In all responses, the original reviewer comments are repeated below and then followed by our response to the reviewer [RR].

Anonymous Referee #1:
General comments:
This paper examines the impacts of deforestation on climate in Amazonia. The authors implement new plant functional types (PFTs) in CLM4.5 in order to represent typical tropical crops, under both rainfed and irrigated practices. Using a fully coupled Earth System Model, the authors compare two simulations: one with present vegetation cover, and one with a deforested Amazon. The differences fall within the range of previous studies in spite of the more realistic prescribed changes from forest to crops.
This study includes a more detailed and realistic depiction of deforestation via the inclusion of tropical crops, and is therefore a welcome addition to the existing literature. The manuscript is generally well-written and I believe that the work deserves publication.

[RR] We thank the reviewer for the kind remarks on the paper.

However, some aspects require substantial improvements and I thereby recommend major revisions. In particular, the authors mainly assume that the observed changes are due to local changes rather than changes in large-scale circulation. Similarly, some aspects such as the cause of changes in the evaporative fraction are not convincing enough at this stage. Finally, apart from the more detailed representation of the crops used in deforestation, the work is very similar to previous studies. It could easily be slightly expanded to include e.g. the impacts of deforestation on temperature extremes, which would certainly be a welcome addition to the existing literature.

[RR] We respond to these points where they are raised again in the specific comments below.

Specific comments:
1. Page 881, line 6: 1991 is now over 20 years ago. Is there any more recent data available?

[RR] A more recent set of references has been added. Inserted at line 28 of the revised document.

2. Section 1.3: The literature overview provided here could benefit from a few additions. For instance, Lejeune et al. (2014) have provided a comparison and synthesis of 23 GCM studies relevant here and provides useful findings in this regard. This study could be relevant to facilitate the comparison to previous studies on page 891, lines 13-19 and on page 892, lines 10-18, as well as when discussing the presence of bipolar temperature changes (p. 890, lines 20-24). Recent studies

that make the link between land-atmosphere coupling and sensitivity to deforestation could also be worth citing (e.g., Lorenz et al., 2014, etc.).

[RR] We agree; Lejeune et al. (2014) has been incorporated into the literature review. The dipolar temperature change found due to initial coupling strength has been referenced in regards to discussion the dipolar temperature change found in this study.

3. Irrigation: I find it quite difficult to understand what and where is irrigated. For instance, was irrigation only applied to "irrigated rice" but not to other crops, or to any crop? I think this requires clarification. For example, at lines 5-12 on page 886 there is an explanation that (1) irrigated area fraction is defined based on a dataset of areas equipped for irrigation, and that (2) irrigation is only applied to the soil beneath irrigated crops. This does not seem to exclude that irrigated area fraction might exceed the irrigated rice PFT area fraction, and thereby implies that other crops can be irrigated (also implied by line 5-6 on page 887). On the other hand, in the rest of the paper, irrigation only seems to refer to rice (e.g., lines 12 p.888; line 26 p.891; line 7 p.892; line 15-17 p.895).

[RR] Yes, irrigation is only applied to rice, which is the only irrigated crop specified. The other crops are rainfed; please see line 153 of the revised document for "irrigation is only applied to the soil beneath irrigated crops." There were both rainfed and irrigated versions of each crop created, however, this study only uses the irrigated version for rice. Each crop has a separate soil column, which is determined by the percentage of each crop in the gridbox (i.e. percent of area irrigated = percent area of irrigated rice), please see lines 152-153 of the revised document.

4. Section 2.3: over which period/with which forcing are the simulations run? The Qian et al. forcing used repeatedly is mentioned for the spin-up simulations but it is not clear to me whether this forcing has also been used for the actual simulations (250 years including 125 last years used for the analysis). A layer of confusion is added at L7 (p. 889) when the year 2000 is mentioned as initial conditions. Please clarify.

[RR] The years of the forcing data used in the spin-up simulations for the land-surface have been added, please see line 233 of the revised document. For years 1 to 600 of the spin-up, Qian et al. (2006) data from 1951 to 1990 were used. For years 601 to 650, years 1951 to 2000 were used. This forcing was only used to get the land-surface's carbon and nitrogen in a steady state; the last land state from the spin-up is then used as the initial land-surface state. Year 2000 is meant to indicate the greenhouse gas concentrations, which has been clarified in the text, please see line 229 of the revised document.

5. Section 3: The analysis only considers changes in mean quantities. I think that showing changing in the distribution (e.g., changing in temperature quantiles from

the distribution of daily temperature) would be a valuable addition to this paper, even if only for temperature. I leave this up to the authors to take up this suggestion but it might also help understanding some of the observed features.

