Ref.: Ms. bg-2021-241 Biogeosciences

Effects of climate change in the European croplands and grasslands: productivity, GHG balance and soil carbon storage

Revision 2

Editor

Comments to the author:

Dear authors,

thank you very much for your thorough revision of the manuscript. Referee #2 states that the novelty of the manuscript still has to be highlighted in more detail. Also, the representation of fertilizer application in Germany appears to be incorrect. All remaining comments of referee #2 have to be addressed before the paper can be published in BG.

Best regards,

Ivonne Trebs

We thank the Editor for giving us the opportunity to review our manuscript. All comments raised by Reviewer#2 and Reviewer#1 have been considered and discussed here in this document (in blue).

Reviewer#2

1. Thanks to the authors for the detailed and extensive response to the reviewer comments. The response addressed as promised all points and clarified several concerns. Nevertheless, several concerns remain, and others occurred during re-reading the manuscript.

We thank Reviewer#2 for the in-depth analysis of our work and for the detailed comments provided, which have contributed to a marked improvement in our work. All the issues raised have been taken into consideration and discussed, as detailed in this document (in blue).

2. Before I comment on the comments to reviewer#2, I would like to pick up a comment to reviewer#1. Regarding the novelty and the comments made to Reviewer#1: I see your point about the model differences, but I am not sure if this point is clearly pointed out in the presentation of your study. If I understand the authors correctly there are two major differences between the here presented study and former studies:

   1.) The here used model approach is a process-based model approach, while ethe former studies are mainly data driven approaches (including more details on e.g. the management).

2.) The finer spatial resolution.

Please make this clearer in the manuscript. I understand that the comment seems to be unpleasant, but your response is only mentioned in comments and not reflected by changes in the manuscript. Considering the list you mention, there are models like DAYCENT and Landscape-DNDC includes, which are also state-of-the-art models on croplands. The combination of grassland data and cropland data might be new, but is not the new innovative step that would be used to address this comment. Please characterise the novelty of the study more clearly in the manuscript, as reviewer#1 seem to miss this (and my comments went in a similar direction).

We want to thank this reviewer for re-highlighting this point. We have given more emphasis on the points of novelty by clearly indicating these aspects when defining the objectives of the paper

*[This research aims to investigate, by means of **process-based simulation models**, the contribution and the impacts of climate change in the European crop and grassland production systems towards the year 2100. The analysis focuses on plant productivity and biogenic GHG (N_2O, CH_4, CO_2) balance, outlining a detailed carbon budget for current agro-ecosystems and with two climate scenarios, an intermediate and a pessimistic one. Through **high spatial resolution** and **detailed management representation**, this study provides projections of key agro-ecosystem variables in the near and long term in order to support and help identify possible actions to maintain productivity and reduce environmental impacts]*

and highlighting this part better in discussions, although these elements were already present. In this regard, we have expanded this section, explaining some aspects that differentiate this study from the past and recent cited literature [L 845-854 of the new version of the manuscript], also reporting - as suggested - the models which were used. To go in the same direction as the Reviewer#2, we point out that there have certainly been other applications with process-based models in the past, but the fact of using rotations deriving from spatialized statistical data, combined with dynamic management (instead of fixed sowing dates or random fertilizations), and integrating two specific models on the same spatial unit, makes this study a novelty compared to those mentioned so far.

The comment is far from unpleasant. The review process allowed the Authors to take stock of the situation, improving the presentation of their work and reflecting on future researches.

3. Concerning the location specific differences in the results, I would like to thank the authors for their explanations. I can follow your explanations. I do not think that the representation of fertilizer application in Germany is correct (too high in the North-East (considering a fertilizer shortage during the GDR times in the 80s and the low fertility of the soils) and too low in the North-West (this might be due to the difficulties to estimate the amounts of applied manure)). The fact that the sharp changes at the political boarders are visible shows the statistical flaws of this data set. A critical view on the SOC values let also assume a fairly high SOC stock for croplands. These are comments to the data sets and not on the study here. The comment on the study concerns a more detailed elaboration of these facts in the discussion. Serval maps allow to recognise the political boarders of countries, which is an indication for the impact of the statistical aggregation of the used data sets.

