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Hydrology and Earth System Sciences Discussions

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.

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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.

Dear editor, We thank for the constructive comments by the two reviewers and appreciate that reviewer 1 notes that it is a "very well written paper" and reviewer 2 that" 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...". We recognize that both reviewers raised some concerns, each which we are addressing below. We

feel that all concerns could either be used to improve the manuscript or in a few cases were based on misunderstandings.

Below we have inserted our response to the comments from referees, marked with "Our response:" below each point made by referees.

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 13C (and 15N?) 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.

Our response: In the referee's kind summary it is mentioned that "but changes with years since conversion were apparent only for C, N and aggregate size". However, it was apparent also for infiltrability and bulk density as can be seen in figures 2 and 3a and in the results section.

In the last paragraph of the abstract as well as within the discussion the reader is suddenly 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.

Our response: In the discussion part we think it is adequate to discuss possible implications and practical aspects of the research. However, we will rephrase the statement to emphasize the need for further research on the inclusion of agroforestry structures in rehabilitation and in agricultural fields.

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.

Our response: We do see the point in aggregating the data for years 57 and 69 to get an even sample size. However, general patterns and trends are evident and will not change, and the strength of the analysis will not improve much, as seen in figures 1 and 2 (and from the subscript letters showing statistical differences). Hence, we prefer to keep sampling years 57 and 69 separate. However, if the editor insists we could agree to pool the years 57 and 69.

In addition, the sample size for the very subjective crop yield is not included within the method section.

Our response: The sample size and methodology for the "crop yield" will be included in the methodology section.

Why is 15N in 3.5 reported but not within the other sections? It is not mentioned in the abstract, too.

Our response: 15N data will be omitted as they are not used in the analyses or discussion.

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.

Our response: The PCA analysis adds a visualization of the relations between parameters. However, we agree that the data is already shown in table 2 and will omit section 3.6 and figure 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.

Our response: Further references to other chronosequence studies will be added in the discussion.

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

Our response: Will do.

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

Our response: We will delete "rapidly".

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

Our response: We will provide more references

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

Our response: For the first sentence we replace "agricultural systems" to "land-use systems" to read: "The means and variances of infiltrability, soil C and other parameters are influenced by management in land-use systems". The second sentence will be deleted.

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 is possible.

Our response: We will refer the meteorological data to Kimetu et al 2008 and Solomon et al 2008, both of which had one co-author in common with this ms.

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

Our response: Will do.

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

Our response: We will delete “soil and site” and add “(ic, BD, %C, %N, macro and micro aggr. ïĄď13C and yield)”.

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

Our response: Will do.

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

Our response: Generally CV’s are higher in forest than in agricultural fields. This info will be included in the ms.

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

Our response: Will do.

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

Our response: We will refer to Ilstedt et al as “a review and meta-analyses”.

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

Our response:See comment above: P.7000-7001. Generally CV’s are higher in forest than in agricultural fields. This info will be included in the ms.

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 relates to your sentence?

Our response: We’ll rephrase that sentence to read: “Decreased infiltration may also lead to less groundwater recharge if the decrease is higher than the difference in evapotranspiration between the systems (Bruijnzeel, 2004; Malmer et al. 2010)”

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

Our response:See general comment above: In the discussion part we think it is adequate to discuss possible implications and practical aspects of the research. However, we will rephrase the statement to emphasize the need for further research ion the inclusion of agroforestry structures in rehabilitation and in agricultural fields.

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

Our response: We are not sure on what part of these paragraphs the referee do not understand but will rephrase to add clarity. Some points in that rephrasing will be: Infiltrability (and C and N) are more sensitive parameters to identify changes in the later part of the chronosequence than BD (and some other parameters. Had there been regular use of C3 plants, e.g. trees or tea, in the agricultural practice lower values and larger variation would have been seen in ïĄď13C. Analogous with other changes in the ms. we will refrain from using the term “rapidly” and instead state “within 40 years”.

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.

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‘.

Our response: We argue that there are patterns. There are consistent (exponential-like) decline in measured values for ic, N, C and macroaggregates and increases for BD, ïĄď13C and microaggregates over time. Walker et al 2010 defines chronosequences as: “A set of sites formed from the same parent material or substrate that differs in the time since they were formed”. They also state that “Chronosequences and associated space-for-time substitutions are an important and often necessary tool for studying temporal dynamics of plant communities and soil development across multiple time-scales”. The term chronosequences is frequently used for space for time substitutions, e.g. Kimetu et al 2008, Ngoze et al 2008, Moebius-Clone et al 2011, Awiti et al 2008, Epron et al 2009, Lebrija-Trjos et al 2011, Lin et al 2011, Creamer et al 2011, and we hence find the term chronosequence adequate to use. However, we do agree with the referee 2 that our study is a space for time substitution and we will explain this clearly in the introduction. We don’t find the term “soil degradation” to be judgmental. It is very frequently used when important soil parameters as soil C, N and infiltrability are severely reduced. However, we are not very rigid on the title of the article; we could, if the editor find that appropriate, change the title to e.g. “Soil property changes in a 120 year forest to agriculture chronosequence in Western Kenya”.

