Ref.: MS. bg-2022-317 Biogeosciences Reconciling different approaches to quantifying land surface temperature impacts of afforestation using satellite observations

Reviewer#1

General consideration:

The manuscript "Reconciling different approaches to quantifying land surface temperature impacts of afforestation using satellite observations" by Wang et al presented thoughtful analyses regarding three different types of temperature effects of forestation that appeared in the literature (potential vs actual) and trying to explain the causes of the different magnitudes. The research is a nice addition to the literature on this topic as it is helpful to clarify the interpretation of different results.

We thank Reviewer#1 for the general positive comments and confirming the additional value of our work to the existing literature. Please find below our detailed responses to the review comments, with original comments in black and our responses in blue.

Major comments:

1. First, I disagree with the authors' interpretation of these results and the claim that the causes of the different estimates are unknown. On the contrary, spatial scale or fractions of forest change matters for interpreting the temperature impact, which has been considered in previous studies. Taking the influential work cited by the authors as an example:

In Alkama 2016, the fraction of forest cover change is explicitly taken into account, and the results clearly indicated that the temperature effect depended on the fraction of change.

In Li 2016, the fractional dependency has been reported: "It should be noted that the estimated impacts also depend on the thresholds used to define forest cover change, as discussed in section 2.2. The sensitivity analysis shows that a higher threshold to define forest change leads to stronger impacts on temperature."

In Duveiller 2018, they used the temperature effect of 100% conversion to avoid the influence of fractional changes.

The strength of this work is that it explicitly addressed this question. Perhaps the authors could consider an alternative title better reflecting this point.

Maybe some improper expressions (very likely they could be lines 25 and 81) in our original manuscript (MS) made the reviewer conclude that we have claimed that “the causes of the different estimates are unknown.” But in fact, we did not explicitly claim this. Instead, we suspected that different forest cover changes in different approaches could potentially influence the LST change, which is one of the motivations for this study.

The reviewer seems to imply that the fraction of forest cover change was known as the cause of different magnitudes of LST change in previous studies. But this is not true. We acknowledge that the fraction of afforestation was mentioned and discussed in previous studies, but none of these studies have explicitly demonstrated that it is a core factor that can reconcile the different approaches until our present work. In addition, the fraction change effect cannot cover the full range of the methodological differences. For example, we are unaware of any studies that have ever verified whether ‘potential’ effect could really be ‘actualized’.

In response to the review comments, we will clarify the following points in the revised manuscript:

(1) We acknowledge that the effect of fraction has been noticed and discussed in previous studies and that we are not the first study to examine this factor. We will emphasize the existing research on forest cover change effect on surface temperature change in the Introduction of the revised MS.
(2) We will also explain that despite afforestation fraction was known to influence surface temperature change, whether it can fully explain the difference among different approaches has not been demonstrated.

(3) We will add a detailed description in the supplementary material to further explain the scope of methodological differences of different approaches, which include but are not limited to the fraction differences.

2. Second, the main finding is that the fraction of forestation (complete vs incomplete) explains the different magnitude of temperature effects. Fraction could indeed have a strong influence on the temperature signal. But it is not the only one. Other factors such as the timing of land cover change, length of the study period, and the spatial extent of forest cover change impact may also contribute.

(1) Taking the timing of de-/forestation as an example, if the change happened in the different years of the two periods of 2002–2004 (t1) and 2010–2014 (t2) (L277), changes in 2002 and 2010 would produce a larger temperature change compared to changes in 2004 and 2014, depending on whether the change signals lasted full three years or just the last year.

Sorry, but we are not completely sure what the reviewer refers to "timing of land cover change" and the example given is ambiguous. We appreciate and would like to provide a specific response if the reviewer gives us a more detailed description. If the reviewer is referring to "timing for a specific season in a given year", this is clearly not our focus because we are considering changes in the mean annual surface temperature.

