EGUsphere, author comment AC2
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Reply on CC1

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Author comment on "Impact of contrasting fertilizer technologies on N dynamics from subsurface bands of "pure" or blended fertilizer applications" by Chelsea K. Janke and Michael J. Bell, EGUsphere, https://doi.org/10.5194/egusphere-2022-511-AC2, 2023

We thank the Reviewers and Editor for their valuable feedback and consideration of our manuscript for publication in *SOIL*. Specific responses to comments from Reviewer #2 are listed below.

This manuscript presents a detailed experiment investigating the interactions between banding and the characteristics of different enhanced efficiency fertilisers including blends of controlled-release fertilisers and nitrification inhibitors on NH4 and NO3 outcomes. This is important as blends have received little research attention to date and the aspects of fertiliser placement have not often been studied in this detail – often they are just tested agronomically.

The experimental design is novel and described thoroughly and in great detail. The results include not only NH4 and NO3 observations within the fertosphere and in concentrical bands around it, but also urea, aqueous NH3 and supporting information on pH and EC to characterise the soil conditions within which nitrification occurs. In addition, the dynamics are observed in two contrasting soils.

The authors do a great job to untangle the effects of slow release and nitrification inhibition by both the changed soil conditions following urea hydrolysis and the nitrification inhibitor. The story is quite complex though – requiring the reader to study the figures in detail and stay focussed for quite some time. Below a few suggestions that may make that easier for the reader.

>> The text in the manuscript will be revised generally, and in specific sections as detailed in responses, to reduce the complexity of the manuscript and simplify explanations / concepts.

In addition, I suggest to not get drawn into a discussion of why reduced N losses may not always translate into improved NUE (section 3.5 and parts of section 3.4). Aside from a note how banded application can delay availability of N, that discussion is not really informed by the results you present. The system aspects have been better handled elsewhere with modelling tools to untangle the complex interactions.

>> This section which discusses why reduced N losses do not always translate into

improved NUE will be revised to include just a short note and reference to texts that better cover this topic.

The side story on biodegradable CRF coatings may at first also seem a little bit of a distraction from the main aim of evaluating the blends. However, it is good to get these results out in the open so that people can start to build a picture of their behaviour.

>> Care has been taken to ensure that discussion on the biodegradable CRF is succinct, as it is important but not necessarily a focal point of this manuscript. In the revised manuscript, this discussion with again to reviewed to ensure it is not unnecessarily verbose.

Section 3.1: Minor edits

Shorten some of the sentences to make them easier to read

>> This will be done in the revised manuscript as part of the effort to reduce complexity.

206: remind the reader that the fertosphere you refer to is the inner circle of the figures

>> Although the fertosphere has been defined earlier in the manuscript, a reminder will be inserted here again.

Section 3.2: Minor changes to figures, inclusion of S2 and some extra discussion

The nitrate concentrations are difficult to see on the same scale as NH4 in Fig 5 and 6. The absence of an x-axis line exacerbates this. While it helps to see the contrast in concentrations between NH4 and NO3, I think it is more important to be able to see at a glance the differences in NO3 in time, with distance, and among products. Including the x-axis will also help visually. Have you tried if figure designs like those used in Fig 3 and 4 would make it easier to see differences between products (i.e. separate figures for NO3 and NH4)? Online version could maybe also be in colour?

>> It's not clear here what the Reviewer means by "x-axis line". Presumably, the Reviewer is referring to the fact the scale (0 - 10 cm) is not denoted under every plate in the figure. Doing this makes the figure unnecessarily "texty". However, we will revise the figure so that the marks which indicate the scale are clearer (bigger, bolder) in each plate. In terms of displaying the NH4+ and NO3- concentrations - having both together helps give a better idea of what is happening within each treatment. Whilst we do also understand the Reviewer's point, unlike urea and aqueous NH3 (Figs. 3 & 4) which can suitably demonstrate processes (i.e., hydrolysis, aqueous NH3 formation) when presented "alone", we think presenting both NH4+ and NO3- in a single figure is more appropriate for demonstrating nitrification dynamics. Instead of separating the two N species, we will revise Figs. 5 & 6 so that NO3- is on a separate scale and trends in this N species can be better observed.

Figure S2 is quite central to understanding the text in this section. Hence, it should be included in the paper, along with a brief explanation how NO3 production was calculated.

>> This data will be presented in a N mass-balance table which will be included in the revised manuscript. A description of how the mass-balance is calculated will be included in the table caption.