Fertiliser application were generated based on the FAO statistics at NUTS0 and NUTS2 resolution and were distributed to crops and regions on the basis of crop requirements (function of e.g. regional yield, nitrogen fixation, deposition and manure availability). This produced a map with 1 km × 1 km resolution which was aggregated to our spatial units. Administrative borders assume consequently an impact and constitute another limitation of this (and other) regionalised studies. Following this comment, we want to add this point also in the discussions regarding the limitation of this study. [L 809-812]

[Another limitation of this and other regionalized studies is the deviation in the representation of the quantities within the administrative units, which is related to the scarcity of management data with fine spatial resolution. For example, the amounts of fertiliser to be distributed on cropping systems, which are provided at a regional level, show little heterogeneity within the boundaries of the region itself, and can mark a sharp transition between adjacent regions.]

Consequently, Germany, as well as other regions in Europe (*e.g.* Po Valley, which is shared between heterogeneous regions with plains and Alps), can suffer about this issue, coming from the nature of the statistical data and not for the interpretation of these Authors.

Please, refer to the comment #5 concerning SOC stocks for croplands.

4. A more detailed characterisation about precipitation driven emissions (South) and more temperature driven effects (North) would be great.

During the historical period, N_2O emissions assumed a positive correlation with precipitation ($r = 0.42$) and with minimum air temperature ($r = 0.46$) for croplands in central European latitudes. During the climatic scenarios a marked dependence of the maximum and minimum air temperatures was established especially for central and high latitudes ($r > 0.9$ for RCP8.5 and $r > 0.5$ for RCP4.5). The dependence of N2O emissions with precipitation resulted significant only for central and high latitudes and only for the RCP4.5 scenario ($r = 0.25$), while was anti-corelated for the RCP8.5 scenario ($r = -45$). Regarding grasslands, N2O emissions were not corelated with air temperatures and precipitation during the historical period, at all the latitudes. A significant correlation with air temperatures occurred for both RCP4.5 ($r > 0.45$) and RCP8.5 ($r > 0.75$) only for low latitudes. Emissions were positively correlated with precipitation only for central latitudes and for RCP4.5, while resulted negatively corelated for RCP8.5 at low latitudes. Furthermore, at central latitudes, N_2O missions for joined cropland and grasslands ranges from 42 g N-N_2O ha-1 / °C in the historical period, rising to 52 and 73 g N-N_2O ha-1 / °C for RCP4.5 and RCP8.5, respectively, in the second half of the century.

We added this sentence in the new version of the manuscript [L 679-684]:

[The projected temperature rise in the climate scenarios has a latitudinal impact with N_2O emissions, which is directly corelated at mid- and high latitudes for croplands ($r > 0.5$ for RCP4.5 and $r > 0.9$ for RCP8.5; $p < 0.01$) and at low latitudes for grasslands ($r > 0.45$ for RCP8.5 and $r > 0.75$ for RCP4.5; $p < 0.01$). Precipitation in the mild climate has a direct influence on N_2O emissions ($r = 0.25$, $p < 0.01$) at mid-latitudes for both production systems, also at high latitudes for croplands. Precipitation is anticorrelated emissions in the RCP8.5 scenario at mid- and high-latitudes for croplands, and at low latitudes for grasslands.]

5. For me the input data seem to be the main drivers for the simulation result. To underline this, I would like to pick up your argument with the organic content in the soils. First, do the authors think that these high SOC values are realistic for cropland soils? There are no cropland specific SOC values available, so it would be good to add a short check of

references that back-up the assumptions for the high SOC values in Eastern Europe (this is especially doubtful in Poland). Are organic soils excluded from the data set? The authors might argue that looking in the difference or changes due to differences in weather data are the main focus, but some of these "doubtful" areas show the strongest response.

