Interactive comment on “Obtaining more benefits from crop residues as soil amendments by application as chemically heterogeneous mixtures” by Marijke Struijk et al.

Response to Flavia Pinzari (RC2)

Transferring knowledge on the decomposition of forest litter to the agricultural sector, to improve organic inputs to the soil by burying plant residues, is certainly an exciting and worthwhile approach. This study is very accurate, scientifically sound and is carried out with great care. The manuscript is well written, and the introduction is very exhaustive. A few comments and some suggestions that could help to improve the clarity of some parts are provided.
Thank you for the thorough and detailed review of our manuscript. Please see below for our responses to each of your comments.

Introduction: The authors well referred to the mechanisms that contribute to the synergistic and not merely additive effects of a mixture of residues. It would be useful to have here a mention of the role of the diversity of decomposers and their niches, and what emerged in studies on forest systems regarding the composition of communities in homogeneous systems compared to mixed systems, not limiting the scenario as purely a matter of microbial carbon use efficiency. The article https://doi.org/10.1111/mec.13739 and other (some quoted therein) give some useful perspectives on dynamics (and complex succession) of bacteria and fungi during de-composition.
We will incorporate the suggestion to include mention of microbial succession during decomposition by adding the following sentence to the paragraph starting at line 72:

“… Other authors have also suggested the possibility of manipulating the functionality of the soil microbial community with soil amendments, such as Li *et al*. (2019) who report that eutrophic microbes are stimulated by organic carbon amendments and oligotrophic microbes are stimulated by chemical fertilisers. Studies have also demonstrated that changes in tree litter diversity affect both fungal and bacterial diversity (Otsing *et al*., 2018; Santonja *et al*., 2018). **Research on decomposition in forest systems indicates a succession in the community composition of microbial decomposers as the decomposition of residues progresses (Bastian *et al*., 2009; Purahong *et al.*, 2016), and this succession is different in decomposition of litters of different qualities (Aneja et al., 2006).”**

Additional references to include:
Aneja (2006): doi.org/10.1007%2Fs00248-006-9006-3
Bastian (2009): doi.org/10.1016/j.soilbio.2008.10.024

Puraong et al. (2016): doi.org/10.1111/mec.13739

Materials and methods: The authors limited the analysis of chemical elements to K, P and Mg, besides C and N. The decomposition of organic residues is also linked to other elements that play an essential role in some enzymatic activities fundamental in oxidative processes (manganese, copper and iron, for example). I find this being a bit of a limit of the work that indeed found some significant correlations, reported in the last table and only marginally commented. The introduction of specific residues may have had an impact on the availability of certain microelements capable of explaining the course of some processes.
L. 64 commented on by the other reviewer has been revised as follows:

> *"These experiments suggest that non-additivity in decomposition rates and changes to soil C and N dynamics could go hand-in-hand"*

This reviewer comment led us to concede that there is only evidence that non-additivity is related to C and N and not other soil properties. While microelements are important mediators of decomposition in some natural ecosystems (e.g. forests), these agricultural soils undergo frequent analysis by the farm and corrective measured introduced if micronutrients are below optimal levels. Therefore, we have not included them in our discussion.

We have, however, analysed the residues for some of these elements, so we suggest including these in Table 3 as follows:

Table 3: Residue characterisation (SEM indicated in parentheses, n = 3).

Nutrient	compost	straw	woodchip
C (g/kg)	322.3 (0.433)	459.0 (1.012)	485.3 (1.121)
N (g/kg)	25.3 (0.167)	11.2 (0.083)	7.6 (0.105)
C:N	*12.7 (0.084)*	*40.9 (0.368)*	*63.6 (0.760)*
P (g/kg)	5.5 (0.076)	1.0 (0.025)	0.9 (0.024)
K (g/kg)	20.6 (0.31)	13.1 (0.22)	5.1 (0.10)
Mg (g/kg)	4.3 (0.014)	0.7 (0.015)	1.3 (0.040)
Mn (mg/kg)	258 (1.68)	41 (1.15)	41 (1.67)
Fe (g/kg)	15.0 (0.051)	0.5 (0.015)	1.0 (0.060)

The recovery of a certified reference material analysed alongside our samples for copper is only 78%. This represents a QC failure and so we are not confident about including the Cu data. The recovery of Mn (92%) and Fe (114%) will be included in the Methods section.

