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Discussions

Interactive comment on "Predicting the global warming potential of agro-ecosystems" *by* S. Lehuger et al.

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Authors' response to the comments made by the Editor and anonymous referees

Overall, the Editor and the referee #3 were rather critical and expressed their utmost concern regarding the results we presented. Referees #1 #2 were less categorical and suggested some revisions to improve the paper.

All comments dealt with the same issues and concern: 1) the measurements were insufficient, 2) the soil C sequestration predicted in one site was unrealistically high, 3) there occurred disturbingly strong discrepancies between measured and simulated data and 4) the results lacked an uncertainty analysis. Our answer to these four points are given below, after which we address more specific remarks.

1)The insufficiency of the measurements The Editor and the 3 referees point out the insufficiency in the gas measurements data sets, since CO2 flux measurements were only available for one site, and N2O measurements were either too infrequent (at the Rafidin site) or too uncertain (at the Grignon site). This precludes a thorough testing of the model used to predict those fluxes. In an ideal world we would fully agree with these comments and simply go for more complete data sets. Unfortunately it turns out that such sets do not exist, to the best of our knowledge. We would like to point out that both datasets (Grignon and Rafidin) are quite remarkable for cropland and presently are the more complete and intensively-monitored datasets of GHG fluxes for such ecosystems in France. In Europe at large, we are not aware of such data sets existing: the cropland sites of the CarboEurope EU project are only beginning to deliver CO2 data, and they are still not combined with N trace-gas measurements. In addition, the simulations of the daily CO2 fluxes with a crop model is also quite original, and we believe that the good agreement achieved by CERES-EGC supports this avenue as relevant in the prediction of agro-ecosystems' C dynamics. However, we agree that the more data we could feed into the model testing procedure, the more reliable the simulations. Since the paper was submitted we have been gathering more data sets on the Grignon site, which could be used to further-test the model. For example, continuous N2O measurements could be available over the 1st half of 2007 for a barley crop and daily CO2 flux measurements could be available for the years 2004-2005-2006 and 2007 to cover the complete rotation (Maize-Wheat-Barley-Mustard). These datasets and new model testing could be added to the next revised paper. We are also seeking data to address the fact that most troubled the referees (the long-term soil C trends output by CERES-EGC). This is detailed in the next point.

2) The C-sequestration rate The most problematic point in the referees' comments involved the C-sequestration rate in the Rafidin soil, which seemed unreaelistically high to the Editor and referees #2 and #3. In addition, they considered that the testing of the model with a one-yr period of net ecosystem exchange (NEE) measurements is not satisfactory for judging long-term C dynamics.

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First, we would like to clarify our objectives concerning the simulations of crop rotations over a long period. The objectives of these simulations over a period of 30 years was neither i) to trace back the historical dynamics of soil C (SOC) over the last 30 years, nor ii) to extrapolate our predictions to a larger (regional) scale, as ref. 1 surmised. Instead, our objective was simply to calculate a potential of C sequestration taking into consideration the cropping practices and the rate of crop residues return over a full crop rotation. Since soil C is very sensitive to residue returns on a crop rotation scale, we decided to run 10 of these sequences to smooth these short term effects of crop residue and to identify more stable variation trends. Secondly, our objective was not to assess the evolution of SOC stocks in croplands at the regional or national scale in France, but precisely for two specific soils for which datasets were available. The goal of our manuscript was thus more methodological, in that it (tentatively) aimed at showing how a crop model could be used to predict the CO2/N2O balance of cropping systems. Although the two soils we tested do represent a significant fraction of the soils in their respective regions, the results can not obviously be spatially-extrapolated to the whole regions. Also because the management practices (and in particular the rate of crop residue return) vary substantially across these regions. Thus, the fact that the Rafidin soil came out as a strong sink for C can not lead to the conclusion that all agricultural soils in France are sinks for carbon.

The C sequestration rate was ˜750 kg C ha-1 yr-1 at the Rafidin site, and this high rate originated from high crop residue returns in field -the straw was left on the soil surface at harvest- and specific soil characteristics. The Rafidin soil was a rendzina with a high carbonate content (˜800 g CaCO3 kg-1 in the first 30 cm of soil) and a high pH value (8.3 in the topsoil). As we mentioned in the manuscript, these characteristics hamper the mineralization of SOC due to physical protection process of organic matter by the formation of calcite on the organic fractions (Ballif et al., 1995; Trinsoutrot et al., 2000). This explains the high potential to sequester organic C. Using a simpler model than CERES-EGC and parameters derived from experiments on such soils, we obtained a value of 580 kg C ha-1 yr-1, which is lower than the CERES estimate, but by only 25%.

