Manuscript “A probabilistic approach to quantifying soil property change through time integration of energy and mass input”, by Christopher Shepard, Marcel G. Schaap, Jon D. Pelletier, and Craig Rasmussen

Decision on the manuscript after 3 reviews and 3 responses.

I thank the authors for their extensive responses to the reviewer’s comments. A number of changes to the manuscript were already announced, and I am looking forward to read the revised text and evaluate these changes.

Additional, I am of the opinion that a number of comments were not yet fully or adequately addressed and ask the authors to respond to these in the manuscript as well.

Re: Anonymous reviewer #2

Discussion on manuscript title. The authors are not consequent in their objectives: the model calculates the soil state (at present), but this is presented as soil change (a.o. in the title). For “change” you need (minimally) 2 values in time, these are the initial value and the final/current value. As the authors state there is currently no role of the parent material in their functions, there is no initial value and thus there can be no “change” calculated. Following the discussion inside the manuscript on reasons for poor model fits, parent materials may enter in future versions of the model and then change may come in. Advice: do not suggest “change” when it is not calculated, certainly not in the title.

Re: reviewer J. Phillips

On the bivariate normal density function. The authors reply that other bivariate distributions are possible, but this does not answer the concern (also by myself) that multivariate pdf could be considered as well. Advice: Add this to the announced change near lines 134-136 and comment on possible problems that this might introduce.

On lines 144-154. The authors state to disagree with the reviewer “as the present approach is effective at prediction soil property across wide variety of environments and ecosystems”. Such effectivity was not present at all sites (e.g. the hot climates in Barbados or Taiwan) and anyway, statistical effectivity of predicting a depth-weighted parameter does not validate assumptions on the presence, absence or mutual compensation of progressive and regressive processes, related to equifinality or pathway dependency. Advice: mention in the text that the correct prediction of depth-weighted soil properties does not inform on progressive and regressive processes that affect the depth distribution of these properties.

Re: review by editor

On the delineation of the model domain: In statistical studies such as these it is a good practice to indicate the domain for which the model was parametrized, for instance to prevent extrapolation. That was behind my question. At the moment the model is fit to a wider data set, it is a new model with a new domain. The current domain seems to be restricted to soils little affected by humans, with little aeolian deposition and for the temporal extents of the chronosequences. Advice: It should be no problem summarizing this.

Response to question 2: The quoted text states that "taking into account the differences in past and present climate would likely diminish disparities" (etc). This statement assumes a perfect model which does not exist and I advise to weaken it because it is not supported by evidence.

Response to question 3: The authors state that age definition is not a problem and suggest it is more easily done than measuring the clay content. This refers to the chronosequences used in the study, where age was determined. But what if the model is to be applied elsewhere? A model that is only valid at the locations of parametrization is not very useful, and I think that the authors do aim at a wider geographical application domain. Thus it is a legitimate question to ask if age determination at such unvisited locations is not more costly than to measure the target parameter directly. Advice: comment on this issue.

Response to question 6: Fertilizer input transferred to EEMT may not change EEMT much, but quite some soil properties may respond strongly (which is a purpose of fertilization). Perhaps not the physical soil properties that were looked at; here we may have another delineation of the model domain. Advice: state in the text that human agricultural impacts were not addressed in this study, and that adaptation of the model to include these impacts may ask for adaptation of the model structure as well as a re-parametrization.

Response to section 3.1, bias: You did likely not introduce bias by the sampling amongst the chronosequences, it is the available data that are biased. Advice: mention in the text that this is an unavoidable limitation.

Response to remarks on predicting potential landscape evolution: The authors are quite defensive in their response. Certainly the present model cannot predict potential landscape evolution including human impact, because human impact is not included as a factor. When it will be included, that's another paper. Also, authors should realize that they do not predict OC nor its feedback with erodibility. It is a model for some soil physical parameters and that does not make it a landscape evolution model. Potential landscape evolution addresses the future. Please comment on how EEMT can be estimated for future scenarios. Advice: be realistic in what the current model can do and what it cannot (yet) do.