Response to reviewer’s comments

We deeply thank the two reviewers for their time and helpful, important comments. Below is our point-to-point response to each comment. The original comments are in bold, our responses are in regular font and relevant changes in text are in italic font.

Anonymous Referee #1
Received and published: 30 October 2015
Dear Authors,
I congratulate you to this paper which elegantly addresses many aspects of deforestation-induced climate change such as local versus remote effects of defor-estation, the nonlinear dependence of the magnitude of change on the scale of defor- estation, a decomposition of the total change into contributions from the three most im-portant biophysical factors, and an explanation of the latitudinal change signal in terms of background climate conditions. I only have some minor suggestions, questions and technical corrections.

Thanks for your favorable comments.

p1899l20–23 “Further analysis [. . .]”: I do not really understand this sentence. Surely, it can be written more intelligibly.

This sentence has been revised:
“Our analysis interestingly reveals that the latitudinal temperature change largely results from the climate conditions in which deforestation occurs, and is less influenced by the magnitude of individual biophysical changes (albedo, roughness, and evapotranspiration efficiency).”

p1903l3–6 So VEGAS does calculate a land surface albedo. Why don’t you use these data directly? Are the albedo changes calculated by VEGAS used as is, I mean, don’t they need to be adjusted (rescaled maybe) to the corresponding satellite observations?

Yes, VEGAS does calculate vegetation albedo (Av), by using a simple empirical formula as a function of LAI:

$$A_v = A_{min} + (A_{max} - A_{min})\exp(-k\,L)$$

where Amin = 0.1 and Amax = 0.45 are the minimum and maximum albedo, respectively, and k = 0.5 is the light extinction coefficient. Here is a detailed explanation on this issue provided by (Zeng & Yoon, 2009): “This simple empirical formula is not sufficient at capturing all the possible processes responsible for the observed albedo, many of which are difficult to model mechanistically at present. For instance, bright deserts with high albedo values often correspond to sand dunes or dry lake beds whose formations are also related to other hydrogeological processes [Knorr and Schnitzler, 2006]. To minimize potential climate drift due to full coupling, only the anomalies A_v' (changes in A_v relative to a control run) are used by the atmospheric radiation module, i.e., the changes in A_v was added onto the observed surface albedo climatology in order to capture the first-order effects due to vegetation change”

$$A = A_{obs} + A_v'$$

And the following explanation words has been added to the text:
"*Vegetation-albedo feedback is treated in the model by introducing albedo anomalies, that is, adding the albedo change due to vegetation change calculated by VEGAS based on a empirical formula as a function of leaf area index, onto the observed albedo climatology used by the atmospheric radiation module (Zeng & Yoon, 2009). This albedo anomalies treatment is designed to capture the first-order effects of albedo change due to vegetation change, since capturing all the possible factors that are responsible for the observed albedo are difficult to model mechanistically.*"

p1903l25–27 Please briefly discuss the limitations this entails. For example, would your results change much if you used a perpetual 30-year (1960–1990) cycle of SST observations? Please also state which SST data you used.

SST data are from HadSST and we have revised the sentence to include this information.
"The model is driven by a climatological seasonal cycle of SST (HadSST, (Rayner et al., 2006)), averaged for 1960-1990 to smooth the influence of inter-annual climate variability."

We also added the following discussion to the choice of SST climatology.
"In the simulation, we used the SST climatology of 1960-1990 with seasonal cycle only that can minimize inter-annual variability and therefore amplify the strength of deforestation signal to climate variability in terms of statistical significance. If a different period of the SST climatology had been used, the simulated climate may have been slightly different including differences in vegetation distribution and deforestation impacts. Nevertheless, our results are unlikely to be substantially changed by the choice of SST climatology, because a background climate change as large as that coming from 1× CO_2 increased to 2×CO_2 can only modify the climate impact over certain transitional regions (Pitman et al., 2011)."

p1904l3–5 This part is not well written. Please polish. Also, I think you should include some observational precipitation data for comparison in Fig. S2. I mean, you allude to the possibly detrimental impact of precipitation biases on the quality of simulated PFT distributions but then it appears as if you tried to get away from this issue as quickly as possible. Please address the issue briefly but properly.

The idea of comparing the simulated precipitation with observation data is very needed for many cases but it may not very necessary in our case because there are issues for such comparison.

First, bias in simulated climate is expected for a model with intermediate complexity. Therefore, the bias, if any, is tolerable for our experiment because we mainly focus on the climate response to vegetation change as well as its mechanisms rather than accurately reproducing historical or future climate change. Second, what we designed is an idealized experiment, for example, using preindustrial CO_2 and seasonal SST data. Strictly speaking, the simulated climate is not comparable to the observed climate in the real world. Due to these fundamental differences, a direct comparison with observations could be problematic and give little help to the paper.

