

Interactive comment on “Use of laboratory and remote sensing techniques to estimate vegetation patch scale emissions of nitric oxide from an arid Kalahari savanna” by G. T. Feig et al.

Anonymous Referee #2

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Use of laboratory and remote sensing techniques to estimate vegetation patch scale emissions of nitric oxide from an arid Kalahari savanna

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Reviewer #2 Comments

Major Points

This is an ambitious paper that attempts to upscale nitric oxide emissions from soils taken from the Kalahari of Botswana to a regional scale using remote sensing techniques.

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1. Pre-treatment of soils prior to NO determination

Soils were collected from the field site and air dried, sieved through a 2mm mesh and stored at 50C prior to the determination of NO flux in the laboratory. Why were the soils sieved? The authors were very careful to select and sample soils based not only on the immediate vegetation cover, but also biological crust type. Sieving is likely to have destroyed the integrity of the crust and in so doing, changed the way in which the microbial biomass reacted to moisture. For example, non-heterocystous species of cyanobacteria (such as *Microcoleus* sp.), common to microbial crusts, are able to fix atmospheric N₂ because they physically create anoxic sites along the filaments allowing the nitrogenase enzyme to function in an oxygen free environment. Disaggregating crusts will flood such sites with air and prevent the enzyme working.

To what extent do the authors think that sieving and the breaking up of the crust will have affected microbial processes, particularly the generation of NO?

2. Pulses following the wetting of desiccated soils

The authors are quite right in allowing for the commonly observed pulse of gas release following the wetting of dry soils in their research design. However, they cite Scholes et al. (1997) where the NO pulse following wetting was < 6% in order to justify why it was not necessary to take into account NO losses in this initial pulse. Work in the Sahel of West Africa, particularly by Delon, has shown that there are significant pulses of N₂O as microbial denitification is stimulated in newly wetted soils. Although a different molecule, the processes and drivers are related. Would it have been better to have quantified the NO concentration in the pulse? Have the authors underestimated NO releases as a result?

3. Length of the methods section

The paper is too long. The level of detail given to describe how NO release rates were calculated is considerable. Can this be shortened, perhaps by referring more to the

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similar and related earlier paper by Feig et al. (2008)?

4. Heterogeneity of the land surface and upscaling of results

The authors state that one of the advantages of the study area is the relative homogeneity of the soil properties (page 4636). Although this is true, when using remote sensing images there may be considerable mixed-pixel issues. At a broad scale, the area is a mosaic of sand and pan soils, but at a smaller scale soils can vary according to their relative position on the fossil dunes - i.e. dune crests, flanks and interdunes. This should be mentioned.

Further to this, biological crust cover is known to be highly variable at the small scale and most of the microbial biomass will be concentrated in this upper surface layer. NO fluxes, therefore, may be linked to crust cover, yet the interesting aspects of inter-patch variability have been lost through the averaging of the fluxes at the patch scale. This is a shame, because there are many assumptions that have to be made when up-scaling the data in the way the authors have, yet a focus on the smaller-scale controls on spatial variability in flux could be a way of gaining insight into the controlling mechanisms.

These assumptions, particularly about how homogenous the surface is, need to be much more clearly outlined. There will be large errors associated with this approach. What the authors have attempted is laudable, but I think the paper would benefit from a more explicit account of the weaknesses. For example, if I have understood correctly, 29 samples were taken to the laboratory and used to calculate flux. The samples were all taken from one farm and these are used as the "ground-truthing" upon which fluxes are estimated for the whole region.

5. Discussion

Much of the early part of the discussion is repetitious and simply summaries the results (for example, pages 4646, 4647, 4649, 4650. This should be removed (and will help in shortening the paper).

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The way in which related studies have been bullet-pointed to facilitate comparison with the authors work is not satisfactory (pages 4652, 4648). This should be properly integrated into the text.

On page 4651, the authors relate their findings to the CO₂ flux work of Thomas et al. (2008) and mention the negative relationship between soil temperature and CO₂ emissions. Wasn't this because in certain circumstances photosynthesis (and thus CO₂ uptake) was stimulated in the crusts? It does not necessarily mean there is less biological activity; the micro-organisms are just doing different things.

Minor Points

- overuse / unnecessary use of definite articles throughout.

This occurs throughout the paper, including, for example, page 4623, line 23 to 26. These two sentences contain 6 uses of "the". All of these could be removed, i.e.

"Biogenic production of NO in drylands is dominated by processes of nitrification. Nitrification is influenced by environmental factors, such as soil moisture, soil temperature and nutrient content."

- The introduction needs to provide details on the biochemical processes leading to NO production and emission from the soils. Currently this is too vague.

- First line, page 4625, "The Kalahari is currently undergoing extensive land use change..."

Most significant changes occurred with the construction of boreholes, allowing access to water and opening up the Kalahari to grazing. This is not a current phenomena.

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