

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

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Dear authors,

it was a pleasure to read your very interesting discussion article.

I would like to make a comment. You wrote in Sect 2.1. “We excluded experiments by applying urea and manure, because these organic N forms have limited implications for the effects of N deposition (Aronson and Helliker, 2010) as N deposition mainly occurs in forms of inorganic N (Vet et al., 2014).” Based on this statement, you excluded one of the most long-term measurements of the effects of N addition on soil CH₄ fluxes from tropical forests (4 years of data, Veldkamp et al., 2013) (as stated on P7/L5 of the discussion paper).

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1) Could you please clarify more specifically where in the cited paper (Aronson and Helliker, 2010) you refer to when arguing that urea has limited implications for the effects of N deposition. When reading that paper (Aronson and Helliker, 2010), I find discussion on the form of N added on P3249-3250. The authors state there that “The effects of N addition could be similar regardless of N form used, due to the presence of microorganisms capable of rapid N transformation by variation in microbial consortia. The timing of fertilization may determine the form of N that methane-cycling microorganisms encounter in the soil more than the actual N species added. The fact that the addition of urea and ammonium nitrate were capable of increasing nitrate availability significantly in Delgado et al. (1996) underscores this point. Therefore, any conclusions of the effects of the specific N species relative to others must be highly qualified, as the form of N that results may be quite different from that added.”

In terms of the tropical N-addition study that was excluded from your review, there is detailed data on soil extractable nitrate and ammonium concentrations that shows that urea additions have chronically changed the soil inorganic N status of the studied forests (Koehler et al., 2009). Therefore, I would like to challenge your decision to exclude the study on soil CH₄ fluxes from the same sites (Veldkamp et al., 2013), based on the argument that N was added in the form of urea.

2) In that same study that you excluded from your review (Veldkamp et al., 2013), N addition did not decrease soil CH₄ uptake, which is in conflict with the conclusion you made in your discussion article (e.g. abstract “However, high-level N addition significantly decreased growing-season soil CH₄ uptake across boreal, temperate, and subtropical forests.”). N-addition to the tropical forest sites in Panama that you excluded from your review (Veldkamp et al., 2013) did not affect soil CH₄ fluxes.

I suggest that you include that detailed study on the effects of N-addition on CH₄ fluxes from tropical forest soils, rather than excluding it with the argument that “the results based on N additions by using urea shed limited lights on the effect of N deposition” (P7/L5-6). Further information on detailed and long-term soil CH₄ profiles (Koehler

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et al., 2012) may assist in discussing that study in the context of your review, and in discussing why the N-addition effects on CH₄ fluxes in these sites may differ from the overall results you found in your review.

Yours sincerely,

Birgit Koehler

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