

Interactive comment on “Land use change associated with urbanization modifies soil nitrogen cycling and increases N₂O emissions” by Lona van Delden et al.

Anonymous Referee #1

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This manuscript reports on a work investigating soil N dynamics and nitrous oxide emissions associated with the change in land-use in a transition from forest to peri-urban turf grass establishment. From an experiment perspective I find that the work has been carried out very thoroughly with careful planning and execution giving rise to a solid set of data on soil N, N₂O fluxes and key soil parameters. Data from similar ecosystems are rare, and I support these should be made available to the scientific community and do suggest publication of the current work in BG. Meanwhile, I also find that the data interpretations in some places are exaggerated with a need for modifications before publication can be recommended.

As pointed out by the authors the observed N₂O emissions from turf grass system is

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possibly the result of a system in transition characterized by poor root development during the first months after establishment and thus high N₂O emissions due to reduced N competition by plants. In a longer time perspective, N₂O emissions from turf grass systems may remain at low levels throughout the year once the grasses have been established. Meanwhile, I think this need to be emphasized more strongly in the presentation of the results as generalization from the current data should be avoided. Thus, I suggest leaving out the correlation plot (Fig. 4) as in this plot you actually compare the established forest and pasture systems with the turf grass supposedly under rapid transition. In this context, I'm also wondering how this correlation analysis can be established on non-transformed data as it is mentioned that data are non-normally distributed. Optionally, the correlation plot may be modified to illustrate partial relationships showing the analysis specifically for the initial transition period.

Secondly, the authors conclude that leaching of NO₃ took place in the fallow plots. However, this process was not investigated in the current work and although the data may imply water-mediated losses of NO₃-N I suggest this statement be modified in the current text.

How were the experimental plots situated in the landscape? E.g. were the randomized plots separated by strips of pasture, and what was the distance between plots? More details on the gas-flux chambers should be included in the text, e.g. were chambers transparent or opaque, how did the chambers open/close. Were fluxes obtained from two pseudo-replicate collars per each replicate plot considered a continuous time-series of measurements, or were they analyzed separately? Please, clarify.

Check citations, if more than two authors only first author + et al. should be mentioned (e.g. Barton, Walm and Colmer, 2006 (P 9)).

In section 4.2 (lines 7 and 14) you refer to a linear increase in N₂O emissions with increasing NO₃ – where is this shown? Please, clarify.

In section 4.3, line 22 it is concluded that land use change results in increased N losses

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from the soil. Is this statement pointing at the N lost as N₂O? Please, clarify

Section 5, conclusions – as pointed out above, I think this section needs to be modified according to the nature of the current study. Also, conclusions about turf grass C sequestration cannot be made from this study and should be removed from this section.

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