Is there a possibility that the results in S2 do not represent all NO3 production, e.g. due to losses? (PCU/POCU are still releasing urea at DAI60, whereas urea and urea-DMPP have released all. Yet, the concentrations of NH4 and NO3 are lower in urea and urea-DMPP?) On the other hand Fig 7 suggests that all N was 100% recovered in either the granules or as mineral N in soil solution. Is that correct, or was only the proportion in the granules measured and related to the initial amount?

>> It is likely that some of the applied N is not accounted for in the total NO3 production data of Fig. S2. This is due to unmeasured losses (gaseous, microbial or via sorption to soil matrix) or some of the N remaining as NH4+, which will vary between the treatments. Inclusion of N mass-balance data combined with discussion on soil chemistry (see earlier responses) in the revised manuscript will address these differences. For Fig. 7 - all recoverable granules were extracted and measured but total N recovery (also including soil N measurements) was not 100%. The data in Fig. 7 is presented as a proportion of "recovered N". Thus, the two components (granule N and soil N) will combine to form 100% of recovered N. Given the confusion of both reviewers around this figure, we will revise to either (i) make clearer the data that is being presented, or (ii) revise so that the presented data is a proportion of applied N, and not recovered N, or (iii) potentially eliminate the figure entirely if deemed unnecessary after inclusion of a N mass-balance table, which will describe all treatments.

Section 3.4: This is a good place to compare the results of this study and what they may suggest with the findings of field experiments that include crops etc.

>> We will revise the manuscript to ensure that most of the discussion on the field / management implications of the findings in our study is placed in this section. This will primarily be done by trimming from Sections 3.2 and 3.3 (i.e., discussion on N dynamics in the two soils).

Can include here the text at the start of section 3

>> Is there a line number missing here? Or is the Reviewer referring to Section 3.4?

Please include a reference to the results on which basis you conclude that there appears to be little advantage in using CRF/DMPP-urea/blends on the higher soil of poor chemical buffering. Was it on the basis of Fig S2? If so, that figure should be included inside the paper. If the similar NO3N production is a net effect that cannot account for any N losses, is it then the right conclusion that there seems little advantage? Possible N losses not accounted for would be worth a discussion here.

>> This conclusion (lines 284 - 287) was made on the basis that there are only small differences in N dynamics around bands of the various fertilizer treatments over time in the Ferralsol (e.g., Fig. 5). This reference will be included in the revised manuscript. However, this sentence was also incorrectly written and will be corrected to indicate "N dynamics and availability" rather than "N losses". In fact, we go on to describe scenarios where some of these EEFs/blends may mitigate losses in soils similar to the Ferralsol (lines 289 - 291), although we avoid excessive discussion on N losses as these are not a focal point of our study.

Contrast with field experiments that obtained benefit on the soils that your work might suggest wouldn't see benefits is a good discussion point. It allows a useful warning that wider system perspectives may overrule the fine scale effects of the bands. However, suggest to not get drawn into an interpretation of the experimental results and the wider system effects.

>> Agreed. Our results are useful for understanding mechanisms of fertilizer dissolution, reaction and distribution under controlled conditions. They can be extrapolated to help

explain field outcomes but can not be used in isolation of other conditions influencing the wider system. In the revised manuscript, we will ensure discussion on wider system outcomes is concise.

l.300 – note that the inability of the crop to take up the N causing losses of N later in the season was not caused by the slow release. It related to N being surplus to crop uptake potential (either for a period or for the season as a whole). Increased losses seen in some experiments later in the season could also relate only to the pathways they measured. They could come about if the CRF protected the N from losses along other pathways earlier on. If the crop is unable to use the initially 'saved' N, this can lead to the later N losses. Losses late in the season when crop uptake is low are likely a consequence of excess N unless the wrong release pattern was used.

>> This sentence will be revised to indicate N in surplus of crop demand, which may occur later in the season, is vulnerable to loss and that the reasons for this a multi-faceted.

Section 3.5: your results do not contribute new evidence or insights to this discussion, so this should not be part of this paper. Implications for short and long season crops would require a more thorough analysis.

>> This section which discusses why reduced N losses do not always translate into improved NUE will be revised to include just a short note and reference to texts that better cover this topic.

Section 3.6: this section includes some speculation – suggest shortening and not get too deep into potential theories without having back-up evidence for them. Brief statements of possible explanations should suffice.

>> The more speculative observations will be revised and made more concise in this Section.

Is the POCU coating designed to swell and release via diffusion through the coating, or is its slow release associated with gradual breakdown, fragmentation of the coating? Are you in a position (i.e. have evidence) to distinguish between the two and conclude that the higher release was due to osmotic induced bursts based on visual observations of retrieved granules?

