Reviewer 1

Comment	Answer	Changes
It is the opinion of the reviewer that only two cases are not sufficient to understand the model performance and potential limitations. I recommend the authors to add 4-6 more cases and design sensitivity studies to test the different hypothesis they made related to the performance of the B90 model.	We partly agree. The main aim of the manuscript is to present the technical developments of the upgraded model and illustrate its application using one case study for a single year, rather than to provide extensive validation results. However, we agree that including a small validation appendix would add value. Therefore, we propose to include additional validation results using available long-term ICOS datasets from Saxony. The developed and adopted parameter sets will be applied to different ecosystems (grassland, young beech forest, cropland, and an old spruce stand), following the study https://doi.org/10.5194/hess-26-3177-2022.	Appendix B, Section 4.2
The manuscript is submitted as a Development and Technical Paper. Hence, the version number of the B90 model needs to be included in the title.	We partly agree. We prefer not to include a version number, as the model name in the title refers to a model family rather than a specific version, with the sub-daily B90 representing a new branch. Notably, not all GMD articles include a model version number in the title.	-
Abstract: (a) It is my recommendation to add another sentence describing why the performance of the B90 model for wet surfaces is less accurate. (b) Consider removing first line of the third paragraph from the abstract. (c) Kindly provide the full name of ICOS.	Thank you for these suggestions. The abstract will be revised accordingly. a) and c) will be adapted accordingly, however we would like to keep b) since we see it as a 'selling point' of this new model version	Abstract
L28: citations to classical papers and a few new papers are needed here	We agree, additional references will be included.	Section 1
L31: a few important papers which use B90 model should be cited, to justify the 'widespread adaptation' of this model.	We agree, additional references will be included.	Section 1
I recommend rewriting the 'Introduction and Motivation' section. This section needs some restructuring, and better (yet short) descriptions of (a) skills of the previous version of the B90 model (b) potential drawbacks (c) why these issues were not addressed by previous researchers and (d) how the authors intend to improve the model. See	We agree, the Introduction and Motivation section will be revised.	Section 1

Comment	Answer	Changes
some suggestions below.		
In my opinion, the first paragraph of the introduction section should discuss hydrological modeling in general and not any specific model development history.	We agree, the paragraph section will be elaborated.	Section 1
L40-45: It is not clear why the model output was daily or why the energy balance was not considered? Computational constraints? A short description is required.	We agree, this point will be clarified in the revised manuscript. In brief, daily temporal resolution was considered sufficient for computational efficiency and for most practical applications in water balance studies. In addition, the energy balance was not explicitly considered because the original B90 model was developed as a hydrological mass balance model.	Section 1
L51, L54: can you find a more recent paper?	We will attempt to identify more recent references; however, studies on fog and dew processes related to water balance modelling are relatively rare.	Section 1
L60: In this paragraph, the Shuttleworth-Wallace evaporation model needs to be very briefly explained with citations, and possible alternatives (if any) should be mentioned	We agree, a brief explanation of the Shuttleworth–Wallace model will be added along with appropriate references and possible alternatives.	Section 1
L65: 'ICOS DE-Tha' - This term is used for the first time in this line. The full name should be mentioned.	We agree, will be corrected.	Section 2
L67: the authors mention about the ICOS DE-Tha station data being used in 'numerous' studies yet cite only one paper.	We agree, additional references will be included.	Section 2
L65-L78: These two paragraphs provide the description of the station. However, it is important to describe the instruments present in this station. I recommend adding a table with brief description of the instruments present here, especially those important for generating the B90 model forcing data.	We agree, a table summarizing the main instruments used at the station relevant for generating the B90 model forcing data, will be added in Appendix A.	Section 2, Appendix A
Figures 3-9: Please increase the DPI and the font size. Additionally, please use the Coblis-Color Blindness Simulator to make the figure more accessible.	We agree, figures will be revised accordingly. The original figures were created at 300 DPI, but the conversion to PDF reduced the quality; this will be corrected.	-

Comment	Answer	Changes
L210: “This is expected…” Citation needed.	We agree, additional references will be included.	Section 4.1
Can the authors comment on the large IQR shown on Fig 6? Is this expected?	Yes, this behavior is expected due to the high turbulence characteristic of eddy-covariance measurements. High-frequency observations (25 Hz) often exhibit considerable variability because of strong wind speeds at 42 m height and the large, heterogeneous tower footprint (up to about 300 m). Fluxes are averaged over 30-minute intervals, and when cumulative values (solid line) are calculated, the interquartile range (IQR) derived from the original high-frequency data also accumulates over the year, resulting in the observed wide range.	Section 4.2

Reviewer 2

Comment	Answer	Changes
Section 4.1. Does subdaily data refer to a 30-min time step ?	Yes, a 30-min time step is used. This will be clarified in the revised manuscript.	Section 4.1
l218-219 : You mention that “The differences between observed and modeled turbulent fluxes are relatively small, indicating good agreement between the two datasets.” However, as shown in Table 1, the differences for LE between B90 and the observations are close to 20%, which does not seem particularly small. What do you mean by June and December? Does this refer to the whole month, or only to the days shown in Figures 3 and 4? You could also show the minimum, maximum, and average subdaily differences between B90 and the observations to better illustrate the variability of these differences.	We agree that the wording should be clarified. Table 1 refers to the entire months of June and December, including all available data (not only the periods with quality flag = 0). This will be clarified in the revised manuscript. In addition, we will include an extra table presenting the minimum, maximum, and average sub-daily errors for LE and H for each month of 2024 in Section 4.2 to better illustrate the variability of the differences between the modeled and observed fluxes.	Section 4.2, Table 2
l254 "The modelled H aligns..." you mean λE instead of H	The sentence will be corrected.	Section 4.2
l280-288. No correlation is found for wet conditions, but the comparison is based on a small number of points, and the dispersion does not appear to be larger than for dry weather. With more points, a correlation might be found. It would be good to add a comment on this subject. Moreover, do the observed λE and H fluxes also have a quality flag of 0 for wet conditions?	We agree that this point requires further elaboration. The number of data points under wet conditions is indeed relatively small, which limits the robustness of the correlation analysis. This will be noted in the revised manuscript. For both dry and wet conditions, only time intervals where both λE and H have a quality flag of 0 were used in the analysis.	Section 4.2