

## ***Interactive comment on “Effects of drought on nitrogen turnover and abundances of ammonia-oxidizers in mountain grassland” by L. Fuchslueger et al.***

### **Anonymous Referee #1**

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In the submitted manuscript, the authors presented results from an in situ experiment of simulated drought in two grasslands experiencing different intensity of land-use and its impact on N cycle key processes and AO. The information presented here is of general good interest and is valuable information. The experimental set-up looks 'clean' and of good quality. Results are generally presented in a pretty clear and interesting way. However, as I develop below, I have two major concerns that would prevent its publication as it is: 1) the too frequent random and incorrect use of references for justifying (sometimes wrong) assumptions and 2) the lack of strength and clarity of the work hypotheses, which to some extent appear as if they had been formulated after the experiment was conducted. Also, I think that the 'recovery' (or not) of the tested

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parameters after the drought ended should be more discussed.

Major points:

1) Here are some example of 'mis-citations': Page 9186, Line 17-22 'archaeal AMO has a higher affinity for ammonia and they seem to have a clear advantage in environments with low ammonia concentrations (Gubry-Rangin et al., 2011; Höfferle et al., 2010; Offre et al., 2009; Tourna et al., 2008, 2011)'

Gubry-Rangin et al. 2011 study the niche specialization of AOA lineages, while Offre et al. study the inhibition of AOA growth by acetylene. None of these references mention AOB or compare AOA and AOB in terms of their affinity for ammonia or 'advantage in environments with low ammonia concentration.

Page 9187, Line 22-26 'Moreover, AOB and AOA seem to differ in their sensitivities to changes in soil water availability (Gleeson et al., 2010; Stres et al., 2008; Szukics et al., 2012), with growth of AOB, but not of AOA, being favoured at higher levels of soil water content (Bates et al., 2010; Szukics et al., 2012).' Page 9197, L10-12: 'Abundances of bacterial and archaeal ammonia-oxidizers have been shown to strongly differ in soil NH<sub>4</sub> concentration optima (Gubry-Rangin et al., 2010; Offre et al., 2009; Schauss et al., 2009)'

Gleeson et al. show a slightly greater impact of WFPS variation on AOA community structure than AOB community structure and NO difference in terms of abundance. Stres et al. study total archaeal and bacterial communities targeting 16S. Szukics et al. did not see significant differences in AOA and AOB numbers (even though the text in this article mention it for one of the soil they tested, standard errors presented in their figure 1 clearly show no difference). If they actually do, it is rather the absence of impact of higher moisture content on AOB abundance while it may have decrease AOA abundance, which is different from what has been stated. Bates et al. does not even study water content or AO!!

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The correct and appropriate use of references should imperatively be thoroughly revised throughout the article to give them back their original meaning and relevance.

2) The first hypothesis is valid, although this should be reminded here that this is because mineralisation is a 'broad' ability while nitrification is a 'narrow' one, if I understood properly the justification of the hypothesis. On the other hand, there is no justification for the second hypothesis in the Introduction section, just the above-mentioned list of citations, which were misused and/or over-interpreted. The rationale behind the 3rd hypothesis is not clearer or stronger: land-use may have an impact on the parameter you state, but what would be the link with resistance to drought, what would be the underpinning mechanism? And you don't precise what impact, just 'stronger impact'. On what?

Minor points: Page 9186, L17-20: convoluted wording. Please rephrase. Page 9188, L18: Please state why this missing information would be important to know. Page 9189, L21: why 'respectively'? Page 9190, L10: why pooling 2 subsamples? Pooling imply that you're aware that spatial heterogeneity might be important. If so, why just 2 subsamples? Page 9191, L14-15: In any case, this will always give you the potential rates only and not the actual processes, whatever the volume of water you add! It is still valuable information per se. Page 9192, L14: Strange dilution, but why not! In any case, you should rather state the quantity of DNA that was added per reaction. Page 9192, L15: Were the plasmid used as standards linearized? Close plasmids may have a strong influence on qPCR efficiency and reliability. Page 9192, L18-19: There is no need for efficiency formula, but r-squared information would be useful. Figure 1: I think that the way you indicate your stat results is a bit complicated and that the figure lacks of visibility. Maybe colours would help? Figure 2: Would it be possible to indicate the stat results on these barplots? Paragraph 3.2 of the Results section and Discussion section: please keep the use of 'potential' when talking about the process measurements that you've performed. Page 9196, L6-7: your hypothesis was not 'distinct response' but 'stronger impact' in the meadow, which is not obvious here since

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potential nitrate immobilisation and potential nitrification seem to be more affected in the non-managed soil. Page 9196, L8-12: Okay, it sounds to make sense. But are you suggesting that the difference between the two sites could be due to mowing? It would be risky since you have not tested this (there was no 'mowed abandoned site' or 'unmowed meadow'... Page 9196, L14: Why 'however'? What is the link between the two sentences? Page 9197, L3: Yes for Zhang et al., not for Gleeson! In any case, do you have any hypothesis about why AOB or AOA may be more responsive to drought stress? Maybe related to the concentration of ammonium in the soil solution with various moisture? Page 9197, L5-7: again here, why Bates??? Also, Szukics may observe higher AOA abundance at their lower water content, in only one of their soil, but it was actually normal moisture for this soil as they state in their M&M (it's WFPS and not WHC!).

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