Results Lines 240-45: "Contrary to our hypothesis, there was a non-additive increase in pH from the mixtures relative to individual amendments (hypothesis 1)". Why should pH increase more in a synergic scenario? How would the authors link pH dynamics with decomposition and the quality of starting materials? This part needs clarifications.
We agree that clarification is needed because the expected effect of the treatments on soil pH has not been explicitly hypothesised. We will therefore revise the manuscript accordingly.

We would expect pH to increase when organic residues are added to the soil since decomposition of residues leads to the ammonification of residue N (Xu et al., 2006), and since faster decomposition is expected in the mixture treatment, compared to the parts, we would expect a greater increase in pH. Indeed, this should be stated explicitly in hypothesis 1, so we suggest to revise L. 114 to read:

> *"In particular, we hypothesised faster decomposition of residue mixtures to result in a higher soil respiration rate in the short term, as well as the release of greater levels of soil available nutrients (N, P, K, Mg) and SOM, compared to individual residues, which leads to greater ammonification of resudie N (Xu et al., 2006),* ***and, in turn, leads to a greater increase in pH*** *(hypothesis 1)."*

This means that the finding that the pH increased is in fact in agreement with hypothesis 1. So we further revise line 240-45 as follows:

> ***"In agreement with*** *our hypothesis, there was a non-additive increase in pH from the mixtures relative to individual amendments (hypothesis 1), although this was not significant (Table 5) and per-treatment results (discussed in next section) show that the pH decreased in all treatments relative to the control (F = 2.238; p = 0.095; one-way ANOVA; Supplement S2)."*

Additional references to include:
Xu et al. (2006): https://doi.org/10.1016/j.soilbio.2005.06.022

Correlations 3.3 - This part would deserve more space. Although it is true that the effect of the synergy of the use of diversified starting materials can be well seen on the quantitative data, I believe that correlations between the amount of nutrients applied and the amount of available K and Mg in the soils are extremely significant. I would be curious to see if the values of R and its sign (+/-) changes when calculated between respiration or SOM and K, Mg, P within each treatment, or between the two main kinds of treatments (mixed or not).
We do not wish to undertake correlations of measurements made on replicate plots within an individual treatment since the value of n would be 4 (or 8 if separate correlations are made on replicates in the mixture and non-mixture treatments). It is not clear what hypothesis we would be testing when conducting these correlations.
Significant correlations between the amount of nutrients applied and the amount of available nutrients, and correlations of Yield with available nutrients and SOM are highlighted in section 3.3. However, as requested, we expand on our description of the data presented in Table 7, to also include the significant correlation between SOM and available nutrients:

"A number of noteworthy correlations may help explain the data and are summarised in Table 7. There were some significant correlations between the amount of nutrients applied and the amount of available K and Mg in the soils at the end of the experiment, which indicates a positive effect of the residue amendments. The amount of C applied via the residue amendments was not correlated with the amount of SOM. Yield was positively correlated with the sum of available and potentially mineralisable N, available P and Mg, SOM and aggregate stability. **SOM was also positively correlated with available N, P and Mg, and with soil respiration.**"

Discussion: The different organic residues may have contributed groups of microorganisms capable of directing the decomposition process. The effect of the founder is a process that has a certain weight in the phenomena of ecological succession, especially in the case of microorganisms (see, for example, this work: http://dx.doi.org/10.1016/j.pedobi.2017.01.004). The manuscript under revision is very articulated and definitely not focused on the more strictly microbiological aspects, but a mention of the possible role of the microbial charge of the source material is necessary for the discussion of the results.

Although this does not contribute to the interpretation of our results, it could certainly be mentioned that the residues introduce microbes that may not already be present in the soil. We therefore agree to revise L. 324 as follows:

> *"As pointed out by Lecerf et al. (2011), niche complementarity effects, in which different groups of decomposing organisms* ***(already present in the soil, or newly introduced via the residues)*** *develop a synergistic association in residue breakdown, tend to advance with time, leading to a generally higher number of long-term litter-mixing studies finding non-additive effects."*

We hope we have responded to these comments in a satisfactory manner.