SOIL Discuss.,
doi:10.5194/soil-2016-63-EC1, 2016

Interactive comment on "A probabilistic approach to quantifying soil property change through time integration of energy and mass input" *by* Christopher Shepard et al.

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Received and published: 2 November 2016

Manuscript SOIL2016-63 "A probabilistic approach to quantifying soil property change through time integration of energy and mass input" by Shepard et al.

The authors present a probabilistic approach to estimate soil properties from energy and mass inputs accumulated over time into a parent material. As such this predictive model builds on the energy models from Runge and later instances by Rasmussen et al. It is parametrized and applied on a number of chronosequences and then onto CZN sites. As such model is not mechanistic in itself although it uses inputs that suggest a mechanistic nature (energy input), I have doubts on the use potential of such model in the CZN environment where process knowledge is being generated and applied.

At best, the model predicts some soil properties at unvisited locations where we have some idea on mass/energy fluxes and the age of the soil. In this respect it may have some value in the so-called homosoil approach in digital soil mapping, when practically no soil data are available. The model does however not generate knowledge on soil (forming) processes although the used wording in the manuscript suggests so; and there are (too) many assumptions behind it.

My conclusion is that this article needs a major revision in the sense that the below remarks need to be addressed and also, that the model application domain should be delineated more precisely (delineated in spatial and temporal extent and parent materials). I advise against keeping in the text that soil forming processes are addressed, the word "implicitly" actually means that only the net effect is dealt with, not any individual process unless perhaps weathering via EEMT and t.

Below I first formulate a number of general questions regarding the manuscript and then address some ambiguities or issues at specific locations in the manuscript. 1. Why is the model forced into a bivariate pdf form? Techniques were on the shelf?

2. Inputs "time" and "EEMT" are assumed to be known. EEMT can be assessed using today's data which brings in the (big) assumption that EEMT is a constant (section 1.1). All climate modelers will tell you that, e.g. due to variations in insolation, EEMT is not a constant. Glaciations were caused by this... Time or soil age is used for the integration. How often does one know the soil age, especially in non-glaciated areas? If it has to be measured on-site via dating techniques, this investment is far higher than direct measurement of the target variables, so why do it?

3. I am uncomfortable with the statements on removal of some soil layers from the chronosequence data. What does this do to t , thus the integration interval, if you remove the upper layers from the data set? Additionally, is the model valid for sites receiving dust influx or is it only valid for sites with prolonged soil development in a single parent material? In some cases buried horizons were removed from the data

set. In case of a recent deposit on top of an older soil, is then the whole profile removed from the data set (section 2.3)? I guess not but how is the decision made?

4. Any non-linear or transient soil development, even when it is due to variations of EEMT (there may be other causes as well that the model does not consider, e.g. bioturbation, frost action) is averaged out during the estimation of parameters of the bivariate pdf by assuming it is constant. Transient soil changes occurring in the past may dominate todays' soil properties (e.g. ripening, erosion). I do not like statements like in line 149 that all processes are implicitly captured. There is no single process captured, only the net effect, made constant over time.

5. Is the soil forming factor "human activity" e.g. via tillage, fertilization, erosion inside or outside the scope of this model. If it is "implicit" as well, then the human influenced (time span of 100s of years) is smeared out over pedogenetic timescales, which is totally unrealistic. Making this point more general (see also point 3): what is assumed domain of validity? Natural soils receiving no dust influx and under fairly constant climate? The authors should comment on this in their discussion section.

6. Section 4.3 addresses quite a lot of the concerns that I make above and below, so the authors do realize these. The feeling remains, however, that the energy model approach because it is implicitly dealing with the mixture of soil forming processes occurring during the formation of the chronosequences, may not be able to deal with other mixtures in the future such as caused by human impacts and transient climate change.

Specific remarks: Line 94-97: If the model would be mechanistic, I would debate if the soil forming factors Climate+Organisms (Ps) are considered exchangeable with Relief+Parent material (Lo) (can they compensate each other). As it is empirical, I accept that the model is a (dark) gray box model with little representation of the processes but would not accept any mechanistic conclusions to be drawn with it.

Line 182-183: Depth weighted texture is in mass%. No account of mass distribution

over depth? In line 238 when converting to mass, a uniform bulk density of 1.5 (quite high) is assumed. As bulk density will vary over depths in many soils and is often related to (diagnostic) horizon occurrence, here we have yet another simplification.

Line 238-240: bulk density always 1500 kg m-3? Yet another simplification. If RF% is a needed input, to be measured on site, what is then the use of the estimation of the clay percentage on the same site (just analyze it, the pit has been dug). RF% is not cheaper to measure than clay%.

Section 3.1. L0 translates here as 3 parent materials. Can we say in general that due to the categorical nature of parent materials, equation 4 is dealing with the soil data in a stratified manner? To estimate the parameters of equation 6 in an unbiased manner, the strata (parent materials/rock types) should be sampled by probability sampling, which is obviously not the case as the input data are from chronosequence studies. The authors should add statements on the possible effect of biased sampling on estimated parameters.

Line 346: The statement on causes for clay content change are in contradiction with line 149 where a larger amount of processes are stated to be(implicitly) captured. As stated above, I think that the authors should not pretend with their approach to capture soil forming processes.

Section 4.1.1 mentions cases where the model does not predict well, which suggests again that the authors should define the model application range and confine the data domain for which it was fitted.

Line 439: It is not true that the strength of the approach is in the lack of assumptions about the initial conditions: parent material/rock type is used for stratification (eq.4).

Line 457: I do not believe that the parametrized approach makes any sense in predicting potential landscape evolution. First, because the accelerating effect of human activities on soil formation and soil redistribution is not properly captured in the model, second because the time integration is done using averaged EEMT. The authors should also state at what temporal extent they expect to be able to make landscape evolution forecasts. I think there could be additional value of this model to produce a covariate map in digital soil mapping that expresses the likely degree of weathering (via EEMT).

Interactive comment on SOIL Discuss., doi:10.5194/soil-2016-63, 2016.