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Interactive comment on “Implications of land use change in tropical Northern Africa under global warming” by T. Brücher et al.

T. Brücher et al.

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Dear Referee #1,

Thanks for your time and effort to improve the manuscript. We are glad, that you like it.

Please find our reply in the following:

General:

... should be pointed out in the abstract and it can also benefit from more emphasis/clarification in the introduction and method parts.

We agree and we will add some clarifications in the abstract.

Specific comments:

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Page 1103, Line 7-8: This sentence about coupling immediately following the sentence on Koster et al. study is not appropriate. The Koster et al. approach indicates the strength of coupling regardless whether the feedback is positive or negative, so the coupling strength index has no sign (it is theoretically positive. Any negative values are considered noise. If the author made this statement based on other studies, a reference is then needed.

Thanks for this very good comment. We will change this part of the sentence to be precise.

Page 1109, Lines 13-15: How might the sharing of moisture between crops and natural vegetation have an impact on the main finding of the study? In reality, this would not happen because the crops and natural vegetation are physically located apart. This was touched upon later in the discussion, but this feature of the model should be clearly pointed out in the methodology part as well.

We do agree! We will add text to the methodology part to explain the sharing of moisture between all tiles within one grid box.

Page 1112, Lines 7-10: The authors think that other models would turn out similar results. How does the dynamic vegetation changes from this model differ from or are similar to results from Yu et al. based on a dynamic vegetation model driven with a large number of 19 GCMs? (Yu M, Wang GL, Parr D, Ahmed KF, Climatic Change, 2014)

Thanks to point at this nice publication. We think that the link to this publication is most appropriate in the introduction part, as the study of Yu et al. belongs to a different topic. Although the experiments described in Yu et al. are based on the same forcing (RCP 8.5), Yu et al. does not account for feedbacks from biosphere to atmosphere, as in these experiments the climate is prescribed as an external forcing for the offline simulations of the DGVM. Also the focus is set on potential vegetation and not on changes in land use.

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Technical corrections:

Page 1102, lines 10-12: "... replace the entire area ... WITH either pasture or agriculture

We will change accordingly.

Line 13, what does "mean agriculture" mean?

We will clarify this.

Page 1106, lines 2-4: This sentence is very confusing. Does not seem like a complete sentence.

We will check and improve the sentence.

Line 11: "properties of grazing ... " should be "parameters of grazing"?

We will change accordingly.

Line 14: "sawing" is a typo.

We will correct it.

Line 19: "both" should be changed to "the two"

We will correct accordingly.

Page 1110, Lines 10-13: This sentence is confusing – not sure what the emphasis of this sentence is. The part about temperature leaves me the impression that is opposite of what I think it meant to be. Should rephrase.

We will rephrase the sentence.

Page 1112, Line 23: "what shouldn't be the case" "what" should be changed to "which"

We will change accordingly.

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Lines 4-6: This will potentially lead to “overestimation” or “underestimation”?

The natural vegetation would benefit from more precipitation and the desert fraction would shrink further. How large the effect of the natural vegetation (forest, shrubs, or grass) on climate would be can not be answered without doing a set of sensitivity studies. Further more, we would mainly see an effect of natural vegetation on climate (which is not the topic of this manuscript) and not of land use change on climate. This is why we would like to leave it open.

With best regards,

Tim Bruecher

Interactive comment on Earth Syst. Dynam. Discuss., 6, 1101, 2015.

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