Book review
Carbon sequestration in tropical grassland ecosystems

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After former US Vice-President Al Gore was awarded an Oscar for his documentary *An Inconvenient Truth*, economist Jeffrey Sachs commented, 'Once you get Oscars for climate change, you've got to know we're on our way.' The Kyoto Protocol is now in force and according to the *Economist* (June 7, 2008, page 42), '[Certified Emission Reductions (CERs)] currently trade at around $40 a ton in Europe'. Everyone, it seems, wants a piece of the action. This book asserts that carbon 'sequestration' by improved forages in the tropics will allow farmers, principally smallholders, to 'contribute to sustainable development, poverty alleviation, and mitigation of the undesirable effects of climate change ….'

The book consists of 10 chapters, divided into the field work and analysis of the results; a socio-economic survey and its results; a consideration of alternative statistical and modelling approaches; an attempt to identify areas in the Neotropics with characteristics similar to the sites chosen for the studies; and some conclusions and policy recommendations.

On the positive side, the book presents some welcome data for carbon stocks in agro-ecosystems other than the grasslands and forests that have hitherto been the main focus in the lowlands of the Neotropics. It also continues to emphasise the importance of carbon storage in the soils under pastures, especially at depth, in marked contrast to many temperate grassland systems typified by the US Conservation Reserve Program (CRP, see below).

Unfortunately, readers familiar with the subject will find this book disappointing. Its major problem is that it fails to address the issue of true sequestration of carbon in the systems that the authors studied. The title of Chapter 3, 'C stocks and sequestration', led me to expect some insights into the processes of carbon dynamics in the systems under consideration. The authors measured total soil carbon (by dry combustion) and oxidisable soil carbon (by Walkley Black wet chemistry), and estimated carbon in the plants growing in the various systems. Unfortunately, they give us no information as to what form that carbon is in. Indeed, it is not clear whether the authors appreciate the dynamics of carbon in the systems that they measured: they use the words 'storage' and 'sequestration' interchangeably throughout the book. In one place (page 63) they incorrectly refer to carbon in fine roots as 'sequestered'. Fine roots are, of course, part of the plant biomass, and not the soil organic matter. Moreover, fine roots turn over rapidly with residence times of only weeks to months.

To put the dynamics of carbon in context, it is commonly accepted that soil carbon occurs in one of three conceptual pools (Parton *et al.* 1988): an 'active' fraction consisting of live microbes and microbial products with a 2- to 4-year turnover time; a 'slow' fraction that is more resistant to decomposition as a result of physical or chemical protection with a turnover time of 20–50 years; and a 'passive' fraction that is physically protected or chemically resistant and has a long turnover time (800–1200 years). The pools are readily estimated by size and density fractions of the soil organic matter. Only the passive fraction can be considered truly 'sequestered', although some might accept that the slow fraction also qualifies. The components of the active fraction are in dynamic flux. My colleagues and I concluded that, 'It is obvious that the extent to which the soil can be a continuing sink for C depends on the balance between the rates of the processes of C acquisition and the rate of breakdown of the resident C and the newly-acquired C (Parton *et al.* 1987). There is little known about either process, especially in the acid soils of the

savannas.' (Fisher *et al*. 1998). There is nothing in this book that addresses these issues.

The carbon stored in a system in which inputs have increased will continue to increase only until a new equilibrium is eventually reached. Likewise, systems that have reached their equilibrium levels will maintain them only if the systems continue unchanged. It is noteworthy that carbon in soils set aside in the CRP, which costs the US taxpayers over one billion dollars per year, has accumulated carbon only in the top 7.5 cm layer of the soil, rarely as deep as 20 cm. This accumulated C, widely touted as an example of C 'sequestration', will inevitably be lost when (not if) farmers return to cultivate the CRP land, for example, to provide maize for the US biofuels program.

