

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.

G. T. Feig et al.

Received and published: 6 March 2009

The authors would like to thank the two anonymous referees for their considered and constructive comments.

Major points that have been brought up by both the referees: The section in the methods and materials section where the laboratory incubation technique is explained is very similar to a previous paper published by the authors in Biogeosciences [Feig et al., 2008]. Both referees suggest that the common techniques used in the previous paper be cited and therefore large portions of sections 2.3-2.6 be removed. This section was originally included to allow the paper to stand alone, but on suggestion of the referees sections will be removed.

Both referees have indicated that there is extensive repetition of results in the discussion section; Referee 1 suggested that the results and discussion sections be combined, this has been done and has shortened the paper and avoids the repetition. We would like to thank referee 1 for the helpful suggestion.

Specific points brought up by referee 1

Referee 1 stated that "lots of the paragraphs have been written in exactly the same way (including the introduction)" We tend not to agree, while there are similarities in the introductions between this paper and the previous paper, the introductions differ in both structure and content. Similarities occur since the same background information needs to be conveyed.

Refer to the importance of pH and soil texture on the biogenic NO emission. In the methods section p.4627 line 24 the following was added "*Soil properties such as the pH, soil texture and nutrient status are known to have an important influence on the biogenic production of NO from the soil [Delon et al., 2007]. Since the laboratory measurement technique that we have used treats the soils as a "black box" these properties are not specifically studied.*"

Referee 1 found lines 16-19 in the abstract p. 4622 difficult to understand This has now been changed to read "Up-scaling the net potential NO fluxes with the satellite derived soil moisture and temperature data gave values of up to 323 g ha⁻¹month⁻¹. Vegetation patch related differences occurred, the highest up-scaled NO fluxes occurred in the Perennial Grassland patches, and the lowest in the Pan patches."

It was noted that this paper used the same flux measurement techniques as the previous paper by Feig et al. and it was suggested that the new features developed in this paper be emphasised. In the introduction p. 4625 line 26 a sentence has been added "*A similar technique has been used in a previous study by Feig et al [2008]. However instead of examining the effects of landscape position in a semi-arid savanna this study focuses on two other important points; 1) the effect of differing*

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

vegetation cover types on the emission of NO along a disturbance gradient, and 2) upscaling of point measurements of NO release to a regional NO soil emission flux estimate. In contrast to [Feig et al., 2008] where local measurements of soil moisture and temperature were used, we now used corresponding derived by remote sensing techniques"

In p.4624 lines 7-10 it was asked weather the sentence brings an important element to the comprehension- We think it is important since the NO flux is up-scaled as a function for the soil moisture and temperature, the previous study sets a range of where we would expect the optimum soil moisture content to be.

p.6425 line 16: referee suggests the use of "following"; instead of "previous" we still intend to use "previous"; since these are studies that occurred previously.

p.4625 lines 16-22: referee suggests we should comment on what methods have been used in the listed studies - we have included "field and laboratory based" in line 16, to indicate that these studies occurred both in the field and in the laboratory, we believe that more information is not pertinent at this point.

Methods and sampling: Large portions of sections 2.3-2.7 have been removed and the reader is now referred to [Feig et al., 2008].

Effect of NO pulse (also mentioned by referee 2) - Yes we agree that the effect of a NO pulse can be very important. In the global modelling paper by Yan et al (2005) they did not consider the pulse of NO in their statistical analysis and the 20% increase due to pulsing is as a result of a pulse emission function being attached to their statistical analysis. Therefore due to the difficulties in accounting for the NO pulse and the fact that the [Scholes et al., 1997] study actually measured the NO pulse at a site in a similar environment we consider the Scholes et al study to be the best available data for comparison.

Part 2.7 Error estimation needs to be shortened- details on where the information

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

for the error propagation data came from has been removed.

Part 2.8 should be removed- section 2.8 has been removed

Section 2.9 referee 1 suggests that the title is inappropriate and recommends that it be changed to "Remote sensing and GIS techniques used for the up scaling" - This has been changed

It is suggested that section 2.9.1 p. 4636- 4637 is too long and should be condensed- Lines 3-5 (p.4636) have been modified and now read "The area chosen for up-scaling was limited to the size of a Landsat image (185km x 185km, see highlighted square in Fig.1)" co-ordinates for the up-scaled region (Lines 6-8 p.4636) have been removed and Line 22-27 (p.4636) and lines 1.2 (p. 4637) have been removed. It was also suggested that lines 19-24 be removed (p.4637), however we believe that this data is important and this is the most fitting place to mention how the spatial resolution between the various remote sensing techniques used was standardised.