[RR] The changes to variability were considered, but it was decided that it would take away from the goal of describing the mean response of local climate. We feel that once one variable is brought up, as the reviewer suggests, we would be obliged to discuss others, considerably lengthening the paper.

6. Section 3.3: The authors argue that changes in net radiation directly drive the changes in the partitioning between sensible and latent heat fluxes (page 892, line 24: ". . .impacts the partitioning between sensible and latent heat fluxes"). I would expect a reduction in net radiation to reduce both fluxes without necessarily changing their partitioning, and the authors do not present any convincing evidence that the reduction in net radiation is the cause of decreased latent heat / increased sensible heat fluxes. Instead, I suspect that reduced precipitation (Fig. 5) is likely to lead to drier soils and thereby reduced evaporative fraction. Alternatively, modified vegetation parameters might also impact evaporation via plant physiology without necessarily impacting net radiation. The authors need to present a more detailed analysis here and/or more cautious conclusions (see also the first few sentences of the discussion and conclusion, which clearly assume that modified albedo directly change the partitioning EF, further changing the climate, although this could also results from indirect effects via precipitation or from vegetation parameters other than albedo). A map of changes in evaporative fraction might be useful here, as it is difficult to assess how changes in turbulent fluxes translate in changes in EF if both are reduced or enhanced at the same time.

[RR] We have conducted analysis of the relationship between the albedo change and evaporative fraction change, as well as precipitation change and evaporative fraction change. Both albedo and precipitation show a noticeable relationship, neither shows a statistically significant relationship though. The conclusions are now more cautious regarding this statement. In addition, following the reviewer's suggestion, an analysis of evaporative fraction has been added to the paper in Sec 3.3 (and new Fig 7).

7. Page 895, line 10-14: The authors seem to imply that changes in precipitation result from local interactions via PBL growth, and vertical moisture transport. What about horizontal moisture transport (i.e., convergence)? It is not clear, in my opinion, whether changes in large-scale moisture convergence due to e.g. atmospheric subsidence can be excluded based on these analyses. Did the authors observe any change in wind, atmospheric moisture and resulting moisture convergence that could also explain changes precipitation (as well or even better than local PBL drying/deepening alone)? Much of the manuscript assumes that changes are local, but in fact non-local changes linked to circulation could also play an important role in the shown changes and relationships between those.

[RR] Analysis of the changes to large-scale circulation features is the topic of a separate paper that has been submitted to the journal Climate Dynamics. A draft of the paper can be found at:
ftp://cola.gmu.edu/pub/abadger/Badger_Dirmeyer_2015.pdf
As would be expected, there are changes in moisture convergence over the region. When analyzing the precipitation over the region, the entirety of the region experiences a significant decrease in convective precipitation, while large-scale precipitation has regions of significant decrease towards the south (SACZ) and north (ITCZ). While both convective and large-scale precipitation contribute to changes over the region, it appears as though the convective (i.e. local) precipitation is the large driver of the change.

8. Section 4 and Figure 9: Much of this section focuses on analysing the impact of irrigation (page 895 line 15 until page 896 line 19). I would recommend having this as an additional subsection within the results (i.e., a new section 3.5) rather than merged in the conclusions. Moreover, Figure 9 is difficult to read, especially the lower row, as many dark dots mask lighter dots, and it is difficult to verify the claims made from p.895, line 25 onwards. Could these results be presented in a different way? For instance, one could use multiple boxplots in the respective colors for different ranges of initial tree cover; the choice is of course left to the authors.

[RR] We feel that by stating the spatial changes in coupling in the results and then providing explanation of them in the discussion along with discussion of irrigation, a better logical flow to the paper is achieved. Figure 9 (now Figure 10) has been modified to make it more readable.

9. Table 1: The date format is not intuitive and changing it would facilitate under-standing. Also, could you distinguish between crops that already existed and those that have been implemented (e.g. with an "*" or with bold or italic font)?

[RR] The date format has been changed. There was originally a vertical line distinguishing new crops in the table that disappeared during typesetting. We would hope to distinguish the new crops by appending "tropical" before their names.

10. Figures 1 and 2 display a smaller domain than the other figures, which extend further south. Although this is not really a problem, I was wondering if there was there any reason for this.

[RR] Figures 1 and 2 have been modified to show the same spatial domain as the other figures.

Technical corrections:
These suggested changes have been made. Again, SD was defined in the original draft form, but was removed by the typesetter. We have re-inserted it.

Anonymous reviewer #2:
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.

