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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.

Anonymous Referee #2

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General comments.

The paper “Quantifying the ‘Energy-Return-On-Investment’ of Desert Greening in the Sahara/Sahel Using a Global Climate Model” by Bowring et al. addresses the problem of estimation of “energetic sustainability” of anthropogenic desert greening. The research is within the scope of ESD.

The idea to look at the desert greening from the “Energy-on-Investment-Return” point of view is new and interesting.

The objectives of the paper are not clearly stated therefore it is difficult to judge whether they are achieved and whether appropriate methods are used. If the objectives are to

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show a way to apply EROI concept to the desert greening the objectives are reached but the paper must be cleaned from redundant analysis confusing readers (by the way the conclusions correspond to such objectives). If the objectives are to study EROI of the desert greening the objectives are not reached because of incompleteness of the numerical experiment set-up.

The authors must decide/clarify the objectives that may influence the title as well.

The conclusions are very conservative and are reasonable because of inconsistency in the experiment set-up. The authors study large-scale desert irrigation (greening) influencing climate feedbacks (including rain increment feedback in the studied region) but in the conclusions they recognize that in reality such large-scale irrigation is problematic. I think the study needs the second step to be more consistent – conduction of small-scale irrigation experiments in the places where the authors found high EROI.

The authors give proper credit to related work and clearly indicate their own contribution. The abstract corresponds to the text. The language is fluent and precise. Most of formulae, units and abbreviations are defined (for explanations see Specific comments (SSTs, K)). In the introduction too much attention is paid to the EROI whilst the methods section lacks description of the model. The references look appropriate.

Specific comments.

Introduction.

The authors formulate the objectives of the paper in the only sentence: “The goal of this paper is to quantify this energetic sustainability for one such process.” I think the objectives must be stated more specifically.

Section 1.1.

Too much attention is paid to EROI. The paragraphs 2-4 (lines 16-29) on p.720 and paragraphs 1-2 (lines 1-14) on page 721 could be deleted or moved to an appendix.

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Section 2.

It looks like the model name PlasSim is incorrect (should be PlaSim).

Some improvements could be implemented to facilitate reading and understanding of the paper. In particular, it would be good to:

- add a simple description of the PlaSim features that are important for this study (e.g., feedbacks surface <-> climate system, how vegetation is modeled etc);
- specify what is “latent output energy of net plant productivity ENPP;
- show the irrigated region on a map.

Section 3.1.

Temperatures are given in K while in the figures the temperatures are in oC. It would be good to stick to a one measurement units.

Section 4.3.

SSTs is not explained.

Section 4.5.

In my opinion it is not evident from the conducted large-scale irrigation experiment that the small-scale irrigation will have similar EROI magnitude and geography because of much weaker surface-climate system feedback. Additional small-scale experiments are necessary to support the discussion and conclusions.

EROIc is not explained.

Interactive comment on Earth Syst. Dynam. Discuss., 4, 717, 2013.

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