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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.

**Anonymous Referee #2**

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The manuscript entitled "A probabilistic approach to quantifying soil property change through time integration of energy and mass input" by Christopher Shepard et al. presents a probabilistic use of the energy and mass transfer model first developed by Runge in the seventies and modified more recently by Rasmussen and co-authors. This use implies an adaptation of the energy and mass transfer model (EEMT) by including the soil formation duration within the model. The proposed adaptation of the EEMT model is then applied to three data set of different natures: a large set of chronosequences gathered from the literature; a compilation from upland catchment from the US NSF Critical Zone Observatory, and a small complex catchment in the Santa Catalina Mountains. The application of the model to this last dataset requires

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further modification of the initial model. As such, this work present an interesting contribution that is worth publishing. However, the objectives of the proposed model are unclear to me. Indeed modelling of soil may have different scopes: the understanding of soil formation, the prediction of soil evolution in future or the prediction of the soil distribution in space. Contrarily to what is stated in the title, introduction and/or discussion, the proposed model cannot be used to either understand the soil formation or to predict its evolution in future as it is not a mechanistic model. Indeed the understanding of the soil formation now a days requires the understanding of the interactions and retroactions among soil processes. Only an understanding may bring some gain of knowledge on the threshold and chaotic aspects of soil formation. Concerning the prediction of the evolution of soils within future, the proposed model has to integrate both the climate evolution and the human activity. As far as I understood, climate is considered as constant (lines 144-145) in the proposed model and human activity is not considered at all. The proposed model can therefore mainly be used to represent spatially the distribution of some soil properties providing that the age of the soil is known. This last data is not easily to obtained and limit thus strongly the applicability of the model. Considering this, the title, the introduction and the discussion of the paper should be modified. Since the paper is based on the EEMT model, this model should be at least shortly presented to facilitate the understanding of the paper for all readers (including those who have not read the papers by Runge and Rasmussen). In addition, the data set should be better described providing the age span of the studied soils, their depth range, stoniness, the vegetation. . .. Indeed the relative variability over the data sets of one parameter compared to the other may explain their relative importance in the response of the model to the different parameters e.g. paragraph from line 319 to line 326. It would be interesting to present a repartition of the number of soil studies per parent material, vegetation, climate types in order to get a better idea of the representatively of the studied soils compared to the worldwide variety and thus of the applicability of the model. In paragraph from line 359 to line 372, the authors show that the clay content is well predicted by the model while the silt and

sand fractions are not, the worst result being for the sand fraction. This result seems expectable to me as the sand fraction contains a large variety of primary minerals that strongly vary according to the geology of the parent material and have strongly variable ability to weather. Therefore, the bad prediction of the sand fraction may be related to the impact of the initial parent material condition on the model. Such an impact is also observed in the bad prediction of the clay content for the soils developed on amphibolite (Fig. 7a). Therefore it is not true to conclude that the model is not sensitive to the initial conditions. This statement should be minored in the revised version of the manuscript.

Please also note the supplement to this comment:
http://www.soil-discuss.net/soil-2016-63/soil-2016-63-RC2-supplement.pdf

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