**Earth System
Dynamics
Discussions**

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

M. Lindeskog et al.

mats.lindeskog@nateko.lu.se

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.

Answer to Anonymous Referee # 2:

At the very least I think the authors should make it clearer how their present study differs from Bondeau et al. (2007), ..., I understand there are a couple of improved crop parameterizations (e.g., relationship between LAI and leaf biomass) in LPJ-Guess-crop compared to LPJ-mL, but I am not sure these justify a new analysis – in any case

their implication for the results shown are not discussed., ..., Many 'factorial' climate simulations are performed here, that are not included in Bondeau et al. (2007), but there are only too shortly discussed at the very end of the paper. More generally, I am not sure I understood why LPJ-Guess had to be used, and croplands parameterizations from LPJ-mL included in LPJ-Guess, instead of using LPJ-mL directly ?

The reviewer voices concerns about what are the novel aspects of the paper. These concerns indicate that in the methods section of the original version of the manuscript, we did not highlight the difference between LPJ and LPJ-GUESS, and the original aspects of our work sufficiently well. We aim to improve this in the revised version. 1) LPJ and LPJ-GUESS differ fundamentally in the representation of canopy dynamics, LPJ-GUESS having features of gap-models. Thus, establishment, competition of resources and mortality of age-cohorts is represented explicitly (in contrast to LPJ with its large-area parameterisation for PFT cover and dynamics, typical of most DGVMs). The detailed approach of our model allows a much more realistic representation of C-fluxes associated with crop-abandonment / reforestation (see Figure 3). 2) While we adopted the CFT concept of Bondeau et al, there are fundamental differences in our approach to representing crop phenology: crops in LPJ-GUESS are less dependent on hardcoded limits like maximum LAI and PHU, which in our views enables a more realistic response of crops to future projections which in general describe environmental conditions outside the range of historical experience and data. In particular, the current configuration aims to simulate potential yields, which (in a near-future realisation of the model) will enable the introduction of coupled C-N physiology in crops, in a similar fashion as is done in many DGVMs also for natural vegetation 3) The mechanism of cropland irrigation is also rather different (– see added text, below) Overall, these specific features of LPJ-GUESS should make the model particularly suitable for the simulation of cropland production and landuse change in future and past environments.

Added text to 2.1: "LPJ-GUESS has been shown to be better than LPJ-DGVM at predicting potential natural vegetation, for example in Europe and Africa (Smith et al.,

2001; Hély et al. 2005; Hickler et al. 2012). Also, African vegetation-atmosphere carbon balance responds differently to climate drivers using LPJ-GUESS compared to LPJ-DGVM (Weber et al. 2009)."

Added text to 2.1: "Crop irrigation is treated slightly differently in LPJ-GUESS compared to LPJ-mL. Irrigated crops can still enter water stress if atmospheric demand for transpiration exceeds a maximum evaporation rate (5 mm/day), and irrigation water is only added if atmospheric demand exceeds plant water supply. Also, pasture grass and cover crop grass is simulated by competing C3 and C4 grass PFTs, while in LPJ-mL, C3 and C4 grasses were grown in separate stands, according to a static C3/C4-ratio for each gridcell." 2.1, p.6, l.

Added text to Results and Discussion – more detailed discussion of the factorial experiments (see below,p.253). Added text to Conclusions: "The updates in LPJ-GUESS to the representation of crops and management developed for LPJ-mL also make for more flexibility concerning future climate and CO2"

The present study by Lindeskog et al. presents essentially the very same elements of analysis, with essentially the same model, only focused over Africa.

As outlined above, we hope that we can show that we are not using essentially the same model. In a recent model intercomparison under the auspices of ISI-MIP showed rather different responses of LPJ-mL and LPJ-GUESS/crops to climate change and changes in CO2 concentration (manuscripts are in preparation). Moreover, as the reviewer rightly points out, we assess in more detail responses of C fluxes to various components of environmental change, including also management options. A more detailed discussion is included in the revised paper (see response to the above). The reviewer is perfectly correct, large similarities exist regarding the model evaluation compared to Bondeau et al. 2007. But following from the differences between LPJ-mL and LPJ-GUESS (which we hope we have made more explicit), evaluation is essential. Similarities on this aspect merely reflect the unfortunate scarcity of the available large-

scale data for such validation. For example, in Africa, the lack of flux measurements and inversions from large towers is obvious.

