
Response to reviewers of the paper entitled
"Importance of surface roughness for the local biogeophysical effects of deforestation"
Ref.: 2018JD030127

We would thank all three reviewers for the time they devoted on reviewing the manuscript, and for their helpful comments.

Below are the reviewers comments (*bold italic font*) and our responses to each point (normal font). All line numbers that we provide here refer to the version of the revised manuscript in which track changes are not shown.

Reviewer #1:

I could basically write two reviews of this paper: one would be accept as is, another would return the manuscript to the authors for major revisions. On balance, I am going to recommend the second of these because if this paper is published it may lead to quite a major impact on how the community explores these issues moving forward and we do not want to publish a paper that does this, but is technically in error. So, I want to see a higher level of evidence in support of these results. On the positive, this paper is well written, nicely illustrated, succinct and polished. It presents a coherent argument, backed by a sound methodology. The analyses are self-contained and I cannot see significant issues in the paper.

We appreciate that the reviewer finds our manuscript well written and backed by a sound methodology, and that the reviewer thinks that the paper may have a major impact on how the community explores these issues.

1. *On the other hand, this basically scaffolds on trusting the model - a course resolution climate model using one land surface model and perhaps critically a land surface models that has not been part of major model intercomparison projects so far as I know - so it did not do PLUMBER (see Best et al. 2015, 10.1175/JHM-D-15-0171.1) and I do not recall it as part of other major intercomparisons. One could "explain away" this paper by asserting that the behavior in the model is anomalous and there is a problem with JSBACH (I assume this is the land model) or the way the land and atmosphere are coupled (again, I do not recall this model being used in land-atmosphere coupling experiments like GLACE-CMIP5 (Lorenz, doi:10.1002/2015JD024053). There may be a counter argument to this - Berg et al., 2016, DOI: 10.1038/NCLIMATE3029 did use the MPI-ESM and it does not demonstrate odd behavior. But, on balance big claims need a high level of proof and the fundamental result here is new and points to the potential that many researchers have misunderstood how land cover change impacts the surface.*

We agree that big claims need a high level of proof, and so it's crucial to also assess the plausibility of the model with respect to comparison to other models and observations. In the main text, we now mention that the MPI-ESM (or its land surface component JSBACH) was evaluated with respect to key characteristics that are essential in representing the deforestation effects, including surface albedo (Boisier et al., 2013; Brovkin et al., 2013b), hydrology (Zhang et al., 2017; Loew et al., 2013) and evapotranspiration (Boisier et al., 2014), soil moisture–climate feedbacks (Seneviratne et al., 2013; Berg et al., 2016). In addition, previous studies compared the biogeophysical effects of land cover change in the MPI-ESM to other models (Pitman et al., 2009; de Noblet-Ducoudré et al., 2012; Brovkin et al., 2013a) and satellite-based observations (Duveiller et al., 2018a). The plausibility of the local

biogeophysical effects on surface temperature from the model with respect to observation-based data-sets is discussed where we refer to Fig. 2 and Figs. S3-S7.

2. *The notion that surface roughness is important is not new:*

Sud, Y. C., and W. E. Smith, 1985: The influence of surface roughness of deserts on the July circulation A numerical study. Bound.Layer Meteor., 33, 15-49.

Sud, Y.C., Shukla, J. and Mintz, Y., 1988, Influence of land surface roughness on atmospheric circulation and precipitation: A sensitivity study with a general circulation model, J.Appl. Meteorol., 27, 1036-1054.

and a result similar to yours was identified by Polcher and Laval in 1995 (see J. Atmospheric Science) where the largest sensitivity to deforestation was linked to roughness.

We now added references to these studies.

We now also refer to the study by Polcher (1995) as one of the studies that contain simulations in which single surface properties are switched from forest to grass values. If we interpret this study correctly, they do not investigate in detail the link between changes in surface roughness and changes in surface temperature, but instead they focus on the link between changes in sensible heat (which indeed may be a consequence of changes in roughness) and deep convective events. From their Table 1, it is difficult to see whether or not surface roughness is more important than the other surface properties, and the results in their Table 1 are difficult to interpret because their sensitivity simulations span only 1 year. To avoid confusion, we do not discuss this issue in our main text.

