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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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Budiman Minasny has, correctly, called the ability to model soil formation and evolution the "holy grail" of pedology. Given the complex nonlinear dynamics and divergent development often observed in pedogenesis, along with issues of polygenesis and multiple causality (not to mention spatial variability and measurement uncertainty), a probabilistic approach to this problem makes sense. The title ("change through time") raised my hope that this paper would be a step in that direction. It is, but only a small step. The contributions of this paper are:

1. An improved correlation of some soil properties with climate and apparent age, based on total pedogenic energy (TPE).

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Discussion paper

2. A suggestion/demonstration that, as might be expected, local variations not captured by the soil-TPE correlation are closely related to landscape position and topography.

Though I suppose one could model change through time by varying the time factor for a given EEMT, this is not done in this paper and would be quite suspect due to the many assumptions and simplifications involved. The approach given here is, to be sure, probabilistic, but in a very traditional statistical uncertainty sense rather than in the more specific change-over-time insight I hoped for when I read the introduction.

I like the straightforward way this approach is linked to the classic factorial approach in lines 85-97. While the state factor model has its strengths and weaknesses, it is an appropriate framework for a model seeking applicability across a variety of environments and locations. The long chain of assumptions and simplifications are reasonable in the context of developing a relatively simple model, but move the final model more toward a black box and less a true factorial model.

Despite the simplifications and assumptions, and that the paper did not live up to my initial (perhaps too unreasonably high?) hopes, this is a worthwhile contribution to pedology, and a step toward true probabilistic modeling of pedogenesis.

Comments (some important, some trivial) on specific line numbers are below:

69-71: Could eliminate the first two “approaches.”

128-131; 152-154: Why the bivariate normal density function? Were other pdf’s considered?

144-154: These are some pretty serious and unrealistic assumptions.

159: “More than” rather than “over.”

161-162: One of the “within the present study” phrases can be omitted.

164-166: Southern hemisphere and mid-continental sites are under-represented.

168-177: EEMT has been successfully used several times in the past decade, and is familiar to some pedologists. However, since the whole rationale of the method is based so heavily on S = f(TPE) = f(EEMT), and EEMT is not familiar to all pedologists, some additional explanation and background is called for.

182: I recommend framing this as examples of change through time rather than proxies, as many chemical and biological changes are not necessarily closely related to textural change.

213: Omit first “terrain.”

232-236; 389-390: The influence of hillslope processes and morphology on soil development and thickness was recognized by the late 19th century. Soil depth does usually vary systematically across hillslopes, but these broad scale variations are often overlaid with complex local variations (see, e.g.. lines 335-6). Thus it seems a major stretch to claim that soil depth “corrects” for hillslope processes.

248-263: This section could use some clarification. The extent to which output from another model is used to both drive and evaluate the output of the probabilistic model is an issue. Perhaps a flow chart would help.

271 et seq.: Please clarify these correlations. Spearman correlations are not included in Table 2; text does not always make clear whether Pearson or Spearman is given.

322: Regression slope, presumably?

359-372: Also, sand and silt are dominantly resistant quartz, and present as primary minerals (or depositional inputs) rather than as a result of weathering or translocation. While weathering would be closely related to EEMT or TPE, residual primary minerals (or depositional inputs) would not be.

377-379: A number of references going back to at least the mid-1980s discuss both the dynamics of soil production and soil thickness, and associated nonlinearities (e.g., Johnson, Phillips, Minasny, D’odorico).

413: This implies some important scale influences worthy of further discussion.

421-424: Agreed. But in areas of complex lithology (most sedimentary rocks) this could be quite complicated.

426-462: I had hoped for something more explicitly probabilistic, such as predicting, e.g., ranges of clay content or the converging or diverging upper and lower limits over time. The approach given here is, to be sure, probabilistic, but in a very traditional scatter-around-trend sense rather than in the more specific change-over-time insight I hoped for when I read the introduction.

464-514: Good discussion of appropriate caveats.

467-470: Or even without controlling for other factors, especially in younger soils.

Table 2: Column headings are not labeled or identified in the caption. For Pearson correlation, why not use the traditional and familiar r, rather than p (or maybe it's supposed to be rho; hard to tell in this font) which normally denotes probability or confidence intervals?

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