To answer to this comment, we detailed here the procedure we used to select representative soil for each pixel. Regarding the SOC values, as detailed in the material and method section (2.2.2) and supplementary material (S.1) they were derived from the European Soil Database (ESDB; Hiederer 2013) at 1 km × 1 km raster resolution. As our simulation grid (raster of 0.25° lat/lon resolution) is much coarser than the soil database grid, a suitable aggregation method was applied. Within each simulation grid, the underlying soil grids were grouped/clustered according to their physico-chemical properties such as soil texture, SOC content, bulk density and pH, for top and sub-soil. So, the class representing the majority was selected first and checked whether it represents an arable/grassland soil or a forest soil due to properties such as bulk density (< 0.76 t m^{-3}) and SOC content (> 30 kg-C m^{-2}). In case the major soil class was identified as a forest soil, it was rejected and the class representing the second major group was selected and checked for suitability. This cut also organic soils. This procedure was repeated until a suitable soil class was identified as representative arable/grassland soil.

With this procedure we were not able to distinguish an arable or a grassland land use. Notwithstanding that, when models' outputs are reported to the surface allocated for arable crops or grassland in each pixel, they assume a proportional weight: high soil carbon for croplands in a simulation unit with high grassland share, assumes low weight. In the paper of Haas et al., 2022 (in press), we used the same soil dataset, reducing the threshold to 20 kg-C m^{-2} in order to address only croplands.

6. I disagree with the conclusion. There are a couple of points:

   - Lines 871-873: this is exactly what was discussed and the authors agreed on. These are not absolute values and absolute conclusions are hardly possible. The simulation results show that the changed climate show a minor impact on the simulation results.

We agree that these are not absolute values, as highlighted in the text at the beginning of the materials and methods (§2). However, in the sentence that the Reviewer#2 mentioned "Results clearly showed that the productions will be stable in the first half of the century, while a strong reduction will occur during the second half of the century, especially at low latitudes, and mainly due to a reduction in the length of growing cycle". We mean that the productions maintain a constant level in the first part of the century (without comparing them with the historical period), and are reduced in the second part. This is mainly due to a reduction effect of the crop cycle length in simulated crops due, mostly, to the effects of rising temperatures, as otherwise described in chapter 3.1.2 and 4.1. We decided therefore to keep this sentence in this present form.

   - Lines 875-877: I think this description is mis-leading. These are managed systems, which need to be characterised by the NBP rather than the NEP. Both systems are there for producing yield and hay. The argumentation yield and hay removal changes the system to a sink sounds a bit odd. Even though the description technical not wrong, it would be great to have a different formulation here.

Thanks to Reviewer #2 once again for his careful re-reading of this manuscript. The quoted sentence has been modified as follows:
[Biomass removal from the agricultural surfaces (yield, hay and animal intake), combined with the removal of crop residues, shift the balance towards a net loss.]

- Line 878-879: this is not a conclusion, but a discussion point. Did you test the changes for leaving more residues on the field?

This option has not been tested in this work. Although the data numerically shown the fact that this can be an excellent strategy to bring the C balance back to a sink (according to the N_2O emissions produced). In agreement with the reviewer, we remove this point from the conclusions, keeping only the sentence suggested to be added in the previous revision (R1) of this work:
[The positive effects of crop residues restitution on carbon and the net greenhouse gas balance need to be investigated with further researches]

- The overall conclusion is for me that the input data and their uncertainty are a more relevant driver for changes than the climate. Only at the end of the century the climate affects strong changes, which might show the potential to show a larger impact than the management uncertainty.

