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Discussions

Interactive comment on "Soil responses to manipulated precipitation changes: A synthesis of meta-analyses" *by* Akane O. Abbasi et al.

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We would like to thank Dr. Dijkstra for providing his valuable feedback on our manuscript. Here is a point-by-point response to his comments and concerns.

Comment 1: Unlike what the title suggests, this is not a synthesis, but merely a summary, which is unfortunate. Response: We attempted to synthesize the meta-analyses in section 3, and also visualized the outcome in Figures 1 and 3, which could be considered synthetic. Based on your comment, we are considering adding a subsection 3.7 to more fully synthesize what we learned from section 3.1-3.6. We would welcome additional input on specific areas that could be made more synthetic.

Comment 2: It provides some research gaps (e.g., lack of data on nitrïïňĄcation, den-

itrïïňĄcation and ïňĄxation), but even there, the authors do not really provide a rationale for WHY more information on this is needed. Response: We believe that filling such research gaps will strengthen the current understanding of precipitation change effects. For example, we know that, in general, decreased precipitation decreases soil N2O emissions, and increased precipitation increases soil N2O emissions. However, we have little understanding of why these changes occur (i.e., because of increased/decreased nitrification, denitrification, mineralization, or combinations of those?). Clarified mechanisms underlying the soil N2O responses may help mitigation strategies and management practices. We are considering adding a discussion of the rationale in section 4.1.

Comment 3: I also disagree about the statement in the abstract that "rates of processes underlying these variables are less frequently covered" than pools. Indeed, respiration rates (Figure 1) have some of the largest observations compared to some of the pools. Response: Yes, respiration is the exception; it is one of the most frequently covered variables. We mentioned "rates of processes" generally in the abstract, specifically listed rates of mineralization, fixation, and de/nitrification as an example. We will clarify that there are some processes that are frequently studied, such as respiration, so there will no longer be confusion. Thank you for pointing this out.

Comment 4: I was further disappointed that no distinctions were made that go beyond effects of decreased and increased precipitation. It is well known that a large number of the 42 soil response variables listed here are quite dynamic in time and depend not only on the overall relative decrease or increase in precipitation, but also on timing, duration and frequency. I believe different soil responses to changes in precipitation among studies could for a large degree be described to differences in intensity and frequency, and I think this is a missed opportunity for discussing these issues in greater detail. Response: This is a great point, and we did initially attempt to look in detail at treatment timing, duration, intensity, and frequency, as well as other methodological and environmental factors. However, covering so many variables both for decreased and

increased precipitation already led to a lengthy manuscript, and we decided to simply discuss the effects of treatment timing and other factors briefly in the text. Therefore, in each section, we highlighted cases in which these factors affected each meta-analysis result. The supplemental information also discusses the importance of these factors in further detail, as well as how frequently they are taken into account in meta-analyses. We believe that another project could more fully incorporate methods and environmental differences. Still, our paper focused on the general effects of precipitation changes that include all variations of manipulation setups.

Comment 5: It was further unclear if only field studies were included when extracting the data from the 16 meta-analyses. I know some of the meta-analyses did include soil laboratory incubation studies,but I am not sure about all 16 meta-analyses. I can imagine that some of the soil variables would respond quite differently depending if they were measured in the ïňĄeld, greenhouse, or lab (and with or without plants). Response: Section 2.1. Specifies that "we collected meta-analyses that included only field studies where the magnitude of precipitation was manipulated." Some meta-analyses showed both field experiments and lab/greenhouse experiments, but we did not include the lab/greenhouse experiments' results. We will add a sentence to clarify this point.

Comment 6: I was unclear what the difference was between "root biomass" and "belowground biomass" (Table 2). How are they different? Response: Belowground biomass was measured by drying soil cores (Wu et al., 2011), and thus includes roots and other plant and animal materials. Root biomass, as the name suggests, includes biomass that derives from roots only. We will clarify this with a footnote in the table.

Comment 7: l. 110: I guess strong agreement is not surprising if the same data are used for different meta-analyses. How much overlap in data used does there exist among the meta-analyses? Response: This is a great question, and we have to note that we did not set out to do our own meta-analysis, and we did not specifically analyze the overlap in data across the meta-analyses we found. However, one can guess the extent of overlap from the sample size and study year. Taking soil respiration (Rs) as

an example, the sample size for decreased precipitation is 96, 72, 21, 8, 132, and 54 from six different meta-analyses. The studies with sample sizes of 132 and 8 probably have minimal overlap, while the study with 132 and 96 may contain substantial overlap. Also, the publication years of the meta-analyses range from 2011 to 2018, and newer studies likely to include data that earlier studies could not have included. We think it is important to show that, while every meta-analysis has a unique sample size and time range, there is typically strong agreement among them for any given variable.

Comment 8: l. 234: "humidity affects P deposition". How? I thought most atmospheric deposition of P was in the form of dry deposition. Response: While some P is deposited through dissolved P in rain, mist, and snow, the amount is typically quite small, and this phenomenon is not critical for this manuscript. We will delete this statement.

Comment 9: l. 268-272: I don't believe microbial community responses to precipitation changes are as clear as suggested here, and probably strongly depend on timing, intensity and frequency of the precipitation manipulation. Response: Thank you. We agree that these caveats are important. We plan to change this concluding remark of this section to highlight these dependencies.

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