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Interactive comment on "Multi-cooperation of soil biota in the plough layer is the key for conservation tillage to improve N availability and crop yield" *by* Shixiu Zhang et al.

Anonymous Referee #3

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The authors present data from a long term (14yr) tillage trail comparing conventional, ridge and no tillage and their effects on soil biota contributions to N mineralization and crop yield. While the paper highlights potentially interesting aspects of biological soil functioning, I am not convinced that the methods that have been applied are justified for the authors' goals.

Specific comments One key concern is that N mineralization is measured under laboratory conditions and then corrected to field conditions, via a solely temperature-dependent Q10 equation (L112-114). It is well known that the simple Q10 relationship does not hold under realistic soil conditions, since temperature is not the only limiting

factor. Soil moisture, substrate availability, etc also strongly co-determine the biogeochemical process rates in situ (see e.g. Davidson & Jansses 2006 Nature 440: 165-173 for SOM decomp). Therefore, I do not believe that the authors can capture realistic N mineralization rates in their field. I think this paper needs a thorough validation of this relationship.

Similarly, I am highly critical of the way the authors attribute N mineralization contributions from different soil biota groups. They use a series of equations from other authors to transform soil biota abundances into process rates (e.g. L170-L176, L177-188, L198-202). Mostly these steps seem to be based on Rashid et al 2014. These steps form the heart of their study. For instance, the conclusion that conservation tillage promotes N min (L21-23), hinges on these equations that all assume that more soil biota lead to more N min. The same goes for the relative contributions of soil biotic groups to total N mineralization (L25-27). The parameter estimates (e.g. Q10 of 3, L116) used come from different systems in other countries, while it is know that N cycling processes are highly heterogeneous in space and time. I am therefore sceptical that the same relations and the same parameter estimates will hold in the system studied by the authors. In fact even in the source paper, Rashid et al 2014, the ecological-production model is an improvement over the standard government rules, but still there is considerable error in the estimates (87-120% of observed N min rates) on the fields they studied. So I think the authors have to spend much more effort on convincing me and other readers that using these equations leads to valid inferences about this particular system. To be honest, as an empiricist, I think that to only realistic way to get to these questions is to use isotopic tracers in the field plots. However, what would help is if 1) we had realistic data on N min rates in the actual plots, and 2) the summed N contributions over the soil biota would have a strong predictive relationship with these independent field data. As it stands such a field validation is totally missing, which makes the study unconvincing.

Data were missing in some months for nematode data and linear interpolation was used to fill these data gaps (L129). I find this a risky approach, especially since nematode population dynamics within season are non-linear, see e.g. the data in Rashid et al, but also other sources. I think the authors also need to show that their conclusions hold if the only work with the months where they have data on all soil groups.

The authors use the ratios of (calculated) mineral N delivery in the conservation tillage (ridge, and no tillage) to conventional tillage in their main figures. However, ratios are biased (e.g. Jasieński & Bazzaz 1999 Oikos 84: 321-326); a log(Treat/Control) has better statistical properties (Brinkman et al 2010 J Ecol 98: 1063–1073). Even better however would be if the main analyses and figures are directly based on the data from the three treatments directly, this approach would even give you a bit more statistical power. In that sense I find the supplementary figures to be much clearer.

In general, I find that the writing is a bit to colloquial in tone and imprecise in many places. See some examples below. Also I find that the presentation of the energy channels to be a bit overstated, there have been many findings of cross-feeding across these ‘channels’, and really I think we need to adopt a network view of the soil community and its links to biogeochemical processes.

Minor comments - L44: what do you mean with ‘special species’? - L51: what are weak root infections - L55: what do you mean by capacity? Use of substrates? Process rates? - L60: I would not use the word conquer here, maybe mediate? - L61: adverse effects on what? - L66: rich in what sense - L68: what is stratrified and in what way? - L80: based on M&M I believe its 14 years, not 15. - L83: what do you mean coupling? How will you quantify that coupling? - L85: it is a bit unclear what you mean by multiple spatial interactions in this hypothesis. How will you test this? - L94: how big were the plots? - L100: what was done with the maize residue?

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