As for "length of the study period", we admit that it can contribute to the magnitude of temperature effects, but the contribution was thought negligible here. For deforestation, e.g., logging and forest fire, conversion between forest and non-forest could be considered instant at the annual time scale of our research, as are the biophysical impacts induced by deforestation (Liu et al., 2018) (Fig. R1a, R1b). In contrast, afforestation often involves the succession of forests from a sparse canopy to a closed dense canopy until it can be observed by satellite as forest and very likely the change in surface temperature will follow the same pattern until it saturates in the closed-canopy forest (Fig. R1c). Global Forest Change dataset we used here defined forest gain as a stable closed canopy that can be distinguished from a nonforest state (Hansen et al., 2013), which gives us the confidence to conclude that the ΔT signal has a good chance of being saturated. In this case, the ‘length of the study period’ is expected to have little impact on our results.

We will briefly discuss and clarify these points in the Methods and Discussion sections in the revised MS to avoid similar confusion for future readers of the paper.

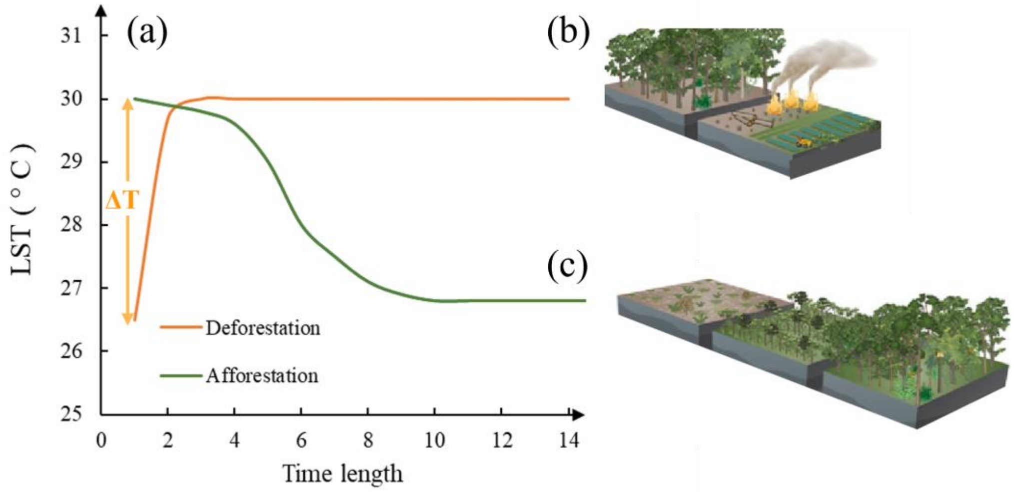

Figure R1. A conceptual scheme diagram showing the land surface temperature change following deforestation or afforestation. (a) Changes in annual mean land surface temperature (LST) following deforestation/afforestation. (b) The deforestation process could be considered as instant consisting of two clearly different stages. (c) Afforestation often leads to gradual forest succession or growth until it can be classified as stable forest cover by satellite data. Here it is represented with a three-stage process.

(2) More importantly, the space-for-time assumption is acceptable, but it is not strictly true in reality. The adjacent two sites did not share the same climate condition (see Chen 2016). This also contributes to the different temperature effects.

We admit that space-for-time is an assumption that cannot be verified on its own, which will inevitably result in uncertainties in the estimated ΔT. But the consistency between ‘potential’ and ‘actual’ effects in our study proves that this assumption is broadly acceptable. These two points will be briefly discussed in the Discussion section in the revised MS.

(3) When the spatial extent of forest change is large, the local and nonlocal temperature effect

appear with heterogeneity which confounds the estimation of the local temperature.

In this study, the temperature effects based on the 'space-for-time', 'space-and-time' and SVD approaches strictly referred to the local effect, without considering any nonlocal effect (Duveiller et al., 2020, 2018; Winckler et al., 2019a). In fact, nonlocal effect is defined as biophysical effects due to changes in wide-ranging atmospheric circulation and advection of heat and moisture, which are triggered by afforestation (Duveiller et al., 2020; Fig. 2 in Pongratz et al., 2021; Fig. 1 in Winckler et al., 2019b). Within a searching window (e.g., 11km×11km in this study), any nonlocal effects cancel out when comparing temperature differences over these neighboring areas since advection and atmospheric circulation have similar effects on adjacent areas (Pongratz et al., 2021; Winckler et al., 2019a). Therefore, the effects derived in this study excluded nonlocal effects.

We will emphasize that biophysical effects here were "local effects" in the Method section in MS to avoid similar confusion for future readers of the paper.

