

SOIL Discuss., referee comment RC2
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Comment on soil-2021-40

Kate Buckeridge (Referee)

Referee comment on "Soil $\delta^{15}\text{N}$ is a better indicator of ecosystem nitrogen cycling than plant $\delta^{15}\text{N}$: A global meta-analysis" by Kaihua Liao et al., SOIL Discuss.,
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In the manuscript, 'Soil and plant $\delta^{15}\text{N}$ have a different response to experimental warming: A global meta-analysis', the authors assess 20 experimental warming field studies and conclude that soil and plant $\delta^{15}\text{N}$ had negative and positive responses to warming at the global scale, respectively.

Overall, the study is a nice contribution because it looks at both plants and soils. But I think the title oversells and misleads. Also, I realize that this is a short communication, but more detail is needed to support the hypothesis, to rationalize why the specific environmental variables were chosen over others, and to relate this study to other results in the literature.

The word 'significantly' is overused in the abstract, and the presentation of results that are not significant as effects is not appropriate. It would be better to not use the word significantly and to only present the significant results (after defining p-value cut-off in methods). In other words, remove the inference from the title and abstract that plant $\delta^{15}\text{N}$ had a positive response to warming – this was not significant. I think the finding that soil $\delta^{15}\text{N}$ is a better indicator than plants of environmental cues is a more appropriate conclusion or story lead. Because really, the pattern of response of plants and soils to environmental drivers tested here was not different, it was just weaker in plants than soil.

Comments by line number:

I. 44. Please explicitly define 'openness' in the introduction (and if possible, in the abstract). Although some readers will understand, those unfamiliar with the $\delta^{15}\text{N}$ literature will read this as jargon.

l. 50-51. This should be reversed: The isotopic fractionation effect results in gradual ^{15}N enrichment.

l. 74-75. The hypothesis is not supported by any rationale in the introduction, please provide some preamble that supports why they should be different.

l. 85-86. It is not clear why temperature gradient studies are being excluded, as they will also include a treatment and control. Perhaps the authors could clarify - do they mean climate gradients (space-time substitution), or lab incubations?

l. 114. I am quite surprised that length of warming was not considered, as multiple studies illustrate different responses in plant and soil CNP for short and long-term warming experiments. Also, see Craine et al (2015), which suggests that soil $\delta^{15}\text{N}$ is directly controlled by soil C and texture, and only indirectly controlled by temperature. Did you consider length of warming, SOC, or soil texture as sub-groups? I see l. 47-67 in the intro provides justification for the subgroups, but I remain unconvinced that the chosen subgroups are more important than the ones not assessed. Perhaps this needs better support from the literature.

l. 176. Actively layer > active layer

l. 177. This is not true for all air warming treatments.

Table 1 should have a column for soil types – at the very least, organic or mineral soil, but especially pH, since this was a main factor in the analysis.

Many grammatical typos, please correct: l. 40, 42, 43, 49, 68, 83, 85, 171