[RR] We thank the reviewer for the supportive comments.

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.

[RR] A portion on evaporative fraction has been added to Sec 3.3, hopefully that can provide some linkage between surface flux changes and the temperature responses. The beginning of the discussion section has been expanded and clarifies that areal averages of the surface fluxes have been reduced. As previously addressed in

Reviewer 1's comments, the convective precipitation decreases across the entire domain while large-scale precipitation does not, this indicates a local driver in the precipitation changes. Further analysis of non-local impacts are the subject of another paper that has been submitted and is in review; A draft of the paper can be found at:
ftp://cola.gmu.edu/pub/abadger/Badger_Dirmeyer_2015.pdf

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 importance 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).

[RR] A true assessment of the improvement of using a more realistic replacement vegetation cannot be done without another model simulation using a homogeneous replacement vegetation. However, the use of heterogeneous crop cover is certainly more realistic. It does provide some seasonal changes that were not previously found, as well as the clear impacts occurring due to irrigation. It is still rather difficult to draw conclusions about using a heterogeneous crop distribution as opposed to grassland cover; we do not feel comfortable drawing such conclusions without doing the appropriate model simulations.

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.

[RR] The new set of parameters detailed in Table 1 show the changes made to create new crop PFTs for CLM. The underlying crop model code is the same, the modification of the parameters in Table 1 detail how the new crop types are different from the pre-existing crop types. The references suggested by the reviewer detail the use of tropical crops in other models and have a large focus on the biogeochemical aspects; the goal of the tropical crop implementation in this study was an improved, more realistic biogeophysical behavior using the existing CLM-crop code. A BGC assessment would be the subject of an additional study. The planting dates are computed from the Sacks (2010) data set; please see line 189 of the revised document. Table 1 shows that some crops have different planting dates for the Northern and Southern Hemispheres, but within a hemisphere the planting date remains the same for the respective crop. The first and last planting dates for each crop are listed in Table 1, this would indicate the time window for planting. After crops are harvested, land cover becomes bare ground until the next planting.

Yield can be computed from the CLM output. The yield for the tropical crops was found to slightly underestimate observations in validation done by others at NCAR (personal communication with Sam Levis). The physical seasonality (planting, phenology and harvest timing) match well with the Sacks (2010) data set. In-situ measurements of tropical LAIs are not readily available, and in our opinion LAI for a managed land cover type can depend on many factors including how densely a particular farmer chose to plant their crops. Single site validation might not prove useful.

The irrigation scheme in CLM simply adds water to the soil when water is the limiting factor for photosynthesis and is removed from the total liquid runoff, simulating removal from nearby rivers. The amount of water added is equal to the water deficit needed to have the limiting factor of photosynthesis be radiation. There are checks within the model that do not allow unrealistic amounts of water be added.

We agree that the crop distributions are not 100% realistic, but they are interpolated from current distributions and do allow for the goal of heterogeneity. As stated, the rice in central Brazil is not irrigated, but it is perhaps plausible in a total deforestation scenario that irrigation would be need due to precipitation and infiltration decreases.

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 (~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").

[RR] The Introduction has been edited to indicate more clearly the "problem" and focus of this paper on changes to the mean climate.
Many of the references suggested utilize a partial deforestation scenario or focus on differing topics than the goal of this study. Lejeune et al. (2014) was added to the list of references for the paper, but we hesitate to expand the literature review too broadly. The idea of high-resolution studies that use "fish-boning" patterns of deforestation that can cause mesoscale circulations is certainly interesting, however this study focuses on total deforestation in a global climate model that does not resolve such patterns. It would certainly be desirable to repeat tropical deforestation studies with mesoscale-resolving or cloud-permitting models incorporating finer land-use heterogeneity than applied here. We now raise that prospect in the Conclusions.

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

[RR] Corrected

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

[RR] Corrected

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

[RR] Corrected

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

[RR] Total deforestation; this has been added to text.

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

[RR] Hopefully it has been cleared up.

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

[RR] "In the model" has been inserted.

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

[RR] Removed “is”

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

[RR] The discussion on the fire model is done because the only references on this issue are via personal communication. By detailing the issue here, we are providing a documented source.

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.

[RR] Adding a discussion of model climatological output in comparison to reality would extensively lengthen the paper. The dry bias was discussed and since this is a sensitivity study, the dry bias would be present in both simulations. This certainly may have some effect of results but it’s impact is lessened when assessing the differences (deforestation minus control).

Section 3.4
At what time scale are correlations computed here? With daily, monthly data?

