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# Reply on CC2

David G. Rossiter et al.

Author comment on "How well does Digital Soil Mapping represent soil geography? An investigation from the USA" by David G. Rossiter et al., SOIL Discuss., https://doi.org/10.5194/soil-2021-80-AC4, 2021

We appreciate the overall positive comments of the colleague and the appreciation for our efforts "attempting to shed some light on the touch points of inadequacy that exist in these products".

- We agree that "distillation of examples would be useful with more contex- tual background to explain what the reader should be specifically looking for in the visual comparison outputs. Many articulate and powerful ex- amples of where these products are lacking could be described for any given soilscape within the CONUS area". We decided in the main paper to concentrate on methods, and placed the four extensive case studies in the companion ISRIC report. These are not sufficient to "distill an overall evaluation over CONUS" as requested. Therefore we provide the code. We hope the work will be taken forward by others, especially within NRCS, to write a paper with this theme.

- The paper states "This (the above attractions of PSM/DSM) removes the need for expertise in discovering and interpreting the soil-landscape rela- tions, also known as the 'paradigm' of soil survey (Hudson, 1992), which is vital for traditional soil survey and difficult to acquire and harmonize among surveyors."; the colleague takes exception to this:

  As one who regularly "actually examines the soil and landscape" of my area, I take great issue with the above statement. It needs to be clarified that it is really only

possible to evaluate the PSM/DSM results if one has the expertise derived from the traditional "paradigm" of soil survey.

We can see how our statement can be interpreted this way, and how incorrect it is from that point of view. We did not intend to imply that DSM replaces the paradigm. In the revision we intend to make this clear.

- The colleague states "A dirty little secret concerning PSM/DSM products is that in spite of their assumed superiority and 'explicitly multifactorial' approach as previously described by esteemed commentators, these products are not 'intelligent' in how they parse soil topographic/soil geomorphic/soil geographic relationships."

  We are not sure how much of a secret this is, at least to us, and we certainly are not the "esteemed commentors"! It may be that DSM has been so presented and thus over-sold. Again, in the revision we will highlight this inherent limitation of DSM.

- The colleague states: "No matter how you slice it understanding local geomorphic relationships and their cut in the process of mapping soils. These complex soil geomorphic relationships live in conventional soil survey products and are largely absent in PSM/DSM products making them substandard in how they capture the reality of soil distributions. You pick the area (any area) and a soil scientist with the ability to read and understand soil geomorphic relationships (the quote traditional paradigm of soil survey) will show you numerous shortcomings."

  The subsequent two paragraphs expand on this point.

  We completely agree and had thought to have made this clear. In fact the motivation for this paper was exactly to highlight this point. Since DSM is so popular it should be critically examined, as we have tried to do. In the revision we willmake sure this point is well brought out.

- The colleague states: "The paper should postulate on why the PSM/DSM products don't measure up to conventional survey. The intent is not to replicate earlier products, but one would hope that PSM/DSM products would reflect a similar lineage in larger structures and spatial patterns expressed within the soilscape. We know these patterns are there so why do we deny them in these new products."

Indeed. We had hoped that the spatial patterns we know from expert soil-landscape analysis would be reflected in the DSM products. As the paper and case studies show, there are serious deficiencies in the examined products, and we expect in all products made by similar methods.

We had not postulated on the reasons. We appreciate the stimulus given by the commentator to do so, and we intend to add this in the Conclusions.

Our first thoughts on this are:

- The dominant DSM methods do not explicitly consider spatial conti- nuity or pattern. Experiments have been started with convolutional neural networks and other methods with varying window sizes of covariates.

- Environmental covariates to represent past soil-forming conditions (the "time" factor) are only available since the satellite remote sens- ing age, very short in terms of soil formation.

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  - Point observations are mostly placed at "typical" or "representative" locations and do not capture the full range of variability along topose- quences.

  - Poor georeference of legacy point observations leads to poor correla- tion with environmental covariates, hence to poor models, hence to much noise in the DSM product, which can obscure patterns.

6. Finally, the colleague states "I think the more pressing and important question is how do we build the intelligence and paradigm from traditional mapping into PSM/DSM approaches so that the strengths of the hierarchical relationships of geomorphology, superposition, fluvial downcutting/cross- cutting, geologic discontinuities of materials are added back into these models to further inform the outputs."

We completely agree, and, along with others, are active in attempting to develop such methods.

It should be noted that the "SolIM" approach of Zhu and colleagues already in 1997 took an expert-based approach to DSM. This is applicable in small areas with detailed knowledge of the soil-landscape relations, but not to wide-area models. This approach still needs covariates for the model, and if these do not cover the soil-forming environment, it will also have difficulty.

Reference: Zhu, A.-X., Band, L. E., Vertessy, R., & Dutton, B. (1997). Derivation of soil properties using a soil land inference model (SoLIM). Soil Science Society of America Journal, 61(2), 523–533.

The work in this paper has been improved and applied in many further works mostly by Prof. Zhu's groups in Madison, Beijing and Nanjing.

Please also note the supplement to this comment:
https://soil.copernicus.org/preprints/soil-2021-80/soil-2021-80-AC4-supplement.pdf