

## ***Interactive comment on “Patterns of water infiltration and soil degradation over a 120-yr chronosequence from forest to agriculture in western Kenya” by G. Nyberg et al.***

### **Anonymous Referee #1**

Received and published: 6 September 2011

The authors analysed and described changes of the soil parameters infiltrability, bulk density, aggregate size, C and N content, and  $\delta^{13}\text{C}$  (and  $^{15}\text{N}$ ?) due to land-use change from forest to agriculture. The study includes agricultural sites with increasing time since deforestation. The authors found that all soil parameters changed significantly from forest to agriculture land, but changes with years since conversion were apparent only for C, N and aggregate size. The paper is very well written, but there are some major and minor concerns I have.

In the last paragraph of the abstract as well as within the discussion the reader is sud-

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denly confronted with the suggestion that landscape planners should include wooded elements in the landscape to increase infiltrability in agricultural sites. The effect of inclusion of wooded elements in agricultural land was, however, not analysed within the reported study. Therefore I suggest to delete these sections and possibly to formulate the need of such studies, or if existent to cite the respective studies within the discussion.

On the analytical side, the sample sizes should be pointed out more clearly (include in Figures etc.). I understood that the sample size for 0, 39, and 119 years after conversion is 12 and 8 for infiltrability and the other parameters, respectively. For 57 and 69 years there are only 6 and 4 samples for infiltrability and the other parameters. As the latter sites have very small sample sizes, I suggest pulling them together for statistical testing in order to increase the test power. In addition, the sample size for the very subjective crop yield is not included within the method section. Why is 15N in 3.5 reported but not within the other sections? It is not mentioned in the abstract, too.

In addition, I do not see the additional value of performing a PCA. The authors should rather point out better the additional value or delete section 3.6 and Fig. 4. Finally the discussion section should be improved in term of including results from different sites (e.g. Amazonia, Asia, not only citing reviews) and if possible other long-term chronosequence studies should be used to discuss the author's results.

Minor remarks:

P. 6994, L13-15: Please sort soil parameters by separating physical and chemical ones.

P. 6994, L16: Delete rapidly or write "within 40 years".

P. 6995, L21-22: Please provide more references as the word "usually" suggests that there are a lot of citable studies.

P. 6995, L23-25: Please provide references for both sentences.

P. 6997, L8-8: These are means of how many and which years? Please tell the reader which meteorological station you are referring to and provide a reference if possible.

P.6999, L11: Please add “t is time for infiltration (h)

P.6999, L13: Please repeat the soil and site parameters.

P.7000, L20: Change “time” to “119 years”

P.7000+7001: If you provide the range of CV than please say whether the higher values were found for forest or agriculture.

P.7001, L5: Add per mill symbol after 18

P.7002, L14: Further studies should be mentioned, or mention that the Ilstedt paper is a review.

P.7002, L18: The variability is land-use dependent. This should be mentioned here if possible.

P.7003, L11-12: Beside the infiltration decrease the evaporation increases from agriculture to forest and, hence, the groundwater recharge is usually higher under agricultural land use. How this does relate to your sentence?

P.7003, L13-17: This was not studied within this manuscript and should be deleted.

P.7003, L19-P7004, L2: I do not understand this sentence.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 6993, 2011.

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