Biogeosciences Discuss., 11, C2521–C2523, 2014
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Biogeosciences
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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.

Anonymous Referee #2

Received and published: 15 June 2014

In this paper the authors present and evaluate at the site-scale a pedogenesis model based on the modeling framework proposed by Kirkby (1985). The model is here compared with different chronosequence data from Hawaii. The model results show that vegetation accelerates soil formation. Additionally the authors found general agreement (between model output and data), but some discrepancies respect to mineral plant nutrients like P and K. The last finding suggests that additional processes to the passive vegetation nutrient uptake as represented in the model must be accounted for in order to better match observations.

The paper is well written and I like the fact that the authors propose using a relatively simple model to advance the understanding of the processes linking soil processes

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and biogeochemical cycling. I think the major strength of the paper is that it explicitly compares model output with observations. However, I have some major concerns and questions that I think need to be addressed before the paper can be published in BG.

1. In general I think that model development has to have a clear motivation and underlying question. The authors claim that the development of their model is motivated by "several important global-scale questions". However, I think that one cannot make a general pedogenesis model that can be used to answer all questions. Only with a concrete aim or question the modeler can decide on the level of complexity and which processes have to be included, while accounting for the computational cost and data availability. Therefore, I suggest the authors to define a clear motivation at the beginning and in the discussion and conclusion to link to the general motivation, clearly stating what are the relevant processes that still need to be consider on the one hand and state the advancement of understanding on the other.

If the authors were considering using their model for other settings then those of Hawaii, which is an erosional landscape, I would argue tectonic uplift should be added in the list of missing processes, and evaluating their results in other cronosequences on continental regions would be necessary.

If the motivation is to build a model that allows understanding the effect of weathering on the long-term carbon cycle, I think one has to include processes at longer time scale as well, for example tectonic uplift, sea level rise and erosion in a more mechanistic way.

For approaching questions related to mineral nutrient limitation in the lowland Amazon Basin (P limiation), I think that one has to consider tectonic uplift and more explicit vegetation dynamics, such as mycorrhizal uptake, root exudation, occlusion processes and exogenous P inputs. I was surprised to find a figure relating their model results to Amazon soils, because I find no reasoning that would allow to use the model framework proposed and tested for Hawaii to the Amazon, which is quiet distinct in its geologic

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settings.

2. The authors use the method of Cosmogenic nuclides to estimate surface erosion rate. From the paper I understood that this method could be used in places where soil have reach a steady-state (P 9 L11). Contradictorily, the authors parameterize this in a merely denudation landscape, where soil production from bedrock does not balance rates of loss due to surface erosion. This is evidence by fact that after few millions of years of soil development the islands in Hawaii disappear.

3. I was not able to fully understand how vegetation dynamics are represented in the model. The soil model drives changes in nutrient availability over time; however, I do not understand how at the same time that the model assumes a constant nutrient carbon stoichiometry in vegetation (and SOM) the productivity is kept constant in over time. Could the authors please explain better how nutrient are balanced in vegetation and how the assumption of constant stoichiometry relates to gross primary productivity, biomass production and soil organic matter decomposition.

4. I personally like modeling studies that provide an overview over the processes that are build in the model and the assumptions they are based on. I think including a diagram (e.g. flow chart) may further help to get an overview over the model structure. Therefore, I suggest including such a diagram and clearly state the model assumptions and processes considered (also with respect which ones have been developed and which ones were already incorporated in Kirkby (1985)).

Interactive comment on Biogeosciences Discuss., 11, 5811, 2014.