Response to Reviewer and Editor comments

We thank the Reviewers and Editor for their valuable feedback and consideration of our manuscript for publication in *SOIL*. Specific comments have been addressed as outlined below.

Reviewer #1 comments

The manuscript deals with important aspects of using Enhanced Efficiency Fertilizers – EEFs related to N-dynamics and N use efficiency, with focus on mixtures (blends) of urea with different EEFs, applied in sub-surface bands. The importance of such a research relates to: The Complex activity of N-dynamics associated with different EEFs (e.g., polymer coated urea-PCU, Plant-Oil coated Urea – POCU, Nitrification Inhibitors -NI with urea) under various chemo-physical conditions in different soils; Application modes; Efficiency of controlling N- release and dynamics; Agronomic cost effectiveness of different EEF technologies; And, environmental aspects related to NUE and to biodegradability of different EEF polymeric coatings (e.g., PCU vs. POCU). Better understating of the interactions between the various factors controlling N-dynamics and NUE can assist improved decision making related to: agronomic, environmental and cost effectiveness for better managing application of mixed EEFs in different soils.

The experiments are well conducted in a laboratory incubation system with 2 different soils under field capacity (FC) moisture. The results provided emphasize the differences induced in N-dynamics by the different urea sources and the two differing soils! And, provide insights to the options of using mixtures of urea with NI-urea and PCU and POCU, rather than using only PCU, which is the most expensive EEF and also based on non-biodegradable polymer coating.

Field capacity conditions (used in this work) may not cover potential changes in soil physical, chemical and biochemical conditions under crop grown field conditions, affected both by plant uptake and irrigation or rain events. This aspect is addressed in line 323, where the authors indicate that moisture dynamics may be an important factor challenging N- synchronization with crop demand. Possible examples for such events are shortly provided in section 3.4 where the authors mention that leaching due to heavy rainfall or extensive leaching may occur and affect the demand for technologies providing better control over N supply.

Considering the said above, I think that potential N losses due to gaseous emissions (ammonia, nitrous oxides, molecular N) are also an important factor to be mentioned/considered when investigating the improved approaches of using EEFs mixtures in different soils. Yet, surprisingly this option is not mentioned in the manuscript. It is indeed expected that band application may reduce part of the gaseous emissions; And yet, the increased pH levels after urea and NI-urea application (Figure 1) and the high initial ammonium levels (Figures 5 and 6) are expected to induce ammonia emissions (e.g. review of Pan et al. 2016, Agriculture, Ecosystems and Environment: 232:283-289). This indeed is more expected in the Ferrasol and can be intensified by NI applied with urea (e.g., Pan et al., 2016).

>> Gaseous measurements were not conducted in this study as the focus was on soil N dynamics around fertilizer bands and the implications for plant availability (i.e., N losses were not a focus). Further, as pointed out by Reviewer 1 in the cited meta-analysis (Pan et al. 2016), gaseous N losses are usually somewhat mitigated when fertilizers are placed in sub-surface bands, and we were therefore not expecting significant loss of N through volatilization. However, the design of this incubation may have created some conditions which facilitated N emissions via denitrification, especially in the Vertisol (see below response). Using a mass-balance approach (see below responses), we can calculate the relative differences between treatments in terms of total N losses and use the closely monitored soil chemistry data to indicate which loss pathway(s) were likely dominant. A table reporting the mass-balance values of NH_4^+, NO_3^- and total mineral N will be included in the manuscript to facilitate discussion on N losses, including gaseous emissions, in relevant sections throughout the manuscript. See below responses for some specific instances where this discussion may be included.

