

## ***Interactive comment on “Quantifying the “Energy-Return-on-Investment” of desert greening in the Sahara/Sahel using a Global Climate Model” by S. P. K. Bowring et al.***

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### **Response to Reviewer 1**

S.P.K. Bowring, L.M. Miller, L. Ganzeveld, and A. Kleidon

We thank anonymous Reviewer 1 for their overall support of our submitted manuscript, and for the help that their detailed, and thoughtful comments have provided us. Below is our response for this review.

The main motivation of this manuscript was to conceptually show how the Energy-Return-on-Investment (EROI) metric can be applied to a large-scale human intervention in the Earth System. Here we have applied it to the particular scenario of desert greening, because previous research on the stability of a green Sahara suggests that it would not be maintained in a steady-state by *natural* means (cf. Claussen et al. (1999)). Therefore, here we ask if the green state could be sustained 'unnaturally' using technological means. As this would involve a continual energy input, we use Energy-Return-on-Investment (EROI) to benchmark the energetic sustainability of this process. This question is of interest because although several researchers have investigated desert greening using regional/global modeling and site-scale experimental approaches, no attempt has been made to determine their energetic 'rationality', and thus whether such proposals are feasible from this energetic perspective. More broadly, the study also:

1. Shows how large-scale global feedback mechanisms can be incorporated into the EROI budget, which has not to our knowledge been previously explored;

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2. Demonstrates that this is both possible and useful in the context of human interventions in said systems and mechanisms;
3. Applies it to a human intervention that has been actively proposed.

The purpose of re-iterating this motivation is the Reviewer's opinion that:

*"[T]he authors may want to reconsider the argumentative framework of their study to convince not only me but also other readers, why it is worth to investigate their question. My major concern is that I cannot clearly see what motivates the EROI quantification..."*

Thus the reviewer has raised a very good series of points regarding the statement and clarification of the paper's objectives, with which we agree. To address this, we have made the appropriate clarifying modifications to the Title, Abstract, Introduction and Discussion sections of the paper. Also based on your comments, we re-evaluated our choice of an appropriate title to better capture this motivation and suggest altering it to

Quantifying the **Applying the Concept of 'Energy-Return-On-Investment'** of to Desert Greening of the Sahara/Sahel Using a Global Climate Model".

Following directly from our under-specification of this study's motivation, the Reviewer makes some compelling points relating to the scope, relevance and rigor of the study, outlined by:

1. *"EROI for solar ..probably larger than for biomass burning.. why.. burn any vegetation instead of growing food?"*
2. *"Energetic sustainability doesn't imply that it would be financially feasible or politically accepted.. [t]he required level of political cooperation is unprecedented".*
3. *"[T]he energy for the construction and maintenance of the whole infrastructure is negelected."*

These comments point to the understandable misconception that we set out to fully and exhaustively quantify EROI of desert greening, and in doing so, implies that our paper is a *bona fide* feasibility study. All the points above are

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interlinked because they imply a level of decision-making by the researcher –regarding infrastructure technology, use of biomass and human cooperation –that is intrinsically political and hugely varied in potential scope and [value] range. This we consider an unnecessary digression from the aim of this paper, which we leave to those assessing a specific desert greening proposal. In response to the above points then:

First, there is no direct conversion of biomass to electricity presumed here, since the paper does not specify what happens to biomass embodied energy once it is grown. We agree that for a complete EROI-feasibility study, any end-use of biomass and its possible subsequent conversion must be accounted for. However, NPP has many potential (not necessarily combustion-focused) applications, whose tradeoffs we do not consider.

Second, there is no doubt that desert greening is socio-politically controversial in and of itself. This paper does not claim to transcend this, but asks whether greening could potentially be sustainable in the energetic sense based only on the derived EROI.

Third, we did not include energetic costs of specific production processes involved, such as fertilizer use and the [overhead and maintenance energy costs for] specific energy-input infrastructure (solar/natural gas?) used in desalination. EROI-estimation of these quantities relates to a second-order question –namely, the EROI of a specific scenario. There are a wide range of variables entailed by desert greening which require decisions regarding plant type, agricultural regime, spatial scale, etc., to be made or presumed by the modeller. This step was beyond the scope of this paper. Instead, our aim was to demonstrate the applicability of EROI in assessing the impact of desert greening on diverse earth system processes energetically, by using a large-scale model experiment to show how land-atmosphere interactions varied EROI as a function of irrigation input, time, geography and climate, which we have done. We agree that fully assessing the energetic sustainability of a given desert greening proposal would require including EROI of technological processes, but that proposal would need to be fully specified. As such, we will further clarify the motivation and applicability of this study to other desert greening studies in Section 4.5, 'Limitations and future research', which will also be referred to at the end of the Introduction section.

