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Interactive comment on "Development of soil biological quality index for soils of semi-arid tropics" *by* Selvaraj Aravindh et al.

Anonymous Referee #1

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The topic presented in this manuscript is of high interest. A valid and general soil assessment is urgently required and there are many attempts to develop soil quality indices (SQI). This publication adds to the necessary discussion, although the suggested SQI (or biological SBQI) appears not to be the final solution. The authors investigated three different long-term fertilization experiments, each with four different fertilization systems. In total 12 soils were used to develop the scoring scheme that was subsequently tested with 25 farm soils. The parameters selected were 1) soil organic carbon, 2) microbial biomass carbon, 3) labile carbon, 4) protein index, 5) dehydrogenase activity and 6) substrate-induced respiration. Data were converted into SBQI, testing five different methods. In his fundamental publication, Nortcliff (2002) stated that any SQI must consider soil functions. Since soil functioning largely depends on

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Discussion paper

soil biota, it is agreed that the authors focus on the soil biological status. However, the parameters selected are all measures of quantities; the dehydrogenase activity and substrate-induced respiration are microbial activities, yet they are so widespread and unspecific that they mostly reflect microbial biomass. That can be seen also in this study from the very close correlation of microbial biomass carbon with dehydrogenase activity and substrate-induced respiration (Tab. 2). All these parameters again very much depend on the content of soil organic carbon. Correspondingly, the loading of all these parameters on the first principal component in principal component analysis (PCA) is very similar (Tab. S4). Anyhow, the authors are able to clearly differentiate the 12 samples from long-term experiments by using PCA. Yet, it must be assumed that this differentiation is mostly due to the systematic differences in the soil samples, which result from the different treatment in the long-term experiments. The authors should test, in how far soil samples are separated solely by soil organic carbon and protein index (and vice versa, whether the other parameters are dispensable). The test of five different methods for deriving a SBQI from the data set nicely shows that it doesn't depend so much on the calculation method. This is shown by the results in Tab. 5 (and partly Tab. 6). Method 1 and 2 as well as 3 and 4 deliver content-related the same data (the regression coefficient is 1 and not 0.97 as written in Tab. 6). Even more relevant and worth to discuss is the interpretation of the derived SBQI. How do we know which value is natural, desirable, sustainable? The authors come up with a "the more the better" target, based on average values calculated from their data. Yet, is for example more respiration better than lower respiration? Here it seems to be a more promising approach to go for ratios such as the metabolic quotient, the microbial quotient or the carbon use efficiency of soil microbiota. The text contains a number of small language errors that have to be fixed by careful proof reading and editing. As was already indicated, the data in Table 6 must be checked and corrected. Normal distribution of data is claimed by the authors. However, the Fig. 1 shows clear deviations from normal distribution. Distributions are left-skewed for soil organic carbon and labile carbon, while dehydrogenase activities show a bimodal distribution. Nortcliff, S. (2002)

Standardisation of soil quality attributes. Agriculture, Ecosystems and Environment 88(2), 161-168.

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