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Interactive comment on "Integrated soil fertility management in sub-Saharan Africa: unravelling local adaptation" *by* B. Vanlauwe et al.

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REVIEW REPORT

Dear editors,

I have reviewed this discussion paper for publication in SOILD. The authors are arguing for inclusion of 'local adaptation' as an essential component of successful ISFM interventions in smallholder farming systems in SSA. First 'local adaptation' is conceptualized and the need for it illustrated with examples of soil fertility patterns within farms, different farm typologies and some limitations to ISFM interventions. The next two chapters describe the impacts of local adaptation on agronomic efficiency (of fertilizer nutrients) at the plot and farm scale. 4 examples are given for the plot scale (liming,

secondary and micro-nutrients, tillage, water harvesting) and 2 examples are given for the farm scale (Zimbabwe, western Kenya). The next chapter discusses how (mostly) research on ISFM and local adaptation can be brought to the smallholder farmer and suggests the use of Decision Support Tools and integrated modeling approaches with examples from the NUTMON and NUANCES frameworks and the Nutrient Expert extension support tool. The final chapter draws some conclusions and suggests some research challenges we still face. I liked reading the discussion paper a lot and only have a few suggestions for possible improvement:

Section 2.1, Lines 9-15: I think it would be worth mentioning all soil forming factors that contribute to the formation of the soil-scape (climate, organisms, topography, parent material, time; Jenny, 1941). As it is written now, only long term weathering and soil redistribution seem to be responsible for a typical soil catena (toposequence). Parent material e.g. is very important in determining inherent soil fertility patterns (e.g. poor sandy soils develop on old African basement rocks whereas richer and more clayey soils develop on younger volcanic materials). → REPLY: Agreed – this comment will be integrated in a revised manuscript.

Section 3.3, Lines 8-11: I am surprised about the association of plough-pan formation with 1:1 clays like kaolinite. Are there other references/data than the Africa Soil Atlas? → REPLY: Kayombo and Lal provide a number of references to articles that have described the widespread occurrence of naturally compacted and hard setting soils in Africa. In their article this is attributed to Alfisols especially, because of their low structural stability and their argillic horizon (or gravelly sub-soil). In the soil atlas these soils are referred to as Lixisols. These soils are prone to structural degradation that may be caused by tilling the soil. The compaction is not necessarily related to the compacting effect of the ploughing per se that you would see in soils with expanding clays, but the effect is the same in that you have a compacted layer below the surface horizon that is tilled and or ploughed.

Section 5.2. Another good example of a DST/integrated modeling framework is the

Tradeoff Analysis model for Multi-Dimensional impact assessment (TOA-MD). This model has been used to support decision making with ex ante impact assessments of alternative practices and/or policies in smallholder agricultural systems in SSA. Examples are the introduction of dual-purpose sweetpotato in western Kenya (Claessens et al., 2009) and tradeoffs in crop residue use (ISFM) in semi-arid Zimbabwe (Homann-Kee Tui et al., 2014). → REPLY: The purpose of the review was not to cover all decision support/modeling tools but provide examples of different tools and approaches and their utility for targeting ISFM technologies. While the TOA-MD approach is interesting, we cannot address it in the paper due to the limited DSTs review scope.

Congratulations with the paper, a very interesting read!

Minor edits/typos, referring to page and line numbers: 1249, 6-8: I don't understand this sentence 1249, 15: 'if done correctly', what does this mean? Maybe add a reference on correct liming? 1250, 10: have has 14: reverse opposite 21: period missing 1251, 8: common in SSA 1252, 8: under in 1254, 15: was used 1257, 13: period missing 19: on for 20: recommendations 1259, 4: crop-livestock-soil? 6: constraints 10: function of 1260, 11: models 13: seasonal seasonal 17: seasons 1261, 19: explain 22: that with 1262, 15: remove period 1263, 7: a balanced 11: what does 'tertiary level' mean? 1263, 20: and improved germplasm? 1264, 7: changes 1265, 13: others → REPLY: All the above will be addressed in a revised manuscript

References Claessens, L., J.J. Stoorvogel, and J.M. Antle. 2009. Economic viability of adopting dual-purpose sweetpotato in Vihiga district, Western Kenya: a minimum data approach. Agricultural Systems 99:13-22. Homann-Kee Tui, S., Valbuena, D., Masikati, P., Descheemaeker, K., Nyamangara, J., Claessens, L., Erenstein, O., Van Rooyen, A.F., Nkomboni, D., 2014. Economic trade-offs of biomass use in crop-livestock systems: exploring more sustainable options in semi-arid Zimbabwe. Agricultural Systems, in press âĂČ Anonymous Referee #2