We added above explanation to the text:
"The vegetation map generally has a reasonable geographical distribution but does not perfectly match modern vegetation of the real world. This is expected because the potential vegetation is derived from an equilibrium state with climate, thus any differences in the simulated climate compared to modern climate or any simulation bias, for example in precipitation (Figure S2) could influence the vegetation distribution. In addition, some bias in simulated climate is expected for a model with intermediate complexity, and that is considered to be tolerable for our experiment because we mainly focus on the climate response to vegetation change as well as its mechanisms rather than accurately reproducing historical or future climate change."

p1904l10–12 What would happen if you replaced the forest by grass? Wouldn't that be the more realistic change? Please at least briefly address this point in the discussion section.
Replacing forest by grass or crop is also a common practice for deforestation experiment in the literature. Compared to the forest-to-bare conversion, the conversion to grass/crop often leads to smaller biophysical changes in albedo and roughness, thus it is expected to have similar but smaller impact.

We added a new discussion to the text for this issue.
"An alternative strategy of implementing deforestation experiment is to replace trees with grass (crop) as it is more "realistic" than replacing trees with bare ground (Davin & de Noblet-Ducoudré, 2010). The conversion of trees to grasses is expected to induce similar impact on climate but should be less pronounced (Gibbard et al., 2005), because the resulting biophysical changes (e.g., albedo, transpiration) are smaller compared to the conversion to bare ground, therefore, the sharp contrast between forest and bare ground is indicative of the maximum impact of deforestation. In fact, both strategies are frequently used in existing literature to represent deforestation and they yield consistent findings as the operating mechanisms and feedbacks are the same."

p1906l6–7 Polish your English here, please.
We revised the text:
"The deforestation impact in the simulation is a very strong signal relative to the small inter-annual variability, making almost all changes over the land statistically significant. For this reason, significance levels are not shown on the map."

p1906l21 Precipitation in W/m2: Please use a more common unit throughout the manuscript or specify the equivalent of 1 W/m2 in a more common unit such as mm/day at first mention.
Thanks for this suggestion. In the revision, we have changed the unit of precipitation to mm/day for all relevant texts, tables and figures.

p1908l2–3 "despite different spatial scales": I don't understand. . .

We revised this sentence to:
"Overall, an amplified temperature change in the global deforestation experiment is expected as it generates a stronger perturbation to the atmosphere, but the latitudinal temperature response is well preserved despite the spatial extent of deforestation increases from regional to global level."

p1908l14–16 Please write this more clearly.
We revise the sentence and provide an additional Figure S6 (using albedo change as a example) to show the non-linearity of the temperature response to deforestation can either arise from the response of biophysical land parameters to deforestation or from the climate response (i.e., temperature response) to biophysical changes.

Relevant changes in text are:
"This nonlinearity can either arise from the response of biophysical land parameters to deforestation or from the climate response (i.e., temperature response) to biophysical changes. We found nonlinearities in both of these aspects (Figure S6)."

