Earth Syst. Dynam. Discuss., 4, C225–C229, 2013
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Earth System Dynamics Discussions

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4, C225–C229, 2013

Interactive Comment

Interactive comment on "Implications of accounting for land use in simulations of ecosystem services and carbon cycling in Africa" *by* M. Lindeskog et al.

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Received and published: 3 May 2013

We would like to express our thanks to the editor and the referees for their careful reading of our manuscript, and the constructive suggestions for improvement. Below we list the changes we have made in response.

Answers to Tom Osborne's comments:

1. The choice to focus on Africa seems a little odd given that LULCC has been relatively minor compared to other regions, and that cropland density is not very high. Which makes it hard to justify as a good region for validating the model.

Although the African continent has experienced relatively little LULCC, and cropland is relatively sparse, we believe that the validation of the model performance on this continent is warranted by the potentially large impact of LULCC in the future, coupled to the fact that climate change can be expected to cause major impacts on crop production.

2. A few more details on the model setup would be nice. What is the time-step of the model? Monthly, daily? If daily, how is the CRU climate data down-scaled? Does the model simulate dynamic or static roots? I appreciate they might be found in the references, but to aid interpretation of the results I think they should be included in this paper.

We have added information to 2.1 like time-step (daily for photosynthesis, respiration and water uptake), down-scaling of monthly climate data and a bit more about allocation to roots. 2.1, p.5-6. Roots of natural PFTs (and pasture grass PFTs) are dynamic in the sense that the carbon allocation to roots is depending on water stress, but this is done once a year, so it is not relevant for phenology. Allocation to roots for crops is done daily, but the root-shoot ratio is only affected by accumulated heat units (starting at 0.4, ending at 0.2).

3. CRU data. In my experience of using the CRU data I have noticed that for some regions there is no inter-annual information on climate. Have the authors checked that this is not the case for much of Africa? It might be impacting upon their simulations.

It is true that historical climate data for Africa is of relatively poor quality. In some parts of Africa, the cru dataset lacks inter-annual variation for the early 20th century. This is mainly affecting radiation in large parts of Africa in the 1901-1930 period, western Africa 1931-1960 and, to a lesser degree, also temperature (central Africa 1901-1930) and precipitation (small parts of the Sahara and Madagascar 1901-1930). This might affect our results, but since radiation is a minor determinant of NECB in our study, the problems should be relatively limited.

4. Validation of maximum PHU values. Their methodology essentially fits the crop

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growing heat requirements to climatology. This leads to a wide rang of PHU for crops in Table 1. The minimum is set to 900. Is there any way of verifying that the maximum values are reasonable (i.e. by comparing to know HU for crop cultivars)? My concern is that they are too large particularly in the semi-arid regions where I would expect crop choice to be based not just on temperature but also rainfall. i.e. cultivars would be grown that would reach maturity during the rainy season.

The climatology-determined PHU values used in our study are not meant to be more than a variable to determine crop development over the growing period. To achieve anything similar to experimentally determined PHU-values one is more or less restricted to the climate at the study area and can not really be used in other parts or the world, in future climates, or with other cultivar genotypes. This is why the original LPJ-mL method of using global PHU values originated from European or Northern American experimental sites for most crop PFT:s was not very sucessful for Africa. Hence we chose to base the crop development on the local climate and a defined growing period length. The arising problem is the need to better specify the growing periods lengths. These are admittedly very crude and in many cases far too long in the current model version. Still, substantial improvement was achieved over the often close to zero yields when using the static PHU values. Using realistic LGPs is probably one of the most urgently needed update to the model at this stage. Currently, the start of the growing periods are OK, and so are the (potential) yields (more or less), but the LGPs are too long, especially as mentioned in this referee's comment, in semi-arid regions. The dynamic calculation of LGPs based on e.g. soil moisture at regions with precipitation-controlled sowing (which make up the largest part of Africa (see Fig.2) will probably solve the issue.

5. Early "green-up" in the model. This seems quite a consistent bias of the model and is not discussed in great detail. I could think of a few potential causes. If the model uses monthly climate data (see point 2 above) then the "smoothness" of rainfall at the onset of the monsoon, when in reality it can be quite erratic, may lead the model to

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simulate growth prematurely. Related to this, if the model has static roots then plants may have access to water prematurely leading to early green-up. Some discussion on these points in the paper is required.

This resumes the argument above. The early-green up is not a problem of the crop phenology, but of the standard LPJ-GUESS natural grass phenology and is not within the scope of this study to resolve. Since pasture grass is modelled using basically the same grass pft:s as for natural vegetation, the pasture land cover will also be affected by this problem. This is why we need to look at selected sites with a high cropland fraction to see the improvement (Fig.5 vs. Fig.4). The monthly nature of the cru precipitation data and the stochastic rain day generation may certainly give large errors in the start of the growing season, but the error seen is systematic, and is probably more related to problems with natural grass pft phenology.

6. Wheat. Figure 8 shows that for quite a few countries the model does not simulate wheat when in reality it is grown. Table 3, footnote 4 implies this is due to a temperature restriction. Could the authors provide further explanation for what the temperature restriction is for.

The original text was unclear. We simulate temperate cereals, not wheat. This means that "the temperate cereals" PFT is not suitable for simulating wheat that is reported to grow at tropical sites, which is why the 15 degree upper limit for the coldest month was used (this is actually an error in the text, which says 10 degrees). Reported wheat areas in tropical African countries are small. Changed text in 2.1: "For temperate cereals, an upper temperature limit of 15°C for the coldest month for growth is set to avoid growing in tropical climates, following Bondeau et al., 2007." Additionally, we added to 3.3 "Temperate cereals were not modelled in many of the countries that report the cultivation of these crops, because of the upper temperature limit in the model (see Methods), but none of these belonged to the countries with the largest reported wheat area. In the remaining 9 countries, all showed modelled yields equal to or higher than reported yields. Moreover, we removed the countries in Fig.8 where temperate cereals

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were disqualified and added text to the caption: “For wheat, modelled temperate cereal yields was compared with FAO wheat yields. Countries where temperate cereals could not grow because of the upper temperature limit are excluded from the scatterplot. In most of these countries, reported wheat-area overall is small” We also changed the pft name “millet” to “tropical cereals” in the text (page 11).

Page 243 line 15. The sentence "The recent local ..." seems incomplete.

The sentence has been revised as: “The relative degree of limitation by temperature and precipitation to the sowing datesâĂŤor the absence of such limitation in perennially moist areas (where incoming solar radiation generally limits plant production)âĂŤwas determined based on the current (1990) local climatology (Waha et al., 2011) (Figure 2).”

Figure 9. Why were these particular countries selected? Were they the ones with the greatest skill in simulating inter-annual crop yield variability.

Changed text in 3.3: “Interannual variability of simulated and reported yields is a further indicator of model performance. Simulated variation in maize yields for the period 1971-2005 shows acceptable general agreement with observed yields, especially for certain countries (e.g. South Africa and Zimbabwe), reflecting a strong climate component to crop yield and probably also better-than-average crop statistics (Figure 9). The results shown for maize is representative of most crops in these countries.” Added to the Fig.9 caption: “Countries were selected that lacked obvious artefacts in the yield interannual data (e.g. constant data and abrupt value shifts) and that showed clear correlation with modelled yields.”

Interactive comment on Earth Syst. Dynam. Discuss., 4, 235, 2013.