(4) The consistency between the actual and potential effect is also scale-dependent. At small scales (e.g., 10m resolution), it would be easier to achieve full change compared to large scales (1km).

We agree that the realization of the full potential effect is scale-dependent and is more feasible at small scales. This comment is related to specific comments#5 and #7 below, to which we have responded with modified sentences. We will modify these statements in the revised MS.

Third, I feel the language of this manuscript should be improved and polished.

We will improve the language of this manuscript.

Specific comments:

1. L102-103 They may not assume 100% complete ground coverage. They used the defined forest and nonforest in the paper. Of course, due to inherent scaling and the mixed pixel issue

in remote sensing, the defined pixels cannot be 100% pure at a given scale. I think many studies were aware of this issue but they did not explicitly address it.

We revisited the related description in Duveiller et al. (2018) (Page 9): "The expected change in variable y associated with a transition from one vegetation type to another at the central pixel of the local window is then the difference between the y_p predicted for each pure vegetation type." Therefore, theoretically speaking the SVD approach quantified the 'full potential effect' by assuming transitions between land-cover types with 100% complete ground coverage, although pure vegetation type observed from satellite is hard to achieve. We prefer not to modify this statement in the MS.

2. L161-162: How are the afforestation and adjacent control pixels defined?

We will add a sentence in the Methods section in the revised MS to more clearly define afforestation and adjacent control pixels: "Here, pixels with F_{aff} > 0% were defined as afforestation target pixels. A searching window of 11 km×11 km was then built centering on the afforestation pixel. Pixels with F_{aff} =0% within this searching window were defined as control pixels and were used to derive ΔT_{res}."

3. L518: What do you mean "extensive variable"?

We double-checked the concept of "extensive variable" in the existing literature (Scheider and Huisjes, 2019) and determined that this term should not be used here. Therefore, this word will be avoided from the revised MS. We will revise the related sentence in this section (L518) into: "This finding is in line with the fundamental fact that surface temperature at a given scale, can be strongly determined by the area fractions of its different components, with each component having a unique surface temperature, which also served as the theoretical foundation for the SVD technique to derive the full potential effect (Duveiller et al., 2018)."

4. L549 to 551: For this fractional dependency, it has been reported in such as Li 2016

We will cite the results from Li et al. (2016) to further support our research: “This is also consistent with a previous study which documented that these effects depend on the forest cover change thresholds used to define afforestation: the higher the threshold, the stronger is the impact on temperature (Li et al., 2016).”

5. L572-573: The actual and potential effect is also scale-dependent, and so is the feasibility of full afforestation in reality. Fully afforested could be easily achieved for a small pixel 30m. And for this pixel, the potential and actual could be similar following the findings of this work. At larger scales, it is more difficult to become “fully” afforested, which leads to larger differences between potential and actual impacts. Therefore, whether “achieving the full cooling potential” is scale-dependent.

We will revise this section (L572-573) into: “Full afforestation is often possible at small spatial scales, but at large scale it becomes challenging. So the realization of full potential effect by afforestation is scale-dependent.”

6. L581-583: I disagree with the authors on this. The potential effect is useful as it measures the possible outcome of full conversion or mostly afforested (depending on resolution and scale), and whether it is realized depends on the fraction of the change. One can take into account the fractional change to convert the potential effect to more reasonable estimation. At least for this reason, it is not misleading. It is about different interpretation and clarification is needed.

We agree. We will revise this sentence into: “Potential cooling effects have a value in that they can serve to establish the envelope of effects and measure the possible outcome given the condition of full afforestation. However, given the challenge of full afforestation at large spatial scales, potential effects should be converted into a more reasonable estimate (i.e., actual effects) by taking into account the intensity of afforestation, to better represent possible policy ambitions and for the purpose of policy evaluation.”

7. L602 to 605: I don’t agree this statement because both the actual and potential effects are scale dependent. Without mentioning the scale, it is incorrect.

To avoid misunderstanding, we will revise these sentences into: “However, the realization of full potential effect is also scale-dependent. At small scales, full afforestation is more likely to occur, and consequently, potential impacts are more likely to be achieved, while full afforestation at large scale may not always be achievable, making it challenging to reach full potential impacts.”

References used in the responses:

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