The manuscript HESSD 10, 8635–8681 "PERSiST: the precipitation, evapotranspiration and runoff simulator for solute transport" by Futter et al. presents a flexible, conceptual and semi-distributed model applied to sub-catchments of the River Thames, UK. The manuscript is well written and the paper is of interest to the hydrological community. However, my main criticism is that the novelty of the paper is not clear and I will detail this below.
Nonetheless, I recommend this paper for publication in HESS subject to the clarification of my comments.

General comments:

- The title and the abstract imply that the paper presents a flexible and directly coupled flow-solute model. However, the reader only discovers quite a bit later that the paper essentially deals with a flexible rainfall-runoff model framework ultimately developed as a delivery model to biogeochemical models such as INCA. Flexible model frameworks are an approach forward to testing hypotheses about catchment functioning and the dominant hydrological processes governing the hydrograph, but it is not clear what the differences, advantages, disadvantages and novelty of the model framework presented are compared to other recent flexible model frameworks such as FUSE, FLEX, SUPERFLEX and most recently DYNAMIT (which incorporated solute transport). I therefore suggest that it should be clearly stated throughout the manuscript that a flexible rainfall-runoff model framework is presented using a more thorough discussion in the light of the wider literature.

- The model calibration strategy seems flawed, which needs at least clarification and/or new analysis. First, the model is calibrated using only the Nash-Sutcliffe performance measure (see specific comments below). I strongly suggest considering using multi-objective calibration especially if peak and low flows are of interest for posterior solute transport modelling. Second, the total number of calibrated parameters is rather large (108) which necessarily results in parameters that cannot be uniquely identified during calibration. Third, the flexible model structure selection procedure is not clear to me. I suggest to clearly show which are the potential model structures, which structure was ultimately selected during calibration and how was this best performing (or most parsimonious?) model structure selected.

- There are some inconsistencies in terms of the paper structure. For example, in the method section there is no mentioning of the sensitivity analysis, but a table (6) and three figures (8a to c) are dedicated to show results from a sensitivity analysis. Further to this, the result section is very brief and could be more elaborate or merged with the discussion section altogether.

Specific comments:

Page 8637, Line 1-3: I think only Hrachowitz et al simulated water and solute transport and with good results.

Page 8639, Line 9: Is the only difference to SUPERFLEX a semi-distributed catchment representation?

Page 8647, Line 18-20: Are those fractions fixed? In this case based on what criteria?

Page 8652, Line 15: The NSE performance measure is biased towards peak flows, but low flows as you state on page 8640, line 2 are important for biogeochemical processes. I invite the authors to consider using a multi-objective calibration strategy evaluating more aspects of the hydrograph other than peak flows.

Page 8652, Line 23: Was really a total of 108 parameters simultaneously calibrated? I strongly suggest revision of this ill-posed calibration strategy.

Page 8653, Line 2-5: Please clarify this sentence.

Page 8658, Line 26-29: These sentences seem identical to page 8638.

Figure 6a and 7a: I suggest representing the maximum and minimum simulations as bands to improve visibility.