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Discussions

Interactive comment on "Climate response to Amazon forest replacement by heterogeneous crop cover" *by* A. M. Badger and P. A. Dirmeyer

Anonymous Referee #2

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(Page and line numbers in this review are from the printer-friendly version of the manuscript.)

In this study, the authors investigate the impact of idealized, complete deforestation on the regional climate of the Amazon in a coupled (atmosphere-ocean-land) climate model (the CESM model). The main originality of their work is that, in an effort to perform a more realistic simulation, they "replace" forests in the model by a spatially heterogeneous distribution of several regional tropical crops, which are then explicitly simulated by the land component of the climate model; most deforestation studies, in contrast, typically replace forests by generic grasslands. In that respect, this study has the potential to represent an interesting contribution to the vast body of work focusing on the climatic impacts of Amazon deforestation.

My assessment of the manuscript, though, is that it requires some major revisions before it can be published. While (mostly) well written and straightforward, in my view it suffers from some significant shortcomings. In summary (and in no particular order of importance), the study lacks process-level analysis of the results presented, needs to better describe/validate/discuss the new crop parameterization introduced, and better analyze the specificities of their results that can tied to these parameterizations.

1) My main concern maybe has to do with the insufficient analysis and discussion of the physical processes linking the change in vegetation cover to impacts on climate. The whole "Results" section (section 3) feels very descriptive, listing changes in different variables in different seasons/regions without a real process-based narrative. For instance, it may feel trivial but changes in near-surface temperature are never really linked to changes in surface fluxes. More importantly, the changes in precipitation are not explained properly – how do the different patterns of change in different seasons/regions come about, in relation to changes in surface fluxes, radiation, etc. The beginning of the Discussion section starts to address this a little bit, but is clearly too little – in addition, in an effort to generalize, it mischaracterizes some of the results, e.g., p.895 line 7: both latent and sensible fluxes are not reduced everywhere (figure 6). Precipitation changes are a major point of focus of Amazon deforestation studies, and the manuscript feels rather weak on that level. Local and non-local impacts of changes in surface properties and variables on moisture and precipitation should be analyzed (e.g., moisture convergence, moisture recycling, etc.). It would also be interesting to see other variables discussed, e.g., cloud cover. Similarly, the section on changes in land-atmosphere coupling (3.1) feels descriptive, and should be better related to actual changes in climate – for instance, changes in variability, or extremes. I find the focus on mean climate a bit limited in this study.

2) Another point of concern, along the lines of the one above, is that while the authors make the (very commendable) effort of introducing tropical crops in the land component of CESM (CLM 4.5), they do not really derive any conclusion regarding the im-

portance of having realistic replacement vegetation (crops) for deforestation studies. As noted above (and as the authors indicate in the title and abstract), the added value of the study mostly stems from the more realistic representation of tropical crops used in their model. Ideally, to illustrate the importance of this, the experiment the authors perform with CESM should be compared to another one replacing forests by grassland with the same model. I understand this cannot be the case here, given the unrealistic "default" vegetation response to Amazon deforestation in CESM. However, I find that the authors' take on this - they simply note that the average annual changes in temperature and precipitation in their simulations are consistent with, while lower than, those in previous studies – is not sufficient. For instance, in this study such as this one I would expect to see the different changes in surface properties and fluxes, and subsequent impacts on climate, discussed in more details in terms of seasonality and in relation with the specificities of different crop phenologies (e.g., planting, harvest). Even in the absence of a 'default' deforestation experiment, the authors should be able to discuss further the specific impacts of crops on climate (compared to., e.g., grasslands).

3) Which brings me to a third point of concern: the lack of presentation, discussion in relation to the literature, validation, etc. of the new parameterization of crops in the land model. Including explicit crops in vegetation models has been a focus of the land modeling community for years and a number of studies present such developments, with varying numbers of parameterized crops and/or geographical scope: see, for instance, Bondeau et al. (2007) with LPJ-LM, Kucharik and Brye (2003) with Agro-IBIS, Gervois et al. (2004), Smith et al. (2010) Berg et al. (2011) and Valade et al. (2014) with the ORCHIDEE land model – some of these studies dealing with tropical crops, too. Only the work of Levis et al. (2012), in connection with CLM, is mentioned here; the manuscript needs to be better connected to the existing literature on this aspect and reference the studies mentioned above. As is typically done in such studies, here the authors need to present the new parameterizations in more details. For instance, how are sowing dates computed? Are they spatially-varying, time-varying? Table 1 indicates "last NH planting date" which suggests some time window is used. What