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In the nearby Picardie region, including similar rendzina soils, a survey of 391 fields in which SOC was measured repeatedly between 1970 and 1998 showed that variations SOC to range between -2,0 and +2,0 t C ha-1 yr-1 over the different fields. Among the latter, 49% were stable over the time-period, 29% decreased and 22% increased (Wylleman R. et al., 2001). Our simulations in Rafidin would definitely be in the upper range of the last fraction, but are still within it - bearing in mind that the rates of crop residue return were likely lower than in our scenario, due to straw burning or removal for instance. We nevertheless reckon that it would be useful to check the CERES model on this long-term data set in a revised version of our manuscript, for both sites. Also, confidence on the modelled trend would be improved by an uncertainty analysis, which we come to in point 4/. A long-term experiment on different soil tillage techniques (plowing, reduced tillage and direct seeding) is conducted with a maize-wheat rotation on the Boigneville site (80 km south of Paris) since 1970. The results between 1970 and 1998 showed that for all the treatments, the C stock increased between +100 kg C ha-1 yr-1 for the plowing treatment and +200 kg C ha-1 yr-1 for the reduced tillage and direct seeding treatments (Thévenet et al., 2002). Our simulations for the Grignon site can be compared to this results. Other studies over Europe also predict a positive soil C sequestration potential when cereal straw were left on soil surface (+ 0,61 tC ha-1, yr-1 for Smith P. (2004) and Freibauer et al. (2004)). Lastly, referee #3 referred us to the Bellamy et al. (Nature 437: 245-248, 2005) paper showing that UK soils were a strong source of C over the last 25 years. The peculiarity of these soils, despite being a stone throw away from those we studied, is that they were apparently very high in C content, initially (being mostly non-agricultural soils or permanent grasslands). These authors went on to regress the C variations against the initial SOC content (or concentration, rather), and if we use their regression for the Rafidin soil, we actually find... a positive rate of change ! [of exactly +0,24 g C kg-1 yr-1, or 950 kg C ha-1 yr-1 in the 0-30 cm layer we used in our simulations]. So the contradiction is not so impressive after all.

The Editor wondered how the model was initialized and suggested to start from an equilibrium rate. Although the latter is a somewhat standard procedure for forest or

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grassland ecosystems, it is not commonly used in arable systems because their soils are not at steady-state (as stated by Balesdent and Arrouays, 1999 for arable soils in France in general). We thus used the alternative option consisting in using laboratory estimates of the partitioning of SOC between the various pools of NCSOIL (the C-N component of CERES-EGC), which is the standard procedure with NCSOIL. It does not prevent some minor degree of numerical instability in the first few years of simulation, and for that reason we actually skipped the first rotation (3 years) in the long-term series. Another way of addressing this issue of model initialization would be to introduce some uncertainties in the initial settings of the SOC pools, which we suggest in point 4/. Also, the initial conditions were not so important because we were mostly interested in the C-sequestration rate associated with a management / soil / climate scenario than in the exact reconstruction of soil C historical dynamics. We considered that the cropping practices over the 30 years of simulation were invariant in order to really predict the potential of C sequestration of that crop rotation and these cropping practices.

As the referees mention, a previous step would have been done to validate the model with long-term datasets of SOM dynamics and history of agricultural practices. Then the model could have been applied to predict the C-sequestration rate. This step could be integrated to a new submission by integrating long-term datasets on soil C dynamic available for the studied regions.

3) The inadequacy between measurements and model The inaccuracy between simulated and observed data of N2O emissions was quite large for the Grignon site but it was in the range of discrepancy that other models return for N2O emissions simulated at the daily time-scale (see section 3.1.3 of the MS). To improve the simulations, we are re-calibrating the parameters of the denitrification and nitrification sub-models of CERES-EGC with a Bayesian method, and this part could be included in a revised paper.

4) The lack of uncertainty analysis Referee 2 and Editor invite us to add an assessment

of the uncertainty of our predictions. In a first step, the application of a Bayesian method to the denitrification sub-model could allow to quantify the model output uncertainty due to uncertainty source of global parameters of these sub-models. This part could also be done in a next submission. We could also assess the uncertainties in the initial settings of the SOC pools, and see how they propagate in the final prediction of C variations after 30 years.

Other comments: Referee #1 complained that the experimental site description is too detailed in comparison with the explanation of the CERES-EGC model in the Materials and Methods section. In this paper, our aim was not to compare different models or to add new developments but instead to apply the model to predict GHG balance.

The CERES-EGC acronym means that our model is a variant of the CERES crop models (Crop Environment Resource Synthesis; Jones and Kiniry, 1986) that was altered to focus on the environmental fluxes in our laboratory. The acronym of our laboratory is "EGC" (which stands for "Environment and Arable Crops", in French). Previous articles on CERES-EGC describe the originality, the concepts, the testing and the application of the model; a publication list can be consulted on the web site of the model: http://www-egc.grignon.inra.fr/ceres_mais/publis.html. In the past, CERES-EGC was compared to different models against the Rothamsted's long-term experiments for C dynamic in Gabrielle et al. (2002b). CERES-EGC was also compared to Daycent model for N2O simulations in Gabrielle et al. (2006a). The different components were described in several papers: the water balance component was assessed in Gabrielle et al. (1995),the nitrogen transfers and transformations in Gabrielle and Kengni (1996). The model was also tested against several datasets over Europe (Gabrielle et al. ,2002b) and, lastly, CERES-EGC has been used to assess the N2O emissions at the sub-regional scale in Gabrielle et al. (2006a). All the same, to improve our "material section" in the present paper we should explicitly detail the major concepts and equations of the different sub-models of CERES-EGC.

Conclusion: The Editor and the referees acknowledge that this paper addresses an important and timely issue, but appear skeptical on both the measurements and simulations presented, and mainly the rates of C-sequestration at the Rafidin site. Their remarks seem to indicate that this submission was premature for publication in BG. Furthering model testing with supplementary datasets might be an important step to improve our predictions and to submit a revised paper.

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