

Interactive comment on “Responses of soil respiration to elevated carbon dioxide and nitrogen addition in subtropical forest ecosystems in China” by Q. Deng et al.

Anonymous Referee #1

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I reviewed the manuscript entitled “Responses of soil respiration to elevated carbon dioxide and nitrogen addition in subtropical forest ecosystems in China” by Deng et al. This work address an important issue about the combined effects of N deposition and elevated CO₂ on soil respiration. The strength of the study is that is unclear how SR will respond to elevated CO₂ under high N deposition and this is relevant for ecosystems that are currently receiving high N deposition rates (e.g. subtropical forests ecosystems in China). In general the manuscript is well written but a few grammar corrections are needed throughout the text. Here I list a series of comments that I hope will help the authors improve the manuscript.

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General comments 1-The title is misleading. This study is based on seedlings planted in 10 chambers with a diameter of 3 m. Thus these chambers do not represent subtropical forests ecosystems in China. I suggest changing the last part of the title to better inform about the experiment and results discussed in this study.

2-The introduction is well written but I would suggest to clearly state: How this study is different from previous studies looking at N and CO₂ addition? Why this study is needed for seedlings of native species of subtropical forests in China? I think the key to these questions are in the last paragraph of page 8373.

3-The major objective is also misleading. Again the study was not designed to assess the effects of elevated CO₂ and N addition on soil respiration in subtropical forests (page 8363 lines 12-13). The study was based on seedlings planted in small chambers and therefore the title, the objective and the discussion should be based on this experimental design to avoid over interpretation of the results. I encourage the authors to revise the manuscript based on their findings and to be careful in over interpreting the results to the ecosystem scale of subtropical forests.

4-I like how the introduction clearly states what the authors examined (page 8363 lines 17-22). I would like to suggest rephrasing this section as hypotheses supported by a few references. In other words, which were the expectations of the authors before performing the experiment? This is important because in the discussion the authors show that previous studies differ from the present results (e.g. page 8376 line1-5). This is also important to highlight for the overall significance of the study.

5-I think more discussion is needed to explain why the combined effect of N and CO₂ increased soil respiration rates. Did the authors expect this a priori? Which are the possible mechanisms that were triggered? This is a study using seedlings...are there any differences with previous studies using larger and older plants (e.g. FACE experiments)?

6-The authors calculated the temperature sensitivity based on Q₁₀ (equation4). How-

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ever, the authors do not present the error bars, the confidence intervals of these estimates, or any statistical test between treatments. This is important because Q10 values vary from 1.5 to 1.84 and it is possible that the error in this calculation is larger than the reported range. If there are no significant differences (which should be tested in a revised version) I would suggest removing or editing section 4.2 in page 8372. In the current version of the manuscript there is stated that a one-way anova test was used to compare the b values among treatments (page 8369 line 4-5) but I do not see the results of this test that would support the arguments presented in this section 4.2.

Specific comments 1-The seedlings used had ages between 1-2 years (page 8365 line 6). These seedlings were randomly collected but it would be important to test if there were significant differences in the biomass of these seedlings. If a chamber was planted with consistently larger seedling then this pre-treatment condition should be taken into account. This probably can be clarified by a line showing the height or biomass of the seedlings per plot with the respective statistical test.

2-Although reporting annual soil respiration calculations are important I would suggest to avoid these estimates for the year 2006. This is important because a treatment could have an effect that is shown in the next year and therefore the response of a variable (i.e. soil respiration) to environmental factors may not be the same for the first year. Maybe a possible test could be a two way ANOVA where the treatment and the year are tested and then the interaction between year*treatment.

3-I encourage to report the F values along the P values for all the results in the manuscript. Also I would suggest being careful in the use of the word "interaction" when referring to the treatment with high CO₂ and high N. The use of that word when reporting statistical results (e.g. page 8370 line 12-13) is misleading and suggest that the "interaction" was a statistical effect (e.g. as in a two way anova). I am not sure if the authors intended to test a statistical interaction or if they were referring to the combined treatment.

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4-The authors state that soil moisture may play a more important role in soil respiration rate as the soil becomes dryer (page 8372 lines 7-8). This result has been shown in many other studies and the authors should also explore what happen at the other end of the moisture spectrum. ...what about the interaction of soil temperature and water? If the idea is to revise interactions I believe that it is important to explore how the combined effect of high CO₂ and high N influence the combined effect of soil temperature and soil moisture on soil respiration.

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