happens at harvest? Crops are replaced by bare ground? Are allocations rules modified? Is some yield computed? Etc. As is done other studies, again, the authors then also need to show some validation of these new parameterizations for different crops, for instance against in-situ seasonal vegetation measurements when available, or satellite data, or even yield data – and discuss the impact/improvements from having real crops instead of grasslands. Note that the one study that uses soybean instead of grassland as replacement vegetation in the Amazon, for instance, parameterizes soybean based on observations (Costa et al. 2007). My point is that readers need to be able to see for themselves how realistic the new crop parameterizations in CLM are. Again, the added-value of the study mostly stems from the introduction of this new land parameterization, so this is essential. Incidentally, here, I am wondering in particular about the realism of the irrigated rice parameterization. Figure 2 indicates that it is a major replacement crops in the model. While I understand this comes from the Sacks et al. data combined with the deforestation/replacement algorithm used, I am questioning the realism of this: I read (for instance, here: http://www.pecad.fas.usda.gov/highlights/2007/03/brazil_rice_30mar2007/), that in Brazil rice in the center of the country is not irrigated (irrigated rice is in the Southern tip of the country, where climate is certainly drier – that is also where most of the rice is). In addition, the authors indicate that irrigation is used whenever the plant is water-stressed in the model, which seems to indicate that there is no constrain on the amount of water added. This could lead to unrealistic amounts of added water (and subsequent impact on climate). In line with my general comment above, I would like to see the authors discuss these aspects in particular in more details.

Some more comments along the text:

Section 1: Introduction The introduction lacks focus and needs to introduce the "problem" that is identified, and how the authors intend to address it, more clearly. For instance, it is never explicitly mentioned that the authors intend to simulate Amazon deforestation, with explicit croplands as replacement vegetation. The focus on mean climate should be indicated. On lines 21-29 p.881, the authors discuss the biogeochemical effects of land-use change, which distracts the reader from the main focus of the study, which is biogeophysical effects; etc. In general the introduction makes some effort to reference previous modeling studies on Amazon deforestation - of which there are many. However, some of the references are a bit dated, and the discussion could benefit from including more recent references: e.g., Lee et al. 2011, Medvigy et al. 2011, Spracklen et al. 2012, Bagley et al. 2014, Lejeune et al. 2014, among certainly others. The latter reference, in particular, provides a very complete overview and discussion of prior deforestation modeling studies that the present study could advantageously draw upon. A broadened literature overview should also allow the authors to discuss/mention issues such as whether current deforestation has already impacted climate (e.g., Lee et al. 2011, Bagley et al. 2014); or the impact of model resolution on simulation results. This is an important issue that the authors should discuss: a whole body of work points to the importance of mesoscale effects of deforestation – e.g., both observations and high-resolution modeling ($\sim$1km resolution) suggest that in regions currently being deforested, deforestation actually has a positive impact of on cloudiness and precipitation (Chagnon and Bras 2005, Wang et al. 2009): small-scale surface heterogeneity caused by deforestation can induce local “vegetation-breeze”-like circulations and, as a result, can increase convection over warmer deforested areas during the dry season (Roy 2009). Although the authors focus on a different problem (vegetation simulation), they need to at least mention this scale issue in their introduction or discussion/conclusion. They touch on it on line 1-5 p.882, but in a vague and insufficient way (e.g., it is unclear what the authors mean by the “local features of deforestation”).

Line 23 p.880: “large portion” -> numbers could be given here.

Line 14 p.881: “is a danger that can” -> can

Line 20: Davin and Noblet 2010: please choose a more relevant reference: this one focuses solely on biogeophysical processes.

Section 1.3: what range of deforestation is considered is all those studies?

Line 23 p.883: description. The authors haven’t said yet what the problem/point of the study is, so this feels somewhat awkward.

Line 24-25 p.885: “most notable difference”: in the model.

Line 1 p.8898: remove “is” in “is centered”. References may be needed for this climatological description.

P.889-890: the discussion on the fire bias feels a bit long and needlessly detailed.

Section 3: Results.

In general, if length requirements allow, it would be nice to see climatological maps of T and P for the model versus some observational estimate maybe, to assess the model’s regional climate – in particular as the authors repeatedly mention a dry bias in the model over that region.

Section 3.4

At what time scale are correlations computed here? With daily, monthly data?

Are changes in L-A coupling significant? Given that these involve changes in correlations and variability, this could be formally assessed, I presume.

Lines 10-15 p.894: the authors should explain how, physically, changes in correlations and variability relate to each other.

Lines 18-19 p.894: this claim needs to be explained in more detail.

Section 4: discussion and conclusions. Line 6 p.895: Note that there could be changes in the Bowen ratio without changes in net radiation.

Line 7 p. 895: not true on Figure 6.

Line 1-2 p.896: is this shown somewhere?

Line 20 p.896: This sentence feels grammatically awkward.

Figures:

Show latitudes on maps (e.g., the text often references the Equator, which is not indicated).

Figure 6: in NDJFM, how does the Northwest region of increased net radiation relates to the positive changes in albedo, as well as precipitation?

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