Another main comment has to do with what exactly is being compared here: are the authors comparing simulations with and without land-use change, or simulations with land-use change but with either "real croplands" (from LPJ-mL) and "approximated-bygrasslands croplands" (default in LPJ-Guess) ? This question is actually kind of rhetorical, along the text one understands it is the former, not the latter, but I am emphasizing this because it could be more explicit, in particular given the content of the introduction (which stresses that "at first DGVMs accounted for LULCC by simulating grasslands"), and the existing study of Bondeau et al. (2007) (which includes comparison of the default LPJ and LPJ-mL). I do think that comparing "LPJ-Guess-crop" and default LPJGuess (both with land-use change) would actually be interesting here, I am wondering why the authors did not do it ?

The manuscript here focuses on questions to be addressed with LPJ-GUESS, concentrating on C-cycle dynamics in a simulation (S0) of potential natural vegetation (PNV) compared to a more realistic representation of combined PNV, pasture and cropland (S1). Fractions of these three land covers and, the relative fraction of the different crop pft:s, were adopted from Bondeau et al. 2007. This involves land use change over the simulation period, such that either PNV is converted to cropland, or the reverse. This has been specified in section 2.1, defining the term "land use functonality" as meaning all of the above in S1. In the C flux section, modifications are made to S1, mainly by keeping one of the input drivers (climate, CO2 or land cover fractions) constant or cycling through the spinup values, or turning off/varying management options (cover crop grass or residue removal) to investigate their relative influence of the cumulative NECB for the 1901-2006 period.

An interesting modification, that is lacking in the original manuscript, is a simulation with grassland representing cropland, as suggested by referee #2. We have included this in the revised manuscript (S2). The role of pasture land in the simulations for NECB

was also not shown, but has now been added. This is important since the pasture area is much larger than the cropland area in Africa. We show the results of the extended simulations in the revised Fig.10, the added Table 5 and Figure in the Appendix B1. Changed/added text in section 3.4: "Land use change alone accounted for a 3.1 PgC flux to the atmosphere (Figure 11a), while the rest of the reduction (4.3 PgC) was due to the differential carbon balance of the simulated potential natural vegetation and managed land (at the 1901 cover). Pasture accounts for the majority of the difference, often replacing natural woodland (S0), which is an overall sink for carbon under present-day forcing, with C4-dominated grassland (S1), which is an overall source (Table 5). An alternative model setup including land use change with harvested grass representing both pasture and cropland (S2) produced a similar NECB (Figure 10)." "The results from factorial driver simulations, shown in Figure 11, suggest that CO_2 fertilisation had a greater influence on African NECB (-22.9 PgC for the period 1901-2006) than climate change (+6.3 PgC), land use change (+3.1 PgC) or alternative cropland management (up to 2.2 PgC). In managed land, the CO_2 fertilisation effect is reduced relative to potential natural vegetation, due to a higher proportion of C4 grasses in pasture and C4 crops in cropland, which lack the strong physiological response of C3 plants to elevated CO_2 (Figure B1). However, there are many discussions on the CO_2 fertilisation effect (Long et al., 2006; Tubiello et al., 2007), leading to some uncertainties on the capabilities of DGVMs to correctly account for it."

A systematic intercomparison between LPJ-mL and LPJ-GUESS w.r.t crops would have its value, but this was not the scope of the paper here. In fact, such a intercomparison is being done as part of the ongoing ISI-MIP exercise, and manuscripts are currently in preparation.

I am slightly uncomfortable with the title of the paper,and how it reflects its content.

The title have been changed to better reflect the actual contents of the paper, removing the "ecosystem services" reference. New title is: Implications of accounting for land use in simulations of ecosystem carbon cycling in Africa

Illustrative of this issue are lines 7-8 in the abstract: the authors claim to analyze "the impact of accounting for land-use on crop production". This sentence is problematic: if there is no land-use, then there is no crop production.

The reviewer is correct, this was not well phrased. Changed text in the Abstract: "The revised model was applied to Africa as a case study to investigate the implications of accounting for land use on net ecosystem carbon balance (NECB) and the skill of the model in describing agricultural production and reproducing trends and patterns in vegetation structure and function."

I think the authors should show somewhere a map of reconstructed land-use change in Africa over the 20th century and discuss the extent, location, etc, of this change – this is expected in this kind of study and would help the reader make sense of other figures and results. In particular the authors also mention cropland abandonment a few times (including in the abstract), I would be somewhat curious to see what area it affects in Africa. Similarly, data on irrigation should be presented and discussed upfront.

