

Interactive comment on “Estimation of pre-industrial nitrous oxide emissions from the land biosphere” by Rongting Xu et al.

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General Comments

The objectives of this manuscript are to offer estimates of terrestrial sources of N₂O emissions during pre-industrial (PI) times, both in terms of the total global sum and the spatial distribution of those emissions from soils across the continents. The complex process-based Dynamic Land Ecosystem Model 16 (DLEM) was employed, and appropriate driver datasets on land use and land cover, N deposition, climate, etc. were derived from literature sources to run the model. Overall, this is a useful exercise that has the potential to make a good contribution to the literature, but I do have several concerns regarding the approach and assumptions.

Specific Comments

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1. With respect to constraints on the overall PI global emissions of N₂O, I have more confidence in the top-down approach using atmospheric concentrations and lifetimes of N₂O, than the bottom up simulations of a highly parameterized process model. The most recent top-down estimate (Prather et al., 2015) is cited in passing by the authors, but the estimates are not included in the present manuscript. The estimates from the IPCC AR4 and from Davidson & Kanter (2014), mentioned in lines 53-54, were based largely on the 2012 paper by Prather et al., but their 2015 paper provides an important update on lifetime estimates and resulting PI emission estimates. They now recommend using lifetimes of 123 years for PI and 116 years for the present (+/- 9 years), and from those lifetime estimates, they derive a new PI emission estimate of 10.5 Tg/yr. Fortunately, this is very close to other estimates, including the one from this study. Nevertheless, it should be specifically cited.

2. The point that the lifetime has probably decreased since PI times should be discussed. As far as I can tell, a varying lifetime cannot be incorporated into the one-box model (line 171) used by the authors. Perhaps the resulting global estimate is not terribly sensitive to this change, but that should be evaluated and discussed.

3. I fail to see how the analysis presented in Figure 7 and Table 1 provides additional confidence in the summed global estimate from this study. I can see the value of a sensitivity analysis of initial PI atmospheric concentrations and lifetimes, which Prather's papers have already done and for which they could be cited. In contrast, the analysis in Fig. 7 and Table 1 is clouded by the unclear source of annual emissions over the simulated time period and the validity of those assumptions. The text (lines 182-185) suggests that model output was used for annual emission estimates: "The mean with 95% confidence intervals, the maximum, and minimum values of estimates from DLEM simulations were applied as initial emissions to calculate the atmospheric N₂O concentration in 2006 as shown in Table 1 (Scenarios 1–4 and baseline), as well as concentration changes from 1860 to 2006, as shown in Figure 7." However, the Fig. 7 captions indicates that the "net additions of anthropogenic N₂O emission amount

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in different years were listed in Syakila and Kroeze, 2011.” I don’t understand which was used to estimate annual increments of N₂O concentration in Fig 7 – was it model output, as indicated on lines 182-185, or was it the net additions estimated by S&K as indicated in the figure caption? Both have problems.

S&K estimated fairly substantial N₂O emissions from agriculture during the late 19th and early 20th centuries, but they also estimated a rather large decrease in natural emissions compared to 1500 (which are very difficult to estimate, see my further comments below), so their estimate of the net change relative to 1500 was small for this time period. However, the starting point for the present study is 1860. Therefore, it is incorrect to subtract this decline in natural emissions that preceded 1860 from the growth in anthropogenic emissions since 1860. S&K did this to show changes since their starting point of 1500, but using their “net additions” column without accounting for a different starting point in the present study introduces a significant bias. It is the net change relative to 1860 that is important for the present study, so the “net additions” estimated by S&K should be recalculated relative to 1860 if they are to be used in the analysis for Table 1 and Fig. 7.

I showed in my 2009 paper, and Smith et al. (2012) have affirmed, that atmospheric N₂O began rising significantly many decades before fertilizer use became common in the 1950s, and so the “net additions” to the atmosphere must have been larger than those estimated by S&K relative to 1500, although they may be similar if they were corrected to be relative to 1860. We speculate that this increase in emissions between 1860 and 1950 was due to mineralization of soil N as agriculture expanded into regions of previously untilled soils, thus mobilizing N for rapid cycling, including a fraction lost at N₂O. I also suspect that the current DLEM may not include effects of soil mining when virgin soil is first tilled, so if Table 1 is based on DLEM simulations, as indicated in the text on lines 182-185, then I suspect emissions from 1860 to 1950 were underestimated, which would affect the slope of the trend line later in the analysis as well.

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I realize that the point of Figure 7 is not the accuracy of the simulated trend line, but rather the end point, but if the trend line agrees so poorly with the observations, then one has to question the validity of the model and the input data, which calls into question the reliability of the end point analysis. I believe that Fig. 7 and Table 1 could be replaced with citations of the sensitivity analyses done by Prather et al. (2012, 2015), but if the authors persist in wanting to include their own analysis, I would suggest that they utilize another source of “net addition” emissions than those of S&K relative to 1500.

4. The change in “natural” emissions before and after 1860 should be discussed. As I noted above, S&K deduce a substantial decline in natural emissions from 1500 to 1850. Similarly, I included a significant change in non-agricultural soil emissions due to tropical deforestation, which began growing rapidly in the late 20th century (Davidson 2009). Whether pre-1850 or post-1950, these changes in natural soil emissions are difficult to estimate, but the uncertainties that they represent should be considered, and biases resulting from how they are or are not included should be considered.

