

Interactive comment on “N₂O and N₂ losses from simulated injection of biogas digestate depend mainly on soil texture, moisture and temperature” by Sebastian Rainer Fiedler et al.

Anonymous Referee #2

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Fiedler et al. submitted a manuscript on N₂O, N₂ and CO₂ emissions in a short-term, climate chamber incubation study addressing effects of biogas digestate (BD) amendment of soil as well as soil type, soil moisture and temperature effects. The subject is of great relevance as the processes following BD and animal manure amendments to soils are still poorly understood and such process knowledge is urgently needed for successful development of knowledge-based mitigation strategies. Therefore the subject falls well into the scope of the EGU SOIL journal. However, as the presented study suffers from such very serious short-comings I recommend rejecting the manuscript.

Major critics – The very short-term, 7-day duration of the study does not at all allow drawing conclusions to the extent as done in the manuscript. Potentially an experimen-

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tal duration of 7 days could permit first conclusions on the important initial phase after BD incorporation into soils but, unfortunately, in addition to the very short duration, the authors have chosen to modify the incubation conditions within that short duration in a way that hampers all reasonable interpretation: in the first four days soils were kept at only 2 °C with the first two days without any measurement (obviously caused by limitations of the incubation systems), so with a gap of the first two days they then looked at processes in the next two days in slow-motion mode caused by the very low temperature. Then, after 2 days of single gas flux measurements per day, the incubation temperature was raised to 15 °C for another 2 days of incubation and gas flux measurements. Then for the last 24 hours, the oxygen supply was turned down to zero for one single last measurement of gas fluxes after 24 hours. In the manuscript there is no explanation for the choice of this set-up. While I see that sometimes limitations by experimental methods require compromising to a certain extent on the experimental design, I have tried hard but was unable to identify any strength in this choice of the experimental design. So I cannot see in which way the design would allow drawing any meaningful conclusions on processes in the first 5 or 7 days after BD application to a soil in any real life situation. The last day of incubation in pure helium, so the first day of incubation in an atmosphere without oxygen, was followed by just one measurement of gas fluxes. This set-up does not allow to see whether anaerobic conditions have already been reached in all treatments, including different soil types, different levels of soil moisture and different levels of BD application. Thus data of the last out of only 7 days cannot be interpreted at all. Throughout the manuscript authors take too little notice of the short-term duration of their study. For example the lack of differences among BD application rates is discussed without any consideration of the fact that only something like the first 3-5 days were studied. Also they don't wonder why with respect to N₂O and N₂ fluxes, soils amended with such substantial dressings as 320 kg N ha⁻¹ hardly differ from unamended soil but discuss results as if the experimental duration would have covered the most relevant period after BD application under field conditions. Thus clear indications of artefacts caused by experimental con-

ditions were ignored and any attempt to see their own methodology in a critical way is missing.

So my clear recommendation for rejecting of the manuscript is based on two highly critical points. First, the set-up suffers from such serious problems that I don't see that these can be overcome at this late point. In a period of 7 days, 2 days were left without measurements the next 2 days were dominated by very low temperatures (2 °C) and the last day compromised by the fact that there is no way to see whether anaerobic conditions have been reached in all treatments. Second, in their interpretation and discussion of results, the authors did not sufficiently consider the short-term nature of their study and the limited relationship to real conditions in the field. Conclusions like "However, the emissions did not increase with the application rate of BD, i.e. a broader spacing of injection slits, probably due to an inhibitory effect of the high NH₄⁺ content of BD." (line 19) or, "Our results suggest that the risk of N₂O and N₂ losses even after injection of relatively large amounts of BD seems to be small for dry to wet sandy soils and acceptable when regarding simultaneously reduced NH₃ emissions for dry silty soils." (line 21) or, "Contrary to our second hypothesis, the gaseous losses of N₂O and N₂ did not increase with the application rate of BD." (line 383) cannot at all be drawn. The respective findings that underlie these statements have to be attributed to artefacts caused by the experimental set-up.

Major points that might be corrected – All emission rates were given per unit of soil surface area. However the incubated soil cylinders had a surface of only 40 cm² and a height of 6.1 cm. Particularly at the background of such small volumes of incubated soil, gas flux data and also application rates of BD (see below) should be given per unit of soil weight. – Throughout the manuscript the authors speak of BD injection. While the details of BD dosing have clearly not been described in sufficient detail (line 100 ff) it seems unlikely that the treatment really simulated BD injection. (In line 100 authors state that BD was mixed with soil and then repacked into cylinders.) In practical farming BD injection always results in a very heterogeneous distribution of BD characterized by

a small band of more or less pure BD surrounded by gradients of very high to zero BD concentration. Based on the description given so far, it seems likely that the set-up corresponds to immediate incorporation of a slurry band into soil, e.g. by a rotovator. So in this point the manuscript could be corrected. – The insufficient description of the experimental BD treatments is another important critique. At no place, the ratio of soil to BD or any exact number of application rates is given. Instead there is a very circumstantial description of application rates in farmer's fields and spacings of injection bands in the field (line 103ff). Also for the reader there is no way to understand in which way the two BD treatments corresponding to 160 kg N ha⁻¹ and 320 kg N ha⁻¹ were achieved in this study. Was it not just simply mixing twice as much BD with the same amount of soil?

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