We have added more information on the land cover database used in the paper, including maps of estimated cropland fraction, pasture fraction and irrigated fraction of gridcells in Africa. We have also illustrated the extent of cropland abandonment by including maps of cropland fraction change in the relevant period (after 1960) as well as the resulting extent of multiple stands with natural vegetation in the model. These maps are placed in the Appendix. Additionally, we add more details how the database was constructed in 2.2: "The historical cropland data set for 0.5o gridcells used in this study was an adaptation by Bondeau et al., (2007) of the cropland fraction for the period 1901-1992 (Ramankutty and Foley, 1999), the distribution of different crops for 1990 (Leff et al., 2004), the pasture fraction of 1970 (Klein Goldewijk and Batjes, 1997) and the irrigated agricultural fraction for 1995 (Döll and Siebert, 1999) (Appendix A). A simplified land cover change model was used by Ramankutty and Foley to extend the 1992 cropland cover, derived from satellite data calibrated by cropland inventory data, back in time. They used historical national and subnational cropland inventory

data and assumed the cropland spatial distribution within these political units to be constant (Ramankutty and Foley, 1999). Bondeau et al. (2007) determined the gridcell pasture fraction after comparing the initial cropland fraction and the "grass and fodder" class of the HYDE data set for 1970 (Klein Goldewijk and Batjes, 1997). Cropland was assumed to expand only into natural vegetation and abandoned cropland was assumed to revert into natural vegetation. The historical cropland fraction was used for the 1901-1992 period and kept at the 1992 level for the rest of the simulation. The relative distribution of different crops (translated into crop PFTs) for 1990 (Leff et al., 2004) and the pasture fraction of 1970 (Klein Goldewijk and Batjes, 1997) for the gridcells was used for the whole simulation period. Irrigation was assumed to occur only for rice in 1901 and then linearly increase to the 1995 value following the linear trend for global irrigation (Evans, 1997). The irrigated fractions of the different crop pft:s were derived by distributing the irrigation according to a priority list, mainly from European agricultural practices (Bondeau et al. 2007)."

In particular, although this is discussed a little bit towards the end of the paper, I think the uncertainties surrounding land-use data in Africa over the last century should be further discussed – how such data is built, etc. (in particular the dataset used here, even if it is described in Bondeau et al. 2007).

We acknowledge that the compilation of land use data for Africa raises a number of issues and contribute uncertainty to our study. However, the data set we used has been previously described by Bondeau et al. 2007, the paper is already lengthy and an in-depth discussion of such uncertainty would be beyond the scope of this paper.

Finally, one point I would like to see addressed is the implications of the overestimated yields (by factor 1 to 6) on the continental carbon balance: if yields are so largely overestimated, doesn't it alter the conclusions regarding the simulated impact of LULCC on the carbon cycle in Africa ? In particular, what about crop management practices in this context (I could see that if crop biomass is overestimated, then the impact of crop residue management, for instance, is also likely to be overestimated).

C217

Added text to 3.4: "Our simulations are of the potential productivity of managed land given plant resource availability mediated by the prevailing climate and available information on irrigation practices. We do not account for fertilisation, suboptimal irrigation and other management aspects that may contribute to reducing actual yields below the biophysical potential (the yield gap). The simulated NECB estimates are thus likely to overestimate the actual carbon storage. A possible overestimation of crop biomass will also be reflected in an overestimation of the impact of crop harvest and residue removal on NECB. However, secondary factors such as losses during transport and storage, which also contribute to reducing the reported yield, will not influence the NECB."

p.242 l.14: at this point PHU is not defined in the text, I believe.

Added definition. (2.1)

p.243 l.28: "2.0" : where does this number come from, and how constant is it in real life ?

The number 2.0 assumes a carbon content of 50 %. This is a very simple approximation for average plant carbon content (45-50% is often used), which of course could be refined for the crop pft harvested organs, if that information is available. However, this uncertainty only affects the yield estimates in our paper, and even if we estimate this error to be about 10 %, this is relatively small compared to the other error sources of both simulated and reported yields. Also, it will only affect the yield gap estimates, and not the correlation with FAO yields. Added to 2.1 "assuming a carbon content of 50 %".

Section 2.2: I assume a weather generator was used. This should be discussed.

We used a stochastic rain-day generator (Gerten et al. 2004) to convert monthly cru precipitation data to daily values and linear interpolation to convert monthly mean temperature and radiation data to daily values. Added to 2.2:"The monthly cru precipitation data was converted to daily values using a stochastic rain-day generator (Gerten et al.