Following the discussion of §4.5, the quantification of the uncertainty of the input dataset is difficult to accomplish, mainly due to the absence of spatialized measurements and information. Accordingly, it is difficult to determine a priori whether input data have a greater impact then the climate data on results. We agree that the uncertainty of the input data has an important weight, and it is our intention to point it out also in the conclusions, as follows:
[This work provides a database on cultivation and management of cropland and grassland at a detailed spatial level. Data can be improved to reduce uncertainty and exploited in future work to test different management options, new or a combination of agro-ecosystem models, climate change projections, crop varieties or floristic compositions, and the support for future actions.]

7. In figure 5, S5 and some others it is not clear enough that the difference between the maps is only the irrigation. I also do not think that all these maps are necessary. The difference between the irrigated and non-irrigated maps is too small to recognise. I think it is an important and relevant finding, but one map with irrigation as example and the rest mentioned as showing a similar los change would be enough. In figure 12 the text is not readable (N2O emissions).

First part of the comment, Figure 5 and S5. These figures show N_2O emissions for croplands and grasslands (Fig. S5 a and b), or combined (Fig. 5), in the European administrative borders (NUTS2). Emissions in the large map are reported for historical period (1985-2004) and in the small panels is reported the difference "Δ" toward mid (2030-2049) and toward the end of the century (2080-2099), for both climatic scenarios RCP4.5 and RCP8.5.

N2O emissions here, or other variable in the respective figures (EF_{N2O} [Fig. 6], CH4 [Fig. 9], NEP [Fig. 11, Fig. S9), are reported with the irrigable scenario. So, they do not report the differences with the irrigated and irrigable scenarios.

We are perfectly aware that these panels in the figures can burden the view. Since we mention these differences (sometime low and sometime high) in the text when we report these results, *i.e.* differences between historical periods and differences between climate scenarios, we are not being able to remove this important information.

Second part of the comment, Figure 12. Thanks, we fixed this issue.

8. The authors mention changes in the growing season of spring crops. Considering that the growing season was derive by phenological models, I would assume a reduction of the growing season as well, as the vernalization would be extended due to increased winter temperatures. Was the vernalization included in the models? I understand the author comments on the second growing season. However, considering the shorter growing seasons for some areas, make a second growing season a realistic option and will affect the overall balance. This should be discussed.

We thank Reviewer#2 for this comment. Vernalization is included in the crop model for autumn-winter crops and plays a decisive role in crop development. The shortening of the growing season due to the increase in temperature slows down this phase and, in combination, reduces the other phases. CERES-ECG uses variety-specific genetic inputs to simulate crop growth development. The model considers different processes as the phenological development (especially as it is affected by genotype and weather), the extension growth of leaves, stems, and roots, the biomass accumulation and partitioning (especially as phenological development affects the development and growth of vegetative and reproductive organs), together with the biogeochemical cycles related to crop growth in soil, water and air.

Second part of the comment, possibility to grow multiple-crops in a year. In fact, as we commented earlier, there is not much data on a second crop in Europe, compared to the tropics (Waha et al., 2020), for example. What we have written remains conditional, as we have not done any testing with this work. Anyway, Mueller et al. (2015) show that longer growing seasons allowed by rising temperatures at the temperate latitudes have made the cultivation of multiple crops in a year more viable. Furthermore, multiple crops can become an alternative in areas with very long growing seasons where water (rain or irrigation) and the availability of light are not limiting factors. Nevertheless, in the warmer latitudes, the growing season of the crops can be considerably reduced (see previous point). This underlines the importance of understanding the growing temperature of crops / varieties, as longer growing seasons do not necessarily translate into an increase in yield. Furthermore, as we demonstrate, an earlier planting date can help mitigate these negative impacts of climate change as it allows harvesting before relatively higher temperatures and crops can benefit from the longer growing season (Liu et al., 2013). We added these comments to the text [L 587-589]

[Multiple cropping can be a viable alternative in regions with long growing seasons where water (rain or irrigation) and solar radiation are not limiting factors (Mueller et al., 2015; Waha et al., 2020), as well as cardinal temperatures for crop and varieties are met.]

