Point-to-Point Reply to Referee #2 (RC C711, received and published: 29 November 2012**)**

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.

I appreciate the opportunity to review Carbon farming in hot, dry coastal areas; an option for climate change mitigation. The manuscript addresses a GeoEngineering type research topic that certainly deserves more research and careful consideration in the future. Thankfully, the authors were also open about the necessary assumptions required to obtain these results (e.g. species selection and biomass accumulation rates, site selection, modifications to the regional model). Overall, in this manuscript I encountered a well-written and intriguing research study that, with a few modifications, clarifications, and additions, definitely deserves publication.

1 Topics that need to be addressed

• Long-term storage - It appears that you are presenting Carbon Farming as a means to avoid the long-term storage complexities related to carbon capture and storage (CCS) (p.1223 line 10-13). Yet although it was not discussed here, Carbon Farming would also require a long-term storage mechanism.

We agree with the statement of the reviewer. Obviously, we did not point out clearly enough that our data and simulations are focusing on 1-3 decades. Beyond that it is difficult to estimate the fate of large-scale plantations. However, we also should not undersell the exciting potential of Carbon Farming as a climate mitigation technique, particularly in comparison to CCS. Research and understanding of the long-term impact of these plantations is important and necessary.

We are now stating in the text that our simulations are reasonable for a few decades and further analyses are required to study the long-term potential. We added the following text passages in the manuscript:

Abstract:

"Our investigations indicate that there is sufficient unused and marginal land for the widespread cultivation of Jatropha curcas to reduce significantly the current upward trend in atmospheric CO2 levels to *have a significant impact on atmospheric CO2 levels at least for several decades."*

Last par.: "In such areas, plant growth and CO_2 storage could continue until permanent woodland or forest had been established. In other areas, salination of the soil may limit plant growth to 2-3 decades whereupon irrigation could be ceased and the captured carbon stored as woody biomass."

Introduction, p.6, 3rd par: "With this approach and the application of available data, we are aiming at an analysis of the performance of the plantation over a time period of 1-3 decades. This may also provide an appropriate basis for the assessment of the fate of large-scale plantations for up to a century in future research."

Section 4, p. 26, starting from 3rd par. – p. 27: "Therefore, carbon storage as woody organic matter may not a permanent solution but at least it would provide a breathing space during which longer term methods of storage and reduction of CO_2 emissions could be found.

The ultimate fate of the plantations proposed in this paper can only be studied by further extensive experiments. It is probable that the trees on many of them would continue to grow, albeit at an ever decreasing rate, until it was no longer worthwhile irrigating them. At this stage, they could either be harvested or irrigation could be ceased. Harvested material could be stored or used as fuel. Using the material as fuel will, of course, release CO_2 back into the atmosphere so, even if the cleared areas are replanted, the potential for net removal of atmospheric CO_2 will be limited to that achievable in the first 20 years of growth.

The option of storing the material could be achieved most easily by simply ceasing irrigation since the sites proposed for the plantations are hot dry deserts. This would be the only option in sites where the salinity of the soil had accumulated enough after 20 years to inhibit optimal growth. Incidentally, this will almost certainly be the case in our test site in Luxor where the irrigation water derived from sewage is relatively saline and may also contain other undesirable mineral compounds. Once again the amount of carbon sequestered is limited to that accumulated during 20 years.

The most optimistic scenario would be one in which the plantations caused sufficient increase in precipitation, as predicted by our climate model, to maintain vegetative growth and establish permanent woodland or forest without artificial irrigation. On a geological timescale this might even lead to the production of coal or petroleum deposits."

The Jatropha curcas biomass accumulation rate appears to have been derived from some 32 month-old samples and some 12-year old samples, then extrapolated to a 20 year-old representative. Without being shown these curves, the connection between future rates of CO2 bio-accumulation and the necessity and rate storage remains ill-addressed — they should not be. By including the curve or fit-equation, not only is the study more reproducible, but it would further highlight the rates of growth and fruit/litter production that could be expected.

The reviewer is raising a very important point here. Since we originally wrote the paper, there has been a significant publication in this area by Hellings et al. (2012). We added the following text on p. 12:

"Recently, Hellings et al. (2012) published allometric equations based on their destructive analysis of Jatropha trees in Northern Tanzania. These authors experienced similar problems to us in that most of their sample trees were 2.5-4.0 years old with just one 7 and one 25 year specimen. However, their results are similar in some respects to ours. For example, their 25 year old tree had a DGL of 433 mm, whereas our predicted value for a 20 year old tree was 412 mm. When we applied their allometric equations to a 412 mm diameter tree we obtained values of 165 and 146 kg for total and above ground woody dry biomass respectively. The relevant equations were:

$TWD = 0.0042\ DGL^{2.8361}$ and

$AGWD = 0.0019\ DGL^{3.0248}$

where TWD is Total Woody Dry matter and AGWD = Above Ground Woody Dry matter.

