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Interactive comment on “Amino acid and N mineralization dynamics in heathland soil after long-term warming and repetitive drought” by L. C. Andresen et al.

Anonymous Referee #2

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General comments: The manuscript describes the results of a ^{15}N flux rate experiment on soils sourced from a long-term climate manipulation experiment in heathland in the Netherlands where slight warming, and severe early-season drought have been imposed in-field since 1999. The main focus of this MS is whether mineralisation of amino acids constituted a significant proportion of gross N flux as measured by pool dilution, and whether gross N mineralisation is affected by drought and warming. The manuscript is concise, and most relevant variables are quantified. However, the standard of English is in places poor, so much in fact that parts of the discussion are very difficult to disentangle. I appreciate the difficulties in writing formally in a language

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other than your mother tongue, but nevertheless, it is important that results are communicated effectively. I also have to question why the paper ends with speculation on the concept of niche specificity when this hasn't previously been brought into the context of the work presented here (and could not be [dis]proven in this study in any case).

Specific comments:

1) The specific question buried underneath all this: how much of the mineral N pool comes from amino acid mineralisation is a very interesting one, I find the introduction and discussion muddled. The contrasting concepts of internal vs extracellular mineralisation are introduced and discussed, yet the methods used in the present study would never allow these questions to be answered directly. The main reason for this is that free amino acids (FAAs – and it should be a capital 'F' in the MS for this acronym) only present a portion of the total N that is able to be taken up intact by microorganisms. Recent work highlights that not only are FAAs one of several organic N species to be taken up by plants and microorganisms, peptides in particular may be responsible for a much larger N flux than monomeric FAAs. Consequently, the authors need to either expand their introduction to better cover the recent literature on non-FAA-N, or better, focus more on the effects of climate manipulation per se, rather than heading into an area that cannot be answered by the techniques used. I would prefer the latter option.

2) Following on from this, there are several parts of the introduction (such as P806 L1) that are factually incorrect due to the absence of any acknowledgement that FAAs are not the only form of bioavailable organic N

3) The statistics and subsequent results in part make little sense. In the sampling section, a rigorous approach using composites of 9 reps per plot (field reps) is described. However, in the calculations section, it is stated that each treatment had two reps. Which is it, and if it's the latter, why? Also, why were t-tests carried out when this is surely a simple 1-way ANOVA design? This is further made difficult by discussion of

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non-significant differences in the discussion – if there is no significance, there is no difference!

Interactive comment on SOIL Discuss., 1, 803, 2014.

SOIL

1, C497–C499, 2015

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