3. *So, I would suggest several major modifications:*

(a) embed this paper more thoroughly in the literature. I do not mean just those noted above, but roughness is not a new concept and it has been commonly disentangled in tropical deforestation experiments - there is a large literature on this. A starting point might be Badger and Dirmeyer (10.1007/s00382-015-2752-5).

In the introduction, we now refer to more studies that highlight the importance of only changing surface roughness. We also refer to the study by Badger and Dirmeyer (2016). However, we would like to stress that our point is not simply that roughness is important in the context of deforestation, but that it is more important that other surface properties changing with deforestation.

4. *(b) Provide a through exposition on how well your modeling system captures the impact of deforestation. Single column runs perhaps, or paired fluxnet sites that demonstrate your modeling system is sound. I do have some concerns -the experimental design is clever, but it is based on making all vegetated surfaces forest, and then removing the forest. That is a major kick to the system. Figure S2 hints at too strong a cooling impact over the northern hemisphere (although its similar to Bright, its anomalous compared with other estimates shown in Figure S2). Figure S3 again shows a very strong responses relative to other estimates, so does Figure S4.*

I should be clear, I am not so much doubting your results - rather I want to know if your results are the result of something unusual in your model and given the results have the potential to drive the community down a specific path I think you need a very thorough and high level demonstration that your model is reasonable.

The reviewer raises two concerns: 1) That it is not sufficiently clear from the text how well the MPI-ESM captures the impact of deforestation, and that the model is anomalous especially compared with the satellite-based estimates. 2) That simulating the difference between a forest and a grass world is a major kick in the system.

Concerning 1), it is challenging to properly assess that the model captures all relevant processes correctly. We now argue that the model response is in reasonable *qualitative* agreement with the observations concerning local effects on surface temperature (right sign of mean change in most regions, amplification of diurnal temperature range, seasonality), and this makes it at least plausible that the most relevant processes are adequately represented in the MPI-ESM. Indeed the simulated response in the model is at the lower end of the satellite-based estimates. We now argue in lines 328-330 that the cloud bias that is inherent in the satellite-based estimates may lead to an over-estimation of their local effects (Li et al., 2015).

Concerning 2), we agree that a large-scale change in forest cover could cause a large perturbation of the climate system. However, at least for the MPI-ESM a large part of these changes is included in the nonlocal effects (Winckler et al., 2018) that are subtracted in our methodology. The local effects within a deforested grid cell, which are the focus of this manuscript, are largely insensitive of how many grid boxes are deforested elsewhere (Winckler et al., 2017).

5. *I would also comment that the design - changing all vegetation to forest and then converting this to grass may have unintended consequences. So, the text around lines 327-244 may relate to how you force a transition from tundra to forest and then grass (for example). I am not sure of the consequences of this - and you do not tease out how uncertain your results are to how you impose the methodology.*

In our methodology, we compare climate in two different simulations: One 'forest world' simulation, and one simulation where surface properties are switched to grass values in 3 of 4 grid boxes. We do not go from tundra to forest and then grass within one simulation, we are sorry if there was a misunderstanding. We are not sure what possible 'unintended consequences' the reviewer has in mind. If the reviewer refers to possible large-scale climatic changes due to the large-scale changes in forest areas between the two simulations, we argue that these are not part of what we derive as local effects, see previous comment.