There was good agreement for the prediction of AGWD but the below ground woody biomass (TWD-AGWD) was substantially lower using these equations (19 vs 55 kg or 13% vs 28% of TWD). The value of 13% was lower than the value calculated directly from the data in Hellings et al. (2012) (28.1% ± 6.5 which includes the 25 year old tree that gave a value of 30.6%), or the data of Firdaus et al. (2010) quoted by Hellings et al. (2012)), which gave values of 24-51%, or that of the data in Table 2 of this paper (average 24.3%).

In view of the above we have used our original estimates in subsequent calculations in this paper viz. a value of 140 kg for the AGWD of a 20 year old tree, which is slightly lower than the value derived from the data of Hellings et al. (2012), and a value of 195 kg for the TWD

of a 20 year old tree which seems consistent with the ratios of AGWD to TWD reported here and elsewhere. We stress that these values are tentative and based on sparse data but subsequent calculations are sufficiently transparent that they can easily be repeated if or when more robust data become available."

• Site selection - I like the fact that you are proposing the use of only degraded land for Carbon Farming. Your extrapolation of small-scale experiments to the larger 109 hectare scale suggests that the previous utilization of these land areas (such as failed agricultural lands that were overly irrigated?) does not inhibit the cultivation of Jatropha curcas at these locations. Is this truly the case?

The Luxor site where we got most of our data is on formerly degraded agricultural land some 10 km from the river Nile. Jatropha grows well there. The sites in Mexico and Oman where we obtained the inputs for the climate model are both on previously uncultivated sandy desert soils. At present nothing grows at these sites owing to the very low rainfall (see Google earth using the coordinates in figs 5 or 6). The only way to find out how Jatropha or any other plant would grow under the irrigation levels proposed in the paper would be to go there and try.

• Salt build-up - Could you please address how long these Carbon Farming regions could be cultivated/irrigated before the salt concentrations of the fertilizers (referred to as the fertilizer's salt index) added to the reverse-osmosis desalinated water have reached soil concentrations that prevent further cultivation in this area?

We agree that this is an important issue. Please see our answer to a similar question raised by Reviewer 1:

We agree that this topic should be addressed in more detail. We are focusing on arid coastal zones, which are deserts since a long time due to lack of precipitation or natural water sources. Therefore, in most of these regions, irrigation was absent and these landscapes became arid by natural processes in the land-surface-atmosphere system.

The main reasons for the degradation of the land are lack of precipitation or natural water sources over a long amount of time. The very low precipitation amounts are due to large-scale suppression of convection, e.g., by Hadley cell subsidence, or local suppression of convection either by upwelling of cold air by ocean currents or by atmospheric divergence

caused by increasing land friction. Therefore, land degradation by high salinity of the soil due to poor agricultural practices such as inadequate irrigation is not the main reasons. We added a corresponding statement on p.7, section 2, 1st par.:" The dry coastal areas have been degraded over a long period of time due to lack of precipitation or natural water sources. The very low precipitation amounts are caused by large-scale suppression of convection, e.g., by Hadley cell subsidence, or local suppression of convection either by upwelling of cold air by ocean currents or by atmospheric divergence caused by increasing land friction. Therefore, in these "natural deserts" land degradation caused by poor agricultural practises such as inadequate irrigation leading to high salinity of the soil is hardly an issue. However, strategies for minimizing contamination of the soil must certainly be taken into account and are discussed in sections 3.2 and 4."

Furthermore, we extended the sentence on p.4, 2nd par.: "However, carbon farming must not compete with food production so that afforestation measures should concentrate on already degraded land areas such as desert *regions that are not likely used for conventional farming and which are not salinated by previous unsustainable agricultural practises."*

In cases where salinity or the accumulation of soluble contaminants is likely to be a problem (e.g., our test plantation in Luxor, Egypt that uses sewage water as the sole source of irrigation) we recommend that only one crop of carbon fixing trees is grown and then the land is left fallow. Even this 'one shot' approach would still fix large amounts of carbon for a period of decades to centuries. The literature is unclear in the sensitivity of *Jatropha* on salination. We added three references on this. Please see p. 10, 2nd par.: "The extensive use of sewage water would also not be recommended in very dry areas because of salination. In the literature, mixed results are found with respect to the sensitivity of Jatropha to salinity. Whereas Tal et. (1979) found a very low sensitity of plant growth on salinity and Silva et al. (2010) found adaptive physiological processes reducing salination-induced stresses; in contrast, Rajaona et al. (2012) observed that salt stress influenced Jatropha's canopy development and the CO2 assimilation rate. Without extensive flushing, it is unlikely that more than one crop of Jatropha curcas could be grown before the build-up of salt in the soil prevented further growth, in which case carbon sequestration could be done only once. However, long-term experience is lacking and there is a need for more experiments and analyses on large-scale plantations."