The book's title is misleading. It does not deal with tropical grassland ecosystems, but with introduced grass pastures sown on lands cleared from tropical forests in Colombia and Costa Rica. The data on C stocks, stated to be 'the core of the book' (page 49) would scarcely be sufficient for a peer-reviewed paper. Description of the statistical methodology is limited to, 'using Dunnet's [sic, it is Dunnett] test, under ANOVA models consistent with the experimental design used.' Conventional statistics (ANOVA) are inappropriate for the analysis of data coming from complex ecological systems. In the case of the data reported here, not only are there strong indications that some of the data are unreliable (for example, Figures 7 and 8 on page 78), but in several sites the variability of the soils overwhelms the processes involved (for example, Table 14 on page 61).

The over-long chapters on the socio-economic evaluation reveal the difficulty of this sort of analysis. The Colombian peso is currently substantially revalued to about 1700 to the US$ (from the 2300 and 2500 used in the analyses, see below) and, as noted above, the price of CERs in Europe is currently $40, compared with the conservative $2.50 used in the economic analyses. A revalued peso makes things look worse, while higher-priced CERs make things look better.

The chapter on modelling, too, is over-long. It attempts to discredit process modelling of soil organic matter, the CENTURY and RothC models in particular. The authors assert that process models do not provide robust estimates of error and have little calibration or verification in the tropics. While both these assertions are true, both CENTURY and RothC have been widely tested and verified in the temperate ecosystems, for which they were designed. However, the use of 'pseudo-chronosequences' proposed by the authors is unlikely to provide viable alternatives to understand the dynamics of carbon in diverse, complex ecosystems in the Neotropics.

There are a large number of spelling and grammatical errors as well as mistakes in substance. For example, the international climate agreement is the United Nations Framework Convention on Climate Change, not the United Nations Conference on Climate Change (page 19) and the *Brachiaria* hybrid known as Mulato is accession CIAT 36061 not CIAT 4642 (page 43, albeit the number 4642 was used internally in CIAT's breeding program). The temperature of combustion is reported on page 42 as 120°C, but on page 80 is a more plausible 900°C. There are two different exchange rates used for the Colombian peso (2300 on page 115 and 2500 on page 122).

The publisher's website (www.wageningenacademic.com/Default.asp?pageid=8&docid=16&artdetail=Carbon&webgroupfilter=5&) carries a brief description of the book, taken from its back cover. The third sentence reads, 'This book presents evidence that tropical grasslands, which cover 50% of the earth's surface, are as important as forests for the sequestration of carbon.' Unfortunate statements like these erode one's confidence in the work. Tropical grasslands cover less than 9% (not 50%) of the earth's surface. In any event, the book has nothing to do with tropical grasslands; as noted above, it deals with pastures sown on lands cleared from tropical forests. Moreover, there are no data that show that a pasture can store the same amount of carbon as a mature tropical rainforest, which ranges from 130 to as much as 360 t/ha C in the standing biomass. As the senior editor himself wrote, '[Even] well managed pastures … are unlikely to replace the above-ground C lost when the forest is cleared.' (Mannetje 2007).

This slim book is overpriced. For comparison, volumes of the *Advances in Soil Science* series, with 20–40 peer-reviewed papers and running 600–700 pages, mostly retail for US$130–150.

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Book editors' response

We thank the reviewer for his positive comments, but we are afraid he is demonstrating a somewhat biased point of view. Since Dr Fisher worked as a consultant on the project whose methodology, research strategy and results are presented in the book, we consider he has a conflict of interest. Dr Fisher cannot be considered as a neutral reviewer.

The research presented in *Carbon sequestration in tropical grassland systems* addresses not only carbon sequestration effects, but prominently also the benefits of improved grassland systems for the local farmer. That economically interesting management systems have also a positive effect on soil carbon stocks is a boon for both the farmer and society in general.

