Earth Syst. Dynam. Discuss., 3, C812–C816, 2013
www.earth-syst-dynam-discuss.net/3/C812/2013/
© Author(s) 2013. This work is distributed under
the Creative Commons Attribute 3.0 License.

Earth System Dynamics Discussions

Interactive comment on "Carbon farming in hot, dry coastal areas: an option for climate change mitigation" *by* K. Becker et al.

Anonymous Referee #3

Received and published: 24 January 2013

This paper addresses the technical and economic feasability 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 down-playing 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: 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 as 2-3 t/ha/yr, with 4-5 t/ha/yr already being a maximum – see various summaries e.g. by van Eijck et al.) Interestingly, the authors call their estimate "conservative" and compare it to desert tree productivity rather than other Jatropha experiments. 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.) 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 luctrative 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 author 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). 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 phosophorous 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. e. The precipitation change found in the climate experiments are not set in relation to Jatrophas water demand.

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 downplaing the problems and side effects. Such a yield may be possible, but this has not been convincingly demonstrated do to the author's reliance on particular values from particular studies which they then apply to the large scale uncritically.

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, depite all of its merits, in my opinion not solid enough for that (yet). It is an exporation, a study adding to the discussion, not more.

ESDD

3, C812–C816, 2013

CC BY

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. The paper makes no argument on dynamics. It addresses the Earth system only be 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.

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. 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, I suggest the authors tighten the discussion, present it as an exploration of the issue, discuss the basis 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 feasability of bioenergy plantations in desert regions.

Interactive comment on Earth Syst. Dynam. Discuss., 3, 1221, 2012.