

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

Anonymous Referee #1

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This is a generally well written paper. The authors use a set of well-designed experiments to show that based on the particular model used, land use type and harvest intensity do not have any major impact on regional climate, and the impact is an order of magnitude smaller than the impact of CO₂-induced climate changes. The authors therefore suggest that climate change can be used as an external driver in land use modeling. This is an important finding as there are many ongoing efforts on incorporating land use prediction into earth system models to address the uncertainties related to climate-land use feedback. Although results may be model dependent, addressing the model dependence will require a community effort that needs to be organized. Results in this paper however may serve as a catalyst for such an effort. I therefore recommend acceptance for publication with minor revisions. My comments are listed in the following:

C483

General: The overall experimental design focuses on the impact of differences in land use type and harvest intensity. It includes very limited changes from forests to grassland or from vegetated to desert (the extent of which is limited to the dynamic vegetation response), which are the type of land cover changes many previous studies focused on. The finding that the impact on climate is small may be partly due to this specific focus on the experiments. This should be pointed out in the abstract and it can also benefit from more emphasis/clarification in the introduction and method parts.

Specific comments:

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.

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.

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)

Technical corrections:

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

C484

ture ...”

Line 13, what does “mean agriculture” mean?

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

Line 11: “properties of grazing ...” should be “parameters of grazing”?

Line 14: “sawing” is a typo.

Line 19: “both” should be changed to “the two”

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.

Page 1112, Line 23: “what shouldn’t be the case” “what” should be changed to “which”

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

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