

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 #2

Received and published: 20 September 2011

The authors studied assorted soil characteristics – infiltrability, bulk density, aggregate size distribution, Corg, Norg, and delta-13 C and delta-15N – on agricultural plots formerly covered by forest; the plots differ from each other with respect to the time elapsed since deforestation, but are assumed (without factual support) to be identical with respect to basic soil properties such as texture, clay mineralogy, and the like. While this study is a welcome addition to the projects that monitor the development of soil properties over time in response to some change in land cover or land use, several issues need to be addressed before an eventual publication.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Title

'Truth in advertising' is an ideal that applies to all consumer goods including publications, although one would hope that in non-profit science this ideal be held in higher esteem than elsewhere. There is, alas, little truth in this title: patterns were not studied at all, nor was there a chronosequence in its original sense: a 'space-for-time' substitution approach is not a chronosequence proper, but a pseudo-chronosequence: the former is the real thing, the latter an article of faith, faith, that is, in the uniformity of soil properties unrelated to the land-use change under investigation across the extent of the investigation. A title that reflects the contents of this manuscript without buzz words or judgmental terms ('soil degradation') would look like this: 'Changes of soil properties over 120 years since forest-to-pasture conversion as deduced from a pseudo-chronosequence'.

P6998, L2: Eight plots of 40m², spread over an area of 8 km², and all 8 of them are supposed to share the same intrinsic soil properties just because the soils of that area were classified as Humic Nitosols (P6997, L13)? The taxonomic level is so high as to be meaningless in the context of this study. The authors will have to do better than this to convince the reader that the pseudo-chronosequence used in this study is as good as the real thing. Further to that the question keeps cropping up why no soil texture data are supplied for those eight plots; after all, the bulk density and soil aggregate data suggest that soils from those eight plots were analyzed in the lab – why not for texture?

P7002/L9-16: What is a 'rapid decline in infiltrability' after forest conversion? If going from 342 mmh⁻¹ to 140 mmh⁻¹ in 39 (!) years is 'rapid', what are we to make of a decline by one order of magnitude within 1 (!) year (Zimmermann et al., 2010) – would that decline qualify for 'mind-boggling'? If anything, going from 342 mmh⁻¹ to 140 mmh⁻¹ in 39 years looks like a gentle adjustment hardly worth mentioning, so beware the hype! As to the discussion of the variability of soil infiltrability, a sample size of n=6 per plot hardly lends itself to address this issue: it is worthwhile remembering Tukey's

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



rule of thumb which says that in order to estimate the mean of a quantity, you need $n=5$, to estimate its variance, you need $n=25$, and so forth for higher-order moments.

P7003/L1-8: ‘Tropical rains are often very intensive; rainfall intensities of $>60\text{mmh}^{-1}$ or even $>100\text{mmh}^{-1}$ are not uncommon in the area for periods up to 30 minutes’ - statements like this need to be more precise to be credible given that the authors base their conclusions regarding the hydrological consequences of forest conversion on them. What is ‘not uncommon’? How frequent is ‘not uncommon’? Why not give the return interval? Far from being uncommon, a rainfall intensity of $>100\text{mmh}^{-1}$ sustained over 30 minutes appears an exception, if not incredible. And a statement such as ‘Such events may contribute an important part of the total annual rainfall’ lead me to believe that the authors have not actually looked at the rainfall record, because if they had, why such vagueness (‘may contribute’)? Either they did contribute ‘an important part’ (what constitutes ‘an important part’? 50%? 60%? 70%?), or they didn’t.

P7003/L4: ‘50% of the cultivated area had infiltrability values below 60mmh^{-1} ’. This statement is not supported by the facts presented: the support associated with an infiltrability measurement is $\sim 315\text{cm}^2$, the plot extent is 40m^2 , so even if all six survey sites per plot had infiltrability values below 60mmh^{-1} , their combined support area would not come close to 50% of a given plot area. Exaggerations like this tend to undermine the credibility of a manuscript.

P7003/L5-6: . . . and after 119 yr the entire agricultural area had steady-state infiltrability below 100mmh^{-1} – so what? Unless compared with rainfall intensities, this number is meaningless, and certainly does not support any conclusions about runoff and erosion.

P7003/L7-8: ‘. . .there is considerable surface runoff and erosion’: The authors did not present any evidence for runoff and erosion, considerable or not, so this statement should be omitted, or be rephrased so as to be obvious as an article of faith.

P7003/L13-17: ‘From a landscape management perspective, the implication is that

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



wooded structures, e.g. tree lines or shelterbelts along contours (Ellis et al., 2006; Stroosnijder, 2009), woodlots or other agroforestry elements, need to be included in the agricultural system, at a scale large enough to create enough high infiltration locations to reduce runoff and erosion at both farm and landscape levels.' Generic statements such as this may appear appropriate for conservation journals, but are inappropriate for a journal such as HESS unless supported by data. Once again, as pointed out above, there are no data on runoff and erosion in this manuscript.

P7004/L25-27: 'Although logical and easy to understand, correlations between infiltrability and the other parameters are rarely presented in research studies.' The authors supply the perfect reason why the correlations are rarely presented: they are well-known, well-studied, have a causal basis – so why report extra data that do not contain extra information?

Conclusions

What is a 'sharp decline'? It's preferable to list again the facts, and to omit value judgments. For a truly sharp decline of infiltrability over a true chronosequence, see Zimmermann et al., 2010. It is good practice to state in the conclusions to which extent the objectives listed in the introduction were achieved. While the authors did a good job regarding objectives i) and iii), they gave short shrift to ii).

References:

Zimmermann B, Papritz A, Elsenbeer H. 2010. Asymmetric response to disturbance and recovery: Changes of soil permeability under forest-pasture-forest transitions. *Geoderma*, 159, 209-215, doi: 10.1016/j.geoderma.2010.07.013.

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

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)