

## ***Interactive comment on “Soil classification based on spectral and environmental variables” by Andre Carnieletto Dotto et al.***

**Anonymous Referee #1**

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This manuscript presents an idea for a new soil classification system based on spectral, terrain and climate data as a new source of soil information and compares it with traditional soil classification system World Reference Base for Soil Resources (WRB). As this method does not rely on tacit and empirical knowledge of soil scientists but entirely on quantitative measurements, the authors claim it is more robust against subjective evaluation and should be fully transferable across different regions and thus will give coherent information. The authors argue that this approach is different from traditional nomenclature as it takes into account climate and terrain information, whereas the traditional one does not. I do not agree as the traditional nomenclature such as WRB always take into account climate and terrain information when naming soils, e.g. Ferasols occur almost exclusively in humid tropics, because certain climate is a basic

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condition for certain soil forming process and soil layers formation. For a good while I was also wondering if the proposed methodology, which is k-means clustering, is an adequate and sufficient method for new soil classes retrieving. The k-means clustering is already firmly adapted in soil science and broadly used for stratification the study area for purposes such as sampling design optimization, or delineating the most diverse classes based on terrain data, etc. In my opinion the methodology presented lacks for deeper quantification of the relations between soil classes and environmental variables. Also, time as one of the key soils forming factors is neglected. Nevertheless, the product presented can be found as a useful extra soil information (treated as environmental co-variate) that can be used within digital soil mapping framework to increase the accuracy of digital soil maps. However, from this point of view, the question remains, whether the classification presented here will do a better job compared to traditional soil taxa (e.g. WRB) or not. One more limitation of the method is that the new classification based partially on soil spectra could not provide quantitative information about soil properties without a proper calibration or linking to known soil taxa. In other words, when applied to unknown area the resulted soil classes will not contain information about soil characteristics. It is a bit unclear how the optimal number of classes was determined. The authors said according to AIC, but there is no equation. Please describe in detail how AIC was computed and provide an equation. Explain why do think the traditional classification system WRB does not take into account climate and terrain information. In summary, the manuscript is written with consistency and clarity, and the overall quality of the presentation and writing is very good. Although I personally do not think that this is the right way that soil science should take further, i.e. to suppress traditional soil scientist knowledge based on experience and replace it with automated procedures, there is certain novelty contained in this study that defends publication in the SOIL journal. Therefore I recommend acceptance after minor revisions.

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