

Interactive comment on “Contributions of secondary forest and nitrogen dynamics to terrestrial carbon uptake” by X. Yang et al.

Anonymous Referee #1

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Comments on “Contribution of secondary forest and nitrogen dynamics to terrestrial carbon uptake” submitted to Biogeosciences Discussions by X. Yang et al.

General comments: In this study, the authors investigated the contribution of secondary forests to the global carbon budget in terms of land use change (LUC) and nitrogen deposition, using a global coupled carbon and nitrogen cycle model (ISAM), in the 20th century. They found that carbon sequestration by secondary forests is one of the major components of LUC-induced biospheric carbon sink and that nitrogen-carbon interaction plays an important role to determine the magnitude and spatial distribution of the sink. The research topic seems timely, because understanding the mechanisms of global carbon budget is critically important for conducting reliable climate projections and effective carbon managements in the future.

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The authors conducted a series of global simulations, using different experimental assumptions, to specify the contribution of LUC, nitrogen deposition, and carbon-nitrogen interaction. They took account of the historical climate and atmospheric CO₂ conditions. Their findings, i.e., considerable contribution by secondary forests and substantial influence of nitrogen deposition, are plausible but not novel; previous multiple studies came to similar conclusions. However, I found additional values of this study, in terms of model-aided aggregation and regional analyses.

The manuscript was well prepared, and I found no serious logical fault. One caveat is that the authors did not clarify the range of estimation uncertainty even for the main result (e.g., 1.22 Gt C/yr emission by LUC, without a range of uncertainty). By addressing uncertainties in the assumptions, forcing data, and model parameters, the authors should evaluate the range of uncertainty in the estimation of carbon budget of the secondary forests. At least, I recommend the authors discussing this point in the section of conclusion.

Finally, I conclude that the manuscript is acceptable after moderate revision, especially in terms of estimation uncertainty. Also, I recommend discussing how the study, potentially, can contribute to climate projection and carbon management in the real world.

Specific comments: Page 2743 Line 1 Excessive nitrogen deposition brings about nitrogen saturation, passively leading to ecosystem degradation. Did your model study consider the possibility of the adverse effect of nitrogen deposition?

Page 2744 Line 12 Does the ISAM consider symbiotic nitrogen fixation?

Page 2744 Line 20–23 Dominant plant functional types change through time, along the secondary successional series. However, I guess that the ISAM model assumed stationary land cover types of secondary forests. Is it correct?

Page 2746 Line 6 ‘and’ should be between ‘cropland’ and ‘pastureland’.

Page 2746 Line 15 Does the model consider the difference between dry deposition

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and wet deposition? They may, more or less, differ in biogeochemical properties. And, you should more clearly explain how the deposited nitrogen is handled in the nitrogen cycle scheme. For example, which compartment the deposited nitrogen goes?

Page 2747 Line 20 and Table 1 It is unclear how the experiment “without nitrogen deposition” was conducted. Did you assume zero deposition? Or, constant (i.e., fixed to the level in 1765) deposition rate?

Page 2748 Line 12 A citation, Van Minne et al. (2009), is not found in the reference list.

Page 2748 Line 22-26 and Figure 3 This part simply describes the spatial and temporal patterns of secondary forests provided by the data of Hurtt et al. (2006), which is not a result of this study. This part should be moved to Section 2.2.1.

Page 2751 Line 8 ‘on’ should be after ‘based’.

Page 2751 Line 20 ‘increase C uptake’ would be revised as ‘increased C uptake’.

Page 2751 Line 25 to Page 2752 Line 2 The mechanisms 1) and 2) can happen to every secondary forests, and may not explain the specific phenomena in southern Europe and India. Instead, you should give further explanation for the mechanism 3), i.e., climatic characteristics in these regions.

Page 2752 Line 26 Disagree. Houghton (2003) estimated LUC-induced emission mainly using inventory data, which implicitly include nitrogen limitation in the real world; he did not assume a nitrogen-rich condition.

Page 2755 Line 20 Correct family name is ‘Klein Goldwijk’ (this may be Dutch name). Please look the original paper.

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