Dr Fisher expected this book to address carbon dynamics in a theoretical rather than a practical way. It seems that he is not aware of the complexities of carbon dynamics. Although carbon dynamics are widely studied in much detail, and some principles concerning stability start to emerge, it would take a full-size book to discuss the ins and outs of carbon dynamics – let alone 'the form in which carbon exists' in relation to this project. Actually, some of this useful research is forthcoming as part of a Ph.D. study. Dr Fisher, who was involved in the early stages of the project on which the book reports, should have been aware that the project addressed practical and not theoretical problems. Anybody who reads the executive summary (pp. 15–19) of the book should be aware of this.

The term *sequestration* may be misunderstood. When it is mentioned on p. 63 that C is sequestered in the root biomass, this is not a fortunate formulation, but it is similar to stating that C is sequestered in (above-ground) biomass in general. With respect to soil carbon, an increase in stocks is a sequestration, because positive changes indicate that the new (future) equilibrium will be at a higher level than the present. This does not depend on the turnover time of a specific fraction. Dr Fisher is aware of this new equilibrium, but seemingly not of the fact that all fractions contribute.

As Dr Fisher states, models use conceptual pools. It is well known that such pools are little more than concepts (*e.g*. Poirier *et al*. 2005; Lützow *et al*. 2007) and that even the most detailed research using grain-size and density fractionation (*e.g.* Liao *et al*. 2006) does not allow estimates that are sufficiently accurate or that can be extrapolated beyond very local systems. Since Dr Fisher's 1998 paper, lots of relevant publications have seen the light.

When Dr Fisher states that soil carbon increases are largely restricted to the top 7.5 cm, he appears to be unaware of the strong effects of deep-rooting tropical grasses, which are known to increase or change C stocks to significant depths (0.5–1 m). The fact that C may be lost again when a land-use system is changed is completely true, but this is not the issue.

It appears that Dr Fisher does not consider a managed grassland as an ecosystem. We wonder who would agree with him.

As the book is meant for a general public, including policy makers, details of statistics have not been presented. Given the fact that two professional statisticians were involved in the project, we are confident that the methods have been appropriate.

Dr Fisher uses two figures from the book to demonstrate that data are unreliable. Given the fact that the authors have presented these figures and have used the information to improve the conclusions, it is difficult to agree with him. We challenge him to find a publication where data variability of trial sites has been discussed more thoroughly. In case soil variability was too large to draw solid conclusions, this has been stated. Actually, some trials were not used because of this problem.

As an essential part of the project was to evaluate improved grassland systems in terms of farmers' incomes, the socio-economic evaluation (method and results cover less than 40 pages) is hardly exaggerated. The economists were, of course, aware of the problem of varying exchange rates and have taken this into account. We wonder whether Dr Fisher could suggest a better method.

Yes, we do discredit process modelling of soil organic matter in the context of this book. SOM models require a large number of parameters, very few of which can be measured (see references cited above). Applying models in such circumstances suggests reliable outcomes that cannot be verified. Suggesting that models such as CENTURY could be adapted easily to tropical circumstances (in four strongly different ecological zones) indicates Dr Fisher's unfamiliarity with the models. What exactly is the objection against pseudo-chronosequences when better information is not available? Using such chronosequences is a common practice (*e.g.* Liao *et al*. 2006).

Indeed, the book contains a number of unfortunate errors. The 'dry combustion' at 120ºC as mentioned on p. 42 is in fact a wet combustion, and this was also the method used for total C as reported on p. 80. The reference to dry combustion is an error.

For the surface area of tropical grasslands we have cited the appropriate sources, while Dr Fisher mentions different numbers without stating their source.

We thank Dr Fisher for pinpointing some real mistakes, but we think that, as a whole, he has misjudged both the purpose of the project and its results.

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Journal editor's comment:

I chose Dr Fisher to review this book because of his international standing in the area of carbon sequestration under pastures and forests and was aware of his involvement with the project when I approached him. I considered that his professional integrity would allow him to provide a balanced and objective assessment of the work. Readers of his review and the response from the book's editors can peruse the book and decide for themselves how objective both Dr Fisher and the editors have been.