Furthermore, we added on p. 26-27 three new pars., which are discussing the limits and optimization of irrigation.

• Relating to CO2 emissions - Comparing a development scale of $0.73 \cdot 10^9$ to offset the 2 ppm increase is good in context, but also assumes indefinite storage of the plant (and all fruit and litter? — this is unclear in the text). I appreciate putting your simulations in context but there appear to be some shortcomings in solidly making this conclusion.

We agree. We pointed out this potential but now we added the required effort in research and implementation. Furthermore, we discussed processes limiting long-term storage potential and measures for extending long-term storage of carbon sequestration. Please also see our answer to the first topic raised by this reviewer.

Furthermore, we added on p.14, 3rd par., the following statement: "The predicted reduction levels in CO2 can be achieved as long as the plants are growing at the expected rate for 1-3 decades. Therefore, carbon farming has the potential to influence the atmospheric CO2 level at least over this time period. The fate of large-scale plantations beyond this time period needs more research and experiments. Some options are discussed in section 4."

• Burn or not burn - Several parts in the text make it unclear if the intermediate results assume the fruit and leaf litter is being burned as the desalination power source or if this material is being accumulated and stored indefinitely. This should be clarified.

We agree that this information needs to be added. We extended the manuscript as follows on p. 15, bottom of 4th par.: "According to Gebel and Yüce (2008), 5 metric tons of dry biomass with a heat of combustion of 18.5 MJ kg-1 becomes available per hectare per year in the form of nuts, leaves, and trimmings. This material from a 10.000 ha plantation could therefore be burnt to produce a continuous heat output of 30.000 kW. This material has not been included in the estimation of carbon sequestration and would provide enough energy to produce steam either as the first stage in a thermal desalination plant or to drive a turbine to generate electricity for a RO plant."

• Fig. 5 - This plot leaves me wondering why the simulated Oman plantation only influences the PBLH within the plantation region while the simulation for the Sonora plantation extends about half the plantation length-scale further. Using a similar scale between these 2 plots may partially clarify this confusion, yet the spatial and quantity differences in precipitation (Fig. 6 - which also uses different scales) seem largely unrelated in comparison to the simple schematic of Figure 1.

The reviewer is raising very interesting points here. It is correct that many complex and non-linear processes are driving the diurnal cycle of the ABL and its 3D structure. However, it is too difficult to indicate these effects in Fig.1.

The scale between the upper and the bottom panels in Fig.5 is very similar - 0.5° corresponds to about 50 km. However, the modification of the ABL depends on the local upstream conditions (wind speed and wind shear), the land surface energy balance closure (EBC, which is quite similar over both plantations), the local vertical stability of the ABL, the strength of the capping inversion, and the subsidence in the free troposphere. The study of land-surface-atmosphere feedback is worth another publication, which is currently in preparation, but beyond the scope of this study.

These processes results in different ABL depths over Oman and Sonora, a different structure of the onset of the thermal internal ABL over the plantages, and a different wake structure downstream of the plantages.

We added a corresponding explanation on p.23, 2nd par: "The modification of the ABL depends on the local upstream conditions (wind speed and wind shear), the land surface energy balance closure (EBC, which was quite similar over both plantations), the local vertical stability of the ABL, the strength of the capping inversion, and the subsidence in the free troposphere. These effects resulted in a growth of a thermal internal ABL over the plantation on the upwind side and wake effects downstream of the plantations. Interestingly, in the Sonora and Oman an increase and a decrease of the ABL were found downstream of the plantations, respectively. These complex and non-linear effects are currently the subject of further studies. The increase of the ABL depth due to the higher sensible heat flux over the plantations caused a strong diurnal cycle. In the Sonora, at local noon, the ABL increased from 1800 m to 2580 m between CONTROL and IMPACT and, over Oman, the corresponding increases were 2000 m to 2750 m, respectively. The mean ABL increased from 750 m to 976 m in Oman and from 685 m to 895 m in the Sonora. These increases mainly occurred during daytime."

