Dear Dr Zaehle

We very much appreciate your very thorough considerations of our manuscript and thank you for your detailed comments. We agree that the paper benefits from the restructuring you recommend and we have added the new sections as suggested. We have also stated in the new discussion section how the results we show demonstrate the usefulness of the model for biogeochemical applications. We hope that this manuscript is now clearer and acceptable for publication.

We reply to each of your comments below and detail each of the changes we make.

Editor Initial Decision: Reconsider after major revisions (20 Aug 2014) by Dr. Sönke Zaehle
Comments to the Author:
Dear authors,

Many thanks for your revised manuscript. After consulting the reviewer comments and your responses, as well as my own reading of the revised manuscript I think that major revisions are required before this manuscript may become publishable in Biogeosciences. My main criticism is that it does not become clear, what the new understanding gained from this new model is. I believe that this is mainly a question of the structure of the manuscript, and I make some suggestions for making the story more transparent below.

I also think that the revised manuscript should contain some of the responses to the reviewer that you provided (see below) in the discussion section.

When revising your manuscript, please provide a point-point explanation of what has been changed.

For somebody, who is not familiar with the Kirkby model, it is hard to see what your new developments are compared to the original model formulation. Try to emphasise more your new parts relative to the original model. I would suggest to move the equations of the original model into a second appendix, summarising only the key points in Section 2, which are required to get what the model is doing. I would revise Figure 1 to illustrate which parts of the model you introduced (maybe as dashed versus solid arrows), such that

the reader can better appreciate your contribution. Figure 2 can be safely moved to this new Appendix, or removed.

We have made a big attempt to move parts of this section into an appendix yet keeping the rest of the section readable and easy to follow.

The changes involve:

Page 5 line 2 we have inserted text which explains that although we base the model on Kirkby's we needed to make many changes to the structure and components in order that we can explore in detail the coupling of biospheric processes with the soil processes and properties. We have removed lines 3-7. We believe that the new model structure is so different to Kirkby's that it is not possible to simply split processes into new and old in the schematic because the processes no longer act on the same properties. We hope that this is acceptable.

Page 6 line 14, we have replaced "The model processes are detailed in the following section" with "The key elements needed to understand the model rational are detailed in the following sections"

Because we shorten many of the sections in section 2 (Model description) we have changed the headers to un-numbered, italics.

Page 7, lines 1-25 (equilibrium description) are moved to the appendix.

Figure 2 and lines 13 onwards of the leaching section (page 10) are moved into the appendix.

Page 11, the ionic diffusion equation (eqn 12) is moved to the appendix.

Page 11, the bioturbation equation (eqn 13) is moved to the appendix. In line 11 (now in the appendix) we have changed Nz to zmax to avoid any confusion with N.z.

The methods, results and discussion section as very interwoven, which may seem logical when you are familiar with the topic, but which makes it hard for a reader to follow, because sometimes it remains unclear whether you state a model assumption, explain a model result or refer to something the model should be doing. I would recommend to more closely follow the standard structure of a journal article (methods, results, discussion).

We agree that this will improve the manuscript and have restructured the sections as you recommend.

In particular, I would move the text in Section 4 prior to Section 4.1 in a new Section 3.1, which describes the model set-up and runs

We have done this. Section 3 (Model set up and simulations) now has 3 subheadings
3.1 Model solution and parameters
3.2 Simulations demonstrating the model processes
3.3 Model simulations and setup for Hawaii chronosequence sites

Similarly for Section 5, the text before p25 line 4 (goes to Section 3.2). The point here is that you not only introduce a new set of site conditions and runs, but a lot of other information, which distracts from your model results. It also introduces a new model equation, which should not be presented here, but where you describe the model.

We have done this. Now section 3.3.

Move the discussion texts in page 19 l 2 ff into a separate discussion section. At this point in the text it is sufficient to write "As discussed in Section X (the discussion section),the weathering sequence of basic oxides displays similar properties than in other studies." or something along the lines. The rest of the text distracts from comparing this result to the other model formulations.

We have done this.

It is unclear to me, why you discuss Section 4.1 in depth, but do not provide a similar assessment for the other terms?

In the new discussion section we expand on the other components of the model and also discuss how these features of the model can further our understanding of long-term biogeochemical cycles.

Similarly the discussion of K and P in the chronosequence p 25 l 12 to p 26 l 4 would be best located in a separate discussion.

Done

I’m stressing the need for a separate discussion, because my impression is that currently the strengths and weaknesses of the model are buried in the results section. Having them altogether is a separate section would make it easier to appreciate the quality of the model (and its limits).

We have included a limitation sub section in the new discussion section.

3) The text following page 28 l 15 in the Conclusion section aren’t conclusions, but a discussion of the results - they should go into the discussion section.

Done and removed lines 15-23 from the manuscript completely.

Please limit the conclusion section to things you have learned from your study, or needs for further research that you’ve identified with your simulations.

We have shortened the conclusion, see above.

Abstract: mentioning “vegetation interactions” implies that the vegetation plays an active role in your model (otherwise it would not be interacting). After reading your model description I think that this is not correct, because vegetation production and water-use are prescribed, and the only thing that varies is litter stoichiometry, which however is not actively controlled by the plants, but simply follows soil nutrient availability. I would therefore rephrase this to be “effect of vegetation in terms of ….”. Only then the meaning last two sentences of the abstract become clear.