1. Overall Comments Provides good synthesis of the status of knowledge on the ben-

efits of integrated Soil Fertility Management (ISFM) that includes fertilizer use at plot level. It goes beyond to highlight the complexity of taking the solutions from plot to farm scale level, complexity that arises from inter – and intra farm variation in soil fertility conditions and household decision process in terms of allocation of their limited resources (fertilizer, manure, labour etc.) and it provides some decision support tool that can be used at both plot and farm scale level in guiding investment ISFM in ways that increase yields and agro economic efficiency. Although the target client of the paper is not articulated, the rich information provided can be used by a wide range of stakeholders, particularly the agricultural research and extension community in sub-Saharan Africa.

2. Specific Comments

State who the target audience of the paper are? → REPLY: The primary target audience is the agricultural R4D community and more specifically those scientists engaging in developing and promoting ISFM options in sub-Saharan Africa. This need was identified through the frequent questions asked at various scientific fora in relation to the specific meaning of 'local adaptation'.

Would be good to indicate the yield gains at scale, if any, from upscaling ISFM? Given that it is a package of interventions and not a single technology, what would be logical progression in 'localizing its adaptation' in a given region and farming system? → REPLY: The logical progression depends on current practices. Farmers already using improved varieties and fertilizer could, e.g., invest in improving their use efficiency but improved integration of organic resources and adapting application rates to soil fertility conditions. Yield gains at scale will depend on yield gains in individual plots which are quite frequently observed under ISFM, since targeting input use to specific soil conditions is inherent to its definition.

P.1244 – home gardens. It is worth mentioning the Chagga homegarden of Tanzania that is much published. → REPLY: We are not sure how this would add value. Note that the exact location where this is proposed to be added is unclear because line numbers were not clear from the comments document.

P. 1248 (3.1 Liming effects) – suggest you indicate the proportion of sub-Saharan Africa's potential agricultural land that is acidic or has al toxicity problems and that requires lime application. This will help put the problem into perspective. Would also be good to mention that the high cost of transportation limits the use of lime. It is generally not an expensive product but its transport is partly because it is often found in areas far from main agricultural production areas. The application rates are also often high, often 2-4 t/ha, adding to its application costs. → REPLY: It is known that Al toxicity is inherent to a number of soil types (e.g., Ferrasols, Acrisols) coupled with high rainfall conditions. An estimate of the area occupied by those soil types is about 16% of total land area in sub-Saharan Africa. One could assume that the potential for creating Al toxicity would be in the same order of magnitude. We agree with the reviewer that transportation costs of lime are high, and the further to the mining location the point of delivery is, the more costly it gets. The rate of lime applications is also high, sometimes higher than the range indicated, depending on the level of the Al saturation. However, the current major issue is that the agricultural lime is not even mined in many countries that have the problem. Most lime use has remained at experimental/research purpose. As indicated in the paper, the adoption of liming would depend on the cost effectiveness of the practice, and this would take into consideration the costs suggested by the reviewer. We'll integrate above discussion in the revised manuscript.

Soil Taxonomy used in the paper: suggest both FAO and its equivalent USDA taxonomy are used. This will enhance the readership of the paper. → REPLY: We would prefer to keep the FAO/WRB classification since this is the only globally accepted taxonomy.

P.1253 – the statement on effect of water harvesting on AE – N. Would this be true for irrigated rice? → REPLY: Water harvesting is not really of relevance to irrigated rice.

P.1257 – Moving knowledge on local adaptation . . ..one comment: Nutrition – good

