

## ***Interactive comment on “Flexible vector-based spatial configurations in land models” by Shervan Gharari et al.***

**Anonymous Referee #1**

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Gharari et al present an application of the VIC model, using Grouped Response Units to define computational units, rather than grids. It is acknowledged that this concept was already presented in 1993. I do think it is justified to re-introduce older concepts if these can serve the science of today, however, then the re-introduction should also deal with some of the challenges of today, and this is currently not the case.

Firstly, the reader has to do quite some searching to fully capture the concept of GRU's, and its comparison to HRU's. Only when the investigated cases are presented it becomes clear what a GRU exactly is and the choices it encompasses when defining GRU's. This seems to be the result of an overall quite weak structure in the manuscript; the introduction does not clearly present the aim or goal, probably because the structural test (case 3, presented in the intro in line 103-110) seems to be completely out

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of context. In the same fashion, 3.1.3 is not well embedded. Furthermore, sections are not logically structured, e.g. subsection 3.3 only consists of 2 sentences while some subsections are longer, and parameter are presented well after the calibration is discussed and the cases are introduced. I suggest restructuring the manuscript, clearly introducing the concepts with simple examples, and omitting parts that do not fit the aim or goal of the study.

One of the key questions in defining the spatial discretization of models is of course the calibration. Whereas the GRU's conceptually might make sense compared to grid cells, it introduces new questions on how to calibrate the parameters, and this is not well explained in the text. Does each GRU receive its own set of parameters? And is this then related in any way to the underlying data? As the authors rightly suggest, parameter ranges can be adapted based on soil type or land use, but it seems this was not done by the authors. Not surprisingly, the results demonstrate some of the already known flaws from calibrating on discharge outlet; the everlasting problem of equifinality and overparameterization. If the authors believe the GRU concept is valuable to re-introduce (and I can see it has potential), this value should be demonstrated in a more sophisticated calibration. If the same calibration is done as for usual grid-models, of course we know we can achieve good model performance because there are enough buttons to push, but what do we learn from it compared to a grid-based model and what does it add? 1000 evaluations in the calibration procedure seems rather limited given the dimensions of the problem; this is understandable from a computational point of view, but also a chance to demonstrate why GRU's make more sense than grids within these bounds, by making use of the opportunities that GRU's offer in comparison to grids.

An example: The results from Figure 4 are criticized in the text as: "The result indicates the two parameters that are often fixed or a priori allocated based on look up tables can exhibit significant uncertainty and non-identifiability". The Brooks-Corey coefficient is from such a high conceptual level that it might be challenging to find good values

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in lookup tables, but Ksat might be able to be estimated. The lookup tables can then provide an indication for a search range for the parameter and decrease the equifinality issues with these two parameters.

Shortly, I can see why GRU's might have added value in land-surface modeling. However, the re-introduction of this concept in this manuscript might not make a very good case to convince people of this fact, given that calibration is one of the main challenges and the potential for GRU's in this context is not well explored.

Other suggestions:

In section 2.3, it remains unclear why structural changes to the model were made. Some of the most sensitive parameters of the model ( $D_s$ ,  $D_m$ ) have been replaced by a linear reservoir coefficient. Furthermore, the description focusses on VIC4 while VIC5 was explored. Why is that?

It is not explained how the parameters in Table 2 were selected for calibration. It is for instance remarkable that no snow parameters, such as snow roughness, are included in the calibration – is this because GRU's focus on soil and land use? Furthermore, it is not clarified to which soil layer  $E_{exp}$  and  $K_{sat}$  refer, or is this kept constant over both soil layers?

Minor for tables and figures:

Table 1 the unit of forcing resolution is missing (degree) Figure 3 the a,b,c labels are missing, the legend is not readable. Figure 4 not sure if this is very informative. More interesting to see a boxplot of every parameter to demonstrate the wide range. Figure 5 Caption says “deviation” but you demonstrate NSE compared to benchmark run, and not the deviation in NSE.

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