Earth System Dynamics Discussions

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Comment on esd-2020-93

Anonymous Referee #3

Referee comment on "Modelled land use and land cover change emissions – a spatio-temporal comparison of different approaches" by Wolfgang A. Obermeier et al., Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2020-93-RC3, 2021

Review of esd-2020-93:

Modelled land use and land cover change emissions - A spatio-temporal comparison of different approaches

Summary

The authors present a detailed analysis of multi-dynamic-ecosystem-model estimates of LULCC emissions under different initial and environmental conditions, with some comparison with recent bookkeeping approaches. A main focus is the contribution of the "Loss of Additional Sink Capacity" (LASC) to emissions, which can be best estimated by dynamic ecosystem models with transient environmental conditions. The authors conclude that LASC is considerable, and has some regional variation. Furthermore, they show that CO2 effects dominate the contributions to LASC over climate effects. They also raise the question of whether LASC is appropriate for a harmonized attribution of LULCC emissions.

Overall review

This is an important contribution to LULCC and carbon/climate science and presents interesting results from the trendy model ensemble. There are a few main issues that need to be addressed, however, prior to publication. These are summarized here, with further detail following:

1) There seems to be a better way to calculate the CO2 vs climate estimates. You have simulations that you can difference directly, rather then using the ratio method that has limitations.

2) The results and discussion of regional LASC are confusing and not to the point. These can be better constrained and made more clear with main points in mind.

3) The suggested "harmonized attribution" of LULCC emissions is not consistent with the paper findings and doesn't differentiate between science and policy. The real issue here is full scientific estimates of emissions, as the authors clearly show require the inclusion of LASC, vs attribution of LULCC emissions for policy purposes, which is a political issue. This paper isn't about harmonizing different model approaches, but rather clarifying and understanding their differences. This provides scientific bases for accurate estimation of emissions, and provides info for political decisions regarding accounting for attribution policy, but doesn't show an answer to the "correct" accounting for policy or whether/how the different approaches should be "harmonized."

Specific comments and suggestions

Abstract

The abstract focuses more on the argument for LASC and DGVMs, than on the results of the study. The argument is better suited for the discussion, with a brief statement in the abstract stating that you use DGVMs to estimate LASC. Then you can add more findings/explanation to the abstract, such as temporal transitions in PTD that support the DGVM vs bookkeeping difference statements.

line 8:

"arbitrary chosen" is unnecessary.

lines 10-11:

Constant environmental forcing does not generate a condition independent of timing and legacy of lulcc. It generates a condition independent of changing environmental conditions, which is how this should be described.

Ensuring that the lulcc magnitude and trajectory are identical across simulations, along with constant environmental forcing, creates a condition almost independent of the timing and legacy of lulcc, as long as the transient dependence isn't the question. Full independence in this case requires that the biogeochemical dynamics are also identical across simulations, such as may be possible when a single model is used. But this is not possible across different models because each model will respond differently to the same

exact lulcc (which is also very difficult to achieve across models), even if the environmental forcing is identical.

I think you mean this in the context of a single-model experiment, with the exact same lulcc and environmental forcing, and not focused on how the LULCC trajectory determines emissions (i.e., path dependence), but this needs to be qualified here as it sounds more general.

lines 21-23:

I am not sure that your analysis calls for this, nor that your approach bridges the two. The issue is a full scientific accounting of emissions vs a partial accounting for policy purposes. While you have evidence for a full scientific accounting, you still have little basis for a partial-LASC approach.