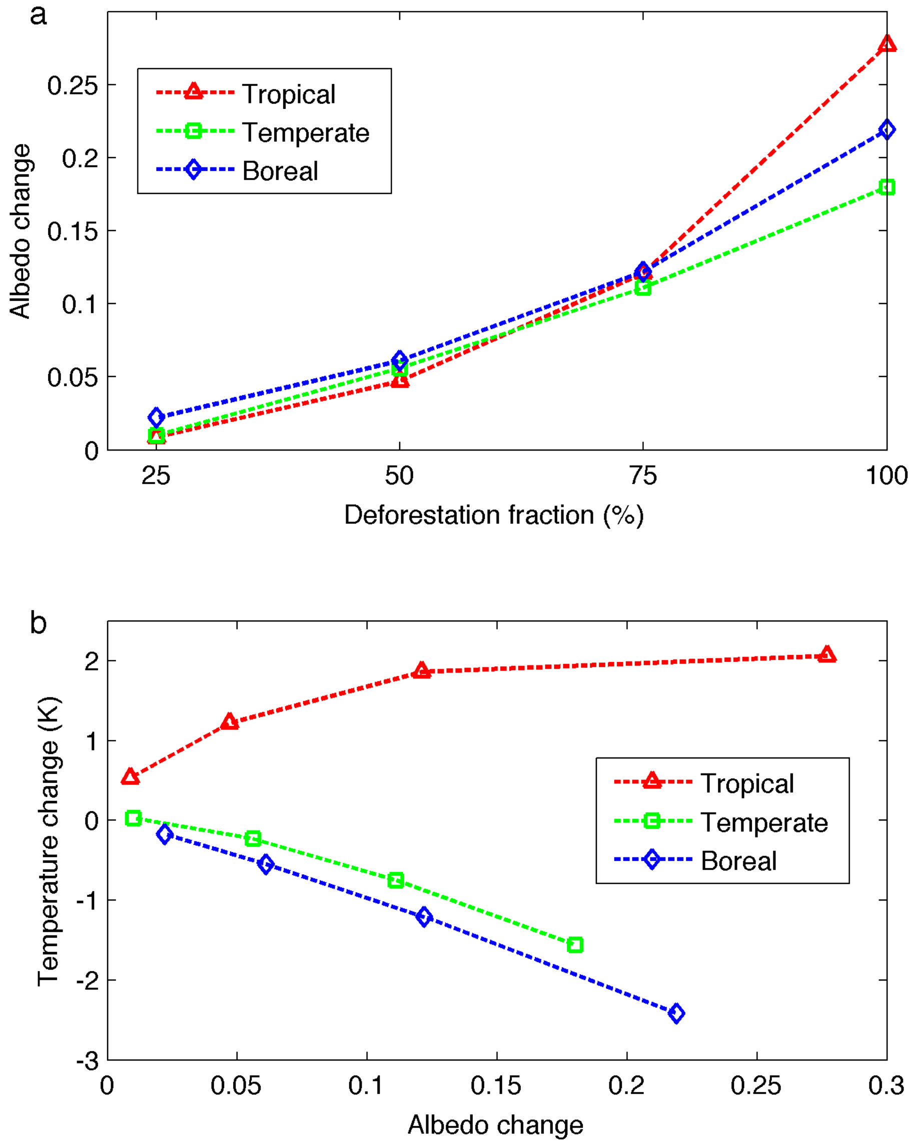

Figure S6 (a) Response of albedo change to growing deforestation fraction from 25% to 100% and (b) temperature response to albedo change under different deforestation fractions. Data points in the figure are from Table 3.

p1910l1 "when ΔH is considered": I suggest to refer to Tab. 2 once again, here.
We have taken this suggestion in the revision.

p1911l11 Shouldn't you better specify the albedo changes in percent, just as you do
for ET?!

Here we want to demonstrate that a given percentage of change in shortwave radiation and ET can lead to the latitudinal pattern in ΔSW and ΔET as a result of background climate. So we use background incoming shortwave radiation and ET for the calculation, multiplied by the percent of change, expressed by albedo and ET reduction rate respectively. In fact, the absolute albedo change itself denotes shortwave change in percent, which is essentially similar to ET reduction rate.

In this calculation we assume changes in absorbed shortwave radiation at surface are solely induced by albedo change. In this case, changes in absorbed shortwave radiation (ΔSW) are:

$\Delta SW=(SW1\downarrow - SW1\uparrow)-(SW0\downarrow - SW0\uparrow)$

"0": before change; "1": after change

And albedo by definition is given by $alb=SW\uparrow/SW\downarrow$

Since there is no change in SW↓ by assumption, $SW1\downarrow = SW0\downarrow$

Therefore,

$\Delta SW=SW0\uparrow - SW1\uparrow = alb0*SW0\downarrow - alb1*SW1\downarrow = (alb0-alb1)*SW0\downarrow$.

This equation indicates that changes in absorbed shortwave radiation (ΔSW) can be calculated by background SW multiplied by albedo change.

p1913l10–12 "[. . .] in the tropical region (Table 4) where its effect on climate can be isolated [. . .]": This holds everywhere, not just in tropical regions, right? Please rephrase.

Thank you for pointing this issue, the sentence has been revised as:

"Effect of roughness on climate can be isolated by the difference All – noRGH. Roughness change as well as its impact are more pronounced in the tropical region (Table 4)."

p1913l16–18 I guess you mean that even if deforestation was not associated with rough- ness change, some parts of the tropics would warm because the reduction in evapotranspiration efficiency would still outweigh the albedo impact in those parts. I don't know how clear you will find this statement but you should definitly rephrase your version.

Yes, that is exactly what it means. We revised the sentence to explain it more clearly:

"Moreover, Figure 8b also shows the combined effects from albedo and evapotranspiration efficiency since roughness effect is excluded. Thus, the existence of a tropical warming in some regions implies that the reduction in evapotranspiration efficiency remains dominant and outweighs the albedo impact in this situation."

p1913l23 "Lower ET" → okay "and higher sensible heat" → not necessarily as you show in Tab. 2.

