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Biogeosciences Discussions

Interactive comment on "The acetylene inhibition technique to determine total denitrification ($N_2 + N_2O$) losses from soil samples: potentials and limitations" *by* R. Felber et al.

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The manuscript bg-2012-50 by Felber et al. reports results of laboratory incubations of soil samples with acetylene (C2H2) in an automated system, which are compared to field measurements of N2O fluxes at the original field site of the samples, a well-studied grassland site in Switzerland. The authors claim to adress potentials and limitations of the C2H2 inhibition technique.

The strong point of the manuscript is surely that the authors do not neglect the published serious drawbacks of the method (like so many others still do), but discuss them. Obviously, this does not improve the quality of the measurements, which -although

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measurements have been carried out adequately, as far as I can judge- is hampered by exactly these drawbacks.

The authors suggest to check the plausibility of results from the C2H2 inhibition experiment by comparing them to N2O emission measurements by static chamber in the field (p. 2854 l. 25-28). In my opinion, this is not a relevant comparison: Field results are not influenced by disturbance of the soil due to sampling (possible compaction, different aeration), may produce (and consume) N2O by several pathways and processes, some of which are (partly) inhibited by C2H2 (nitrifier denitrification, for instance, which is not even mentioned in the paper), may have different amounts of nitrate and ammonium present due to microbial and plant processes during incubation, are influenced by the plants and roots (information about how plants were treated in the lab incubations is missing: were the aboveground parts removed? if not, how was the increasingly tall vegetation over the season and consequent reduction of headspace volume in the incubations treated? Was there any light for photosynthesis?,...) and may have different temperature conditions (what was the incubation temperature in the lab?). Furthermore, the plausibility would only be affected if field measurements of N2O would be larger than lab measurements of N2O + N2. This does not say anything about the validity of results, though. It would be much better to compare results of the C2H2 inhibition method to other methods of measuring total N2O + N2 losses from soil, e.g. isotopic methods or He incubation methods.

I hesitate to suggest the manuscript for publication with major revisions. The advantage of publication would be that maybe some more scientists became aware of the drawbacks of the method. However, as the authors do not discuss alternative methods (which exist, but are more work intensive) and conclude that 'a lower estimate of the N2 loss can at best be provided' (p. 2870 l. 21-22), i.e. are still quite positive about the method, this would probably not lead to a change of perception of the method. As a method is applied that is known to have serious drawbacks, I therefore suggest rejecting the paper in the current form.

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Specific comments:

p. 2856: Taking of soil cores: How were the soil cores transferred to incubators? How tight was the fit to the incubators? Were the samples disturbed (maybe dependent on water content) during transfer? What happened with the vegetation layer?

lab incubation: What was the incubation temperature and how did this compare to field conditions? What was the concentration of C2H2 used? As far as I can see, this is not mentioned. l. 20: Cleaning of the tubes and addition of C2H2 is also important in the other case to avoid dilution of C2H2. Was this possible with the system? Was C2H2 readded after each sampling event?

p. 2858: Switch to measurement with GC: Normally, GC measurements take longer than photoacoustic measurements. Did this lead to less measurements or an increase in incubation time? Please provide more detail.

p. 2859: comparison C2H2-free and -treated samples: This seems like a small amount of samples given the normally large spatial variability in the field (which you show as well). You should comment on that.

p. 2861 l. 2: How often was 'occasionally'? This should be made transparent. I generally have a bad feeling when data is removed from a data-set without good reason (e.g. the knowledge that something went wrong during measuring). Neglection of results has often hampered scientific development and I strongly recommend to discuss and use these data in a more critical way.

p. 2862: isotopic measurements: This information is not at all suficient: How were samples taken? How did you measure isotopic composition? What were the standards used? Please provide more detail!

p. 2865: isotopic results: What are the standards here? How do you explain simultaneously decreasing del15N and increasing del18O values?

Table 1: I guess the lab fluxes were C2H2 treated fluxes, but this is not totally clear. x-*

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is not explained.

Fig. 2: I guess the lower S'2.2 should be S2.2?

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