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Comment on gmd-2022-123

Peter H. Verburg (Referee)

Referee comment on "CLUMondo-BNU: Adaptive determination of conversion orders in CLUMondo for simulating land system changes with many-to-many demand-supply relationships" by Peichao Gao et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2022-123-RC1>, 2022

As one of the developers of the CLUMondo model it is nice to see others studying our model in a lot of detail. As the model is published as open source with detailed online documentation and manuals we have seen many groups making small or large improvements. As the model is published under the creative commons license it is good practice to make such a deviant version of the model distinguishable from the original developer version of the model. The authors of this paper have not done so, calling it version2.0 is not really something distinguishable from the main versions of this software.

The authors have done a good job in analyzing the source code and the ways they explain the model in a more technical manner as compared to the original paper is mostly correct. The claim that there is no good explanation is not fully correct: the main mechanisms are well documented in the published papers (there are a lot more of these than the authors refer to), their supplementary materials and the user manual available online.

The authors justify the 'new' version by making a very small 'improvement' to the source code. This does not change the functioning of the model. Where a hierarchy of land systems in the original version of the model is manually set by the user they have programmed an automatic approach to determine the hierarchy. They claim different results. This is not correct. The hierarchy can be manually set exactly the same as with their automatic approach (which is basically comparing values and setting the order according to their size). The model should then give exactly the same result. So, the claims are not justified.

Moreover, the accuracy of the simulation is determined by standard kappa values. There is a large literature (Pontius et al.) that has shown that kappa values are not appropriate for validating land use models. Authors have completely ignored this while this is very common knowledge in the literature. Moreover, better performance in one case would not mean 'universal improvement' as the authors are indicating.

But, most important. The 'improvement' is not an 'improvement'. As the authors site the 'Asselen and Verburg 2013' paper for the reasons for setting this manually indicates there is a good reason for enabling the user to set these values manually. As the land system classification and ecosystem service data used are not always fully aligned in terms of resolution, approach etc there are sometimes some small or big biases in the average provisioning of services that a land system provides when this is determined by an overlay method (as applied by the authors). If these would lead to some weird conclusions: a pasture land providing more carbon storage than a peat forest, the user can manually modify this hierarchy. Such a situation can happen as maybe current pasture land is only a few pixels in very special conditions. The automatic method would promote the conversion to pasture land to fulfill a carbon demand. By setting the hierarchy manually this can be avoided. Also, the threshold used to distinguish small from big difference depends on the distribution of values, units etc. Therefore, we have in the past changed the automatic procedure we had in the beta version to a manual version that still allows the same settings, but provides the user with more flexibility. The proposal to make this automatic again is thus, given the history of the model, a step back! (and decreases the flexibility of the approach and user control on the functions).

The authors rightfully claim that an automatic method makes it easier for the user, and the user would need less expertise.

Land systems are complex. Modelling these appropriately requires a deep understanding of system function and model representation. 'making it easy' leads to applications without sufficient understanding and thus flawed results. If one wants to do things 'easy' we refer to the IDRISI land use modeller which is made for that purpose.

Finally, there are many small errors in the manuscript and the writing is far from clear, obscure sentences include: 'We present in this section a subtle and feasible method for the establishment, first qualitatively and then quantitatively. The qualitative establishment involves two steps: generating land systems based on scale transformations and defining the 305 services of different land systems. The quantitative establishment is to quantify the land system-dependent supply (in 2010) of and the aggregated demand (in 2020) for each land system service.'. There is no explanation what this 'scale transformations' are and how this is all incorporated. To me there remain more question marks on how the authors have implemented the model.