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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 #2

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This manuscript reports on a synthesis or meta-study of studies that report the effects of N additions on soil CH4 fluxes. The authors did a literature search using several sources, calculated a response variable, stratified results according to dose and biome, and conclude that low-level N addition increased Ch4 uptake in boreal forest while decreases occurred at high N addition levels and in all other biomes.

A meta-study is only as good as the sources where the information comes from, and therefore studies used in such a meta-study should be generally accessible and peer-reviewed. Unfortunately, several of the studies used in this meta-study did not meet these criteria. I find it problematic that apart from ISI Web of Science, also Google Scholar and China National knowledge Infrastructure are used to search for literature. To my knowledge, these two latter sources also list reports that are not peer-reviewed.

I strongly suggest to ONLY use ISI as a literature source, because here only peer-reviewed sources are listed. Studies that are not peer reviewed should not be included in a synthesis. In your meta-study I suggest to exclude the following studies listed in the supplement: - two of the studies listed are a MSc thesis (Wang, 2012, Pan, 2013). Such a thesis is not considered peer-reviewed, please exclude them. -all studies that involved incubations, instead of field measurements. For example, the study by Chen et al. 2017 mentions that they did laboratory incubations. You even state in Page 3, line 16 that you only included studies that used closed static chamber technology. Apparently that is not true. There may be more studies with incubations, I did not check them all. - Please, exclude studies published in Chinese (or other non-English publications) with only an English abstract. I do not consider such studies as generally accessible. For example, the study by Hu et al., 2011, is only accessible in Chinese and there may be more in the list.

You use the 60 kg N ha-1 yr-1 as an arbitrary cut-off between 'low' and 'high' level N addition. Did you calculate also the background N-deposition in the N additions? For example, the study by Li et al., 2015, mentions that there is a background N deposition of more than 30 kg N ha-1 yr-1, while the treatment is 40 kg ha-1 yr-1. Together this would be more than 60 kg N ha-1 yr-1 and the 40 kg treatment should be grouped as 'high' level N addition. I suspect that is not how you did this and it just illustrates how arbitrary the choice of 60 kg N ha-1 yr-1 is.

You did not mention any other criteria for inclusion or exclusion of studies. However, I think you should define what you consider a sufficiently large plot and a sufficiently wide buffer zone between treatments. Also, were all studies having true replicates? This kind of important information on the quality of studies is completely ignored in your manuscript.

I found it very adventurous that you excluded the only peer-reviewed study conducted in tropical ecosystems (Veldkamp et al., 2013) with the argument that urea as an organic N form has 'limited implications for the effects of N deposition', then cite Aronson and

Helliker, (2010) as the source for this statement (page 3, line 20), and later lament that there are no studies conducted in tropical areas and then even extrapolate the results from subtropical forests to the tropics. -First of all, while urea is strictly spoken an organic N source, it is quickly hydrolysed ($NH2CONH2 + H2O \rightarrow CO2 + 2NH3$) after application and the gaseous NH3 reacts with water to form ammonium (NH4+). Only on soils with a high pH there are significant losses through volatilization. -Second, in the paper by Aronson & Helliker (2010), I did not find any statement that urea has limited implications for the effects of N deposition. In contrast, they also analysed studies with urea additions. They found no difference between Urea and other pure N fertilizers. They also concluded that 'any conclusions of the effects of specific N species relative to others must be highly qualified, as the form of N that results may be quite different from that added'. Therefore, to quote the Aronson & Helliker (2010) paper as the source why studies that add urea should be excluded is misleading. -Third, ignoring the only tropical study and later filling up the gap with studies from subtropical areas is very adventurous. Especially since the study conducted in the tropics did not following the hypothesized trend across biomes.

The hypothesis to be tested (Page 2, line 21 and further) is weak and based on incomplete assumptions. You simply assume that N availability increases from boreal to tropical biomes, which is not true. If you read publications by Vitousek more carefully you will see that the main factor is not the biome but how heavily weathered soils are. More than half of the tropics is located on soils that are not heavily weathered (e.g. montane forests) and N availability is expected to be as low as other biomes where young, less weathered soils dominate.

You group all forest ecosystems together and make no difference between natural forerst and plantations or managed forests. Tree plantations typically have significant growth rates and are almost always N limited, also in tropical and subtropical conditions. Ignoring this may lead to wrong conclusions.

In summary, this synthesis is poorly conducted. There are studies included that are not

peer-reviewed and there were no quality criteria for the studies that were included. The hypothesis is weak and based on incomplete assumptions. The distinction between 'high level' and 'low level' N addition is arbitrary and the background N deposition is ignored. Finally, I could not find any objective reason why studies where urea was added were excluded. Given these weaknesses of this synthesis I strongly doubt the validity of the conclusions and I recommend not to publish this manuscript in Biogeosciences.

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