Since the manuscript does not provide an estimated or calculated mass-balance of the mineral-N applied in the different treatments, I tried to estimate it for the Urea and NI-Urea treatments. This was done on the basis of total nitrate-N produced after 60 DAI via Urea treatment in the Ferrasol (Figure S2) and the estimated values of nitrate-N and ammonium-N after 60 DAI roughly evaluated on the basis of Figure 5 (provides only rough estimates since the concentration range in the figure are very large : 0 to 3000 mg-N per kg soil!!!). Nitrate-N from Data in Fig. S2 is estimated in the range f 450+ mgN/pot! From Fig.5 the ammonium-N appears somewhat higher than the 450+ mgN/pot of nitrate-N. Yet, when considering that the total input of urea was 1350 mgN/pot a question is raised about the N balance: there seems to be unaccounted N in this treatment. A similar trend is observed for treatment of NI-urea after 60 DAI in the Ferrasol (Figs. S2 vs Fig.5). For treatments of PCU, POCC and 1:2 DMPP-urea:PCU in the same soil, both nitrate-N and ammonium-N appear to be higher (Apparently closer to the initial value of 1350 mgN/pot), but there is no N-balance estimation or calculation which indicates the levels of total mineral N in the soil after 60 days and its comparison to the initial N-input.

>> Calculating the mass-balance of N in these pots/treatments based on the data in the supplied figures is very difficult, especially when using Fig. 5 where concentration data is presented for each zone (e.g., 0 cm, 2 cm, 4 cm, etc.). The reviewer would have needed to calculate the volume of soil in each increment in order to convert the concentration values to absolute 'mg' values, and this would not be possible (or very difficult) with the supplied information. A brief review of the mineral N mass-balance in all treatments and soils shows that at no point did total mineral N exceed that supplied in treatments (i.e., 1350 mg N). Recoveries of mineral N were ~82% for urea and NI-urea at 10 DAI in both soils and decreased over the 60 d incubation. Recoveries for the controlled-release fertilizer treatments can be calculated when including the granule N contents and are very close to 100% at 10 DAI in both soils and remain high relative to the uncoated fertilizers.

A mass-balance was not originally supplied as gaseous N emissions were not measured, meaning N unaccounted for could be found in several pools (i.e., lost via emission,

microbial consumption or sorption to soil particles). Whilst providing mass-balance values for the mineral N does not facilitate additional understanding in terms of N availability (compared to the already presented datasets), it does provide useful insights into loss dynamics and pathways, which we had not previously considered. As indicated in the prior response, we will include a mass-balance table (including the % recoveries of applied N) to facilitate discussion on N losses.

When comparing mineral-N for treatment DMPP-urea in the Ferrasol to that in the Vertisol: nitrate-N levels at 60 DAI appear close (450+/- mgN/pot), but ammonium-N appears to be significantly higher with the DMPP-urea compared to the urea treatment (Figure 5). This again, indicates that there is missing N is the urea treatment which should be shown and explained!

>> By 60 DAI, there are indeed large differences in total mineral N between urea and DMPP-urea treatments in the Vertisol (Fig. 6) but not the Ferralsol (Fig. 5). In general, recovery of mineral N in both treatments remained high (67 - 70% of applied) over the 60 d incubation in the Ferralsol but decreased significantly in the Vertisol (to 20% and 54% for urea and DMPP-urea, respectively). Given the differences between urea and DMPP-urea (where a large portion of N was preserved as NH_4^+) in the Vertisol and the high volumetric water content of this soil at field capacity (the incubation conditions), it's likely N in the urea treatment in the Vertisol was lost via emissions in the denitrification pathway. Significant volatilization losses were not likely to be a major contributor as the conditions for volatilization were greater in the Ferralsol (i.e., higher and persisting increases in pH, coarser textured soil) and a relatively smaller amount of N was lost from treatments in this soil. Although, volatilization may at least partially explain the small losses of N from urea and DMPP-urea treatments in the Ferralsol. This discussion and explanations will be included in the relevant section(s) of the revised manuscript.

