General comments

The manuscript “Quantifying the “Energy-Return-on-Investment” of desert greening in the Sahara/Sahel using a Global Climate Model” by S. P. K. Bowring and colleagues analyses large-scale irrigation simulations with regard to the question if an artificial greening of Northern Africa would be energetically sustainable. I have the impression that the experiments are reasonably performed and analysed and that the conclusions are valid, given the scope of the manuscript and its assumptions and limitations.

However, I find it difficult to see the relevance of the research question and results. As, admittedly, the assessment of relevance is probably rather subjective, I do not see this as a sufficient reason against a publication of the material. Nonetheless, the 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 and how it adds to our understanding of the Earth System. Sect. 4.4 provides four aspects, but not an overarching motivation. Hence, it has not become quite clear to me what type of study the paper is meant to be. In particular, how serious should the reader take the scenario of a large-scale irrigation of the Sahara and Sahel? If it is really meant as a feasibility analysis, I can imagine many reasons why the scenario is unrealistic, for example:

- The EROI for solar power in the desert would probably be larger than for biomass burning (b.t.w. I suggest to provide EROI numbers for photovoltaic technology and other forms of solar power)? So why should one burn any vegetation instead of growing food? After all, the advantages of the scenario (“providing food, fuel and carbon sequestration”, p. 722) cannot be realised all at the same time. As the authors assume that all biomass is converted to energy, the arguments of food security and carbon sequestration seem questionable in this context. There can only be an irrigated Sahara if the people there have enough energy as well as food.

- As far as I understand, the energy for the construction and maintenance of the whole infrastructure is neglected. Also, what about the efficiency of converting biomass to electricity?

- Energetic sustainability doesn't imply that a project would be financially feasible or politically accepted (other aspects of sustainability!). Who could and would bear the immense investment costs and risks?

- The required level of political cooperation is unprecedented, and arguably impossible to achieve, especially in a region as unstable as Northern Africa. Local cultures and traditions are completely ignored by this geo-engineering project, so who should ensure its operation? I admit that this may change in the farer future.

Hence, even if the authors were right about EROI > 1 (and I am not convinced of that), what is the relevance of this information in the light of the other restrictions? I acknowledge that several of these issues are mentioned in the paper, but the authors do not seem to see that in conflict with their approach. This would be completely legitimate, if the authors followed a different line of argument, but then I suggest to put more focus on that.

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. I see the advantage that such a local irrigation scenario is much more realistic and therefore more relevant than the large-scale geo-engineering. Although the

authors state on p. 728 that “Given the simulation setup, it is not possible to identify the highest EROI, and thereby the most sustainable location(s) for desert greening”, I wonder whether this is strictly true:
1. If NPP was calculated diagnostically (feeding the offline vegetation module with the unirrigated PlaSim climate and a perturbed soil moisture), wouldn't the resulting NPP-map allow such a site-specific assessment (assuming that small-scale changes do not affect climate)?
2. Using the same climate model, Bathiany et al. (2013) show a possibility to find the most susceptible region diagnostically from long stationary simulations. May there be a similar way to determine the location of maximum EROI and the interdependencies between the regions?

Specific comments

Sect. 2:
- I think that the authors should add information on the vegetation model they use. I suggest to explicitly state that NPP is simulated by the climate model, and how. I guess there is no Jatropha in the vegetation model, so what justifies to use the model's NPP but the energy content of Jatropha in the same equation?

- The logic of the two different EROI versions is not clear to me. In Sect 4.5 it is stated that EROI_B includes the precipitation feedback while EROI_A does not. First, it would be helpful to provide this information already in Sect. 2 to make it clearer. Second, I cannot reconcile this information with the equations. As far as I understand, IR is externally prescribed to the model (artificial influx to soil moisture box), while climate and NPP are dynamically simulated as a response. Due to the irrigation and the resulting changes of surface properties, the model climate will change and in turn affect NPP. Hence, 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. After all, an increase in rainfall would reduce the necessary amount of irrigation, but not increase the energy output of the system.

- Why do you need an additional control simulation? Is that not the same as IR=0?

- Why do you base the choice of the irrigated region on output of a different climate model and not some other criterion within PlaSim? I propose to show the irrigated region in at least one of your maps (maybe in all of them).

- Why do you not account for the need of fertilisers and suitable earth to grow vegetation (and the related energy demand)?

- Given the brevity of the results section I think that Sect. 4 is too long and could be combined with Sect. 5 to give a Section with clearer focus.

- To my taste, the introduction (Sect. 1) could also be shortened. Although the theoretical remarks on the EROI concept may be interesting, I wonder if they are really necessary in this context.

- p. 728: How realistic is the year-round monsoon effect in the model? Is there really an AEJ in the model?

- Sect. 4.3: The reference list could become a bit broader in general, but especially in this section (frequent citation of Ornstein et al., but few others). For example, concerning the shutdown of the AEJ the authors may consider to cite Patricola and Cook (2008). Concerning the coastal ocean circulation feedbacks (Ekman pumping) the authors may consider to cite Liu et al. (2004) and Braconnot et al. (2007).

- p. 732/733: “the precipitation (Charney) feedback”. The atmosphere-vegetation feedback does not only consist of the Charney feedback! Charney (1975) and Charney et al. (1975) only considered the effect of surface albedo. However, there are other factors, especially the changes in the water cycle (evapotranspiration and latent heat release). The role of evapotranspiration was investigated in Charney et al. (1977).

- p. 732/733: I suggest to discuss the uncertainty of the atmosphere-vegetation feedback in general, not only the extreme case of a self-sustained green state.

- p. 733: I find the paragraph about EROI_C hard to understand. It would help to make the main point (inclusion of radiative forcing reduction due to C sequestration) at the beginning of the paragraph, not at the end. Regarding the sequestration argument, would the carbon stock of the Jatropha plantations really be significant compared to anthropogenic emissions (global emissions in general, but also the emissions due to the irrigation infrastructure)?

- Acknowledgements: The authors could mention how their work is funded.

Minor comments

- Some formulations are not clear to me and could be made more precise, e.g.
p. 725: “more water is more energetically sustainable as less water”
p. 726: with “portions” you mean regions or areas?
p. 726: “confluence of low insolation (and so NPP)”
p. 726: “corresponding to a decrease in NPP over the irrigated region”; but NPP increases over the irrigated region (but not as much as it would without lateral transport – is that what you mean?).
p. 728: What exactly is meant by “broad-based inferences” and “subjecting these to thermodynamic rules and climatic knowns”?
p. 732: “As Desert Greening can be viewed as... These types of...” I do not understand the message of these two sentences.

- Fig. 1: The caption starts with “December to February...” which is a bit confusing. I would get a clearer impression if it started with what is shown, then followed by the details.
In Fig. 1b, title and vertical axis label say “latent heat flux”, but the caption says “NPP”. A similar confusion happened with Fig. 2b.
- Fig. 3 and 4: The text (titles, subtitles, especially colour bars) is very small. I suggest to avoid repetition of “irrigation rate = ...” and “summer / winter” by putting it only once above every column of figures. Instead, the variable displayed could be named on or above every panel (not only in the caption).

- There are several typos, e.g.
p. 723: “M J kg-1 The” (missing period)
p. 724: “PlasSim” should probably read “PlaSim”?!
p. 726: “Figure 3 illustrate”
p. 728: “African Eastern Jet” is usually called African Easterly Jet
p. 732: “Jackson (2011) These” (missing period)
p. 732: “A related discussion related to”

References

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