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Interactive comment on “Interactive effects of belowground organic matter input, increased precipitation and clipping on soil carbon and nitrogen mineralization in a temperate steppe” by L. N. Ma et al.

Anonymous Referee #1

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The present study deals with carbon and nitrogen dynamics in steppe grassland soils as affected by driving factors. The stated purpose is to evaluate the effects of 3 treatments: 2 changes in C inputs (either through direct incorporation of litter into the top soil or by clipping of aboveground biomass) and changes in precipitation (simulated with irrigation), and possible interactions.

While the study does address a relevant topic for the study of ecosystem biogeochemistry (i.e. the dynamics of C and N in soils under simulated changes in precipitation and C inputs), and involves a large amount of measurements and a reasonable experimen-

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tal design, it has a number of major flaws. The first and most important is the lack of clear hypotheses to be tested, with only the objective of finding effects of the mentioned driving factors on C and N mineralization but based on no specific mechanism.

A rather large amount of soil biological and chemical properties are measured and correlated with mineralization rates. Because there are no clear questions, all variables are included in several multiple linear correlations which (almost necessarily) result in some significant models that relate the spatial variability of fluxes with mostly soil moisture and microbial community properties. These correlations are interpreted as causative and the treatment effects explained through them. While a relationship between microbial properties and respiration is expected, they may both be the consequence of changes in other underlying driver factors such as SOM quality and quantity, or changes in soil physical characteristics. It should be noted that incorporation of litter in the study will change soil density and water retention characteristics (and its effects on respiration) and these are not well considered in the study.

Although ST and SM were measured in the field, the mineralization measurements were done in the lab and it is not clear what moisture and temperature are being related to these and why a T and M model was not applied to estimate their effects throughout the year in the field. It is also not clear when and how often SCM was measured.

Apart from a descriptive analysis the study does a poor job at explaining the actual underlying connections between factors. The large number of factors included without an a-priori hypothesis linking them makes the interpretations in the discussion very weak and speculative. The lack of relevant results is noticed in the closing paragraph, which states that effects were observed but gives no further conclusion.

The English of the manuscript still requires considerable efforts to be understood. Many grammatical mistakes are found and many passages need revising. I recommend that the paper is revised properly by a native speaker before the next submission. Also

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some terms were not defined (light and heavy organic matter) and some are confusing (e.g. SOM input is used to refer to addition of litter to topsoil, but clipping will also change below-ground SOM inputs). In general, the connection of ideas throughout the text should follow a more rigorous logic.

The large number of bar plots is non informative.

The study finally shows that changes in C input and precipitation have an effect. This is not at all new nor unexpected. They also show significant interaction effects. However, there is finally no convincing argument explaining these. E.g. moisture and temperature conditions in the field at different depths were likely changed with both clipping and SOM addition (changes in water retention, root water uptake, etc.). These conditions throughout the soil profile, which are main drivers of SOM decomposition, could alone be responsible for the observed interactions, but this more detailed ST and SM information is not given. ST and SM being known as the major drivers of SR should be measured in detail at several depths in such type of field experiments.

I would suggest that the authors focus on specific mechanisms relating their treatments with the response variables and derive hypotheses that can be tested with available or new data, and that they avoid over-interpreting significant multiple-correlations that are common in soils.

Some specific comments:

L28 Change 'SOM input' to some other expression throughout the manuscript, more specific to the treatment (e.g. litter addition). SOM input is too vague and can refer to above or below-ground inputs. L 28 Do not use the expression 'SOM inputs (or increased precipitation)' as the two are very different factors even if they showed similar effects. They are not interchangeable so should not be 'or' but rather 'and'. L30 The first sentences here sound too repetitive. L34 year, and L35 NNR was not defined L37-38 again parenthesis problem. L79-80 Sentence is not clear. L101-104 Both question 1 and 2 are the basically the same.

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