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Interactive comment on "Higher response of terrestrial plant growth to ammonium than nitrate addition" *by* Liming Yan et al.

Anonymous Referee #2

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My comments on this manuscript echo those of Anonymous Referee 1. The paper highlights an important issue in the interpretation of the effects of N deposition on ecosystems, especially given the different sources of ammonium and nitrate, and the changes in emissions that are expected over coming decades. In principle, a meta-analysis of the type described by this paper would be a valuable contribution to increasing our understanding of the relative effects of these two forms of nitrogen.

However, I am concerned, like Anonymous Referee 1, that the way in which papers are used in this study does not provide a robust basis for the analysis. Of specific concern are:

- The duration of the experiment. It is not clear from the methods description how this is handled, as the methods only refer to amount of N in g m-2. Is this the total N

added over the course of the experiment, or the annual addition? The authors appear to include experiments of quite different lengths in their meta-analysis, but how this is taken into account in their analysis is not specified. This needs to be clarified.

- The dose of N added. The references listed include studies using an N addition well above the realistic range of atmospheric deposition, which at a global scale might be 0-50 kg ha-1 yr-1. It is not reasonable to assume that plant growth will respond linearly (as I understand the author's analysis assumes) across a wide range of doses, and it would be informative to constrain an analysis informing deposition responses to the range of likely deposition.

- The counter-ion used. Studies with ammonium, for example, might use sulphate, chloride, phosphate etc. and the study design needs to ensure that any growth responses are only due to the ammonium itself. However, having checked references cited as studying ammonium responses in the paper's reference list, I am not convinced this is the case. I expected, for example that all the studies labelled as NH4 would apply this with an inorganic anion, but they seem to include studies applying fertiliser with a mix of nutrients with N in the form of NH4. The authors need to include full details of the chemical form of the applied N in all cases, and to ensure this is included as a factor in met-analysis.

- It is clear from the sub-sample of papers included in the meta-analysis that I examined that the studies vary from pot experiments in a greenhouse using seedlings to application to mixed plant communities under field conditions. It is very unlikely that growth responses are going to be the same under these very different conditions and very different plant growth stages. The authors need to focus on field studies of established plants, to provide a clear guide to likely responses under field conditions. If other studies under controlled environments are to be included, this needs to included as a separate factor in the meta-analysis.

- As clearly argued by Referee 1, plant growth responses to N can be constrained by a range of other environmental factors, such as climate and water availability. Soil characteristics are also likely to critical in determining responses to added nitrate and ammonium. These factors need to be considered in the authors' analysis.

It is also surprising to me that several relatively recent long-term field experiments designed to compare the effects of ammonium and nitrate directly are not included in the papers used in the analysis. These include studies of an ombrotropic mire (Sheppard et al., 2014, mentioned in the intro but not included in the meta-analysis), of a Tibetan alpine meadow (Song et al. (2015) and maquis vegetation (Dias et al., 2017). Since these studies directly compare the growth and ecological effects of the two forms of nitrogen, they seem a more important source of information than a comparison of independent studies of the two forms which may be biased by the many differences in experimental design which are not accounted for in the authors' analysis. The authors also present in Figure 2 the global trends of the ratio of ammonium to nitrate over the period 2010-2100. However, the description of how these trends were derived is very limited, referring both to the use of environmental driver sets and the use of linear regression. The latter approach seems questionable since the trends in emissions of NH+4 and NO-3 over this period are unlikely to be linear, and the authors make no clear statement of the scenario assumptions used to derive these future trends. Furthermore, if a trend in the ratio is to be useful interpreted, the reader needs to know what total N deposition is in these areas (the ratio will be much more relevant where deposition is high) and what the baseline ratio in 2010 is on from which these trends are based (e.g. a trend to a lower ratio, with nitrate dominance, might be from a baseline of high ammonium dominance or a baseline already with nitrate dominance. If the authors wish to include this analysis, it needs much more detail and context than is provided in the current version of the paper.

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