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.

Our response: We do find the soil classification appropriate. It was not done in our study, but in other studies on the same chronosequence, Moebius-Clune et al 2011 and Solomon et al 2007, studies in which one of the co-authors of this manuscript (J. Kinyangi) was also a co-author. The appropriate references will be added.

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?

Our response: Point taken. We did not perform texture analyses. In previous work by Kimetu et al 2008, from same chronosequence and with the same common co-author, it was concluded that textural differences were small along chronosequences (that study included several chronosequences) and that those small differences were due to length of agricultural cultivation rather than intrinsic soil property differences. This will be added in the ms.

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!

Our response: Point well taken. We will change “rapidly” to “gradual”.

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 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.

Our response: Variance is analyzed on all values for respective sampling years, i.e. n=12 for years 0, 39 and 119, whilst 6 for years 57 and 69. The total numbers of ic measures in the variance analyses is hence 48, i.e. enough to address differences in variation. For both means and variance the size of difference is important, and hence, "a rule of thumb" is not universally applicable.

P7003/L1-8: 'Tropical rains are often very intensive; rainfall intensities of >60mmh-1 or even >100mmh-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 >100mmh-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.

Our response: We thank the referee for pointing out the exaggerated values we quoted. We'll change the text to read: "Tropical rains are often very intensive; rainfall intensities of > 60 mm/h are not uncommon in the area for period up to 15 minutes and occasionally rainfall intensities reach >100 mm/h occur (Moore, 1979).

P7003/L4: '50% of the cultivated area had infiltrability values below 60 mmh-1'. This statement is not supported by the facts presented: the support associated with an infiltrability measurement is _ 315 cm2, the plot extent is 40 m2, so even if all six survey sites per plot had infiltrability values below 60 mmh-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 100 mmh-1 – so what? Unless compared with rainfall intensities, this number is meaningless, and certainly does not support any conclusions about runoff and erosion.

Our response: Rainfall was not measured, but the reference above (Moore 1979) does give rainfall intensities from the area. We do believe in the representativity of the random sampling of the plots, 6 sample points per plot. However, for clarity we'll change the text to read: "After 69 years since conversion, 50% of the randomly sampled points in the cultivated area had infiltrability below 60 mm/h, implying that 50% of the agricultural area had infiltrability below 60 mm/h; after 119 years all the agricultural sample points had IC below 100 mm/h (table 1)".

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.

Our response: Since the runoff and erosion was not measured we refer to an increased risk of runoff and erosion. The sentence should read: " As there are very few agroforestry structures that could act as "funnels" for infiltration (high infiltrability spots/areas) there is considerable risk for surface runoff and erosion and hence less water available for plant production (Stroosnijder, 2009)".

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

Our response: Albeit a generic statement we think it is adequate to raise this kind of discussion (in the discussion part of the ms.) also in prominent scientific journals. Analogous to the above comment we would like to insert “the risk for” for the sentence to read: “From a landscape management perspective, the implication is that 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 the risk for runoff and erosion at both farm and landscape levels”.

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?

Our response: Insert “conceptually” to read “Although conceptually logical and easy to understand . . ...” In land-use change literature we do not find a lot of such correlations and actually infiltrability is often not included as a measured parameter. We agree with Zimmermann et al 2010 when they argue about the need to include infiltrability measures in landscape studies “As discussed above in connection with the low-flow problem, quantification of land use-induced changes of soil hydraulic properties is important because of their essential role in the hydrological cycle. Ilstedt et al. (2007) pointed out that any model of forest–water relations should include soil physical quality such as infiltration levels before and after reforestation. Their claim is supported, for instance, by the fact that hydrological catchment models behave very sensitively to soil parameterization in relation to land use (Bormann et al., 2007).”

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.

Our response: Point well taken: replace “sharp decline” with “gradual decline”

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).

Our response: We realize that objective ii) is not well formulated. It is either too general; and then not very meaningful, or too specific; and then demands much more extensive studies then the present. Hence, we will remove objective ii) (Introduction, p. 6996).

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