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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.

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1.Specific comments One key concern is that N mineralization is measured under laboratory conditions and then corrected to field conditions, via a solely temperaturedependent 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.

The lab incubation method and the in situ method are the most common method used in the researches to investigate the soil N mineralization rate. But, both methods have their own limitations (Hanselaman et al., 2004; Wienhold, 2007). So, obtaining the absolute real value is impossible. We used the inorganic nitrogen content measured in fresh soil every month instead of this amount of mineralized N obtained through lab incubation to indicate the status of soil N under different tillage systems during soybean growth. What we want to emphasize is that obtaining absolute value was not our goal, making a comparison between different farming systems, specifically, tillage systems was our core objective. Since the same test method was used for all tillage systems, errors or biases caused by the test method would be the same for samples collected from different tillage systems.

2.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 ecologicalproduction 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.

Researchers have using the theoretical method to quantify the elemental energy flux of soil food webs for more than thirty years. The parameters, such as assimilation efficiency, the ratio of C:N of predator or prey, and feeding preference and so on, used in this method were almost constant over the past thirty years. The classic literature is de Ruiter et al. (1993), Didden et al. (1994) and Hunt et al. (1987), and the recent literature of Andrés et al. (2016, Soil Biology and Biochemistry), de Vries et al. (2013, PNANS) and Schwarz et al. (2017, Nature Climate Change) also used this method to explore the C or N flow through soil food web in the grassland ecosystem of America, agroecosystem of Europe and the forest ecosystem of America. The method is well established and accepted by researchers. So far, as far as we know, there is no research using this theoretical method to quantify the energy flux of the soil food web in Asia or China. In the revised manuscript, we re-calculated the N mineralization of soil organisms according to Ruiter et al. (1993).

3.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 nematode populations for non-sampled months were estimated by linear interpolation between adjacent sampling dates. This method is usually used in the literature (Didden et al., 1994; Berg et al., 2001; Zhang et al., 2019), which assumed that there is a linear course in biomass or abundance of soil organisms between sampling dates.

This method can not track the trend of nematodes population changes, but can yield a more accurate mean value during the studied period.

4.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. Jasienski & 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.

Thank you for your suggestion. The tables and figures were reorganized in the revised manuscript.

5.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.

The soil food web was rebuilt in the revised manuscript. Sensitivity analysis was conducted to test the influence of the uncertainty on the result of N mineralization. All ambiguous results were deleted, and the discussion was rewritten to obtain a concise and logical conclusion.

6.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?

These inappropriate points were rewritten and the missed information was added to the revised manuscript.

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