9. Lines 520-533 and figures 10 and 11: I am not sure, if this is a good way to discuss this aspect. The yield needs to be removed to provide a more correct estimate for the "real" input into the system. It is possible to analyse the NEP for the changes, and, assuming the harvest index will be constant, the relative changes will stay the same.

However, I think the NBP would be a greater interest, as it indicates if the cropland/grassland systems are an overall source or sink. As I assume that the authors might have other thoughts on this, the yield and/or the NBP should be added somewhere (also the supplement would be fine with me).

The choice to discuss the NEP derives from the fact that this term expresses the amount of carbon potentially available (for storage, removal or loss) in an ecosystem. Thus, it does not consider e.g. organic fertilizer addition, biomass removals, residues recycled into the system, which would be used to calculate the NBP. NEP is a term that is more suitable for us for comparing the two systems in terms of potential carbon storage, and finally to compare them also with the literature (e.g. direct measurements with flow towers, often used for the calibration / validation of models).

We therefore preferred to insert the aforementioned addition/removals (which would have been used for the calculation of the NBP) to calculate the annual net greenhouse gas balance (NGB), which we finally find more interesting to discuss than the NBP (§2.3). Following Ammann et al., (2020) NGB is the most adequate indicator for assessing and comparing the overall GHG effect of the managed agro-ecosystems. To complete this answer, our calculation pass through the intermediate term of net GHG exchange (NGHGE), as proposed by e.g. Soussana et al. (2007). This indicator is reported but not directly discussed in the paper, since does not consider, as also the Reviewer#2 pointed-out, the carbon exported as production/intake or added by residue and organic fertilisers.

Finally, we preferred to discuss NEP as the potential carbon stock of the agro-ecosystems, and considering the import/export of C fluxes, as well as the GHG, in the term NGB, which is suitable to account the overall GHG balance.

10. Lines 784-785: Please sort out the abbreviations and its definitions. First, NGHGE is not the balance, but the exchange. Second, the NGB is the GHG balance, not only C forms (I prefer fluxes). Third, the removed residues needs to be added, while the remaining residues stay in the system.

Thanks to the reviewer for pointing out these mistakes, which we have corrected and reformulated in the new version of the paper.

[For both cropland and grasslands, CO_2 storage potential (estimated from NEP) provided the largest term in the net greenhouse gas exchange (NGHGE), confirming the statement by Jones et al. (2016). The NGB, calculated by subtracting the other non-gaseous C fluxes (i.e. export by harvest and crop residues, import by manure) from NGHGE, indicated that European agricultural surfaces are a net C source.]

11. It looks like the maps are created by a GIS tool. Please change the legand numbers to suitable step-sizes and remove the high number of positions behind the comma (e.g. in figure S1).

Done.

12. The nature of a review is often the critical sound and comments. I also would like to highlight the positives, as the responses to the comments were very good and especially the specification about the spin-up and the main objective. In terms of critical

analysis and novelty I would like to have seen more changes in the discussion and conclusion section (more parts of the comments moved to the manuscript). I thank for the discussion and looking forward to the comments.

Thank you for this positive comment on our review work, which was done thanks to the great support of this Reviewer#2 and for the very helpful comments.

Reviewer#1

I appreciate the effort done by authors to improve the manuscript quality. I still remain not fully convinced by final outcomes, especially since a large scale analysis such as that offered in this study provides a level of uncertainty too high to be translated into real-word findings. The outcomes based on model vs estimates (and not vs observed data) strongly reduce the level of confidence of this approach. I'm not sure this is the right approach to produce consistent information that, when correctly investigated, would be used to overcome environmental and agricultural issues Europe are experiencing. However, since authors strongly improved their work through further and better detailed analysis, and their approach agrees with existing "scientific" methodological approaches, I reccomend acceptance of this work in its present form.

We would like to thank Reviewer#1 for considering and reading our manuscript. The new version of this article further discusses the aspects related to the scales of analysis, input data, the uncertainty of estimation and the validity of the outputs.

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