The manuscript “Impact of model structure on flow simulations and hydrological realism: from lumped to semi-distributed approach” reports on the performance of three different versions of the MORDOR hydrological model. The model versions consist of the initial lumped model MORDOR v0 developed in the 1990s, the revised lumped version v1, which includes new formulations for calculating evapotranspiration and a more complex snow model, and finally the version MORDOR SD, which is basically a semi-distributed version of MORDOR v1. The evaluation of the model performances is based on a multi-criteria split-sample test, utilizing a comprehensive data set from 50 different catchments in France. For the evaluation of runoff, 5 different streamflow signatures are used, of which three are applied in the objective function for model calibration. Additionally, an independent evaluation is performed with MODIS snow cover data, measured snow water equivalent time series and actual evapotranspiration data of the MOD16 AET product. The results show noteworthy improvements of the simulation performances for the versions v1 and MORDOR SD, especially when evaluation the streamflow signatures. MORDOR SD logically also outperforms the lumped models when it comes the simulation of snow cover and snow water equivalent. Although the new model versions show improved representation of actual evapotranspiration, no clear advantages are found for the semi-distributed version of the model. Surprisingly, the simulation results showed good agreement with the data sets not used for model training.

I enjoyed reading the manuscript. It is straightforward, logically structured and mostly well written. The presented models are not necessarily revolutionary but are used in operational hydrological studies and flood-forecasting activities in EDF (French electric utility company), which belongs to the three largest energy producers on the globe. It is of interest to read about model development activities outside the classic academic circle, having in mind that the applied models have clear implications on society and decisions made in practice. The topics and findings are therefore of interest for the (scientific) community and a publication is sincerely recommended.
There are however some points which need to be addressed and where additional clarifications and analysis would be useful and are needed.

General comments:

1. The authors show interesting and relevant simulation results that are in good agreement with data sets not used in calibration (FSC, SWE, ETA). These detailed results are however only presented for 6 out of 50 catchments. It is clear that (i) snow is not relevant in all catchment, (ii) availability of SWE measurements may be limited and (iii) that the ETA estimates from MOD16 are unreliable in alpine regions. It would however be of interest to see results of the model performance for more catchments in this context.

2. The paper reports on different versions of the hydrological model MORDOR. It would therefore be important for more clarity to add two overview tables, containing (i) model parameters, units, range of parameter values and description and (ii) model fluxes and states, also including units and a description of the variables.
3. MORDOR v1 and SD include a modified and improved snow routine. How is snow sublimation considered in the models?
4. From the manuscript it is unclear, what temperature data is used.
5. How is PET / ET0 estimated? How large are the differences between the PET values in version v0 compared to version v1/SD? This is crucial since it will influence the AET results.
6. What is the reason to use KGE in the objective function and NSE for evaluation?
7. The authors state that the model runs at different temporal resolution. Are the calibrated parameters comparable between the different temporal resolutions?
8. In the Appendix the model formulations are given. Here it would also be interesting to give some technical details on the models: In what language are they written? Is there an internal time discretisation implemented? How long does a run take?

Specific comments:

P1L22: semi-distributed

P2L1: Most studies

P2L18-19: It is unclear why the two best solutions are selected (I presume the presented MORDOR v1 and SD) and why these three new formulations are then compared with the historical version.

P2L23: …in quality and length of available records.

Table 1: The number of free parameters are not underlined in the table.

Table 1: Could you please clarify what is to be understood under “adjusted PET from a statistical formulation driven by temperature”?

Figure 1: It would be good to highlight and name the catchments shown in detail in the analysis.

P5L23: Why not call the section 3.1.2 simply MORDOR V1

P5L24: revised model formulation

P6L18: … time-step based on air temperature.

Figure 2: A subdivision in 10 elevation zones is shown. Is this the standard number of zones used for spatial discretisation, also for the other catchments?

P8L7: The term “streamflow interannual daily regime” is not easily understandable.

P8L8: Do you mean the flow duration curve with “streamflow empirical cumulative distribution”?

P8L9: How are the streamflow recession periods defined in practice?

P8L10: What is meant with the 1st-lag streamflow derivates and how are they calculated?

P8L11: … performance results are resumed using… What is meant with resumed?

P8L27: (i.e. 100 simulations)

P9L8: in figure 4

Figure 4: Legend is missing

Figure 7: Why is R^2 shown and not NSE (as in other plots)?

P12L7: (see Figure 2?)

P12L20: What is the second order impact? Not being important?

Appendix A

Additional tables as mentioned above showing parameters, fluxes and states would very much improve clarity when reading the appendix. It is very frequently unclear what the used variables mean. Figure 10 does not help very much in this context. Following are a few things a noticed, being however sure, that the list is probably incomplete:

P13L31: What is meant with "flow length of each gridcell to the outlet", since we are talking about zones?

Eq. A1 & A2: Please check equations. They seem to be erroneous.

Eq. A3: Why ET0 and not PET? This is not consistent with the other parts of the manuscript.

Eq. A4: Should K_c and R_{pot} not have a time component?

Eq. A5: It is unclear, for what A5 is needed.

Eq. A11: K_f: Is the time component missing? It is unclear, what the difference between k_f and k_{fp} is.

Eq. A14: From the equations it is unclear, how $u_i(t)$ is calculated.

Eq. A21: From the equations it is unclear, how $z_i(t)$ is calculated.

Eq. 23/25: What is s_i?

Eq. 24: What is k_N?

Eq. 26: What is fl?

M. Herrnegger, 22.02.2017