale analysis is essential to properly address this issue.

Furthermore, MATLAB and ArcGIS are commercial applications with may limit its accessibility. The broader user base would greatly benefit if these tools were developed using free and open-source platforms like Python. Is there a specific reason for not using free software in this case?

Regarding Equation 4, there is no explanation provided for how the authors derived this equation. A reference or a detailed description of the underlying assumptions is necessary for clarity and validation.

Similarly, for Equation 7, why is a linear relationship assumed? What is the accuracy or error associated with this equation? These critical aspects need to be justified for the methodology to be considered robust.

Line 15: Specify which dataset is being used.

Line 17: Clarify what the term 'forecasting' refers to in this context.

Line 97: The statement, “Typically, modelling a catchment to incorporate such transmission losses involves aggregating land into sub-catchments with uniform runoff-loss characteristics.” is misleading. This is not typically how transmission losses are estimated.

Line 109: “Create a model to compute generated runoff using global precipitation and curve number datasets” – there are many existing models that do this.

Line 110: “Model flowpaths from points where runoff is generated to the catchment outlet” – all spatially distributed models already perform this function, so why is this presented as a research goal?

Line 112: This is not an appropriate way to evaluate transmission losses.

Line 114: The statement is too vague. Specify which characteristics you are referring to.

Lines 116–117: “The novel contributions of this work lie in the use of fully distributed data sets” – using high-resolution datasets is not novel, as many models are already capable of using them.

Line 118: “In arid and semi-arid regions, there are far fewer rainy days than in humid regions. Only some rainy days create direct runoff. Even fewer rainy days are responsible for runoff reaching a collection point”. How are these be assumptions? The authors should clearly state which processes they are attempting to model with this methodology (e.g., infiltration excess, overland flow).

Line 121: “Within such ephemeral systems, baseflow is less significant, or largely absent, compared to more humid regions”. How are ephemeral streams defined here?

Line 122: “The method described here exploits these characteristics of arid zone hydrology” – in what way?

Line 123: “generating runoff using daily precipitation data, while surface flow (and hence transmission loss) is modelled as a singular annual event”. Why is this approach taken?

Line 124: “Such an approach negates the need to route hydrographs hence sub-basins do not have to be created and catchments can be modelled at relatively high spatial resolution”. The meaning of this statement is unclear.

Line 146: What is the difference between an in-stream cell and an overland flow cell? These terms are critical to the methodology and should be clearly defined upfront.

Line 249: “The calculation of the stream network travel time is more complex and is described in the following section”. Why is this considered complex?

Line 259: “- more complex –” Is it necessary to highlight this? Is the method really complex, or are the authors referring to computational demands?

Line 517: The term 'flow paths' refers to all routes water may take to reach the basin outlet. This could mislead readers, and the authors do not provide evidence of addressing this issue in the manuscript.

Line 468: “these paths pose reduced resistance to open channel flow and simultaneously, result in fewer transmission losses due to the relatively higher saturation of the ground”. This statement is unclear. The streambed of ephemeral streams can become saturated and still lose water, and transmission losses can be influenced by stream stage, due to the hydraulic gradient across the streambed.

Line 471: “this approach restricts the number of datasets, each carrying its own uncertainties, to three”. There is no analysis to support this conclusion, and it could be one reason for the model's low performance.

Lines 481–482: “the contributing factor to suboptimal results may not be the sheer size of the catchment, but rather the potential for larger catchments to be more diverse and complex.”, Do you mean heterogeneity?

Line 490: “One possible explanation for this is that the observed discharge data incorporates flows beyond those generated solely by precipitation within the catchment boundary”, This statement needs more specificity. Are you referring to human interaction?

Line 495: “One potential rationale is that the radar technology employed to generate the SRTM product encounters difficulties in penetrating vegetation”. As stated, this does not seem sufficient. How does this impact the calculation, and what about the influence of the CN values?"