6. *(c) A lot of the analysis here depends on how albedo and roughness actually changed over the model grid squares. Obviously, if a modeller changes forest to grass and it happens that this does not change albedo, but does change roughness then the former will not have any impact and the latter will dominate. So, how were albedo changed and how did roughness change? There are observational data sets of both. You could evaluate your albedo changes with MODIS and estimate a roughness change from the new vegetation height data. You clearly need to demonstrate that how you perturbed these parameters is reasonable.*

We now provide values and a map of deforestation-induced changes in surface albedo in the MPI-ESM (Tab. 1 and Fig. S1) as well as values for changes in surface roughness (Tab. 1). We also cite a study that compared land-use-induced changes in surface albedo between models and observations (Boisier et al., 2013). In their study, they indeed show that the change in surface albedo in the MPI-ESM is on the lower range of the investigated models. We now discuss this in the main text (lines 472-475) and acknowledge that our conclusion, that surface roughness may be more important for the local effects than surface albedo, may depend on the employed climate model.

7. *(d) Line 380-82. This is surely testable with existing data?*

We now also provide a supplementary figure on the combined local plus nonlocal effects (Fig. S9) and discuss in the main text that in agreement with previous studies (e.g., Davin and de Noblet-

Ducoudré, 2010), surface albedo dominates the biogeophysical effects while, as we show in this study, the surface roughness dominates the local biogeophysical effects in the MPI-ESM.

Minor comments

8. *Line 60 - remote changes - these remain uncertain and may be artifacts of how modellers have used statistics. See Lorenz et al., doi:10.1002/2015JD024357. This just needs a little editing.*

Thanks, reference added.

9. *Line 63 - the statement local effects are "largely independent of the spatial extent and location of deforestation" is defended by reference to the authors own paper - but there is plenty of contrary evidence and this statement is not balanced.*

We are sorry if this was a misunderstanding. Indeed, previous studies showed that the local effects are different in different grid boxes. Furthermore, a lot of previous studies that the total (local plus nonlocal) effects strongly depend on the extent of deforestation, but this dependence originates mainly from the nonlocal effects (Winckler et al., 2018). Here, we meant to refer to the local effects within a given grid box. To prevent such a misunderstanding, we now state in line 67 that the local effects *within a grid cell* are largely independent of the extent and location of deforestation *elsewhere* (Winckler et al., 2017).

10. *Line 204- why focus on zonal plots? I thought the supplementary maps were more useful. I accept this is a value judgment but I am not sure the zonal maps really communicate your results well.*

We thank the reviewer for this suggestion. We prefer to show the zonal means in the main text because the observations are a bit 'patchy' in some regions (e.g. Fig. S3, boreal regions in 'Alkama'). We think that our main conclusion from this part of the manuscript, that the local effects on surface temperature in the MPI-ESM at least qualitatively largely agree with the observations and that a large share of the local effects in the model can be explained by only changing surface roughness, can be conveyed well by Fig. 2 in its current form.

11. *Line 287 - the figure above this - the legend says MPI-ESM results are zonally averaged where there is at least 1 data set available - this was not clear to me. How the zonal figures were actually constructed is not reproducible from the text.*

We rephrased this sentence. The areas that are used for the zonal means in Fig. 2 can also be seen in the maps e.g. in Fig. S3, from which only the non-gray grid boxes in the MPI-ESM maps are used for the zonal averages. 'The zonal averages of MPI-ESM simulation results shown here are obtained exclusively from locations in the zonal band where at least one of the four observation-based data-sets provides a value. A separate comparison of the MPI-ESM results to each of the four data-sets is shown in Fig. S7.'

12. *Line 352 - I think there is a little poetic licence here - these authors use evapotranspiration for convenience noting the impact on sensible heat. Again, just a little finesse of the English is all that is required. At line 360-62 is a similar issue ... these analyses commonly use partitioning or evaporative fraction and so while I think the point made is legitimate it is a little one sided.*

We restructured the respective paragraph and rephrased these sentences.

13. *Line 358 "mere plausibility" is a little strong. And the authors should be careful not to through an accusation of "mere plausibility" at others when their own text (373-377) sounds very much like "mere plausibility. I think this is English ... for the authors benefit the term is somewhat derogatory and I doubt it was intended.*

We are sorry if this sentence came across as an accusation, which was indeed not at all what we intended to convey. We removed the 'mere plausibility' term.