but it is important to state that the negative nutrient balances will result in positive yield gains if fertilizers were applied. → REPLY: The translation of negative nutrient balances into yield gains is not only going to depend on increased nutrient application but there are other contributing factors including soil nutrient stocks, status of land degradation, fertilizer rates and balanced nutrient management. We have not included the statement to relate nutrient balances to yield due to many complex factors that determine crop productivity in smallholder farming systems in SSA in addition to increased nutrient use. State clearly the utility of the model for farm level decision-making. Has any of the models – Nutmon, Nuances, NE, etc. been used at scale by both public and private sector institutions in guiding the role of ISFM practices? If not, what would it take to do that? → REPLY: We stated clearly that relatively complex farm-level modeling tools have been uses as 'a platform for research to improve understanding of the complexity of smallholder farming systems.' There are efforts underway to translate the complex modeling tools into simple decision support tools that can be used at scale by extension systems, development programs and the private sector. This includes the example provided on Nutrient Expert. An important step in scaling out is developing simple guidelines for extension systems to refine the general ISFM recommendations and develop targeted ISFM options for specific sites and farms. While there has been limited direct use of DST at large scale, the knowledge generated has been instrumental in raising awareness of import issues including the status and factors driving nutrient depletion and land degradation, the limitations of use of both organic resource and fertilizers and hence the need for ISFM, and the importance of fertilizer and improve crop varieties as entry points to increasing crop productivity in SSA. This knowledge is the basis of current efforts to implement the 'uniquely African green revolution that recognizes the heterogeneity of Africa's agroecological environments and soils' – Annan 2008. AfSIS deserves a bit more mention since it is now used widely in Ethiopia for diagnosing soil fertility at scale. → REPLY: Recently develop soil mapping approaches used the Africa Soil Information Services (AfSIS) project including compilation of existing soil survey information, data generation using infrared spectrometry, geo-spatial statistical analysis and remote sensing have enabled the rapid and cost effective development digital soil maps. This has offered opportunities to accelerate data collections for accurate diagnosis of soil fertility constraints and improve targeting of technological options. AfSIS has supported the development of new soil maps for Ethiopia that has been used to develop site-specific fertilizer recommendations for different regions in Ethiopia. We'll integrate this in a revised manuscript.

3. Conclusion Good but would be great to include the human and institutional capacity development needs to further improve the local adaptation and scale up of ISFM technologies. Is the limited uptake of ISFM technologies related to limited human capacity research and extension? If so, what numbers do we need to train? Some concluding remarks on this issue would add value and enhance the readership of the paper. → REPLY: These are very relevant and important issues but beyond the scope of this paper.

âĂČ Interactive comment on "Integrated soil fertility management in sub-Saharan Africa: unraveling local adaptation" by B. Vanlauwe et al. R. Voortman

Dear editors,

A colleague called my attention to the above paper and I take the liberty to provide some comments. In short the authors strive to improve scientific practice in the field of development of fertilizer technologies that are locally adapted and appropriate for Sub- Sahara Africa. This is urgently needed indeed. Overall, I think that the authors could develop their ideas somewhat further, while maintaining theoretical rigour. Doing so may bring great benefits for African farmers. Below I summarize the ideas that come to my mind on this issue. → REPLY: The paper is not aiming to improve science practice for the development of fertilizer technologies. While fertilizer use is an essential component for ISFM, it is certainly not the only component.

The authors state that: i) Most of the commonly applied fertilizer in SSA contains mainly N, P, and/or K, which do not replenish SMNs under continuous cropping. ii) But indeed

the reverse is more likely to be true: where SMN deficiencies exist, they can limit response to NPK fertilizers. iii) management of Al toxicity has received little attention in recent years in SSA These observations are very true. Given the many research findings in the past, it could be concluded that there has been an over-emphasis on high N and P doses in agronomic research in SSA. These high doses are unlikely to be affordable for African farmers and Figure 3 of the paper also shows that high doses are ineffective in raising yields. The emphasis on N and P as well as the lack of attention for liming materials is also remarkable from the theoretical point of view since the plant content of Ca, Mg and S is usually as high as P and frequently even higher. In addition, even though required in very limited amounts, micronutrient deficiencies can impose serious restrictions on crop yield. → REPLY: We agree that SMN response may be due in large extent to inherent deficiencies and in many cases limit NP responseâĂŤyet it is still true that NP(K) fertilizers do not replenish SMNs under continuous croppingâĂŤor any cropping, for that matter. We agree with the reviewer that there has perhaps been an over-emphasis in the past on NP nutritionâĂŤthis manuscript brings to the fore yield constraints related to soil acidity and other nutrients.

The implication for the development of appropriate fertilizers, at least in my opinion, would be that from the outset the working hypothesis would be that any of the essential plant nutrients can be the most deficient or most toxic. However the authors do not really make that change of mind-set. They still mention: i) Much of the evidence relates to N fertilizer applied to maize as N is the most limiting nutrient in many African soils, ii) Blending commonly available NPK fertilizer with SMNs is a cost-effective process to achieve these benefits. iii) to assess the economics of incorporating secondary and micronutrients into NPK fertilizers iv) In the described nutrient omission trials N and P were not omitted (Figure 6) v) In Section 5.3 Nutrient Expert algorithms are recommended to determine N, P, and K fertilizer requirements under specific field conditions. Such observations and recommendations still constitute an emphasis on N, P and K as being the basics, while the use of other nutrients is considered as something that comes in addition. Theoretically the mere addition of SMNs to NPK is also problematic, because of the many interactions between nutrients where uptake by plants is concerned (antagonisms and synergisms). For instance, on soils low in Cu and Zn it may not be wise, and it is likely to be inefficient, to apply their antagonist P. → REPLY: We agree with the reviewer that much past research has concentrated on NPK, and that this research has been done without consideration of other nutrients and soil acidity constraints. It is highly likely that different optimization results would be obtained from modeling NPK response models if secondary/micronutrient deficiencies and soil acidity constraints were simultaneously addressed. While "theoretically" nutrient antagonisms may occur, it is not our practical experience that this is a serious problem, as we have had positive response in applying for example small quantities (less than 0.5 kg/ha) of Cu and Zn as coatings to diammonium phosphate. Practically speaking, SMNs need to be added somehow, and adding them to NPK fertilizers either as blends or coatings has proven both efficient and economic. N and P were not omitted in the omission trials, because we already knew from both soil analysis and previous experience that N and P were deficient. The objective of these trials was to determine if additional nutrients would substantially improve yield response over current NP recommendations.

Section 2.1 line 9-15: There are some serious ill-conceived generalizations here that affect the credibility of the paper. The main natural factors determining soil fertility are soil parent rock composition, rainfall amount and time. Weathering of parent rock produces soil and in young soils this is still an ongoing process (there are still weatherable minerals in the soil profile). However soil fertility per se, the type of nutrients and their levels, is initially determined by the original parent rock. Depending on the age of the soil and the amount of precipitation (in combination), the soil fertility may be altered by leaching. Soil nutrients have differential leaching rates and therefore not only the level of nutrients changes, but also the proportions of nutrients present in the soil (stoichiometry). Furthermore, soil types are only infrequently associated with slope position (in case of catena's). This is only the case when parent material is rather homogeneous over larger areas and if this occurs in combination with old-age topography (rather flat).

Consequently, hills do not occur. Near the highest position at the interfluves soils are not necessarily gravelly and thin with rock outcrops. This is only the case when pockets of more resistant rock occur in the host rock, as is the case for instance in Sukumaland. In fact, at the top of interfluves soils may be deepest of all. Also, further downslope soils need not be more fertile. In fact lower slopes may be shallow and gravelly just above seepage zones, due to redistribution of iron (laterite gravel). In such landscapes the bottomlands (dambo or mbuga) are also not of alluvial origin: they are not deposited by rivers. → REPLY: We agree that this specific statement is rather general and an oversimplification of reality (although the mentioned example does occur). We do not agree that this affects the credibility of the paper since the main factor affecting soil fertility status that is considered in this paper is related to management practices. Inherent soil fertility is indeed determined by parent rock quality and soil-forming processes but long term soil management practices create important soil fertility gradients. As the work on granite sands from Zimbabwe shows (cited in the manuscript) these soil fertility gradients can be created in as short a period as three years.

A similar observation on nutrient gradients as apparently mainly observed in Zimbabwe is in place. Some soils are considered as being degraded and strongly depleted of nutrients and where no significant response to "standard" fertilizer can be observed. At the same time it is mentioned that such soils occur on sandy soils developed from granite. Could it be that such soils are neither degraded and nor depleted (from something that was better), but that they are simply a different soil developed from different soil parent material. Indeed this is what must be suspected in case of soil textural differences. This being the case, one would indeed suspect a different set of nutrient deficiencies. → REPLY: There is no doubt that some of this variation in soil fertility on farm is caused by land use and land management history as the gradients can occur over distances as short as 50 m and detailed investigation has demonstrated that all major soil physical variables (texture, drainage, soil depth) are similar across the gradients (Rusinamhodzi et al, 2013). There is also no doubt that this occurs in many countries with high population densities, including Rwanda, East DR Congo, Northern

Nigeria, Burkina Faso, Western Kenya, etc, etc. The example from Zimbabwe shows that good maize yields can be obtained near the homesteads. Rusinamhodzi, L., Corbeels, M., Zingore, S., Nyamangara, J. & Giller, K. E. (2013). Pushing the envelope? Maize production intensification and the role of cattle manure in recovery of degraded soils in smallholder farming areas of Zimbabwe. Field Crops Research 147: 40-53.