5. While the top-down approach of Prather et al. (2012, 2015) and the one box model used in the present study help constrain total PI emissions, the soil emission estimate must still be made by difference between total emissions and oceanic emissions. While the AR5 estimate of 3.8 Tg N₂O-N/yr (range: 1.8 - 9.4; Ciais et al., 2013) is widely cited for emissions from the oceans, it is highly uncertain, so simply subtracting 3.8 (or 3.5 – 4.5 as in Table 1 of the present manuscript) from a total PI source estimate of about 11 Tg N₂O-N/yr (+/- 1) doesn't really narrow the confidence estimate of the PI terrestrial source a great deal. Indeed, I just discovered a curious inconsistency between the AR5 best estimate of 3.8 with a review paper by Voss et al. (2013), which cites that same 3.8 value for N₂O emissions from the open ocean, but then adds another 1.7 Tg N₂O-N/yr for emissions from the continental shelf regions. I don't know if the AR5 review of the literature failed to adequately represent continental shelf regions or if Voss et al. might be double accounting. If Voss et al. are correct, the AR5 estimate of oceanic

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emissions may be biased toward the low end, which would mean that the terrestrial PI source may more likely be in the range of 5 Tg N₂O-N/yr or less. In any case, this highlights how uncertain the oceanic estimate is, which means we have to have similar uncertainty in the estimate of the PI terrestrial source. The narrow range of uncertainty in the present study's PI terrestrial source (6.03–6.36 Tg N₂O-N/yr) reported on line 331 is unrealistically small.

6. The authors have misunderstood the emission estimates from my 2009 paper, which they incorrectly describe on lines 299-301: “However, the indirect emissions from the riverine induced by the leaching and runoff of manure applications in agro-ecosystems, legume crop N fixation, and human sewage discharging have not been addressed in Davidson (2009).” On the contrary, I derived emissions factors from a statistical model that was constrained by the historical record of atmospheric concentrations and fertilizer and manure use, so the emission factors derived from that analysis necessarily included all of the emissions, direct and indirect, that could be statistically correlated with historical fertilizer and manure use (“The sources attributed to fertilizers and manures include indirect emissions from downwind and downstream ecosystems, including human sewage.” Davidson, 2009). Therefore, it is incorrect for the authors to calculate an additional indirect source (line 305) using IPCC default factors to add onto the estimate that they took from my paper that they misunderstood to be only direct emissions. They could either use an unmodified estimate from my paper or they could derive a new one, based on IPCC default values for both direct and indirect emissions based on estimates of BNF, fertilizer-N, and manure-N for 1860. Furthermore, note that the 0.42 Tg N₂O-N/yr that they extracted from my paper for 1860 was for anthropogenic biological emissions (i.e., soils) only, and that there were also some other anthropogenic emissions at that time, such as biomass burning (see SI for Davidson 2009).

7. The authors should also acknowledge that there were anthropogenic effects on the N₂O budget before 1860, so the 1860 fluxes don't necessarily represent only “natural” emissions. This includes some N₂O from agricultural expansion that mined soil N and

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also added BNF, some biomass burning, a tiny amount of industrial and transportation sector emissions, and possibly a loss of emissions from degraded natural soils that had been plowed for centuries or millennia, some of which were highly eroded.

8. Although my comments above all focus on the PI global total estimate, perhaps the more important contribution of this manuscript is the simulated spatial distribution of those PI soil emissions. It is not surprising that the model simulates the majority of the soil emissions coming from tropical forest soils. That is also true today for non-agricultural soils. There are a few curious details that jump out at me from the map (Fig. 4). Why are emissions from the Amazon Basin and SE Asia so much lower than from the Congo Basin? Other models that I am aware of don't show that difference (e.g., Zuang et al., 2012; Stehfest & Bouwman, 2006; Potter et al., 1996). Which of the datapoints in Fig. 3 are from tropical forests and which continents are they from? Is there validation support for the Congo having much higher emissions than the Amazon or SE Asia? More discussion would be helpful to interpret the variation shown in this map, such as where agriculture was or had been, where wetlands are, and where there are hot spots other than tropical forests. For example, I see a bunch of small red spots that appear to be near the Andes range, which puzzles me, but perhaps there is a good explanation. Ditto for why Northeastern Brazil, which is generally rather xeric, shows up as a hot spot. Also curious are the hot spots in southwestern China and the southeast coast of Australia.

Technical Points

line 41: This statement ignores that some anthropogenic emissions were already present prior to or at the beginning of the industrial revolution.

line 55: Add recent results from Prather et al. 2015.

line 70: Change "is" to "are" because the word "data" is plural: "the data are"

line 178: Use estimates from Prather et al. 2015.

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line 312: Consider other estimates, such as those of Voss et al. 2013.

Figure 2. I don't understand the units. How can these units of crop area apply to each individual pixel?

Figure 3. The data used for this graph should be referenced.

Figure 5. The bottom panel is all that is needed. The top panel is redundant. However, you could also add a panel of mean flux per hectare, which would be useful, because it is difficult to compare fluxes across continents when the contents have such different total areas.

Figure 6. The two panels are largely redundant. The pie chart could include both the percentage of the total and the estimate of Tg/yr, which would obviate the need for the upper panel. However, again, the mean flux per hectare by biome would be an interesting panel to add.

Table 2. The number of significant figures shown is excessive. I suggest rounding to the nearest Gg. The uncertainties are such that any fraction of a Gg is meaningless.

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Respectfully submitted,

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