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## ***Interactive comment on “Compound-specific $^{15}\text{N}$ stable isotope probing of N assimilation by the soil microbial biomass: a new methodological paradigm in soil N cycling” by A. F. Charteris et al.***

### **Anonymous Referee #1**

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In their manuscript “Compound-specific  $^{15}\text{N}$  stable isotope probing of N assimilation by the soil microbial biomass: a new methodological paradigm in soil N cycling”, Charteris et al use the addition of  $^{15}\text{N}$ -labeled tracers followed by compound-specific  $\text{d}^{15}\text{N}$  analysis of amino acids to track N amendments into newly synthesized protein. The method is shown to be more sensitive than other tracer methods because it only detects added N that is incorporated into new microbial proteins. It also reveals interesting patterns of which amino acids are synthesized and when, which varies depending on the N species added. The paper should be of interest to the soils community, and I found the writing and organization to be very clear and straightforward.

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My main criticism is in the framing of the novelty of the approach. If I understand correctly, the stable isotope probing technique was published by the same group 5 years ago in Knowles et al. 2010, which included detailed methodology for both the experiment and calculations. The main difference in the present work is the use of  $^{15}\text{N}$ -labeled  $\text{NH}_4$  and  $\text{NO}_3$  tracers in addition to labeled amino acid tracers as in the earlier work. I appreciate that this work is aimed at a broader audience than Organic Geochemistry but I don't think that constitutes "a new methodological paradigm" as the title suggests. For the same reason, I'm confused about the general framing as a methods paper. The results are interesting in their own right (especially the contrasting incorporation dynamics of organic vs. inorganic amendments) and presented very clearly to a new audience, but the method and basic approach has been published in more detail for 5 years. The manuscript would benefit from a reframing from the novelty of the approach to the novelty of the results.

Specific comments:

Abstract: the abstract describes the results and capabilities of the approach in the broadest of terms. I think it would benefit from specific results, e.g. the interesting contrast in uptake patterns after  $\text{NH}_4$  vs.  $\text{NO}_3$  addition.

Pg 1138, line 19-24: consider removing. The description of this pathway is not really relevant here and is repeated in the discussion

Pg 1143, line 14: this sentence is not clear as written

Pg. 1145, Line 5: why report both SD and SE here? Mostly redundant and makes the sentence hard to read

Section 3.1: this section demonstrates the flaws of previous methods but doesn't show this method is better, falling short of what's promised in the section subheading. I suggest adding a paragraph here about how much of the labeled substrate was assimilated in the experiments

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Pg. 1148 line 14: probably overstated, this isn't the first study of soil N turnover

Pg. 1148 line 25: should read "this approach"

Tables: Again, why report both SD and SE? They should be offset by a constant factor of 6 (for n=36) so reporting both is redundant. Actually looking closer at the SD and SE in Table 2 some numbers seem off: %TN is only offset by a factor of 3 and SD for leucine appears to be off by an order of magnitude.

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Interactive comment on SOIL Discuss., 2, 1135, 2015.

## SOIL

2, C622–C624, 2015

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