Report #1

- Abstract L15: "We explore the use of local precipitation collection complement with multiple sources of global reanalysis data". I think this is rather overselling what was done, which was to use local rainfall data with data from a single reanalysis product. Reword as "We explore the use of local precipitation data in conjunction with data from a global meteorological reanalysis" or similar.

Statement changed as the suggestions.

- Abstract L16: "Our results show that the coarse resolution of rainfall data is the main reason for reduced model performance." As with the previous iteration of the manuscript, I think this is a rather bold conclusion. I would be happier with "Our results suggest...".

The statement “rainfall data is the main reason for reduced model performance” is removed due to the change on results.

Rainfall data

- L43: "runoff fluxes are simulated in gridbox with rainfall data from the nearest monitoring station (Campinas, Atibaia, and Nazare Paulista) of Campinas Agronomic Institute (Campinas-IAC)". For years with missing data, rainfall "is replaced by using the time series from the nearest Department of Water and Electricity (DAEE) station". As there are 5 DAEE stations shown in Fig.1, I am wondering why you didn't use those for rainfall, rather than using the 3 Campinas-IAC gauges. Perhaps the DAEE stations have more missing data (though they are being used to replace missing data)? Only gauges from the Atibaia catchment have been used - could gauges from adjacent catchments also be used to get a better idea of the rainfall coverage (perhaps with a more sophisticated analysis than the nearest-gauge approach currently employed)? Given that one of the main conclusions of the study is that better rainfall data are required, it would seem sensible to explore all possible sources of data. Similarly, there are global rainfall products and (meteorological) reanalysis products that might be considered. Although these might be of questionable value over such a relatively small catchment, and their value likely depends on the quality and number of local observations that are incorporated in them, other studies have shown that these can be useful sources of input data - and they can have advantages such as spatial representivity, complete high frequency time series, and consistent relationships between variables. These data might or might not improve the modelling results, but given the lack of current in situ data, their use should be considered. Ideally this would be part of the current study but

otherwise some of these possibilities (or others) should be discussed. The last two paragraphs of Section 3.2 (~L160) could be expanded to better signpost this possible future direction; at present this is scarcely touched with L167 "There is a possibility for the model to be further improved once more adequate rainfall data is available."

Due to the high variation of rainfall, we thought the data from adjacent may not be an improvement to the model. Instead, we explore the use of the data from 5 DAEE stations in the basin. We use linear regression for the missing data and found that the results could be improved (especially in the year 2017 & 2019). The method is rewrote as “For each sub-basin, the surface (Qsurface) and sub-surface (Qsubsurface) runoff fluxes are simulated with rainfall data from the monitoring station (Campinas, Atibaia, and Nazare Paulista) of Campinas Agronomic Institute (Campinas-IAC) and from the station of the Department of Water and Electricity (DAEE, 2022).” The model performance is good at most of the years. Therefore, we thought that rainfall data is not the major source of uncertainty. We removed “rainfall data is the main reason for reduced model performance” and the related statements.

Other points

- L92: "Mean value and standard deviation of the topographic index data is obtained from Marthews et al. (2015) as follows:" - the text that follows actually described something else (related, but not the mean and std dev).

Description added “Numerical integration using a two-parameter gamma distribution can be found …”

- L100: "Soil hydraulic characteristics can be estimated using the relationship of Brooks & Corey (1964) or a more robust formulation of Van Genuchten (1980)." Do the PTFs of Hodnett and Tomasella provide parameter values for both of these hydraulic parameterisations? Which approach was used in the JULES modelling?

Soil hydraulic characteristics are estimated using the relationship of Van Genuchten (1980) in our study.

- Calibration (sensitivity) is assessed only at the basin outlet, but the same parameters are then used for all sub-basins. It would be interesting to know if calibration at other gauges would return similar parameter values (backing up your use of the outlet alone) or might suggest spatial variation of parameters - e.g. from lowland to upland regions, which might be expected to behave differently. Given that several flow gauges are available (and used) why not at least explore whether a better model set up is possible?

We explore the sensitivity of hydrological parameters in the upper, middle, and lower basin

in Section 2.3.

