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Interactive comment on "Effects of nitrogen deposition on growing-season soil methane sink across global forest biomes" *by* Enzai Du et al.

Anonymous Referee #1

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Dear authors,

I had the pleasure of reading your article entitled "Effects of nitrogen deposition on growing-season soil methane sink". The manuscript is definitively a good effort to investigate the effect of nitrogen deposition on atmospheric CH4 uptake by soils across forested ecosystems. This topic is very hot in the literature at the moment. The manuscript is very well written and easy to read and has a short but sufficient extent. That said, I found some fundamental flaws in three different aspects of the manuscripts: the assumptions you made by extrapolating your results, the datasets you employ and how the data was collected, and some problems with the general structure.

Major comments

âĂć The definition and usage of the growing season extension is not well justified, nor correctly employed. Firstly, you never state why you are using the CH_4 uptake of the growing season and not for the whole year. Most of the papers you revise have data for more than a year. Also, the growing season varies greatly across and within ecosystems, thus cannot be set to a single value by biome. Growing season can be defined by multiple variables (temperature, precipitation, number of frozen days), thus the usage of a simple single value for each biome is simply not acceptable. As a result, there is no justification to consider the effect of nitrogen over the CH_4 uptake only during the growing season. Why not simply consider the whole year? If the growing season has importance for your analysis it is not reflected correctly in the manuscript and is never justified.

âĂć Secondly and possible the strongest criticism, your assumptions about the positive effect of nitrogen in the soil CH_4 sink in the boreal forest cannot be sustained with just four papers. The results are a wild extrapolation from studies that are not sufficient, nor analyzed correctly in your literature review. To be more precise: the work of Gulledge and Schimel (2000) found that nitrogen inhibit CH_4 consumption in boreal forest; Maljanen et al. (2006) found perform a factorial experiment with nitrogen and ashes and while nitrogen alone did increase the uptake it was not statistically different; additionally, ashes with nitrogen decrease the CH_4 uptake. Xu et al., 2014, found and increase in the uptake in the lowest N concentration (10 kg N ha^{-1} $year^{-1}$) but a negative effect with 20 and 40 kg N ha^{-1} $year^{-1}$ which are in the low N category you consider and do not be reflected in the results you obtained (why is the BF bar in figure 2 not going all the way to negative number based on this?). Finally, your fourth work Gao et al., 2013 is not available on google scholar. Based on this, the evidence to argue that boreal forest (the only ecosystem) presented an increased uptake due to nitrogen is not sustained at all. The other two ecosystems presented a decrease in all conditions, which is not novel.

âĂć Thirdly, your definition of the of low and high N categories seems completely arbitrary and not justified based on literature. Why this threshold and not another?

âĂć Finally, you are extrapolating data from 5-10 points, which are highly aggregated spatially to assume a biome-level behavior, which is incorrect. In other literature reviews of the topic, the authors revise around 35 papers to propose general mechanisms that can be scaled. In other words, you are trying to extrapolate a pattern based on few observations, without the proposition of an underlying mechanisms to support the increase in spatial scale.

Minor comments

âĂć There are some mistakes defining the sign for the CH_4 sink (both negative and positive signs are used along the paper). It needs to be consistent.

âĂć Page 2, line 27, you started to talk about China as a hot spot for N deposition with no previous justification and not using this particular region in the paper. If you are focusing on a global scale, you should either give more examples or eliminate the regional-level comparisons.

âĂć Page 6, line 25, the argument about the CO_2 equivalents to measure the N effect over soil CH_4 sink is absolutely out of place. 1) You cannot predict the effect of N deposition in the next 100 years using current values, 2) you are using a GHG potential of 25 (should be 28), 3) you create very strong arguments with very little evidence and not sufficient data, 4) finally, CO_2-eq are not really used any longer, as the relationship of GHG to CO_2 is not linear.

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