Atmos. Chem. Phys. Discuss., 6, S2810–S2812, 2006
www.atmos-chem-phys-discuss.net/6/S2810/2006/
© Author(s) 2006. This work is licensed
under a Creative Commons License.

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Methane emission from tropical savanna *Trachypogon sp.* grasses" *by* E. Sanhueza and L. Donoso

Anonymous Referee #2

Received and published: 30 August 2006

General Comments: The authors have reviewed data from earlier experiments in Venezuelan savannas and found that these data supported the contention that both live and dead plant material can emit methane. This is an important finding. The quality of the paper is acceptable but the authors ought to moderate their conclusions. It is highly inappropriate to extrapolate from a few square meters of experimental plots measured over a few days to global savannas. This is precisely the claim made in the abstract.

"The dry/green mixture of grasses produce methane at a rate of 10 ng m-2 s-1, which extrapolated to the global savanna would produce an annual emission of 5 Tg, much lower than the production recently suggested in the literature. On the other hand, during the wet season savanna soil consume CH4 at a rate of 4.7 ng m-2 s-1, producing

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

a global sink of 1.3 Tg yr-1. 10 Therefore, the tropical savanna soil-grass system would make a modest contribution to the global budget of methane."

Rather than extrapolate to global savannas in their abstract, the authors should report their basic findings and indicate that these findings support the general notion that methane is produced by plants but at lower rates that those found by Keppler et al. (2006). Global extrapolations are appropriate in the discussion section given that the authors have qualified the nature of the extrapolations.

Specific Concerns:

The authors show two groups of measurements. At first glance, these measurements appear to disagree. In the first group of intact plots there is clear production of methane while in the second group the fluxes are close to zero. However, inspection of the experimental clipped plots shows that soil consumption was negligible during the first period whereas it was high during the latter period. Soil consumption could vary as a result of soil moisture as the authors claimed. Therefore, the variability inherent in the measurements provides additional evidence supporting a plant source of methane.

The authors claim that their dark chamber measurements are representative of light conditions. They make this claim because Keppler et al. (2006) found that light caused elevated fluxes under laboratory conditions for up to 15 minutes after the light was turned off. Given that the mechanism for the methane production and emissions from plants is not well understood, this reasoning is not very strong. I suggest that the authors moderate their comments and simply admit that they do not know what sort of response would be found if the samples were illuminated with sunlight.

The authors should be aware that a paper was recently published by Kirschbaum et al. in Functional Plant Biology, 2006, 33, 521-530. This paper evaluates the global extrapolations made by Keppler et al. (2006). The paper should be available from the author <Miko@Kirschbaum.id.au> and ought to be consulted.

ACPD

6, S2810–S2812, 2006

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 6841, 2006.

ACPD

6, S2810–S2812, 2006

Interactive Comment

EGU