14. *Line 421-423 - this is true in your model. I think it is important to reinforce this is true of your model, but not necessarily the real world. Similarly, line 429-432, this is true in the model.*

We now added a 'in the MPI-ESM' at the respective lines to clarify we are referring to the processes in this particular model.

Reviewer #2

15. *This study presented some novel and interesting findings of the large contribution of surface roughness changes in the local temperature effects of deforestation, unlike previous modeling studies in which the local and non-local effects are mixed. The manuscript is well-written and the results are well-supported. I only have some comments listed below:*

We are happy that the reviewer finds some of our findings novel and interesting, and that the reviewer considers the manuscript to be well-written and the results well-supported.

16. *L100: The paper by (Li 2016) also used the same method to separate the contribution of roughness.*

We are sorry that we forgot this reference and we now include it. Thanks!

17. *L130: The paper by (Liao 2018) also found the important role of 'aerodynamic resistance term' which supports the finding of this study although they argued that Lee's method overestimated roughness contribution.*

We now included this reference and state that the roughness an bowen ratio term are not independent of each other (Rigden and Li, 2017; Liao et al., 2018).

18. *L243: How is this method different from the other decomposition methods mentioned in the introduction?*

We now state that the surface energy balance decomposition is a tool to diagnose which changes in energy balance components are responsible for a given change in surface temperature (line 305). On the other hand, the IBPM/TRM methods from the introduction aim at identifying surface properties that are responsible. So the different methods concern different boxes in the yellow part of Fig. 1 in the main text.

19. *Section 2.1: Deforestation scenario can be implemented either as from forest to grass, or from forest to bare ground. The latter should give larger changes. How does this affect the conclusion of this study?*

Most studies that we cite investigate transitions between forest and short vegetation, not bare ground, and this is the more likely scenario in real world. Both transitions from forests to managed grass and from forest to croplands are important because they cover large areas (3.1-8.3 mio km² and 7.9-10.8 mio km², respectively (Tab. 1 SOM in Luyssaert et al. (2014)). Lee et al. (2011) considers the difference between forest and grass, and Bright has all transitions – we pick grass because many models do not have a separate crop plant functional type but use grass instead.

20. *Similarly, the nonlocal effects increase with spatial extent while local effects are not much affected. How does this affect the roughness contribution? For example, under the 1/4 deforestation case.*

We did not perform simulations where we change only albedo and only surface roughness in one of four grid boxes. However, we think it is plausible that not only the surface temperature response but also the underlying mechanisms within a grid box are largely independent of deforestation elsewhere. E.g., the local effects for affected surface energy balance components within a grid box are largely independent of deforestation elsewhere (compare Figs. 4a vs 4b in Winckler et al. (2017)).

21. *Fig 2. Could you please provide the albedo changes?*

A map of the albedo changes is now provided in Fig. S1. We also discuss in lines 372-375 that the change in surface albedo is on the lower end of a range of climate models and observations (Boisier et al., 2013).

22. *Fig 4: The dashed lines have different colors in different panels*

We now state in the figure caption that the colors of the dashed lines refer to the legend in panel a).

23. *L306: "if all surface energy balance components were kept fixed", how valid is this assumption?*

We now state that the energy balance decomposition method is a tool that allows to diagnose the relative contributions of changes in the surface energy balance components to the realized change in surface temperature (line 305). Of course a change in any of the energy balance components during a running simulation would trigger changes in all other energy balance components, but the energy balance decomposition is only performed as a diagnosis tool after the simulations are finished.

24. *L342: "it" here is not correct.*

Removed, thanks.

25. *L345: The authors could not rule out the possibility that the strong contribution from roughness change might be model-dependent, a feature of the MPI model. This needs to be addressed or acknowledged.*

We now added in some sentences in the Discussion and conclusions that our results refer only to the MPI-ESM, and we added that our conclusions probably depend on the parametrization of surface roughness and surface albedo in the respective model (lines 467-476).