Considering such observations I think that the authors should provide a mineral N-balance calculation based on the measurements of N that they have done and accordingly try to evaluate in which treatments there might appear data of N losses, potentially, due to gaseous emissions. Under the experimental conditions (FC) ammonia has the potential to be the largest loss option and particularly in the Ferrasol. In Fig. S1 , there is a clear reduction of nitrate-N in both tested soils without addition of fertilizers. This is particularly evident in the Vertisol where nitrate-N reduces from around 15 mgN/kg-soil to zero, thus indicating also potential losses due to denitrification under the experimental conditions!

>> We agree with Reviewer 1 in that providing mass-balance and % recovery data in the form of a table will facilitate discussion on potential loss pathways, including gaseous emission.

Remarks for specific points in the text:

-Lines 285-287: " there appears to be little advantage in using a CRF, DMPP-urea or blends of the two (cf. urea), in soils of high permeability and poor chemical buffering" This

statement does not consider the potential gaseous losses (e.g. ammonia) which according to the above presented estimates indicate lower levels of missing mineral-N in the in EEF treatments (particularly with PCU, POCC and 1:2 DMPP-urea:PCU).

>> As indicated in earlier responses, we will include discussion on potential loss pathways, including those relating to gaseous emissions, in relevant sections alongside the mass-balance data. This will be one of those sections which includes discussion on potential losses from the differing fertilizer treatments.

-Lines 330-332: " By 35 DAI, NH4-N concentrations in the 0 – 5 cm zone of the POCU band were slightly lower than that of PCU in the Vertisol, suggesting rapid nitrification of the NH4- N released earlier in the POCU treatment" Yet, this could also be a result of higher N-gaseous losses (e.g. ammonia or even potential denitrification when high levels of oxygen are consumed by the fast oxidation of nitrate to ammonium) which were not evaluated in the research.

>> See above response.

-Lines 343-345: " The greater volumetric water content of the Vertisol at field capacity (cf. Ferralsol; Table 1) may have contributed to more rapid water uptake and hence more frequent rupturing of POCU granules, resulting in the initially higher urea-N and NH4-N concentrations in this soil (Figs. 4, 6)" This seems a bit problematic assumptions: Indeed the water content of the Vertisol at field capacity (FC) is higher, but the "water holding capacity" (water potential) at FC of the two soils is supposed to be close and this is the physical parameter which controls water uptake. Yet, since the POCU has a biodegradable coating, the microbial activity in the Vetistsol (much heavier soil, with more organic C and N) may be higher and thus affect the stability and degradation of the POCU coating.

>> The Reviewer raises a good point here. We will revise to indicate that potentially greater microbial activity and / or closer soil-granule contact - both associated with the higher clay content of the Vertisol *cf.* Ferralsol - may have facilitated more rapid degradation of the biodegradable coating, resulting in a higher prevalence of burst granules and the observation of initially higher urea and NH_4^+ concentrations in this soil and treatment.

Lins371-374: " The plant-oil coated urea product that was evaluated initially released more N due to a higher prevalence of 'burst' granules, which was likely an outcome of somewhat poorer tolerance of increased osmotic pressure within granules (cf. PCU). However, overall dynamics and proportions of N in mineral forms (NH4 + and NO3 -) were similar to that of PCU, suggesting this technology may be a suitable option for managing the competing requirements......." The conclusion is indeed right! And yet there were some differences between the POCU and PCU in terms of higher pH increases with POCU (Fig.1) particularly in the shorter time of 10 DAI which could affect ammonia losses (and possibly affect microbial activities). In Fig.3 much higher values of aqueous ammonia concentration were obtained with the POCU in the short range in the Vertisol which may

also indicate potential of higher losses of gaseous ammonia. Such options were not considered/estimated in this work, and no N-balance was performed which could allow estimating N gaseous losses in the different treatments.

