Point-to-Point Reply to Referee #4 (C817, received and published: 24 January 2013**)**

We thank all reviewers for their thoughtful and important comments. In the following the citation of our modifications in the manuscript are related to the new, revised version.

This paper addresses the technical and economic feasibility of extensive Jatropha plantations in desert-like coastal areas, to be supplied with irrigation water from seawater desalination plants, with respect to extracting carbon from the atmosphere to counter anthropogenic emissions. It also explores the implications for local climate. The paper addresses an exceedingly relevant and important topic. It is very well written and a stimulating read. However, I have several quite strong reservations about publishing it in ESD as is, as follows:

1. The paper presents what in my evaluation is an overly optimistic scenario throughout.

It does not seriously address potentially more negative effects. While Jatropha is undoubtedly a very interesting plant full of potential for biomass production under harsh conditions, the literature on Jatropha has been plagued by uncritically optimistic assessments that usually combine optimistic expectations of yield with optimistic downplaying of environmental factors required – the claim that high yields can be achieved with little water, few nutrients and little management is not scientifically verified and probably not readily realised (even in ecology, little is for free). This paper follows to some extent in this vein of combining numbers and extrapolations in an optimistic manner:

We would say that the writing style used is direct and concise rather than over optimistic. However, we have done our best to address uncertainties more clearly now. Uncertainties in science always exist but this does not mean that our estimates are unrealistic and not significant. We are convinced that our results are accurate enough to motivate a scientific publication about our new interdisciplinary analysis. In any case, we have toned the writing down in places where it might be considered to approach hyperbole. Please see our reply with a couple of examples to Reviewer 2 who also criticized us for the use of over optimistic language.

a. Jatropha growing in irrigated desert for 3 years were extrapolated to growth after 20 years using data from Brazil – a very different environment; Jatropha productivity strongly varies with climate and soils as is the case with all plants and has therefore to be treated in a spatially heterogeneous manner rather than, as here, applying numbers from one site to another, then inferring results for global production from that by multiplying with area. Yields of 4.9–6.9 t/ha/yr as assumed here neglect a real possibility that yields are lower (some studies have found yields to be as low. Interestingly, the authors call their estimate "conservative" and compare it to desert tree productivity rather than other Jatropha experiments.

It is correct that the data were compiled from different regions. However, we disagree that it is not possible to derive an estimate of plant growth. We are convinced that this is scientifically acceptable, if these difficulties are mentioned and uncertainties are addressed. Similar uncertainties exists also for all other geoengineering options, which should not distract us from estimating their potential using the best data we can get. ***Research on biogeoengineering in the era of climate change is just too important for us to refrain from studying potential set ups and evaluating their impact.*** **It is particularly new and essential to set large-scale plantations in context with their effect on local weather and climate.**

Here, we needed figures for yield that were reasonable so that we could perform our estimations. Indeed, it proved extraordinarily difficult to find relevant information. Most data were too short term to be of any use and usually gave dimensions of trees rather than actual weights. Inevitably, we had to use data from different locations and climate zones but concentrated on data from our own experiments. If more relevant and reliable data become available, the subsequent calculations are sufficiently transparent for new results to be obtained and new conclusions to be drawn. We also make the point that our calculations need not necessarily be applied to ***Jatropha*****, it is simply that this plant is the one that we are most familiar with. The climate and economic models we devised could equally well be applied using data from other tree/plant species. These points are all made in various parts of the paper and reiterated and emphasised in the conclusions. We are convinced that our results and our uncertainty analyses are important and concise enough to deserve a publication of carbon farming as an interesting biogeoengineering option.**

Interestingly, a paper by Hellings et al. (2012), which has been published since our paper was originally written, contains data that support our original estimates of biomass production. We have now incorporated this information (pages 12-13) and discussed the problems and shortcomings of our estimates. Please see also our reply to Reviewer #2.

b. Salination of irrigated soils is a very common problem not addressed but of long-term impact on yields. (The authors also seem to have an unproblematic view of using urban waste water based on this use in Luxor, their test site; experience from previous decades in Europe shows that the negative effects of land poisoning through assorted toxins in the water can lead to long-lasting and serious environmental problems in areas originally billed, as in this paper, as improving through the use of these urban wastes.)