26. *L380: In Fig S7, it is surprising that for the non-local effects, the albedo contribution becomes much larger? What is the cause of this? Is that possible it is an outcome of the experiment design, because at the deforested grids, the local effects are the residue of "all" effects subtracted by the non-local effects, this may effectively take away the albedo effects from the "local effect" of the deforested grids and unintentionally assign it to non-local effects somehow, as the albedo and roughness changes are referred to as radiative and non-radioactive processes respectively (Davin 2010).*

We agree that the main part of the albedo contribution in the MPI-ESM is included in the nonlocal effects on surface temperature. This was also shown in a previous study (Winckler et al., 2018) where it is shown that within a deforested grid cell, the loss in shortwave net radiation is largely compensated by the latent and sensible heat fluxes, and this leads to cooler and drier air which then also leads to a cooling elsewhere due to a decrease in longwave downward radiation (consistent with the mechanism that was proposed in Davin et al., 2010).

27. *When local and non-local effects are combined, will you get similar results as previous studies that albedo effects are more important? If this is the case, then it can reconcile the seemingly inconsistent conclusions.*

We now provide Figure S9, showing that indeed for the combined local plus nonlocal effects, our results are in agreement with previous studies (e.g., Davin and de Noblet-Ducoudré, 2010) showing that changes in surface albedo dominate the total biogeophysical effects on surface temperature. We now discuss this in line 449.

Reviewer #3 :

28. *The objective of this study was to identify the underlying driver(s) of the local effects of deforestation. Several model simulations were carried out with different parameter settings. Local and non-local effects of deforestation were separated. Simulations were compared to observations. Comparison of different simulations led the authors to conclude that surface roughness largely controls the local effects of deforestation across almost all latitudes, at least in the model. I think that this manuscript has the potential to become an interesting and useful study. However, there are several issues that the issues should address.*

We are happy that the reviewer thinks that this manuscript has the potential to become an interesting and useful study.

General issues include:

29. *The Introduction was problematic. Some key studies were not mentioned. Numerous statements require clarification. Because no research questions were presented, it is not easy to evaluate this study.*

We revised the introduction. As suggested by the reviewer, we included some key studies, clarified some statements and now explicitly state the research question. We provide more detailed answers to the corresponding specific comments (below). We hope the introduction is now more clear.

30. *The authors have shown that a change in roughness is sufficient to explain the simulated temperature changes, but they did not show that it was actually necessary. At the very least, this limitation should be made clear in the text. If possible, an additional simulation should be done with all parameters changed except roughness.*

We now acknowledge that our set-up only allows us to conclude that a change in surface roughness is sufficient to explain the simulated effects on local surface temperature, but we do not show that it is necessary, see also answer to comment 45.

31. *The comparison to observations used different grid cells in the model and observations, and so this comparison is difficult to evaluate.*

We added Fig. S7, showing a consistent comparison between the MPI-ESM and each of the four datasets. We added the following: "The above results are still valid when looking at a one-by-one comparison between the MPI-ESM results and each of the four data-sets and obtaining zonal averages for the MPI-ESM only at locations in the zonal band where the respective observation-based data-set provides a value (Fig. S7)".

32. *The discussion was a bit thin. In particular, further discussion is needed as to why roughness turns out to be a critical parameter.*

We re-structured the first discussion paragraph and discuss more clearly the two points that explain why surface roughness turns out to be a critical parameter. We argue that this is 1) due to our focus on the local effects (while the nonlocal and total effects are dominated by changes in surface albedo (Winckler et al., 2018)), and 2) concerning the local effects, albedo-induced changes in surface net radiation are largely compensated by changes in sensible and latent heat fluxes, while no such compensating mechanism exists for the changes that are locally triggered by changing surface roughness.

Furthermore, we discuss the role of surface roughness for the nighttime cooling, daytime warming and the role in different seasons in the context of previous studies (lines 489-499).