>> The slightly wider zone of pH increase in the POCU (cf. PCU) is a result of greater urea hydrolysis (this reaction increases pH) as indicated by higher NH_4^+ concentrations and is due to the higher initial release of urea from burst granules. However, the difference in pH dynamics between PCU and PCOU only occurred in the Ferralsol (where pH buffering capacity is poor) and only related to the volume of soil impacted (i.e., the magnitude of pH change was similar between treatments). In the Vertisol, restricted solute movement and a high pH buffering capacity meant that high aqueous NH3 concentrations formed in only a very small volume of soil (0 - 1 cm zone). Whilst the conditions within the POCU band in the Vertisol may have initially been conducive to NH3 volatilization, mass-balance data does not suggest that any substantial differences in recoverable N (in soil and persisting granules) exist between PCU and POCU at this time. In short, whilst some differences exist between PCU and POCU in terms of soil chemistry and N dynamics shortly after fertilizer application, the practical management implications of buried fertilizer bands (which generally do not enable large amounts of volatilization losses) means that we consider the above discussion unnecessary in the manuscript at this point.

Remarks to figures:

Figure 4: Urea treatment is, expectedly, not presented in this figure, but it is introduced below with a circular dark symbol.

>> The urea treatment is either missing from the figure (and should be included) or hidden behind other treatments. This figure will be revised to ensure that the urea treatment is visible.

Figure 5: The maximal concentration levels in the figure are around 1000 mgN/kg soil, but the scale is 3000. It is recommended to use a lower scale for this soil, which will better enable estimating the trend and changes.

>> The reason for setting the graph Y-axis maximum at 3000 was so that it could be directly compared to Fig. 6, where N concentrations were higher. However, in response to the reviewer's comment, we will revise to a lower maximum in order to better demonstrate trends.

Figure 7: It presents the percentage of total recovered N in the soil and in the coated granules. Yet, it is not indicated on which basis are the data provided! Are they based on the total initial N input of 1350 mgN/pot. If that is the case, the data indicate 100% recovery of the initial N input!! Yet the figure is presented without any statistical analysis/information. Once there will be an estimated/calculated N-balance it is worse to provide clear information about the meaning of total recovered N and the way it is estimated at the end of the experiment.

>> The figure caption (end of manuscript) explains that this graph is presented with each component as a percentage of the total recovered N (i.e., N from soil and N from granules). The figure caption and Y-axis label will be revised to make this clearer.

Reviewer #2 comments

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.

