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Biogeosciences Discussions

Interactive comment on "The greenhouse gas balance of European grasslands" *by* P. Ciais et al.

Anonymous Referee #1

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The study presents a comprehensive account of the greenhouse balance of European grasslands - including not only sources and sinks of carbon (C) but also the emissions of nitrous oxide (N2O) and methane (CH4) - that is developed around four key questions (p. 6001, l. 20 ff.). The work is essentially based on meta-analysis of previously published results. From a methodological perspective, the new contribution is the targeted combination of sparse experimental data with European-wide results of simulations with a process-based grassland simulator. For the time being, this is probably the most promising approach to overcome the weaknesses of entirely experimental or purely modelling studies. Formally the paper is very well written, with a clear setup, a very good use of the English language and an appropriate selection of figures and tables.

There is, however, a critical aspect that can not be set aside and put key parts of the paper on a weak ground. The main result (Abstract, p. 5999, l. 6) is namely

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that grasslands are a net carbon sink, with a Net Biome Productivity (NBP) of 74±10 g C m-2 yr-1. This estimate is obtained according to equation (3) (p. 6007) from a combination of ratios of NBP/GPP observed at 11 sites across Europe, and European-wide simulations of the Gross Primary Production (GPP). Are these 74 g C m-2 yr-1 a reliable estimate? Is the uncertainty of only 10 g C m-2 yr-1 given by the authors reasonable? At least four reasons seem to require reconsideration of these values.

Firstly: the representativeness of the sites. To conduct the analysis, the authors selected 11 so-called "golden sites" (p. 6001, l. 16). Among these sites, the one with the largest NBP is Malga Arpaco (Tab. 2, p. 6042), a high-elevation pasture grazed at a low stocking density but receiving 90 kg N ha-1 yr-1 in form of mineral fertilizers (Tab. 3 in Soussana et al., 2007, op. cit.). Already in many of the figures in Soussana et al. (2007, op. cit.) this site displays an anomalous behaviour, which could be the consequence of the management. This suggests, along with the data in Tab. 2, that the site might not be representative in the way other sites are. If Malga Arpaco is removed from the selection, reducing the number of "golden sites" to 10, the corresponding mean NBP lessens from 67 g C m-2 yr-1 (Tab. 2) to 38 g C m-2 yr-1, i.e. roughly the half. If extrapolated (admittedly in a very sloppy way here) this could imply a European-wide NBP of only 40 g C m-2 yr-1.

Secondly: representativeness of the measurements in time. For many the sites, measurements of the relevant C fluxes are limited to a few years, which after all may have been characterized by unusual weather conditions. In particular, all data presented in Soussana et al. (2007, op. cit.) encompass the heat-wave of 2003, an event that had profound impacts on the C cycling (Ciais et al., 2005). This implies that the results might not be representative of the long-term behaviour. In fact Ciais et al. (2005) pointed out that the heat wave was responsible for a marked reduction in GPP and an anomalous net source of CO2 (Admittedly, this would imply a higher NBP than estimated here, contrasting what discussed in the previous paragraph).

Thirdly: uncertainty in the NBP estimate from equation (3). Actually, the authors seem

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to be aware of the limited representativeness of the data. In fact, on p. 6007, l. 13 (PDF fro print) they state that “Removing one year randomly at each sites to calculate NBP, defines an uncertainty in NBP from equation (3) of the order of 100 %”. How, then, it is possible to conclude that the uncertainty in the estimate of the European-wide NBP is only of the order of 10 g C m-2 yr-1 or 14 % relatively to the estimate of 74 g C m-2 yr-1?

Fourthly: uncertainties in the modelled GPP. The GPP values used to up-scale NBP according to equation (3) are those from simulations carried out by Vuichard et al. (2007a and 2007b, op. cit.). In Vuichard et al. (2007a, op. cit.) a verification of the grassland simulator was carried out only for three of the sites appearing in the compilation of Tab. 2 and only for NEE, rather than GPP. Is this sufficient to ensure that the model performs well on a continental scale? In any case, Fig. 4 of Vuichard et al. (2007a) provide some evidence that the model has difficulties in simulating the carbon cycling of grazed grasslands, with e.g. systematic biases at the French site of Laqueuille. The authors nevertheless assume the same relative error in simulated GPP for both types of management (cutting and grazing). What are the implications for the application of equation (3), in view of the fact that pastures and meadows are treated distinctly there? Is it really possible to estimate NBP at the continental scale with an uncertainty of only 10 g C m-2 yr-1?

As an aside to the first comment (representativeness of the sites), I would like to mention that Malga Arpaco appears to be anomalous also with respect to N2O emissions. In fact, according to Tab. 4 in Soussana et al. (2007, op. cit.) these amount to only 1.2 mg N2O-N m-2 yr-1, in spite of an input of 90 kg N ha-1 yr-1 from mineral fertilizers plus the N inputs from the animals, implying an emission factor substantially lower than for all other sites. As a second aside to the first comment, I would like to also point out that the representativeness of eddy-covariance measurements of the Net Ecosystem Exchange (NEE) in grazed systems has never been discussed in depth, not even in the publications on which the present paper build upon. Given such constraints as (a) a

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measuring height of typically 2 m above ground, (b) footprints of the order of a few ten to a few hundred m, depending on stability, (c) grazing patterns that are not observed but are likely to be heterogeneous, (d) the necessity to protect measuring systems from the animals, can operational eddy-covariance systems really sample CO2 from animal respiration in an adequate and representative way?

Another critical proposition appearing in the Abstract is the one on p. 5999, l. 20, namely: "The carbon sequestration efficiency of grasslands, defined as the ratio of NBP to NPP, amounts to 0.09±0.10. Therefore, per unit of carbon input, grasslands sequester 3–4 times more carbon in the soil than forests do, making them a good candidate for managing onsite carbon sinks". As formulated here, the fact that grasslands sequester 3 to 4 times more carbon than forests per unit carbon input appears to be a logical consequence ("Therefore ...") of the fact that the carbon sequestration efficiency (CSE) is of the order of 0.09±0.10. However, this is not really what stated in the main text, as on p. 6024, l. 21 ff., it is said that "The CSE of grasslands is (slightly) lower than the CSE of forests .... Such an apparently lower ability of grasslands to store carbon for a given NPP must be tempered by two remarks. Firstly, ... it is more relevant to compare ratios of NBP to carbon input to the soil, rather than ratios of NBP to NPP". So, if the ratio NBP/NPP is not the relevant quantity to look at CSE, how can the results of the present analysis support the conclusion of a higher CSE in grasslands than forests? And if carbon inputs to the soil are the key quantity, where are the pertaining data?

In summary, while the paper clearly represents a step forward in the effort to provide a European-wide estimate of the greenhouse gas balance of grasslands, the analysis does (overall) not go sufficiently in depth to support the main conclusion, namely that grasslands represent a carbon sink of the order of 70±10 g C m-2 yr-1. Questions concerning the representativeness of the sites, the representativeness of short-term the measurements in the long-term, the effects of the management, or the performance of the modelling system, should be tackled more firmly, and the uncertainty range

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evaluated accordingly. A more critical approach should also be taken regarding the carbon sequestration efficiency and the ensuing conclusion that grasslands are "a good candidate for managing onsite carbon sinks" (Abstract, p. 5999, l. 22).

Based on this evaluation, I therefore recommend resubmission of the paper.

Minor remark

The last line in Tab. 2 gives the standard deviation rather than the standard error.

Reference

Ciais, P., Reichstein, M., Viovy, N. et al.: Europe-wide reduction in primary productivity caused by the heat and drought in 2003. Nature 437, 529-533, 2005.

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