“We evaluated the sensitivity of hydrological parameters of PDM and TOPMODEL to determine the most suitable model in the upper (3E-063), middle (3D-006), and lower basin (4D-009) using the simulated results from the first year.”

And the results are show in Section 3.1

“We examined the model performance with a combination of soil depth, shape factor, and the minimum soil water content, and found that the highest performance with combination dz=1.0, b=0.5, s=0 in the lower basin, which altered the shape factor alone from the default setup. In the middle and upper basin, an increased value of the minimum soil water content simulated higher performance (dz=1.0, b=0.5, s=0.1). Therefore, we run the full-time series of modelling with these parameter combinations.”

Figures and Tables

- Please indicate the gauging station or part of catchment used in each figure and table. e.g. sensitivity results in Fig.3 is at outlet (I think). L149 says Fig.5 is for the lower basin - this should be included in the caption for Fig.5. I'm guessing that the later plots are also for the outlet - but that should be clear.

The part of basin is included in the description (Fig 2-7)

- Figure 2 doesn't add much - I would consider removing it.

Figure 2 removed.

- **Minor points and language**

The manuscript is written in reasonable English, but the phrasing is rather odd at times. The meaning is generally obvious, but a fluent speaker of English could tidy the manuscript, possibly with relatively little effort.

Here I list a few examples of bad phrasing here, but there are more:

L25: "Up-to-date, a few research activities"

L28: "In which, a commonly used"

L160: "Despite the highly variation"

L139: "more intense rainfall" - better as "more rainfall". "Intensity" is usually used when characterising shorter timescales, e.g. the rainfall rate during a rain event, not an annual total.

Citations: Some of these are not formatted correctly. e.g. L82 Clark consistently appears as "Clark, Douglas B.".

Relevant changes are made.

Report #2

- The authors have nicely addressed most of the comments from my first review. However, I still disagree with the statement from line 17 that 'Our results show that the coarse resolution of rainfall data is the main reason for reduced model performance.' This is certainly suggested by the results but I don't think the results - as shown here - make the argument sufficiently strongly for the quoted statement to be true.

The statement “rainfall data is the main reason for reduced model performance” is removed due to the change on results.

A few additional points:

- line 43: Suggest replacing 'in gridbox' with 'in each sub-basin' for clarity
  line 44: 'The year' should be replaces with 'Years'
  line 88: 'The parameter is initially set...' - please make it clear which parameter is being referred to, and suggest replacing '0.1/0.5' with '0.1 or 0.5' or similar
  line 99: Suggest 'We evaluated the sensitivity of modelled streamflow to the hydrological parameters shown in Table 1..'

Relevant changes are made.

- line 101: Which soil physics scheme did you use here?

Description added: Soil hydraulic characteristics are estimated using the relationship of Van Genuchten (1980).

- line 105: The sentence 'For each basin...' is a bit confusing

Description added: For each sub-basin…

- line 107: The notation in equation (3) needs some clarification - e.g. what is n here? And what does the (t-ti) notation represent? Similarly, make sure all symbols and notation are explained in eqns (4) - (6).

Description added

- Fig 3a. Suggest making sure all the values of dz are in size order in the legend to make

interpretation easier. Also please make clear in the caption that these are daily mean flow values in figure 3.

Order of values adjusted, and “daily flow” are added.

- Line 126: Should '4' be replaced with '0.4'?

Yes. The value is replaced.

- Line 132: '..which altered the shape factor alone.. ' - does this refer to an alteration from the default values? Please clarify.

Description added: which altered the shape factor alone from the default setup.

- Line 145: 'Several authors...' but only one reference given. Are there others?

Statement removed due to the updated results, which shows improving performance.

- Fig 6: please label panels a, b and c.

Labelled (New fig 6 and the others)

- Line 154: '..from..'?
  Line 159: Replace 'expect' with 'except'
  Line 165: Please clarify which gap you are referring to here.

These sentences are removed due to the rewrote discussions.

- Line 166: Is the quoted NSE here for the SWAT model or your work? Suggest presenting both values here.

Description added: “The model performance for daily flow in our study (NSE=0.74) is higher than the SWAT model's estim0ation (NSE=0.61 in the validation period) (dos Santos et al., 2020).”