The original sentence has been revised as:

"The conversion of forest to bare land favors more turbulence energy to be transferred in the form of sensible heat rather than ET, resulting in higher Bowen ratio."

p1914l22 "perhaps": You don't need to be so cautious here, do you; doesn't Sect. 3.4 strongly support this statement?

This is done as suggested.

Tab. 2 The caption is not entirely precise. I assume the Δ values of a column refer to averages over the respective latitudinal band specified in the top row? Are these land surface variables? (These two questions also pertain to Tab. 3.) What exactly are the turbulent flux and the available energy?

Caption of Table 2 has been revised to clarify these issues and turbulent flux and the available energy are explained in the table. Similar changes are also made to Table 3.

Revised caption for Table 2:
"Table 2 Changes in key climate variables from regional and global deforestation experiments. "Δ" denotes change relative to the control experiment and value for each climate variable is the area-weighed changes over deforested areas for different latitude zones. The symbol "↑" denotes upward and "↓" denotes downward. Units are W/m2 for energy flux, K for temperature, mm/day for precipitation, and unitless for albedo."

Revised caption for Table 3:
"Table 3 Changes in key climate variables from global deforestation with different deforestation fractions. "Δ" denotes change relative to the control experiment and value for each climate variable is the area-weighed changes over deforested areas for different latitude zones. The symbol "↑" denotes upward and "↓" denotes downward. Units are W/m2 for energy flux, K for temperature, mm/day for precipitation, and unitless for albedo."

Moreover, I wonder why there is a difference in Δalbedo between regional and global deforestation scenario runs. Where does this come from?
Yes, there is slight difference for Δalbedo between regional and global deforestation experiment over the same region. This difference is very small indeed, ~0.01 or less, see Table below. And we have changed the unit for Δalbedo in Table 2 and 3 from percentage to absolute change and redo the rounding.

Surface Albedo change	Regional Deforestation	Global deforestation	Difference
Tropical	0.264418	0.276663	-0.0122
Temperate	0.169792	0.179784	-0.0100
Boreal	0.217404	0.218868	-0.0015

We are not sure about the exact reason for this difference. We list some possible causes.
(1) Rounding error.
(2) ΔAlbedo induced by vegetation change is the same between regional and global deforestation experiment before it is passed to atmosphere model for radiation calculation. There might be some other processes (not surface albedo) that lead to the slight differences in the climate response between regional and global deforestation experiments that can also influence shortwave radiation (e.g., relating to scale extent). Because albedo shown in the table is calculated by SW↑/SW↓, therefore, a tiny

difference in SW↑ or SW↓ between regional and global deforestation experiment can result in a difference in Δalbedo.

Fig. 6 Why do you show the solid curves for all four deforestation fractions? If I understand the corresponding part in the text correctly, then the curves are just scaled versions of one and the same curve in all subplots, so it would suffice to show only subplot (d). (Okay, I see that the relative changes of ET and SW differ from fraction to fraction. Well, your choice whether to leave it as is or not.)

Four subplots in Fig. 6 show the calculated ΔSW and ΔET with different combinations of ET reduction rate and albedo change. We want to retain these four calculated scenarios because they indicate the good correspondence between calculated and simulated ΔSW and ΔET are not coincidence from a single parameter combination.

We revised caption of Fig 6.
"Figure 6 The latitudinal pattern of ΔSW and ΔET calculated by multiplying their background climate values with different rates for albedo (red number, from 0.02 in a to 0.23 in d) and ET changes (blue number, from -15% in a to -75% in d). In (d), dashed lines are simulated changes from global deforestation in comparison for the calculated changes (solid line)."

2 Technical corrections
p1902l18–23 Please explain all the abbreviations in the model names.
Done as suggested.

p1913l2 does → did
Done as suggested.

p1917l7 Could you please give a reference for LUMIP.
We Added the website for LUMIP (https://cmip.ucar.edu/lumip), since there hasn't been any papers published yet.

Fig. 4a Please append a ↓ to ΔLW in the figure legend.
It has been corrected.

Fig. 5 Where you write (e, f) in the caption I guess you mean (d–f).
It has been corrected.

Fig. S2 Wrong unit, I suppose. Fig. S5 Unit missing.
We changed unit for Fig. S2 to mm/day.
And we added unit (m) into caption for Fig. S5.

Moreover, in all map plots, the grid cells seem to be shifted relative to the coastlines by one or at least half a grid cell (certainly for the longitudes, maybe also for the latitudes). Please fix that.
Thanks for pointing out this latitude shift issue. We replace all spatial figures with this issue solved in the revision (see these figures in the revised manuscript).