This problem is mentioned in the paper (pages 10 and 26-27). To reiterate, the proportion of hot, dry coastal areas that have saline soils is small and those that could be irrigated by urban waste even smaller. If used at all we envisage that these areas would be applied only once to produce Jatropha or other appropriate trees over a period of 20 years. Irrigation would then be ceased and the above ground vegetation removed or allowed to dry out in situ. There is also potential for developing intercropping systems. The carbon sequestration in these areas would therefore only be temporary (decades up to a couple of centuries: Bird (1999)) but provide a breathing space before other options concerning reduction of greenhouse gases can be brought into action.

c. The economic evaluation seems to me to be superficial, drawing on a single set of numbers that are not further explained or justified, except with reference to the Luxor test case, which is only one particular case under particular circumstances and not fully developed. Estimated costs of 1060 euro/ha/yr of cultivation seem optimistic – the value of land targeted for Jatropha cultivation will not remain at 10 euro/ha/yr once found suitable (why would land remain cheap once it is part of a very lucrative energy market set by high fossil fuel prices?), "erecting and running the irrigation system " with just 100 euro/ha/yr also is low – maintenance problems have haunted irrigation systems in such countries for a long time and tend to be more expensive, and fertilisation is a major cost factor, as is manual labor required in cultivating the plantations as they mature (just leaving them to grow is not optimal). Large-scale monoculture application is not the domain of the small farmer, as is possible with test fields, but requires industrial-style production. The numbers used here seem to come from the Luxor experiment, but the authors show no effort to investigate whether they can be applied to arrive at such general statements for the global case, as are made in the conclusions; there is no effort, for example, to estimate the cost of collecting litter to meet desalination energy demand. There are more in-depth and careful analyses of the management and labor demand of Jatropha plantations in the literature than are behind this analysis here. Again, an extrapolation of one case, Luxor, to the world, in a spatially homogeneous manner (a spatially heterogeneous model would be welcome).

We disagree with the Reviewer that our analyses are not significant. We state that these are very sophisticated. Again, uncertainties in economic analyses, which always exist, should not distract from the pioneering interdisciplinary analysis presented in this manuscript. The economic analysis that appears in the paper is a précis of a much more detailed analysis of experts in this field. In order to keep things precise and readable we did not think it would serve any useful purpose to put any more details into the paper itself. This analysis includes all capital, running and maintenance costs, depreciation costs over 20 years for equipment or replacement costs based on appropriate life expectancies (power plants last longer than irrigation systems), interest on loans etc. Basic data were acquired from many sources which are detailed in a bibliography.

d. Nutrient supply and pest control are not discussed at any depth – the authors merely mention that under operational conditions, nutrients would have to be added to the irrigation water; it is impossible to grow and remove large amounts of biomass from plantations growing on soils that are marginal for a reason (not just water) and not to expect extensive chemical management, a crucial aspect also financially (the phosphorous market could well be tight, for example). Jathropha grow under poor conditions, but not very well, and respond strongly to fertilisation, which hence is a very important factor for viable production levels. As for pests: Jatropha monocultures are as prone to pests as other monocultures and require management for this, the authors merely acknowledge this but do not link it to their economic analysis.

Nutrients (fertilisers) and pesticides are allowed for in the detailed economic analysis, but based on the prices at time of writing. Of course prices will vary but hardly in a predictable fashion. Again, we are convinced that this uncertainty should not prevent us from publishing these analyses, which are fundamental for assessing carbon farming as a biogeoengineering option.

e. The precipitation change found in the climate experiments are not set in relation to Jatrophas water demand.

This is a very good point also mentioned by Reviewer 2. Please see our response to Reviewer 2 and the text we added on pp. 24 (last par.) - 25, 1st par.

2. Much of the argument presented is based on drawing values required for the analysis from one or the other publication (e.g. extrapolation of growth in Luxor from 3 years to 20 years, irrigation demand, costs of desalination etc.) but the reader is not given any sense of how these numbers were produced, whether they are reliable, how large their uncertainty is, and whether there are other numbers in the literature. The conclusions reached are strongly based on these assumptions. My feeling is that optimistic economic numbers are combined with an optimistic extrapolation of a 3-year experiment to arrive at an optimistic yield while downplaying the problems and side effects. Such a yield may be possible, but this has not been convincingly demonstrated due to the author's reliance on particular values from particular studies which they then apply to the large scale uncritically.

That conclusions are based on assumptions is just a common scientific approach. We do not think that our economic analysis is overly optimistic as pointed out above. This economic analysis alone is a new contribution to analysis of biogeoengineering. It can be adapted immediately, if new results are available. Shortcomings are acknowledged in the paper as is our inability to overcome them with the data currently available. Our plea in the conclusions section is not that our proposed scheme be globally and immediately implemented but that a sufficiently large pilot study should be undertaken to test our assumptions, to provide more and relevant data and to establish the extent of the variations to be expected.

