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Interactive comment on "Insights into biogeochemical cycling from a soil evolution model and long-term chronosequences" *by* M. O. Johnson et al.

M. Hoosbeek (Referee)

marcel.hoosbeek@wur.nl

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Soils are usually included in biogeochemical models as rather static entities, i.e. soil texture, porosity, hydraulic properties, mineralogy, horizons, pH, CEC, etc. are given and do not change during simulations. Only pools of carbon, and at best one or two nutrients, are simulated. Due to the slow nature of most pedological processes, this is usually not a problem at time scales of decades. However, at longer time scales and in situations where ecosystem development is essential, a soil evolution model is more appropriate. Also, a soil evolution model needs input data on the initial geological substrate, which may be more homogeneous and easily accessible at regional and global scale than detailed soil information.

In 1985 Mike Kirkby published a seminal paper on “soil profile modelling in a geomorphic context” that has been discussed in many pedology courses. The authors build on the model initiated by Kirkby and either extended or added processes dealing with biomass allocation to litter pools, bioturbation, organic matter decomposition and nutrient cycling.

All processes are well described, although a few minor questions remain:

Section 2.1.2. In case evapotranspiration surpasses precipitation, it an upward water flow possible? For instance, is calcium accumulation in the top soil possible in an arid environment?

Section 2.5.1. With nutrient cycling included in the model it seems tempting to make biomass production (Np) dependent on nutrient availability (through stoichiometry). As stated in op page 5823, line 25, this may improve early-stage ecosystem development.

Section 2.5.2. Root respiration (Rc) is now a number taken from the literature. But, Rc is of course related to Np. And with Np related to nutrient status, vegetation-soil interactions may become even more dynamical. Not a necessity for the current model (and manuscript), but rather a thought for the future.

Section 2.6. Do I understand correctly that nutrients are released into soil solution based on the stoichiometry of fresh litter? So, SOM does not approach, for instance, the C:N ratio of microbial populations of over time? Because, it takes nutrients to store C in the soil (lower C:nutrient ratios over time), the nutrient availability may be overestimated in the model.

Section 4.4. The belowground C stocks presented in figure 6 are compared with data from forest plots near Manaus. This seems a bit odd. Earlier in the model description section (and later in section 5) I had gotten the impression that the model input data were taken from a chronosequence on Hawaii. Moreover, based on figure 6 it was

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concluded that “the decreasing decay rate with increasing soil depth is perhaps the most realistic formulation”. But, if a soil with less SOM below 1 m had been selected, would the conclusion have been the opposite?

Section 4. The evaluation of the effects of the step-by-step addition of processes (Figs 2 – 3) on simulation results makes sense to people with sufficient pedological knowledge and experience. But, it is rather subjective and hard to verify. Section 5, page 5833, line 27. “The depth of the vertical model layers is increased to 0.25m . . .” Should this be 0.025 m? As compared to Zr and other parameters, 0.25m seems too thick.

Page 5836, line 11. “is still is still”

Section 5. The model evaluation based on the Hawaiian chronosequence is informative. Model advancements and limitations are well described, although in this type of study the evaluation of results is inherently subjective.

Because model evaluation is limited, I think emerging “insights” should be taken cautiously. I think the title is overstating this aspect. New “insights” are not the major result of this study, as suggested by the title. Still, the presented study is an interesting addition to earlier work by Kirkby and others in this field of science.

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