Introduction

lines 28-30:

awkward sentence. delete “e.g.” and use three clauses: deforestation…high lats, tropical deforestation, and recent forest expansion in high lats.

line 31:

"…contributed approximately one-third of global anthropogenic…"

line 35:

delete "in line." then: flulcc may also gain…

line 42:

"…but also change in…"

line 50:

delete "allow to"

lines 69-72:

The language here starts to confuse the definition and example of LASC. It sounds like just having the potential vegetation or the foregone sink is part of LASC, in addition to the difference of environmental effects on the managed vs unmanaged vegetation. These two sentences should be reworded to clarify that "assuming potential vegetation" and "capture

the foregone sinks a given LULCC event destroys" do not obfuscate the definition of LASC.

lines 71-72:

this statement is not correct. there is no guarantee that the change in environmental conditions is increasing LASC at the time of change. so "same direction" does not make sense. I think you are still referring to the example here, but this is a more general concluding statement.

it is more correct to state that these emissions "can" accumulate in the absence of further LULCC, depending on the environmental conditions. which you then give examples of in the next paragraph.

lines 98-99:

I don't agree that constant conditions create a condition independent of lulcc timing. regardless of the forcing, the time since disturbance makes a difference, especially in the context of DGVMs where ecosystems are not static. A subsequent LULCC has a different emission depending on the period between it and the previous LULCC. And an event early in time on 'pristine' land may affect a different biomass then one later in time, even if the conditions are identical. Even with a spun-up model, the biomass may not remain constant during a simulation, and depending on the resolution and application of LULCC, a later LULCC may or may not be applied consistently to 'pristine' or 'managed' area (and corresponding biomass), or some combination of the two.

I think this description should be changed to "independent of environmental trends" or something like this.

line 108:

delete “in line”

lines 109-110:

unclear: “and consequently into the natural land c sink.” how is this related to ‘included or excluded?’

Data and methods

lines171-172 and 178-179 and 194-195:

These really seem like a single equation each.

line 188:

again, this isn't true. timing and legacy of LULCC are inherently critical and determining elements in any transient LULCC simulation. these simulations are indifferent to environmental trends or changing environmental conditions.

line 219:

"…C stock changes…"

lines 220-240:

Why did you do it this way, particularly for climate effects? You can get co2+ndep-related emissions directly by subtracting S0 and S1, which would be interesting to compare with your estimate of co2 only emissions using this ratio method.

And you can get the climate-related emissions directly by subtracting S1 and S2, including interactions with CO2, without using this indirect ratio method. and your assumption regarding zero interactions between climate and co2 is not sound because S2 includes both at the same time, and S1 includes only co2. I suggest you use the actual nbp difference of these 2 sims to estimate the climate effects.

Also, you don't report on the co2+no2 effects at all, so why is this simulation listed in the methods?

Results and discussion

lines 246-247:

unclear: "…to consistently use the same models for the flux and bias estimates on a spatio-temporal level…"

line 254:

did you use the trendyv9 version of sdgvm throughout? if so, you need to state this in the methods.

lines 256-257:

not necessarily. the single year pre-ind estimate is closer to bk estimates, and is equally close to gasser as the present-day for the decade.

line 259:

you haven't shown the cumulative yet

line 260:

awkward. try "…in the 1950's and again at the end of the simulation…"

line 272:

"…comes into play…"

lines 272-273:

awkward first half of sentence.

line 277:

during this period (first half of 20th century)

line 279:

"these peaks"

lines 281-284:

maybe because the LULCC occurrence is the same between simulations. but post-peak carbon stocks might have been lowered enough differentially between the sims to reduce the post-peak difference because carbon reductions are generally implemented as fractions, in which case the same LULCC would reduce more carbon in the pd sim than in the pi sim because of higher carbon stocks in the pd sim. and the eed peak is actually after the other peaks and stretched out. so is this really independent of the lulcc timing, as there is a peak in lulcc area that coincides with this emission peak.

line 293:

post 1950

lines 296-320:

This section is difficult to follow, and it isn't entirely clear what the main point is, other than that LULCC dominates the EED pattern. But more importantly, LULCC seems to drive the fLULCC pattern also, since the EED and the fLULCC patterns also match. temporally, which is does not need a regional breakdown to be shown. And it would appear that the difference in biomass between Pi and Pd is a main driver of the corresponding difference in fLULCC. This boils down to biomass+LULCC controlling fLULCC estimates, with some regional variation in how this relationship contributes to the global estimates.

