



EGUsphere, editor comment EC1
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Comment on egusphere-2022-511

Steven Sleutel (Editor)

Editor 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, EGU sphere, <https://doi.org/10.5194/egusphere-2022-511-EC1>, 2022

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 NH₃-concentrations were predicted from pH and NH₄⁺ 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 NH₃-aq, urea-N, and NH₄⁺ and NO₃⁻ 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. 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).

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.

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. I also agree that 3.5 is best omitted entirely, though you may want to recycle the main message in 1-2 sentences elsewhere. Lastly some suggestions are given to shorten 3.6 as well as the conclusion section – these

need to be taken into consideration.

Some extra minor comment:

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

L19 define 'CRFs'

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

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

L104 should be 'cm⁻³'

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.

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

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.

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