We agree that the term vegetation interactions may be misleading so we have amended this as you suggest.

Minor comments.

Section 2.6. The model seems to be partitioning the Np into different pools, but how this is done remains unclear. Please specify.

We state on page 14 line 6 and within equation 20 that Np is allocated to the different pools by the allocation coefficients in Table 1. However, we have now also added “using allocation coefficients from the literature” to page 13 line 15.

You added in response to the reviewer comments at the end of this section that the litter stoichiometry is flexible. However, it would make more sense to state this directly in page 16 line 11, because the less diligent reader will likely miss this.

Done

Please also clarify what happens to the other nutrients if nutrient i is deficient - does this then imply that all other nutrients would be taken up less as well, or do you then break the stoichiometry?

We have updated page 16 line 11

Section 4.4 should not be labelled “Vegetation interactions”, because the model does not simulate any interactive vegetation, since Np is prescribed, and so is root respiration. In terms of nutrients, because the vegetation does not actually influence litter production or soil moisture or soil C interactively, the vegetation simply acts as a buffer mechanism. I would suggests “Effects of vegetation” or something along the lines.

We agree and have changed the section to Effect of vegetation

I agree with reviewer #1 that the inclusion of a comparison to Manaus soil data is odd (and not helpful - see reviewer #2’s comment). I appreciate that there is a need to evaluate the soil C profile and that the Uni of Leeds has good data from Manaus, but I cannot see the relevance of a unique soil profile in the middle of the Amazon for modelling the chronosequence of soil C in Hawaii - this is simply too random a site selection to be useful. I would recommend to remove Figure 7, and shorten the text referring to it to state that the soil profile agrees in general of what you would expect from old tropical soils (refer to the Manaus data here), but in the absence of good data from Hawaii not much more can be said. I would also caution a too firm statement regarding the need for a depth dependent k, because your model might have other biases (missing/wrong bioturbation, depth-distribution of litter input), which could contribute to the misfit.

We have removed this section from the manuscript.

I don't see the need for Figure 9 and the associated sensitivity study, specifically as no data are presented to evaluate the profiles. Aren't these results trivial (and thus don't really need a figure to illustrate? What do we learn from this that we need to interpret the results in Section 5? Given that the model does not assume any vegetation feedback and the Np is prescribed, the soil CO2 efflux in equilibrium must by definition independent of the CO2 profile.

We included this work to demonstrate how sensitive the soil CO$_2$ concentrations are to the vegetation parameters (which could represent different biomes), since we know that the modeled profile responds significantly to changes in pH. For example for the same carbon input and respiration, deeper roots significantly alter the concentration profile of CO$_2$, with much higher concentrations at depth due to the lower pore space at depth and subsequent reduction in diffusivity within the profile. However, perhaps this does over complicate the manuscript so we have removed Figure 9

In Section 6: "importance of vegetation". As far as I understand it, this is your main conclusion and novel finding. It merits somewhat better explanation (what is it that matters and why). To my understanding, the major effect resides in the dependence of soil pH on root respiration? Somewhere then there needs to be discussion as to whether it's realistic to assume that changes in pH only occur due to biological activity, or whether the low pH of precipitation or acidity resulting from weathering processes should not also play a role.

As well as root respiration the pH also responds to the decomposition of carbon entering the soil from the litter and from root turnover (difference between old Figure 9 a and b). In the new discussion section we have stated that we have not explored the contribution of rainwater pH or acidity from weathering so it is not possible to say how much greater a role the vegetation plays. We instead refer to other studies which also highlight the acceleration of weathering due to vegetation.

Figure 3-4: The text does not make any reference to the 10k results, and they are also not strikingly different from the 20k. I would recommend to merge Figure 3 and 4 so as to represent only the 20k results. This also avoid plotting the pH results from 20k twice.

We have combined Figure 3 and 4 as you suggest into what is now Figure 2.

Figure 10: I'm surprised that your soil CO_2 efflux is only 2.5 kg. Assuming steady-state and a constant Np of 1 kg C I yield 1/12*44 = 3.6 kg CO_2 efflux. What am I missing?

This is likely due to how fast the rate of diffusion manifests the flux of CO_2 out of the profile. The soil pore spaces and hence diffusion coefficient are not at steady-state.

Answers to reviewer comments:

Your answers to reviewer #1 1-5 comments should be reflected in the text as a discussion of the current model caveats. You can rightly say that you will be addressing this in a future version, but I think that it is still important to acknowledge that the current version is very limited in the way "vegetation" is represented.

We have included a paragraph explaining this in the limitations section of the new discussion section.

Your answers to reviewer #2's comment on Hawaii as an erosional land scape should likewise be included in the revised manuscript.

In the new discussion of erosion we have stated that we will need to model erosion in a more mechanistic way. We state that tectonic uplift, also an important process can be formulated similarly to the current erosion.

<u>Additional changes</u>

Page 20, line 22 – page 21 line 1 moved to discussion section.

Page 25 lines 13-23,
page 25 line 28 to page 26 line 4
page 27 line 14-22
all moved to new discussion