[RR] Monthly data were analyzed. That the last 125 years of monthly data used for analysis is now stated more clearly in the text.

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

[RR] Significance was assessed using correlation. For the control, a significant correlation implies significant coupling. For the change in coupling, a significant change in the correlation implies significant change in the coupling.

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

[RR] For land surface anomalies to have an increased impact on the atmosphere, increased coupling, the combination of correlation and variance (product of correlation times the standard deviation of the flux) must increase; an increase in either term is not a guarantee. This is now stated more clearly in the text.

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

[RR] We have added “based on the spatiotemporal correspondence between the two”.

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

[RR] The added discussion and analysis of evaporative fraction should clarify this.

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

[RR] Areal averages of LH and SH; this has been clarified in the text.

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

[RR] Yes, this is shown in Fig. 10. Hopefully the redesigned figure makes this more apparent.

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

[RR] The sentence has been restated: “Even using a realistic heterogeneous crop distribution in the Amazon region, there is still general agreement with previous modeling studies.”

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

[RR] The equator is now indicated on all maps.

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

[RR] This is an interesting question. We believe this change is predominantly atmospherically driven by large-scale circulation changes, particularly lower-level convergence and weakening of the Hadley circulation. Another attribution could be an ocean-land contrast response; previous studies did not use an interactive ocean as this study did and this region being on the northern edge near the ocean, the region could be effected by oceanic changes. Further model runs would likely be necessary to pin this down.

Anonymous Referee #3:
The authors provide a study to assess the climate feedback in Amazon with complete deforestation. The major claimed contribution is the use of realistic crop

modules, compared with the previously used grassland PTFs. The other two reviewers have provided indepth comments on various aspects of the study. I want to comment more on the aspects related to crop model, land cover scenario and seasonal scale variation (esp. in hydrological variables). In general, I concur with the other two reviewers to recommend major revision for this work before it goes to the next stage.

First, since the major contribution of this work is the use of realistic crop models, there is a need of validation for the performance of the model, which is missing here. Ideal situation is to use the flux-tower based measurements to validate the crop module, or use some country- or provincial-level yield data to validate. Another alternative would be to use large-scale GPP data (e.g. from spaceborne fluorescence) for the existing crop fields. Without the validation, this claimed novelty is rather weak.

[RR] As stated previously, the goal of the new tropical crops was to have a more realistic physical seasonality (planting, phenology and harvest timing). Comparing to the Sacks (2010) data set, this goal was achieved. In-situ measurements over tropical croplands are not readily available, and in our opinion, measurements of such for a managed land cover region can depend on many factors including how densely crops are planted. Single site validation might not provide a useful comparison.

Second, I don't see any reason for a complete deforestation scenario here. I understand that's what people did before in their literature, but if the authors tried to propose more realistic scenarios, they should follow the type of work by Soares-Filho et al. (Nature, 2006) with more realistic landcover projections. The authors provided little rationale for why the scenario is more "realistic". Besides, why irrigation should be considered is also unclear for me. There is little evidence that Amazon agriculture are using irrigation even for now. The whole "realistic" scenario (Fig. 2) is very confusing and lack of justification.

[RR] The simulation is more realistic in the sense that if total deforestation did occur, heterogeneity of the land-surface would be present, not homogeneous bare soil or grassland as used in previous total deforestation modeling studies. As reviewer 2 had stated, irrigation is used in the southern portion of the country. In a future climate that is drier due to total deforestation, it is realistic to expect that irrigation would be needed to supply water to crops that can no longer receive enough moisture through precipitation. A subsequent paper (J. Climate, submitted) examines the impact of intermediate levels of deforestation including variations in its spatial distribution. In particular, we investigate whether the climate demonstrates non-linear responses to progressive deforestation, which requires assessing the extremes. This paper, in addition to the stated goals here, sets the stage for that investigation.

Third, the authors used CESM with the CLM as the land surface model. CLM model has a well unknown issue that it could not well reproduce the ET seasonality in wet Amazon regions. Some possible mechanisms are missing (e.g. groundwater storage). I suggest the authors also look at the seasonal-scale change in hydrological variables (e.g. ET, precipitation, Bowen Ratio) before/after deforestation. It should be fine that the work won't resolve the CLM issue, but it would be useful to include the analysis for seasonal variations.

[RR] The reviewer is correct about the stated model bias. However, this is a bias that would be similar in both simulations, meaning its impact would be reduced when taking the difference between the two simulations with the same model. As also recommended by reviewers 1 and 2, a section on evaporative fraction has been added to discuss seasonal scale changes in hydrological variables, which gets at the issue of ET seasonality in CLM.