3. The conclusions drawn from the study are, in my view, not as robust as stated. For example, stating that a carbon price of 42-63 euro/tCO2 "compares favourably" with a CCS price of 54 euro/tCO2 is wrong: it does not compare "favourably" but is merely similar. With respect to mitigation effects, carbon losses caused by the whole production system (e.g. in fertiliser production) and its side-effects and the inefficiencies in conversion of biomass were not taken into account: the authors assume a perfect extraction of the biomass carbon and apply that number to mitigation potential, which is not realistic (I would think that 2/3 efficiency can at most be achieved if one takes all into account – which actually is pretty good). The role of litter and fruits enriching the soil vs. being used to fuel desalination plants remains uncertain. In summary, I think the authors provide a comprehensive discussion, but build their argument in a manner that is not completely free of handwaving in putting together some experiments (without knowing how representative they are, and with 3-year plants representing 20-year plantations through allometric extrapolation from a different

location with different conditions), some literature numbers (on irrigation, costs, available land, carbon effects) and some models (also using a number of set parameters) to extrapolate a global picture of the issue. This is perfectly ok when presented as an exploration and in a cautious manner, reflecting more of the literature; but the authors present it as a solid analysis of potential, costs and effects, and draw quite fundamental global-scale conclusions – the analysis is, despite all of its merits, in my opinion not solid enough for that (yet). It is an exploration, a study adding to the discussion, not more.

Again, we would say that our conclusions are not overly robust, simply concisely expressed. But we concede that the phrase "compares favourably" is a slight overstatement, and have amended the text accordingly. We have also mentioned most of the potential negative aspects such as the possibility of infestation by pests but in most cases, 'mentioning' is all we can do because it is not possible to include accurate quantification of these problems in our models.

4. The figures are rather general and mostly not very illuminating for a scientific paper. Fig. 1 is ok but very general and unsurprising, Figure 2 is illustrative, Figure 3 merely depicts the technology of seawater desalination, which is not the topic of this paper, figure 4 merely reflects prescribed parameter changes in the model. Figure 5 and 6 actually contain information. Finally: I am not certain why this paper was submitted to "Earth System Dynamcs" to begin with.

We disagree that Fig.1 is general because it includes in a convincing way the importance of interdisciplinary work as well as the consideration of local climate modification and the relevant processes. We consider this figure as a very elegant overview of our work. As desalination of sea water is essential, we consider a corresponding figure as appropriate. Figure 4 not only shows the changes of surface parameters but also their coverage and locations. Of course, the readers want to know where the large-scale plantations were simulated. Concerning Figs. 5-6, we appreciate very much that the reviewer acknowledges their information content.

This manuscript has been submitted to Earth System Dynamics because our interdisciplinary approach contains dynamic analyses of different Earth system components over various temporal scales such as vegetation dynamics and land-surface-

atmosphere dynamics as well as land-surface-atmosphere feedback. We consider Earth System Dynamics as a very appropriate journal for publishing this kind of work.

The paper makes no argument on dynamics. It addresses the Earth system only by inference. This is a paper on bioenergy technology. As such, it is part of potential societal dynamics shaping the future of the planet. But that is not the main aspect being discussed. This paper would have a perfect home in journals such as “Global Change Biology Bioenergy”, “Applied Energy” or others where this specific community publishes.

We are presenting various dynamic analyses over different time scales (see above). We do not think that the proposed journals are better suited because we are simulating a coupled land-surface-atmosphere subsystem including economic aspects. Furthermore, the weather and climate simulations fit very well in Earth System Dynamics.

In conclusion: I do not recommend publication in ESD due to this combination of partial topical misfit to ESD, broad discussion drawing from many insufficiently described sources without giving a sense of critical judgement, and a lack of looking for negatives while overemphasizing solidity of the conclusions.

We disagree with the reviewer here for the many above mentioned reasons and based on our arguments presented above.

To be clear: I like the work, I find it highly stimulating and relevant, and really important, but it needs to be exposed to the community of bioenergy technology experts and not be superficially tied to ESD. The paper is well written and better situated elsewhere – at least in my personal judgement. If the editors of ESD decide it should be published with revisions, of their numbers taken from the literature and their potential uncertainty, take a more critical view of life-cycle carbon balances and economic costs, and then draw more balanced and careful conclusions, avoiding sweeping statements. Then their contribution could indeed be an interesting element in a debate about the feasibility of bioenergy plantations in desert regions.

Thank you for all your comments. We are confident that we have made sufficient changes and put up a robust enough defence to make you change your mind.