Specific comments:

33. *Line 37: Evapotranspirative efficiency should be defined. Furthermore, I think previous usage has been "evapotranspiration efficiency" (as it is in Figure 1).*

We now provide a short definition of 'evapotranspiration efficiency' (lines 37-38). We now refer everywhere to 'evapotranspiration efficiency', consistent with the paper by Davin and de Noblet-Ducoudré (2010).

34. *Lines 56-57: Momentum advection can also be important. For example, see the work of Jaya Khanna (Khanna and Medvigy 2014, Khanna et al. 2017, Khanna et al. 2018).*

We now mention that momentum advection can also be important and cite these studies (Khanna and Medvigy, 2014; Khanna et al., 2017).

35. *Lines 56-60: When discussing non-local effects, I am surprised that you do not cite the pioneering work of Roni Avissar and collaborators (Avissar and Schmidt 1998, Avissar and Werth 2005).*

We now cite the study by Avissar and Werth (2005).

36. *Lines 62-64: This statement is either incorrect or requires clarification. In Amazonia, the local effect of deforestation is scale-dependent. Again, see the work of Jaya Khanna, Somnath Baidya Roy (Baidya Roy and Avissar 2002), and Roni Avissar.*

We are sorry that our unclear formulation obviously caused a misunderstanding. We only refer to the local effects as described in section 2.3. The sentence now reads: 'In contrast to the nonlocal effects, the local effects on surface temperature within a grid cell are largely independent of the spatial extent and location of deforestation elsewhere (Winckler et al., 2017).'

37. *Lines 70-72: The authors should either re-think or clarify this statement. Consider, for example, two locations that have been deforested. We can call them A and B. In both climate models and observations, deforestation at B exerts a non-local effect on the temperature at A. So I cannot understand how climate models and datasets exclude non-local effects by construction. Unless, of course, carefully prescribed techniques are employed.*

Of course in climate model simulations, both local and nonlocal effects are present and the nonlocal effects are only excluded if focusing on the local effects. We now clarify that this statement (absence of nonlocal effects) in this case refers to the observation-based datasets. In the respective line we cite studies based on satellite observations, that have indeed reported that their methodologies exclude what they called the "atmospheric and oceanic feedbacks" (Li et al., 2015), "second-order effects" (Alkama and Cescatti, 2016), or "large-scale teleconnections" (Duveiller et al., 2018b). This is because in these studies the local effects of deforestation are extracted by comparing changes in climate variables over neighboring forested and deforested areas. Therefore, any nonlocal effect triggered by deforestation elsewhere disappears when temperature differences over these neighboring areas are compared.

Consider, for example, that A is a large forest area, and B and C are forest and grass locations that are next to each other. The deforestation of the large area A could have substantial nonlocal effects on both B and C. However, e.g. the methodology of Li et al. (2015) only considers the temperature difference between B and C, and this temperature difference would be largely unaffected by deforestation of A because temperature of B and C are affected in nearly the same way.

Although it is based on in situ measurements, nonlocal effects are also not in the dataset of Bright et al. (2017), whose authors recognize that the "large-scale climate feedbacks" are excluded.

38. *Lines 74-76: This statement is unsupported, and in fact contradicted by statements made a bit further down in the Introduction. Quite a number of studies, going back to the 1980s, have linked changes in surface properties to changes in temperature.*

We now state that there are two types of studies, one explicitly simulating changes in single surface properties and the other one simulating concurrent changes in all surface properties. In addition, we now also refer to some earlier and more recent studies that change only one surface property (lines 107-114).

39. *Lines 76-82: Again, these ideas are quite old. I suggest citing some of the original papers that first described the ideas, either instead or in addition to, the 2018 study. Just citing a 2018 study give the misleading impression that these ideas are new.*

The study by Duveiller et al. (2018b) refers to the local effects inferred from observations and is in agreement with earlier model-based studies that introduced these ideas. We reformulated this paragraph accordingly.

40. *Lines 91-93: But other studies have, as the authors themselves acknowledge in the next paragraph.*

We clarified that this statement refers to the studies that analyzed changes in the surface energy balance components. The studies that we mention in the next paragraph analyze which surface property was responsible for a change in surface temperature, but these studies did not focus on how a change in one surface property influences the surface energy balance components.