Reviewer 1 makes a good point that we could have looked more closely into small scale irrigation.

*"The question of identifying the best locations for (small-scale irrigation) seems a very interesting aspect the authors may consider to put more focus on."*

We fully agree that this would be fundamental to the practical investigation of optimally locating realistically sized plots in which to undertake desert greening. However, our decision not to do so is based on the objectives of the manuscript, as given above. Additionally, while certainly of interest to the authors, successfully identifying small-scale plots involves order-of-magnitude increases in model runs than those needed to answer our objectives. The authors

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stress that NPP is not calculated offline, but rather has a dynamic (and hence scale-related) effect on the timestep irrigation and precipitation that is available, including the associated dynamic climatic conditions of clouds influencing incoming solar radiation and evaporative cooling of the surface due to water availability. Please see the summary of the vegetation model used below.

We have incorporated your helpful comments as modifications to the manuscript. As such, we plan to add: "*While these results hold for the large-scale scenario used here, we stress that isolated irrigation of the high-EROI areas identified by our experiment is unlikely to produce the same absolute or relative (to other grid cells) EROI values. This is because the precipitation feedbacks involving irrigation and subsequent surface-atmosphere interactions depend strongly on the scale at which these processes are resolved by the applied model system. In other words, the spatial variance in EROI in our results are specific to the large-scale nature of our experiment. Further investigation would be required to identify high-EROI grid cells that are relatively independent of irrigation scale; this would require a far greater array of experimental runs to determine than the number employed here.*"

Reviewer 1 also requested clarification of the distinction between the two calculated EROI values:

*"I have the impression that EROI\_A already includes the atmosphere-vegetation feedback. Concerning the step from EROI\_A to \_B, I also don't understand the logic of adding an energy equivalent of the precipitation increase to the energy output."*

The logic behind EROI\_A versus EROI\_B can best be seen by the fact that we close the EROI-calculation system spatially around the irrigated region. By irrigating it, we use energy which supplies water to the atmosphere. This water is otherwise considered lost (implicit in EROI\_A); however, if the albedo and evapo-transpiration dynamics it sets off increases the amount of rainfall in this (bounded) system, it can be counted as desalination-energy equivalent 'return' of energy to the system. Water is provided 'for free' –water that would otherwise cost energy to supply. This 'free water' is thus excluded from EROI\_A, but included in EROI\_B.

Reviewer 1 also notes some confusion regarding our reason for introducing a third EROI quantification, EROI\_C. Our motivation for including this additional EROI\_C value into our analysis relates to:

1. Clarifying that while this particular large-scale Earth System intervention does result in a change to the regional and global radiative forcing components, quantifying this change is not captured in either EROI\_A or EROI\_B
2. Further illustrating the relevance of EROI to the climate science community which, to our knowledge, has thus far not made use of this metric.

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The second point above is also a response to the Reviewer's suggestion that EROI is somewhat over-explained in the Introduction section. We appreciate the opportunity to clarify this from the text in the Discussion manuscript, and will add the following text to the revised manuscript: "*Because radiative forcing is given in units of  $W m^{-2}$ , and the reductions entailed by greening would need to be converted to energy (in Joules), some multiplier representative of the mean energy required to otherwise reduce one unit of radiative forcing could allow this factor to meaningfully enter the EROI calculation.*"

The reviewer also asked for more information regarding the vegetation model utilized in our simulations. To add a more detailed description here (to be summarized in the revised manuscript), the Simulator for Biospheric Aspects (SimBA) was used, with a detailed description available in Kleidon (2006). SimBA is a terrestrial vegetation model that dynamically responds to the combination of land surface properties and influencing climatic conditions. These influences thereby inhibit/enable plant growth, which then alters the surface albedo, aerodynamic roughness length, and rooting zone depth. This affects the energy and mass exchanges within the PlaSim climate model. Gross primary productivity (GPP) is also constrained by these influences, wherein the net primary productivity (NPP) is assumed to be half of the GPP. Specific to such a desert greening type scenario as explored here, we needed to relate this NPP to a representative energetic content ( $J kg^{-1}$ ). We chose to use the energetic content of the arid plant *Jatropha* as representative of vegetation in this area, while forcibly altering its natural per-unit-land area productivity by prescribing additional water to the soil layer (*i.e.* irrigation). While having clear limitations and dependent on a number of assumptions, this approach satisfies the need for dynamic land-atmosphere interactions, while enabling the concept of EROI to be applied to such a large-scale human intervention of the Earth System.

Of additional note, Reviewer 1 noted several inconsistencies in units, abbreviations, lack of explanation regarding model description, and additional reference sources. Though not described herein, these specific comments will be addressed in the revised manuscript and we appreciate the reviewer's attentiveness.

The authors thank the Reviewer once more for their substantial contribution to the revised manuscript.

## References

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