If the EED pattern is the main point, then take out the other regional plots (5 and 6), as figs 3 and 4 and 7 and 8 show this. The regional plots are difficult to interpret, and so the discussion is helpful, but it should be more clear how the regional patterns contribute to the global patterns, as figure 8 can show (and 7 can show magnitudes better is the scales are matched).

Plots 9-11 are relevant, but they are not used to explain why the EED pattern dominates fLULCC, but they can support the basic relationship between biomass, LULCC trajectory, and fLULCC.

There are too many plots that are too difficult to read, and their referencing is difficult to follow also.

lines 302-303:

The regional comparison plots (figures 5 - 7 and the supplemental) are difficult to compare because they are all on different scales. while it may make it more difficult to see some of the individual lines, putting them on the same scale is the best way to show the differences, which I think is the point.

You can split these plots into multiple figures also because they are so small, which also makes them difficult to read. If that is too many figures, then maybe all the regional plots should be supplemental.

lines 321-352:

This section is difficult to follow, and it isn't clear what the point is of the comparison between LASC and EED. LASC is a component of fLULCC estimation, and EED is a difference between two estimates of fLULCC that don't include LASC. In general, the dynamics discussed here are still driven by biomass+LULLCC, since the forests are the high biomass areas and where LASC will have the most pronounced influence. While the regional breakdown and LASC discussion is somewhat informative, as the LASC dynamics are interesting and a key part of this study and since there is some regional variation, the relationships between LASC and EED seem to still be similar across regions, in that LASC tends to grow over time due to LULCC timing, while EED is driven mainly by biomass differences and the fixed LULCC trajectory. So is the point here to just discuss LASC dynamics, or to support the hypothesis that LASC is critical to fLULCC estimates because static biomass conditions do not include temporal effects of changing conditions (and drive errors associated with these biomass conditions)? I don't think you need the EED stuff here, as your larger point about estimation methods is discussed elsewhere (section 4?).

lines 353-377:

This discussion is a bit more clear as it focuses on LASC dynamics and doesn't mix in references to other calculations. I suggest combining this section and the previous section and focusing just on LASC dynamics. And the last sentence seems less a concluding sentence and more an evidence sentence to support the discussion.

line 383-384:

What about LULCC effects due to abandonment of agricultural land? This also contributes to higher biomass in some places, and isn't trivial. You show some evidence of this in figure 12, where it can outweigh the climatic effects in even the long term, but your net results over the whole period do not show how regrowth has contributed more recently to increased biomass in places where this doesn't outweigh previous emissions.

lines 416-445:

This section seems to be referring to a political question for emission attribution to regions for policy, rather than a scientific question of what are LULCC emissions.

So you should separate these two questions/recommendations. Is your scientific recommendation to discard some LULCC emissions? It doesn't seem to be so as you clearly show that LASC is an important component in "Accurate quantification of the net carbon flux from land use and land cover changes…" (line 447). Whether this makes sense for regional carbon policy is a different question.

From a policy standpoint, the question here is then why include only part of the total emissions from LULCC (i.e. LASC from second half of 21st century) while including some estimate of the directly effects of changes all the way back to 1850? If it isn't ok to include LASC back to 1850, why should any effects of pre-1950 LULCC be included? Why not just start full attribution of emissions at 1950?

liens 467-472:

Here you clearly state that full accounting includes LASC. Then you vaguely backtrack and mention the derived LASC without context, which is the political context of attribution for policy. The “arbitrary length of their simulation period” is a given fact for all modeling and is handled by explicit definition of the period in question. For example, climate change is defined in reference to the “pre-industrial” period (usually pre-1850 equilibrium).

Figures

Your figures and supplemental figures are out of order with respect to references in the text.

Consider using the same scale across the regional comparison plots, both in the paper and in the supplemental.

Appendix A1

lines 484-485:

This statement appears incorrect based on the discussion that follows.

It appears that the variation may arise from models with different ecological and productivity responses to the cycled environmental conditions.