Results

Referee 1 suggests that the results and discussion should be condensed to avoid repetition- This has been done and has shortened and improved the structure considerably

p.4639 lines 14-line 4 p.4640- this paragraph has been modified during the restructuring

3.2 vegetation cover- Figure 2 is now mentioned

p.4641 lines 2-8 - Lines 2-8 have been modified

p.4641 line 12-20 - Lines 12-20 have been modified during the restructuring

p.4643 line 26-p.4644 line 4- This has been changed, by merging the results and discussion sections

BGD

5, S3274–S3280, 2009

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



p.4644 lines 6-9 - Lines 6-9 have been changed in the restructuring

Sections having the same name- Repetition of headings has been changed in the restructuring

p.4645 line 7-8 - Reference has been corrected

p.4645 Title of figure 10- This has been corrected

p.4646 paragraph 3.6.4 - This has been modified during the restructuring

p.4646 lines 25-26 - This has been changed during the consolidation of the results and discussion sections

p.4647 lines 8-20- This has been changed during the restructuring

p.4655 lines 16-18 referee 1 disputes the statement "the other major source of NO in African savanna ecosystems is pyrogenic emissions from biomass burning". Fuel combustion is an important additional source but this is not from savanna ecosystems. The estimates by [Jaegle et al., 2005] have now also been included.

Specific points brought up by referee 2

Comment 1: Pre-treatment of soils prior to NO determination, to what extent do the authors believe sieving and breaking up the crust will affect microbial processes especially NO generation? The soils were sieved to standardise the soil samples for all properties except for their intrinsic characteristics [Gödde and Conrad, 2000]. Since it is known that the majority of the NO production and consumption occurs in the very top layers in the soil [Galbally and Johansson, 1989; Remde et al., 1989; Rudolph et al., 1996], the disturbance has a minimal effect on the NO emission. The only way to quantify this is to make direct comparisons between the NO emissions measured in situ and the laboratory technique, and it has been shown that there is reasonable agreement between these techniques [Meixner et al., 1997; Van Dijk et al., 2002]. As far as other microbial activities are concerned, (especially the activity in

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

soil crusts, which is not of interest in this study) sieving is expected to have a strong negative effect on their function.

Comment 2: Pulses following the wetting of desiccated soils This has been dealt with in the response to referee 1

Comment 3: length of the methods section This has been dealt with in the response to referee 1

Comment 4: Heterogeneity of the land surface and up-scaling of results The authors agree that during the up-scaling process a lot of information does get lost, through issues such as mixed pixels, heterogeneity within each of the pixels and through patch diversity that we were not able to capture during our sampling process. Our method does account for some of this diversity, firstly the resolution of the land cover information was originally 28m by 28m and this is well within the size of the vegetation patches. Secondly we attempted to account for differences within the patches by measuring the vegetation cover and measuring NO emissions under differing vegetation units, and using pooled samples (made up of 10 subsamples) to account for the diversity within each of the vegetation patches. Thirdly the smaller scale information on the net potential NO flux is still included in the results and is therefore available to the reader to consider. However in response to the referee's comment we will include in the results and discussion section (section 3.6) "In any up-scaling process it must be borne in mind that important information is lost through factors such as mixed pixels, vegetation and landscape heterogeneity within each of the pixels and vegetation patch diversity that we were unable to capture during our soil sampling process".

Comment 5: Discussion The repetition has been reduced by combining the results and discussion section as previously mentioned in the comments to referee 1. The bullet points have now been incorporated into the text. In the discussion the negative relationship between the temperature and CO₂ emissions presented by [Thomas et al., 2008] This sentence has been removed.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Minor points

Overuse of definite articles- the manuscript has been checked and many unnecessary "the" have been removed.

Biochemical processes leading to NO production and emission from the soils- this has been dealt with in a number of excellent reviews namely [Conrad, 1996; Galbally et al., 2008; Ludwig et al., 2001; Meixner and Yang, 2006] so it is not necessary to go into too much detail, although mention will be made of the reviews in the introduction

The Kalahari is currently undergoing extensive land use change- will be changed to "the Kalahari has recently undergone extensive land use change"

Interactive comment on Biogeosciences Discuss., 5, 4621, 2008.

BGD

5, S3274–S3280, 2009

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper