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Hydrology and Earth System Sciences Discussions

Interactive comment on "Soil bio-engineering for risk mitigation and environmental restoration in a humid tropical area" *by* A. Petrone and F. Preti

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RESPONSE TO ANONYMOUS REFEREE #1 AND #2

Response to anonymous Referee #1

1. General comments: This interesting site-specific project report deals with a highly relevant topic deserving much attention. A big effort is also made to discuss the involvement of local communities in defining and implementing the project. Therefore, this study has the potential to become of significant value both to scientists and managers working on bio-engineering techniques for land rehabilitation in harsh conditions in the tropics, in collaboration with local communities. Nevertheless, if the intension

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of the authors is to publish this manuscript as a scientific paper in HESS, it will need a serious revision. In that regard, it still suffers from important, especially technical, shortcomings.

We thank Referee #1 for the appraisal of the work and for the constructive criticisms. We revised the manuscript following the general guidelines indicated. In the following we provide point-by-point responses to reviewer's comments.

1.1 First of all, English language needs serious improvement. I made a few suggestions for correction, but these are far from complete: it is strongly advised to have the entire manuscript thoroughly screened by a native English speaker, preferable someone who is familiar with the research theme.

Amended in revised mansucript.

1.2 Furthermore, I have a few concerns with regard to methodological aspects (See also "specific comments").

Manuscript has been improved: please, see answers to specific comments and to Referee #2.

1.3 Ultimately, I believe the currently available literature dealing with these topics is underutilized. Many reports are referred to, but few recent publications are mentioned. (See also "specific comments").

References have been improved in the revised manuscript.

1.4 In conclusion, I would like to encourage the authors to seriously revise the present manuscript, in order to increase the impact of their valuable work and to address a wider scientific public.

We hope you'll find the revisions and clarifications in response to review comments adequate.

2. Specific comments

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2.1 What are the provenances of the plant material used? - No details on the number of plant replicates per species/treatment? Statistical results, together with means, standard deviation and no of replicates could be presented in a table for the merit of the potential reader.

Manuscript has been improved. See answers to specific comments and to Referee #2. A table showing all requested data has been added to the revised manuscript.

2.2 Since survival and plant growth performance are expected not only to be species-specific but also to depend largely upon the growth conditions, it would be interesting to get some more information on the soil environment of the exact places where the installations have been built: at least: water availability, texture, soil type? - In that perspective, especially towards practical recommendations, we wonder why potential environmental effects have not been elaborated upon (phenotypic plasticity?).

All requested data have been added to the revised manuscript and we discuss the influence of these parameters on the different behaviour of Erythrina fusca in the live palisade and in the live crib wall.

2.3 With regard to the measurements of height and diameter: were the initial dimensions also assessed? If so, it would be interesting to present growth rates. If not, how did the authors correctly differentiate initial differences from changes caused by differences in growth environment? - Given the importance of the studied species for e.g. firewood and fodder, it seems very relevant to me to determine (or estimate through allometric relations) biomass development. - In conclusion, I believe more relevant growth variables could be evaluated, statistical analysis could be expanded (maybe taking into account other potential sources of differences) and presentation of the data needs improvement.

In fact the measurements of the height and diameter were referred to the terminal shoots of the planted cuttings. The terminal shoots were not present at the initial phase of plantation. Biomass development is a relevant issue but in this paper we

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focused more on the bio-technical aspects of the plants (more directly related to soil bioengineering purposes).

2.4 Better linking the technical and social methodologies The effort made to involve the local communities in defining and implementing the project, is highly appreciated since of uttermost importance. Nevertheless, a few concerns can be raised:

2.4.1 The species selection criteria used in this study are merely based on financial viability and suitability for soil bioengineering techniques, whereas towards application it is important to consider a much wider range of ecological, socio-economic and even socio-cultural criteria. Although the intensions of the authors to consider the problems of land degradation in an integrated way are made clear, I could not find any concrete results or recommendations in that regard. Even if not dealt with in this study, I suggest at least to shortly addressing this issue in the discussion.

The choice of the species was made first of all by asking to the local population which plants had certain characteristics described in paragraph 2.3 Plant Used. In the study area it was rather easy to detect these species with the help of the local population: actually one of the most common practices is making “cercas vivas”(in English “live fences”), driving cuttings into the soil and then fencing them with wire. Obviously, the species used on this purpose are easily found in the area, they propagate from cuttings with excellent results and, once adult, not only they can be used as fence but they also provide wood and, in some cases, fodder. As a matter of fact, the plants suggested by the population have already been selected by the population itself for their utility: with this work we try to understand which among these plants have also the characteristics fitting soil bioengineering aims.

2.4.2 Furthermore, the image of “consensual (village/local) communities” – where the authors depart from (at least as suggested in their methodology) - has repeatedly been shown to be a poor reflection of empirical reality (see e.g. Leach et al. 1999; Olivier de Sardan 2005). In fact, the authors point out in their introduction that this image does not

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strike with reality in their study area (see rich landowners versus (land-)poor). All negotiation processes reflect prevailing power structures (Leach et al. 1999), hence also the participatory processes set up by the authors to get to know the priorities of "the local communities". We are therefore wondering whether and how the authors have tried to avoid the "hijacking" of their participatory forum by local elites, and the exclusion of the poor? Or in other words: in how far do the site-selections by the community leaders, the species selection by the communities and the decision to contribute labor to the project reflect the interests of the poor/farmers and not e.g. of the large landowners?

The raised question is actually of great importance and the authors agree that the participatory processes risk to be hijacked as described by referee #1. In spite of this, it is necessary to underline that, as far as the choice of the intervention site is concerned, the firs proposals have been made by the DIAF's team and then submitted to the approval of the community. The planners wanted to carry out some interventions able to fix some types of erosive processes common in the area (and so potentially replicable), with a good visibility and allowing the use of a certain variety of Soil Bioengineering techniques. The role of communities under this point of view has been to confirm the interest in proposed intervention sites. As far as the used species are concerned, as we underlined above, communities suggested to use plants well known in the area for other purposes and we think that this point is not influenced by the issues presented by referee #2. In the end it is necessary to make clear that when in the paper it is stated that "the communities gave their contribution by providing their labour force", it is meant that the large landowners paid the working days of their labourers, so that they could work at the construction of the Soil Bioengineering works.

2.4.3 Are the proposed soil bio-engineering measures for implementation in private or communal land?

In the intervention area most of the lands are private properties and the works were realized in private areas except one, implemented in the water collecting area of the municipal water supplying society of Rio Blanco.

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3. Technical corrections (typing errors, etc.) General remark: Only a few suggestions for language improvement are made here, but as suggested above, the entire manuscript needs to be screened in detail by a native English speaker. Everywhere: - Replace “vegetable” by “plant” and “vegetable matter” by “vegetative material or plant material”

Amended.

3.1 Abstract: - line 4: autochthonal (not authochtonal): how do you define this? Why not using “indigenous”? Or is there a difference in definition? - line 5: these two issues - line 5: divulgation: use a better term - line 7: tropics

In fact we use “autochthonal” as a synonym of “indigenous” describing something originating where it is found.

3.2 Introduction: - Pg 5141 line 21-22: no recent works? - line 27: idem

The FAO publications concerning the use of Soil Bioengineering are not recent but, considering the Institution they are from, they are anyway an important reference. Recent works are reported at page 5142 line 4-8.

3.3 Pg 5150 line 6: which diameter was measured? Diameter at the base?

The diameter was taken at the shoot’s base.

3.4 References: - Pg 5159 first line: not Poisen but Poesen Figure layout: - Figs. 7 & 8: remove the decimals from the units in the Y-axis, and remove the grey background

We have improved the paper considering the suggested technical corrections of Referee#1.

Response to anonymous Referee #2

General comments: This is an original contribution, especially due to the rarity of studies related to the theme in Latin America. However, the study is lacking in some as-

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pects to achieve a scientific level. Original methodological questions (during the experiment's installation phase) do not permit arriving at safe conclusions. The authors need to better describe the methodology and improve the discussion of results to take better advantage of the information raised. The economic analysis, as conducted, lacks justification.

We have improved the paper considering some of the comments of Referee#2 and we'll explain in this response why we do not agree with some affirmations of the Referee#2.

Specific comments:

1. The methodology used does not permit a comparison between the studies' results. The data do not come from a controlled experimental situation, but rather from plants developed in the studies. This does not permit any analysis of data from a methodological point of view. There is no known number of repetitions. The conditions for each plant are different, and there is no statistical design. Nevertheless, it still needs a better description of data taking conditions and methods: site's exposure, weather-climate information, type of substrate (soil), data collection periods, etc. The authors also fail to describe what exact type of material they used for initial vegetal propagation: stakes?what sizes? what age? how, planting inclination and proportion? Although the study does not originally have a statistical design, within these limitations, the presentation and analysis of gathered data could be expanded and better presented.

The data come from a controlled experimental situation and maybe this fact wasn't clear as we decided not to report the data sheets concerning every job mentioned at page 5149 line 20. In these sheets we detailed the number of repetitions, site's exposure, the type of substrate and the data collection period. We'll summarize these information in a table in order to clarify these issues in the revised manuscript. The weather-climate information were described at page 5143 lines 24-26. The study has a statistical design that consists in monitoring certain parameters of cuttings that were in the same initial conditions. Regarding the choice of the parameters, we report the

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explication given in a previous paper cited in bibliography (Petrone and Preti, 2008), The three parameters used are: survival rate, length of apical shoot, diameter of apical shoot. As far as the first one is concerned, not many explanations are needed: in fact, it is important to assess this parameter in order to privilege species with higher values. According to Schluter (1971), for example, only plants with survival rates of at least 70% should be considered for use in soil bioengineering practice. As far as the second parameter is concerned, the relationship between shoot and root development allows estimation of overall rooting performance and consequently, the anchoring and soil binding functions of the plants (Lammeraner et al., 2005). Not only is the measure of the third parameter needed in order to assess the degree of vegetative development of the plant, but it is needed above all to understand how long will the shoot remain flexible: in fact, in case of intervention on the bank of a river it is of vital importance that shoots keep their elasticity and bend themselves to the running water without making resistance. It is generally considered that a 4 cm. diameter constitute the upper limit for not losing flexibility. Substantially, the measurement of the shoot diameter development provides a fundamental information on how frequently to perform felling operations. The same initial conditions were of course different in the two test sites, the live palisade along the road from Rio Blanco to Wanawas and the live crib wall along a riverbank. Nevertheless all the cuttings planted in the first site were in the same initial external conditions (soil type, meteorological conditions) and the same we can state for the second site. As far as the description of the exact type of material used for initial vegetal propagation is concerned, at page 5150 line 1-2 we refers to cuttings. The initial size of the cuttings will be reported in the revised manuscript. Planting inclination and proportion were the standard used in the realization of these kind of works cause we didn't want investigate these parameters in this study. In conclusion the study has originally a statistical design as presented above and we'll improve the presentation of the initial conditions and of the collected data as suggested by the referee #2.

2. In item 2.5, "Monitoring and statistical analysis", the authors state that shoot diameter (but they do not explain where this diameter was taken) is directly related to

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development of the root system. An affirmation like that must be confirmed and justified through citations of other studies where this was observed. This relationship is not so clear and unanimous in the references. Besides the diameter of the shoots' base, the number of shoots also plays a role. The average diameter of the shoots' base is not very related to the root system. A variable that is also part of the number of shoots, such as: the sum of the shoots' cross-sectional area has a relationship that is much more explanatory and related to the root system and plant vitality.

The diameter was taken at the shoot's base. In fact in item 2.5 page 5150 lines 7-9 we state that "The extent of the second parameter is particularly important because it is directly correlated to the development of the rooting system. Therefore, such a property shows the ability of the cuttings to serve as retainers of the soil's superficial layer." The second parameter, length of terminal shoot and not diameter of terminal shoot, is reported at line 5 of the same page. As the anonymous Referee #2 writes, the relationship between shoot diameter of a seedling and root system is not unanimous. Anyway, there are studies showing correlation between these parameters and also between shoot length and root system. Just to name few: Duryea and Dougherty (Duryea and Dougherty, 1991) state that "Diameter often reflects seedling durability and root-system size"; Baninasab and Mobli (Baninasab and Mobli, 2008) report that "In Pistacia vera, shoot length and diameter were significantly correlated with leaf number and root number" and that "In Pistacia khinjuk, shoot diameter was also significantly correlated with root number, root length. . .."; in Rauf et al. 2007 we find that "Shoot length showed highly significant and positive correlation with root length,. . .fresh root weight and dry root weight". Anyway, apart from such correlations, morphological attributes such as shoot height and diameter are on the front line of seedling evaluation, and account for most of the variability among them (Bernier et al. 1995). Moreover, quoting again Duryea and Dougherty, "probably the single most useful morphological measure of seedling quality is diameter" and also "seedling with large diameters are better supported, resist bending better, tolerate more insect and animal damage, and are better insulated from heat than those with smaller diameters".