C218

2004). The monthly mean temperature and radiation (percent cloudless) data were converted to daily values by linear interpolation."

p.248. Line 26-27: a important limitation of that improved fit is that because standardized anomalies are used, we can tell it affects variability, but not mean values. This should be explicitly stated (the authors do mention this in section 2.3, but it should be associated again with the results here).

We accidentally submitted a figure showing a comparison of standardised anomalies using the mean and sd of all the monthly values for a site, adding inter-annual variation. This is not what we wanted, so we have corrected Figure 6. The map is similar to the one in the previous ms, apart from a region in southern Africa, where the pasture and crop representation in the current GUESS version obviously also improves on the inter-annual variability compared with satellite data. I also added a 2% cropland cut-off in this figure for clarity. Changed to: "Altogether, simulations including land use improve the FPAR vs. NDVI fit of standardised intra-annual variation (seasonality) in the most crop-intensive regions across the entire continent, with the exception of the Nile valley and delta (Figure 6)." 3.1, p.10, l. Changed Fig.6 caption: "Difference in distance index (di) for monthly observed NDVI and modelled FPAR by adding land use functionality with cropland and pasture for the period 1982-2006. Cropland is simulated without cover crop grass. Negative values are improvements to the FPAR vs. NDVI fit of standardised seasonal variation. Gridcells where the year 1992 cropland fraction is below 2 % is masked out."

Section 3.1: lines 24-25 p247 + lines 1-2 p248, and lines 3-5 p248, seem contradictory to me: either natural vegetation and croplands have similar phenology, either they don't. Maybe some point-scale plots of crop/natural seasonal cycles would help illustrate the differences.

The two sentences are rather confusing, we have removed the first sentence and changed the second one slightly: "Adding land use functionality to LPJ-GUESS brings

C219

rather minor changes to these continental-scale seasonal FPAR patterns, reflecting the relatively small cropland fraction of Africa, which when averaged over the latitudinal bands does not exceed 9% of the area (Ramankutty and Foley, 1999). However, when focusing on a number of locations in regions with a relatively large fraction of cropland (Table 3, Figure 2), the difference in leaf area development over the course of a year between managed land and natural vegetation is evident (Figure 5)."

Section 3.2: Why focus only on the Sahel ? What about the rest of Africa ? The Sahel does not show a particular strong impact of LULCC on simulated interannual variability (actually, virtually none), so I don't understand the regional focus. Why not focus on regions with a higher rate of LULCC ?

The focus on the Sahel is because we wanted to follow up our previous work on this subject. This previous work described reproduction of the greening trend of the late 1980s and 1990s, where LPJ-GUESS was used without explicit crop representation, and an effort was made to correlate deviations between modelled LAI and satellite NDVI with population density and cropping and grazing density. Although we show that the pasture and cropland addition to LPJ-GUESS does not cause a big difference to the inter-annual variation in the Sahel, we still think this is a relevant, even with a "no finding" in the context of this paper.

Section 3.3: so for wheat, shouldn't the temperature be set higher ? Interannual variability of FAO data can be problematic for many countries and crops (i.e., constant data, abrupt shifts, non-climatic factors contaminate the data, etc...). Were the countries shown on figure 9 selected in any way for the quality of their data ? If yes it should be mentioned.

We try to specifiy this more clearly in the revised paper. With the crop PFT concept, we simulate temperate cereals, not wheat only. The current parameterisation 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 spelling

C220

mistake in the original text, which said 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." Added text: "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. Removed the countries in the wheat scatterplot in Fig.8 where temperate cereals are 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." We also changed the pft name "millet" to "tropical cereals" in the text.

The reviewer is correct in that there are issues with some of the country statistics reported to FAO. It is however difficult to find objective criteria for either inclusion or exclusion of data. We revised text in 3.3 and Figures as: "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." p.251. Lines20-25: I don't quite agree with the "comparable" word choice (and the "similar" in the abstract). The total effect of LULCC is 8.3PgC, which is several times higher that the effect of cropland management options (that, in addition, offset each other). Even if the direct effect alone of LULCC may be only 4.4 PgC, it is still 2-3 times higher. See also comment above on yield overestimation.