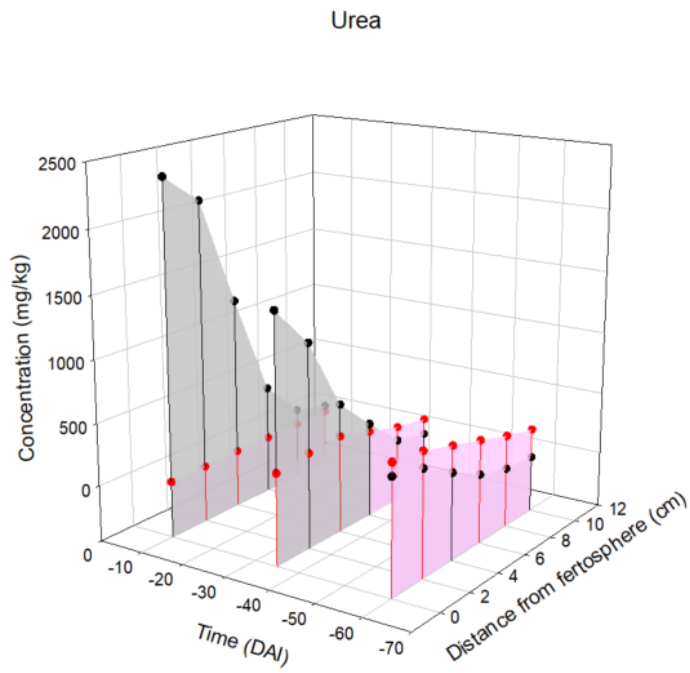
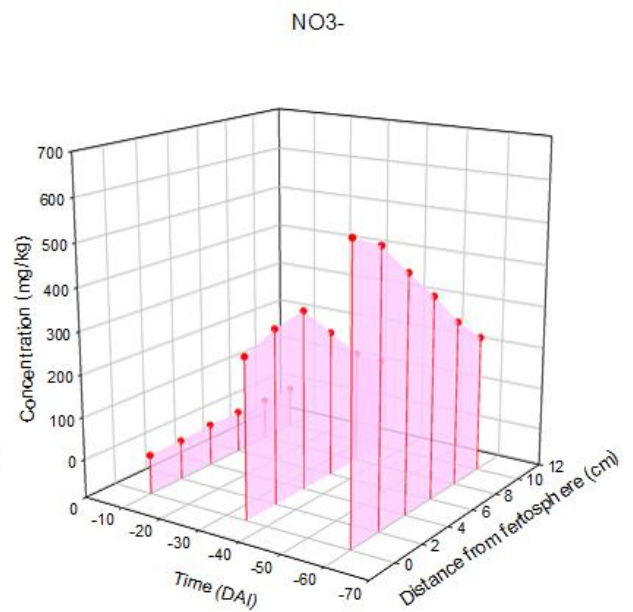
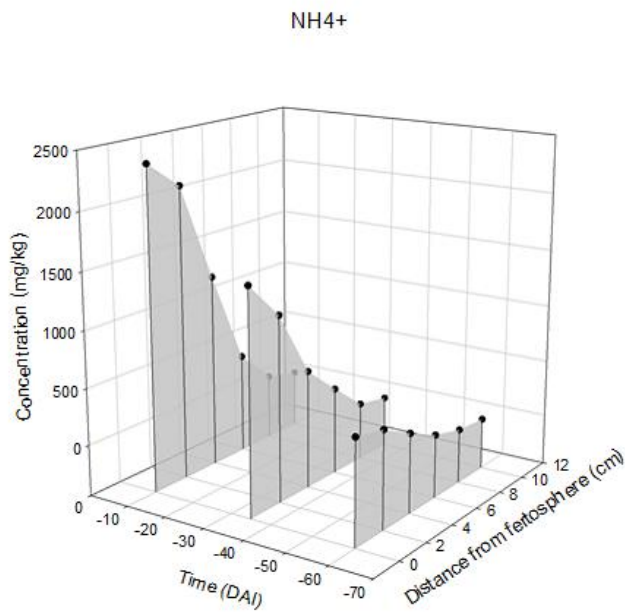
Abstract - check for implications from any of the above comments

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

Editor comments

The present study does not particularly report on a particularly novel topic, i.e. comparison of N-release of several slow-release N-fertilizers, but it does distinguish itself in the robust way how this was done in practice. Much due attention has gone into assessing the spatial distribution of mineral N-release surrounding the centrally inserted N-fertilizer with a custom-developed soil sampling scheme. The study also has merit in that next to just measuring mineral N-build-up also urea-N concentrations were simultaneously monitored and NH_3-concentrations were predicted from pH and NH_4^+ levels. The topic is very well introduced and description of the applied methods is clear and complete.

The data presentation of EC and pH changes in 'heat maps' is appealing, but presentation of 'N'-data is less efficient. The presentation of NH_3-aq, urea-N, and NH_4^+ and NO_3^- is less clear. For example, perhaps 3D graphs with vertical axis concentration, horizontal-axis time and the 'depth'-axis distance from fertosphere would be clearer. Results of statistical comparison between treatments or time are also best presented in these graphs.