Section 2.1 line 24-28: This sentence is not clear. → REPLY: This sentence will be rephrased in the revised manuscript.

Section 3.1 Line 2-3: A potential risk of liming at high rates is that it reduces the availability of all micronutrients except Molybdenum. → REPLY: This is correct; one must take care to lime to a correct level.

Section 3.1: No mention is made of Gypsum which can reduce Al saturation (to deeper levels than lime), maintain micronutrient availability, decrease surface sealing, improve water infiltration, decreases soil erodibility, allows deeper rooting and improves Ca and S nutrition. For instance in Brazil gypsum is considered a valuable soil ameliorant. In this context it deserves mentioning that the shift in African agronomic research from SSP to TSP is deplorable, because SSP actually contains gypsum (Ca and S). Again, in Brazil about 90 percent of P is applied as SSP. → REPLY: The problem in the African context of gypsum is primarily a matter of economics and availability. Gypsum is generally less available than lime. It is not a cheap source of sulfates or Ca in the sub-Saharan African context as are other available sources such as lime and ammonium sulfate. SSP is a good fertilizer but high transport costs limit its economic use to coastal areas. SSP is actually produced in Kenya and is available in some other countries such as Mozambique.

Section 3.2 line20: I do not think that it is wise/correct to state from the outset that multiple SMNs are the norm. This will have to be established by research. It would be interesting and very useful though, to investigate if there are unifying principles in the occurrence of multiple SMN deficiencies reflect common parent rock mineralogies that

derive from the physical laws of nature of rock formation. → REPLY: We believe SMN deficiencies to be the norm. In the past 3-4 years, large areas of Uganda, Ethiopia (all), Mozambique, Rwanda (all), and Burundi (all) have been mapped, and indeed SMN deficiencies are the norm in these countries. We have also found this to be the norm in smaller mapped areas of Zambia, Kenya, and Tanzania. We see response to SMNs in all of these countries, confirming the soil analyses.

Section 3.2 line 24: It is also important to know which plant nutrients are present in excess. Therefore, I suggest to change the text 'demonstrating the importance of including all potentially deficient nutrients in an omission trial (Fig. 6)'. To: demonstrating the importance of including all essential plant nutrients in an omission trial. → REPLY: There are many essential plant nutrients, so it only makes sense to include those that are potentially deficient in omission trials, as the trials would become unnecessarily large and complicated. This is easily determined from prior soil analyses. One cannot include an "excess" nutrient in an omission trialâĂŤthough we do note from soil analyses potentially toxic elements (Mn, Al in acid soils; Fe in flooded soils; Na in high pH soils).

Section 3.3 line 1-3: All issues mentioned can also be addressed with gypsum, or more properly said by reducing the relative amount of Mg and Na at the exchange complex. → REPLY: This sounds quite unlikely and we know of no published evidence to this effect.

Section 4.1: One of the questions the text of this section raises with me is the extent to which nutrient and organic matter accumulation is merely a question of human influence. Could it not be that initially these soils were also of inherently better quality and were these therefore preferred for cultivation. It also would appear that the level of resource endowment as among others expressed in manure availability is then not an independent variable. Could it not be that because poor people have only poor soils that the ensuing resource endowment is an endogenous variable, relating to original soil quality. Using resource endowment as an independent factor then sort of represents circular reasoning. → REPLY: Resource endowment operates at a within-community scale where farmers commonly have access to the same (combinations of) soils. As stated above, for the sites where this has been intensively investigated in Zimbabwe there is no evidence that poorer farmers are situated on inherently poorer soils.

Section 5.1: Would it not be meaningful to involve the farmers in local adaptation. Farmers are likely to avail of knowledge on the diversity of their soils and their spatial distribution. Such knowledge might be a meaningful point of departure for a scientific characterization and subsequent research. → REPLY: Paragraph 3 of the Conclusions section specifically addresses this issue.