41. *Line 95: Evapotranspiration efficiency itself is not a "particular" surface property; rather, it represents the combined effects of several particular surface properties.*

We now clarify that evapotranspiration efficiency is composed of several surface properties.

42. *Lines 118-120: This is not a novel idea (Dickinson and Henderson-Sellers 1988). Previous work should be better recognized by the authors.*

Thanks for mentioning this study, we apologize for having overlooked it. We now mention that this idea was already brought up in the study by Dickinson and Henderson-Sellers 1988.

43. *Lines 132-134: This statement requires clarification in two ways. First, the relation between "the roughness term" and "surface roughness" is in fact explicitly indicated in the supporting information of Lee et al. (2011); personally, I do not view the Lee et al. relation as being unclear. Second, because the authors speak of climate models in general, the statement is so vague that it is impossible for me to evaluate.*

We added a sentence to clarify in what respect we think the correspondence between the Lee et al. terms and the surface properties in climate models are unclear:

'However, the roughness and bowen ratio terms are not independent of each other (Rigden and Li, 2017; Liao et al., 2018) and thus it is unclear how these terms (e.g. 'roughness term') relate to the surface properties (e.g. 'surface roughness') whose influence was investigated in the climate models. In addition, the IBPM terms may include other interactions between the albedo, bowen ratio and roughness terms. For instance, an isolated change in surface albedo in a model may influence the amount of shortwave net radiation at the surface. This may influence latent and sensible heat fluxes differently (e.g. in a water-limited region) and thus lead to a change also in the bowen ratio, so the 'albedo' term in the study by Lee et al. (2011) (comprising only changes in shortwave net radiation) may not be conceptually identical with the changes that would be triggered by changing surface albedo in a climate model (comprising also changes in latent and sensible heat fluxes).'

We agree that the statement refers to climate models in general, but we think that being 'vague' here is appropriate because we describe the conceptual difference between the two different methods (the IBPM method as in (Lee et al., 2011) vs. the method of changing single surface properties in climate models).

44. *Lines 135-145: The text reads like it is new idea that the roughness term is important. But it is not a new idea. It goes back at least to Dickinson and Henderson-Sellers (1988). Also, more recent work by Jaya Khanna suggests that the "contradiction" can, in part, be explained by the spatial scale of deforestation.*

This paragraph refers to the local effects only. To clarify this and to acknowledge the previous studies, we rephrased the first sentence: 'The importance of the 'roughness term' in the IBPM studies seems to suggest that surface roughness is important not only for the total (local + nonlocal) biogeophysical

deforestation effects Dickinson and Henderson-sellers (e.g., 1988); Sud et al. (e.g., 1988); Khanna and Medvigy (e.g., 2014) but also for the locally induced changes in surface temperature.'

45. *Lines 147-154: This is a reasonable simulation design, but it is incomplete. By carrying out a simulation in which only the roughness parameter is modified, one can draw conclusions about whether a change in roughness is sufficient to produce a particular change in temperature (for example). However, one cannot draw conclusions about whether a change in roughness is necessary to produce a particular change in temperature. To draw conclusions about whether it is necessary, you would need to change all parameters except roughness. Consider carrying out such a simulation if it is not too computationally onerous. At least, recognize this limitation in the Discussion..*

We now acknowledge this limitation in the discussions section. 'Because we do not perform a simulation in which we only change evapotranspiration efficiency, we can only conclude that changes in surface roughness in the MPI-ESM are *sufficient* to explain local changes in surface temperature, but we cannot conclude that they are *necessary*.'

46. *Another problem with this paragraph is that it only states what the authors are going to do. It would be helpful to explicitly describe the research questions associated with the analysis. What I want to know is whether the plan (described by the authors) actually makes sense in terms of the driving research questions (which are not explicitly listed).*

We now explicitly state the research question (which was previously somewhat hidden) and adjusted the last two paragraphs of the introduction accordingly to establish a clear link between our driving questions and the associated analysis. We also slightly changed the formulation of the abstract to clarify which question is addressed in this study.