<center>C221</center>

We have changed this to a more careful wording. However, if we consider sub-optimal irrigation and low harvest efficiency also, we end up with a higher range of management-derived C flux uncertainty, but we decided not to include these results because of space limitations. Changed text in Abstract, p.2, l. : "Cropland management options (residue removal, grass as cover crop) were shown to be important to the land-atmosphere carbon flux for the 20th century." Changed text in 3.4: "). In our model, alternative cropping practices thus had the potential to influence the biosphere-atmosphere carbon balance significantly, underlining the need for valid characterisation of..."

p.253 lines1-3: so is IMAGE a better dataset ? But the authors used Ramankutty and Foley here...

Ramankutty & Foley 1999 has probably too low a rate of cropland expansion (or deforestation, since that database does not consider conversions from PNV to pasture) during the 1980s, but, at least compared with Houghton 2003, Ramankutty & Foley has a higher global cropland area for the whole historical period, so it depends on whether one is focusing on the direct effects of LULCC, or the indirect, long-term effects, where a correct level of cropland area might be as important as when the LULCC have occured. Changed the sentence in 3.4: "...more in agreement with high estimates based on book-keeping..."

p.253 lines 16-17: I don't understand this statement; -0.5 and +0.09 sound hardly similar. In absolute values, -0.5 and +0.6 are actually closer.

This was unclearly stated in the text. Replaced text in 3.4 with: "the relative NECB contribution of both climate and CO2 (+0.6 PgC yr-1 and -0.5 PgC yr-1, respectively) was much higher than that of land use change (+0.09 PgC yr-1)."

p.253 lines 18-23: this should be discussed in a lot more detail: what processes are involved here, what about radiation effects, etc... There are lots of simulations behind fig.11a, I would expect these results to be discussed further. In addition, as mentioned

<center>C222</center>

above, this aspect was not addressed in Bondeau et al. (2007).

We add some more discussion on this topic, as well as figures showing the relationship between the simulated yearly NECB-values for Africa and precipitation, temperature and radiation. We obtain similar results as those published by Ciais et al. (2011).

Added text to 3.4: "Over the period 1901-2006, the modelled net cumulative NECB in our study is strongly affected by the rising trend in annual mean temperatures, but the net effect of precipitation and radiation for this period is close to zero. NECB in tropical regions is positively correlated with temperature and negatively correlated with precipitation when modelled by 10 different global dynamic vegetation models (Piao et al., 2013). This is also seen in our study for Africa (as in the study of Ciais et al. (2011)), but after 1991, the correlation with temperature is reduced by the strong effects of rising precipitation (Appendix). Modelled gross primary production (GPP) and NECB of the inner tropics of Africa appears to be limited by radiation in some vegetation models, e.g. LPJ-DGVM, but not by LPJ-GUESS (Weber et al., 2009). This is also reflected by our results, where modelled NECB for the African continent is uncorrelated with radiation (Figure B1)." Added Appendix Figure B2 showing time-series of NECB and climate variables and associated correlation.

Figure 1: what do the dotted and full-line arrows stand for ?

Changed caption: "Crop phenology in LPJ-GUESS. The feedback between leaf area and leaf carbon mass via NPP is denoted by full-line arrrows and the heat unit sum control of the carbon allocation and leaf LAI is denoted by dotted arrow. *HU sum: heat unit sum (dynamic potential HU adapted to local climate); LAI: leaf area index; HI: harvest index, NPP: net primary production."

Figure 4: labels should be explained better (what does LPJ-GUESS LU stand for ? It is possible to guess, but still...)

Changed caption: "Standardised anomalies of monthly observed NDVI and FPAR mod-

elled by LPJ-GUESS with (S1, "GUESS-LU") and without (S0, "GUESS") land use functionality, relative to the gridcell mean and standard deviation, averaged over latitude bands for the period 1982-2006."

Figure 6: what values are blanked out ?

In the new figure, gridcells with <2% cropland fraction are masked out.

Text on figures 9 and 11 is a little too small.

Text on figures 9 and 11 has been made bigger.

Figure 12: what difference is plotted here, explicitly ? The caption could have more information, as the reader is looking at differences of negative fluxes, etc....

Changed caption: "A. Difference in modelled Net Ecosystem Carbon Balance (NECB) between LP-GUESS with and without land use functionality (cropland and pasture representations and land use change). Positive values indicate gridcells where land use reduces the carbon sink compared to potential natural vegetation only. B. Carbon fluxes associated with land use change (not including carbon added to litter). The values are the means for the period 1961-1990. Positive values represent a flux to the atmosphere."