Section 6 line24-27: I fully agree that nutrient omission trials are instrumental for developing site-specific and fine-tuned fertilizer technologies. However they are a first step only. Also this first step can still be improved compared to what is presented in Figure 6. The following questions can be raised. Why are there no all-N and all-P trials reported? The all-dolomite experiment may not be very informative as it contains both, Ca and Mg, and further a host of micronutrients, as well as toxic substances like Cd and U (thus calling for careful consideration of liming). Why have Fe, Mn and Mo not been tried? Moreover, Figure 6 gives only averages across sites. This means loss of information. The responses on individual sites is far more informative, certainly if combined with soil chemical analysis for all essential plant nutrients. Sensible interpretation can then be made. I would also suggest that the subsoil is analyzed, because topsoil properties are always somewhat equilibrated due to nutrient cycling. In fact the differences between topsoil and subsoil themselves may be informative on limitations for nutrient uptake. In this way one would be able to simultaneously address the issue of local adaptation as well as the generalization of overarching principles that are evident in the data collected. Interpretation of results (Figure 6) is also not an easy task, because, for instance, the yield decrease when B is withheld may reflect a B deficiency that is due to high N, K and lime applications, because N, K and Ca are antagonistic with B. Similarly the yield reduction when Cu is withdrawn may result from the Mg, N,

P and Zn applied, while in case of Zn the antagonists consist of Ca, Cu, Mg, N and P. It is well possible that with lower levels of notably N, P, K and Ca similar 'All' yields would be obtained without a B, Cu and Zn deficiency to be evident. As such nutrient omission trials are a first entry only that serves to develop hypotheses that require verification, but now within the context of a well targeted research agenda → REPLY: There are many questions here, so each is addressed in turn: 1. N and P were not included in omission trials because we knew from soil tests that they were already deficient, and the objective of the trials was not related to NP response. Rather the objective was to see what yield increases could be made over commonly available NP fertilizers. 2. Dolomite was the available lime source in Burundi. It is not completely informative as one does not know if the effects are due to Ca, Mg, acid-neutralizing potential, or some combination. But it is informative from the perspective that one can determine if the amendment increases yields and is economic to apply. 3. We thoroughly analyzed the available lime source for a host of other nutrients, including Zn, B, Mo, Si, and S, and found no substantial quantities that would meaningfully contribute to plant nutrition. 4. Mo probably deserves some consideration; Mn and Fe were not included as they were found to be well within sufficiency ranges in the soil analyses. 5. We present averages across sites as a matter of brevity for this publication. All nutrient/amendments mentioned contributed significantly to yieldsâĂŤmore details such as the reviewer suggests will be in the publications relating to these trials, which will be submitted in 2015. 6. While we have considered the possibility of antagonism, believe that the responses evident in the omission trials are not a product of antagonism. Our working hypothesis is that nutrients shown deficient in a soil test are indeed deficientâĂŤthose are the nutrients we evaluate in the omission trials. The amounts of nutrients we apply are relatively modest and would bring soil levels up slightly but not to excessive levels that might induce severe antagonism. In these particular trials, Cu was applied as a foliar, so we cannot have direct antagonism at receptor sites (the common mechanism for Zn/Cu antagonism). Additionally, we have seen positive response to both Zn and Cu in omission trials applied together as fertilizer coatings. If antagonism were in play between Zn and Cu, we would expect omission of one or the other to increase (rather than decrease) yields. Our experience therefore suggests that antagonism is again not in play. Boron levels are extremely low and we have observed B response (as well as Zn and Cu response) without limeâĂŤthis is strong evidence that the B response is genuine. More practically, applications of N, P (K) and dolomite in these trials can be best described as modest and necessaryâĂŤand therefore whether or not antagonisms are induced (and we strongly suspect not for reasons stated above), they still require correction as there is no other option. 7. At the same time, we acknowledge that plant tissue analyses would contribute to better interpretation of trial results.

Generally, it is a pity that no references are made to the Brazilian literature, where ample experience with SMN is reported. Maybe Portuguese is a problem, but there are also papers in English. There is also no reference to relevant French literature. I would recommend: Boyer, J. 1978. Le Calcium et le Magnésium dans les sols des régions tropicales humides et sub-humides. Initiations-Documentations Techniques No. 35, ORSTOM, Paris. Interactive comment on SOIL Discuss., 1, 1239 → REPLY: Again, this is a matter of maintaining a degree of brevity appropriate for this manuscript. SMN responses are noted the world over. Indeed we eliminated the majority of citations from SMN research on the African continent itself from an earlier draft.

Interactive comment on SOIL Discuss., 1, 1239, 2014.