47. *Line 163: Substantial trends in what?*

We are not sure whether we understand the question. We now clarified that the analyzed variables refer to surface temperature and the surface energy balance components, and that the 'period' means the 200-year analysis period.

48. *Lines 171-176: More details are needed here. First, the parameters should be more precisely defined. For example, when the authors change "surface albedo", are they changing upward shortwave radiation divided by downward shortwave radiation? Or are they changing a leaf optical property like leaf reflectance? The distinction is important; surface albedo depends not only on leaf optical properties, but also the amount of vegetation cover, soil water content, and other factors. In the tropics at least, the fact that grass ecosystems have lower leaf area index than forest ecosystems contributes to the higher albedo of grass ecosystems. Second, the parameters need to be set into the context of model equations. At the very least, references to papers that describe the model's land surface parameterization should be provided. Third, for the purposes of reproducibility, the numerical values of the changed parameters (at least albedo and roughness) should be provided.*

We now provide a short description of how land surface albedo and surface roughness calculated (lines 199-214), provide references to studies containing the respective model equations, and a map showing the change in surface albedo in the MPI-ESM (Fig. S1).

49. *Lines 202-204: I think it would be better to say that non-local effect would be reduced, rather than be absent. For example, Alkama and Cescatti (2016) looked at temperatures before and after transition; in doing so, they do not account for temporal trends in background climate.*

We rephrased these lines in order to better explain why indeed most of the nonlocal effects are not included in the observation-based datasets, as these studies acknowledge themselves. See also comment 37.

50. *Lines 224-225: This method of comparison seems problematic exactly for the reason stated by the authors: each dataset is being averaged over different grid cells. At least in the supplement, a more completely consistent comparison should be presented. That is, when the comparison is with Bright et al., use only those grid cells used by Bright et al. When the comparison is with Li et al., use only those grid cells used by Li et al. And so on. Such dataset-level comparisons are probably worth some discussion in the text.*

We added Fig.S7, showing a consistent comparison between the MPI-ESM and each of the four datasets. We did not add a discussion of this dataset-level comparisons but instead state that this comparison agrees with the results in Fig. 2 that the MPI-ESM results are largely within the range of the four observation-based datasets.

51. *Lines 279-281: Where is this demonstrated?*

We added an explanation that we here refer both to the local deforestation effects on surface temperature and their seasonality, see Fig. 8 in Meier et al. (2018).

52. *Lines 276-286: I think that the text exaggerates the quality of the fit. It is a stretch to imply that the model is "right" (line 282), or even that the model is in "broad agreement with the observations" given that the simulation results are typically at the very lower end of the data products.*

We re-formulated this paragraph to avoid the impression that we think the MPI-ESM is 'right'. For instance, we now state that results of the MPI-ESM are *qualitatively* largely in line with the observations. We now also acknowledge explicitly that an agreement between a model and observations does not allow to judge that all processes are correctly represented in the model (line 352-353).

53. *Lines 407-412: I don't see why this is technically challenging. Start with the "ALL" run, and just change the roughness and the albedo, right?*

This is how previous studies simulated the evapotranspiration efficiency (e.g. Davin and de Noblet-Ducoudré, 2010). We now state more clearly that their approach does not exactly provide the effect of only changing evapotranspiration efficiency but includes also interaction terms with the two other surface properties (Stein and Alpert, 1993).

54. *General comment on the Discussion: I am really missing a more specific description of how model parameters are linked to model equations. For example, it is unclear to me whether the authors really changed the albedo, or whether they changed the leaf reflectance. The authors should at least qualitatively describe what is changed in the model when albedo is changed and when surface roughness is changed. At least references to the full model description should be provided.*

In the methods section we now include a description of how surface albedo and surface roughness were changed, see also comment 48.

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