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3. In item 2.6, the authors say the economic sustainability of the interventions is important. In other words, the studies should have a justifiable cost. Most certainly, low cost is needed for these studies to actually be used in developing countries. However, the authors do not explain the technical, social or scientific importance when comparing costs between Nicaragua and Italy. An analysis that compared the costs for this type of intervention with the costs of a traditional intervention (both in Nicaragua) could provide more useful information than a comparison with costs in Italy. Or, it would be even more useful to conduct economic analyses that demonstrated that this type of intervention has costs compatible (or not) with Nicaragua's economic reality. This idea could be worked on when analyzing intervention costs compared to local economic indexes, such as: average income, sums invested by public authorities in works with the same objective, or in palliative measures or recovery of damages resulting from a lack of interventions.

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We refer to the comparison between the costs in Nicaragua and in Italy as two representative cases of low income countries and high income countries. The analysis comparing costs of this type of intervention with the costs of a traditional intervention is obviously very interesting and in fact the authors have already presented and discussed this issue in a previous publication cited in bibliography (Petrone and Preti, 2005). As reported in the text at page 5155 lines 12-16: "The analysis of the interventions' costs allowed to reach a wider awareness of the financial bearableness of soil bioengineering in the "impoverished" (better than "developing") countries in comparison to previous works (Petrone and Preti, 2005, 2008) which considered only the realization of vegetated live crib walls compared to classical interventions such as concrete walls and gabions." We suggest the anonymous Referee #2 to have a look at this work for further information about this topic. When the anonymous Referee #2 states that "it would be even more useful to conduct economic analyses that demonstrated that this type of intervention has costs compatible (or not) with Nicaragua's economic reality." we definitely agree with him. For this reason in the manuscript we have already presented such an analysis. After presenting the prices paid in Nicaragua and in Italy we proceed to a further analysis described in the text (page 5151, lines 20-26): "Once the unit price was obtained, we converted the amount in EPP Dollars (Equal Purchasing Power). EPP is an artificial dollar whose purchasing power is equal in all countries, as its value corresponds to the weighted average of the world prices of 151 kinds of goods. This instrument is commonly used by international Organizations such as UNDP (UNDP, 2006) and International Monetary Fund, only to mention a few of them. It is a way of comparing prices paid in different geographic areas and understanding their actual entity (Petrone and Preti, 2008)." In conclusion, the suggestion of the anonymous referee #2 was already included in the text.

4. Terms such as "satisfactory", "very bad", "good" etc. always need a justification. Why satisfactory? Why very bad? Why good? Affirmations like these given an idea of subjectivity, so they should be avoided or supported by information (citations) that backs them up.

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Every time we used these terms, we have been trying to justify them except in those cases that we can consider self-explanatory. We used the word satisfactory twice in the Discussion: the first was at page 5154 line 13 and the second one at page 5155 line 3. In the first case we say that "The behaviour of Gliricidia sepium and Tabebuia rosea in the live palisade (Fig. 9a and b), with regard to the survival rates, is more than satisfactory: they both showed rooting percentages exceeding 60%." We can back up this by quoting Lammeranер et al. 2005 (paper cited in bibliography) and we thank the referee for the suggestion. In the second case we state that "it is possible to consider undoubtedly satisfactory the behaviour of the three species from the point of view of the growing rate's trend." We admit that this gives an idea of subjectivity but we also stress that, at the moment, studies on the use of these species in soil bioengineering in the Humid Tropics simply do not exist. Consequently we do not have citations to back these statements up. At page 5154 line 20 we used the terms "very bad" to qualify the survival rate of a specie that was 14%: we think that this is self-explanatory and does not require more comments. At page 5155 line 10 we refer to the "very good" performance in the bank intervention of Erythrina fusca: this has to be intended not as a "very good" performance in absolute terms (again we do not have similar studies in order to compare these results) but more as "very good" compared to the behaviour of the same specie in the live palisade. Maybe this point was not clear in the text and so we will change the expression "very bad" with "better" in the revised manuscript.

5. Technical Corrections Cite the scientific name of vegetal species in the text in a complete manner, at the least the first time they appear. Complete botanical names should make reference to the nomenclator. For example: Erithrina fusca Lour. / Gliricidia sepium (Jacq.) Steud

Scientific and common names are used alternately in the text and figures. This makes it difficult for the reader. Sometimes, in the very same sentence (page 5153) a comparison is made between two species referring to one by the common name and the other by the scientific name ("...is the one between Madero negro and Tabebuia roseal..."). I

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suggest the authors make reference to the scientific x common synonymy at the beginning of the text and then adopt a standard, always referring to the plants by the same name, in the text, graphs, tables and figures.

We have improved the paper considering also the suggested technical corrections of Referee#2.

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