>> We believe the current presentation of the data is in the simplest format for identifying trends via quick visual comparisons of treatments and N species. We attempted to graph data as suggested by the Editor and found it difficult to interpret. Nonetheless, we have put together two examples of what 3D figures for N species could look like. In one example, we have both NH_4^+ and NO_3 in the same figure. In the other, we separate the two N species, as suggested in prior comments by Reviewer 2. In the examples, data for the urea treatment only is shown. These individual figures would need to be re-drawn and presented for each treatment. Whilst the figures are more visually engaging and do allow trends to be seen, identifying exact values of N specie concentration is difficult. Further, it's unclear how the presentation of data in this format improves the ability to present statistical comparisons. In fact, in the current figures, SD bars have been omitted as the graphs become overly complex and overlap between 'series' obscures some of the data. We're happy to further discuss data presentation with the Editor as necessary.

Example 2

The interpretation of the provided wealth of data was very well made, but sections 3.4-3.6 and the conclusion are lengthy and deviate regularly from the actually carried out measurements (see comments below).

>> As mentioned in responses to Reviewer comments, much of the manuscript, especially in the Discussion sections, will be condensed with focus primarily on measurements conducted as part of this study, and their direct implications.

Two comments were posted, one by an anonymous referee (RC) and another public one (CC). Both are positive in their assessment, but nevertheless they raise relevant points which the authors need to consider before the manuscript is to be accepted. A response needs to be given to both RC and CC comments. Clearly indicate where you disagree with proposed changes to the manuscript.

I agree with the RC, that it is indeed particularly relevant to provide further reflection on potential NH3-volatilization losses for the various treatment x soil combinations. Also adding in a basic mineral N-balance could be informative.

>> As noted in response to Reviewer 1, a N mass-balance table for all treatments will be provided in the revised manuscript and will facilitate discussion on potential losses, including NH3 volatilization.

In the public CC comment other valuable points are made: section 3.4 is a really interesting read, but it is not entirely informed by your study. A good effort needs to be made to shorten such text parts in 3.4, while at the same time in the CC some valuable advises to alternatively build this section are given.

>> Response to comments from Reviewer 2 are noted above.

I also agree that 3.5 is best omitted entirely, though you may want to recycle the main message in 1-2 sentences elsewhere.

>> To condense and focus the manuscript, section 3.5 will be removed. Although, some of the key messages may be included in relevant sections elsewhere.

Lastly some suggestions are given to shorten 3.6 as well as the conclusion section – these need to be taken into consideration.

>> See above responses.

Some extra minor comment:

L12-14 Lengthy sentence best split + strange to use Laboratory incubations as subject here

>> As indicated in earlier responses, the manuscript will be reviewed in its entirety to ensure that sentences are not too lengthy and / or complex.

L19 define 'CRFs'

>> The definition will be added into the text here.

L21 'the greater impedance of solutes' will certainly not be clear to readers. Look for a more accesible explanation

>> This explanation will be simplified to something like, 'reduced solute transport' or similar.

L73 'In the instance of poor early-season N supply …' do try to come to a less complex formulation of your message here.

>> This will be simplified.

L104 should be 'cm-3'

>> This will be corrected.

L176 seems rather bold to assume that N-immobilization was limited. Solid phase sorption would be unimportant for mineral N as the KCl anyhow displaces any mineral N-species from the exchange complex. So just omit 'solid phase sorption was minimal and / or' from this sentence.

>> 'Solid phase sorption was minimal etc.' will be removed from this sentence.

L186 first time the interesting term 'fertosphere' is used, deserves some explanation for non-specialist readers.

>> The fertosphere is first mentioned at line 130, and a simple definition is given. This can be expanded to be somewhat more descriptive (e.g., 'the volume of soil within 10 mm of the fertilizer band).

The sort of 'disclaimer' given in L195-200 is a bit of a strange way to start a discussion here and is best moved towards the end of 3.

>> This brief discussion on the limitations (or context) of the research will be moved to the end of Section 3.

The quality of Fig. 5 & 6 – at least in the pdf-version – was very low, needs to be resolved

>> The original TIF files for these figures have excellent resolution. These can be re-exported from the graphical software, if necessary. Although we suspect the quality has been lost during condensing into PDF. We're happy to work with the editorial team on this, if the manuscript is accepted.