2 Minor requests for clarification

• Table 1 - I interpret the table's original intent (p.1226) was to illustrate the land area potentially available for Carbon Farming. Instead, I find a lot of other information that isn't directly applicable and is rather confusing. For example, instead of seeing the surface area of Saudi Arabia, I would prefer to know the coastal spatial extent of potential regions where Carbon Farming could be implemented.

My recommendation would be to either clarify why Table 1 is included, modify it, or remove it entirely.

On balance we agree with the reviewer that Table 1 does not add much useful information especially since there are many countries with large hot, dry coastal areas that are not included. We have therefore removed the Table and renumbered the others.

• "Positive-negative" types of phrasing - I would prefer that you closely read the manuscript to identify phrasing such as, "These effects can reduce the amount of water for irrigation and improve the local climate" p.1227 and "...would have fewer potential negative impacts on the environment" p.1242. Assuming I was deploying concentrated solar power in this area or refining a method for decentralized carbon capture and storage, I may have a contrasting opinion to your own as to what positive or negative suggests. The scientific validity of the study is already strong enough without adding these adjectives.

We apologize for a bit of 'tunnel vision' on our part here. We have used more neutral terms where appropriate throughout the manuscript. See, e.g., p. 8, 3rd par., where the sentence "These effects can reduce … improve the local climate" has been modified.

• Summer - Instances like "Overall, summer precipitation increased," and "...deep convection during summer time" occur throughout the manuscript, yet it is unclear if summer indicates June-July-August or is another time period.

The increase of precipitation occurred mainly during summer time. A minor increase was found in spring but this was hardly significant (a few mm averaged over the plantation and the season). In both regions, summer is defined as June - July - August and this has been made clear in the text (see p. 23, 3rd par.)

• PBLH increase - Noting a mean increase of 250m over the plantations would be more informative if you could additionally specify the percent change this represents.

Of course, these data can be provided (see comments above). In the Sonora, during local noon, the ABL increased from 1800 m to 2580 m between CONTROL and IMPACT and over Oman, the increase was from 2000 m to 2750 m, respectively. The mean ABL increased from 750 m to 976 m in Oman and from 685 m to 895 m in the Sonora, respectively, which mainly occurred during daytime. It is important to mention that this change in ABL depth has a strong diurnal cycle (now mentioned in the manuscript) and is maximal during local noon. From these absolute values, the percentage can be derived.

• Self-stability - Given that you have WRF simulations of the 2 regions, you could quantify the soil moisture increase related to the changes in precipitation, in that way giving insight as to when self-stability might be reached. My assumption is that the diurnal timing of the precipitation has a large influence here (e.g. afternoon precipitation on a 40C sand surface vs. evening precipitation on a 25C sand surface) but the similarities and differences between these 2 regions would be quite informative and the answer is already in your model output file.

This is another interesting point. We get an increase of precipitation due to modified land-surface-atmosphere exchange over the plantations, which amounts to 11 mm in Oman and 30 mm in the Sonora in summer, respectively. As the reviewer points out correctly, this is not the amount, which is ending up in the root zone for irrigation, but it can contribute to a reduction of irrigation amounts. However, as this requires analyses of single precipitation events and their impact on soil moisture, which is beyond the scope of this work and currently the subject of another publication in preparation, we added the following text on pp. 24-25:

"In any case, the precipitation increase of 11 mm and 30 mm in Oman and the Sonora, respectively, would spare some of the water needed for irrigation by the plantations in the summer season, which was estimated to be 25 mm. It is important to quantify this in connection with the "self-stability" of the plantations. However, it should be noted that the precipitation increase is not the amount that ends up in the root zone for irrigation. This value depends on the runoff, the interception, the evapotranspiration, and the infiltration into the soil. The precipitation increase exhibits a diurnal cycle and, as it is driven by the increase of sensible heat flux, the accumulation of water in the soil depends on the diurnal cycle as well. Therefore, the increase in soil moisture and thus the reduction

in water demand depends in a non-linear manner on the temporal and spatial evolution of the single, induced precipitation events. These analyses are beyond the scope of this work and subject of another publication, which is in preparation."

• Fig. 2 - The information density of including these 5 pictures in this configuration seems quite limiting. If you could instead eliminate one, a 4 panel view of the photographs would convey much more information to the reader.

We would like to keep it. This series shows nicely the development of plant growth.

• Precipitation effects - You quantify the 11mm and 30mm precipitation increases in Oman and the Sonora for the summer? Given that you ran the simulations for all of 2007, are the only changes to the precipitation occurring in the summer (see also 'summer' confusion above) or are other changes to the precipitation also occurring but to a lesser quantity?

Yes, the onset and increase of precipitation occurred only during summer. During the other seasons, precipitation changes were hardly significant in the model output. This is confirmed in the text.