>> We do not have information on whether the POCU coating is designed to swell and release N, as many PCU products are. However, given the observation of burst granules (splits in the granules with the coating otherwise intact), osmotic pressure increases are more likely than rapid degradation (which would presumably result in degradation that is more uniform and look less like a 'cut' or 'tear'). We are therefore reasonably confident that POCU granules are more susceptible to failure under increasing osmotic pressure (cf. PCU). However, as we did not directly observe/test the breakdown mechanism(s) of these granules, the Reviewer is correct in that we cannot be fully conclusive. Since the study requires some explanation for the initially higher NH_4^+ concentration from POCU (cf. PCU) we will keep our present hypothesis (i.e., granules bursting due to osmotic pressure increases) but revise the manuscript to indicate that this is a hypothesis and requires further validation.

Unless there are contact issues, water absorption would be determined by gradient in water potential – not the water content. The potential gradients should be similar for the two soils given they were both at field capacity and primarily driven by the high concentration of urea inside.

>> A similar point was made by the other Reviewer also. We intend to revise this comment to reflect that closer soil-granule contact in the higher clay soil (Vertisol) may have facilitated more rapid water uptake in CRF granules in this soil. Further, there is potentially greater microbial activity due to the higher clay and OM in the Vertisol, possibly contributing to more rapid degradation of the POCU granules in this soil. It is these factors (rather than the water content) that may have driven differences in water uptake and release dynamics between soils and CRF products.

The comment on earlier crop-availability of the N from POCU compared with PCU requires that the early differences in N dynamics were statistically significant and that you indicate (with data on crop N uptake) which crops have such early N demand that the differences would have an impact.

>> Whilst we do qualify this statement with a requirement for "studies in which realistic plant N demands are placed on similar POCU products" we will remove this sentence to avoid unnecessary speculation.

You mention on a few occasions (including in the Introduction and Conclusion) a concern that N delivery from CRFs may be too slow for early crop N demand. Crop N stress could indeed occur if there was a mismatch between release pattern and N uptake pattern or if the gap between them was too short to allow transformation of the released form of N into a crop-available form. However, I have not seen any studies demonstrating this happening. My understanding is that the early N demands for most field crops appear to be small and easily met by starter N and/or stored soil mineral N. Often the peak N demand period may not start until 30 or more days after sowing/planting. By then many of the commercial CRF would have released 30-50% of their N. A generic statement [that time of release and time required for transformations into a crop-available form need to be taken into account for synchronisation with crop uptake] can be made and would make sense given some of the banding effects seen. However, if you want to express it as a concern (or as an advantage of POCU, l.352), this will need to be backed up through comparison with crop N uptake data.

>> Whilst there are several studies (cited in the text) which demonstrate differences in the responses of long and short season crops to CRF products, the Reviewer is correct in that these studies do not provide enough information to confidently suggest that this is due to delayed N availability (from CRF cf. urea) and not other factors (i.e., loss events, band conditions, etc.). However, we think it is worth noting that field conditions do not have constant moisture contents and profile distribution, and this can impact release dynamics and N availability from CRFs. For example, soil drying in top layers of the profile between rainfall events could quite easily restrict N release from CRFs. In contrast, urea-N may have moved into deeper soil layers and diffused through a larger volume of soil, increasing crop access. We will revise the manuscript in the relevant places to include a more moderated statement around ensuring N release / availability matches crop demand, and that water dynamics in the soil profile of the field may affect N availability from the differing fertilizer types.

Conclusions: A few suggestions for consideration:

Focus on the findings from this paper only. The aim of the paper was to find out whether the blends provided a case of being more than the sum of its parts so this should be a focus of the conclusion section.

>> As part of the revision to condense this manuscript, we will focus on the findings directly derived from this study, and refer back to the original aim of the paper.

Note the issues mentioned above on early season crop N demand, late N losses (indicates excess N relative to crop potential), and osmotic pressure causing burst.

>> See above responses.

Other editorial comments and suggestions:

219 – causal instead of casual

>> This will be corrected.

195-200: leave until later in Discussion – discuss first the results and do not upfront discount them. (Note Bell et al 2021 seems missing from ref list)

>> This section will be moved to later in the discussion. The references for Bell et al. (2021) will be checked and included in reference list.

Many sentences are quite long. Some could be simplified (e.g., "are deployed in fertilizer products which" in l.34-35 could be removed without changing the meaning of the sentence). In other places sentences can be broken up.

>> As part of condensing and simplifying the manuscript, individual sentences will be shortened and / or split.

It would also be useful to break up some of the paragraphs (e.g., l.65 – 94)

>> We will consider this as we revise the manuscript.

Abstract - check for implications from any of the above comments

>> The Abstract will be revised with respect to all revision made in the main body of the manuscript.