Ref.: Ms. bg-2021-241 Biogeosciences

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

Reviewer#1

General consideration:

This manuscript shows the impacts of climate change in the European cropland and grassland production systems towards year 2100. Specifically, by using two biogeochemical models, the work focuses at assessing changes in plant productivity and biogenic GHG (N2O, CH4, CO2) balance. The paper is well detailed and structured, the language is appropriate and is fluently flows. However, to my opinion, some very important weaknesses are presents:

We thank the Reviewer#1 for the in-depth analysis of our work and the detailed comments which allow us to amend and improve the manuscript. All the critical points raised have been taken into consideration and addressed/discussed, as detailed in this document (in blue).

1. The novelty of the paper is quite low. Further modelling studies have been carried out at larger scale to assess changes in productivity and GHG emissions. The 2100 threshold is quite far and, even in the view of the EU policies mostly focused at nearest GHGs thresholds, the provided results risk to be very speculative and not very close-to-reality to provide suitable information for policymakers as suggested in line 74-75.

First part of the remark, novelty of the work. Literature reports similar European-scale studies that aim to estimate crop and grasslands production, GHG emissions and carbon storage at regional scale (e.g. Lugato et al., 2017; Lugato et al., 2018; Leip et al., 2008; Chang et al., 2017; Blanke et al., 2018). Unlike these studies, our study is based on two state-of-the-art specific models for the two systems under study, with detailed management options and producing both separated and joint results on croplands and grasslands. Moreover, unlike the mentioned studies, our work has a finer spatial resolution (see Vuichard et al., 2007, Leip et al., 2008; Ciais et al., 2010; Lugato et al., 2014; Iglesias et al., 2012) agreeing with the study presented by Chang et al. (2015a and 2017) which, however, deals only with grasslands. Furthermore, our work considers a variety of pedoclimates, albeit aggregated in simulation units, and is not only based on an extrapolation of a few points or on a single European area (Myrgiotis et al., 2019; Kutsch et al., 2010; Ceshia et al., 2010, Soussanna et al., 2010; Lugato et al., 2017; Lugato et al., 2018). Our work considers dynamic management by crop type or grasslands in comparison to e.g. Lugato et al. (2017), Ciais et al. (2010) and Leip et al. (2008). Finally, regarding novelty, we use specific crop rotations which are essential for understanding the dynamics of C storage and N_2O emissions in soils (13 crops in specific succession for each simulation unit) and not based on one or a few specific crops, as reported by other Authors (e.g. Sansoulet et al., 2014, Yan et al., 2019).

Second part of the comment, 2100 threshold. The use of quantification methods based on process-based biogeochemical models, as we used here, represent a suitable tool to estimate the long-term effects of management options or climate perturbation on productivity, GHG emissions and carbon dynamics in the long period (e.g. Farina et al., 2020). In fact, the quantification of SOC dynamics from agricultural surfaces over space and time is challenging as the effect of management practices may take years to occur and experimentation became difficult for testing multiple agricultural practices over large areas (Heiskanen et al., 2012). Similarly, GHG emissions, although more realistic in the near future, are still related to long-term dynamics (Guenet et al., 2021), justifying their representation over long-term.

As remarked by Reviewer#1, the horizon of 2100 is far to guide European policies and the quoted sentence "*This study represents the baseline to support and identify future actions targeted to maintain productivity and reduce environmental impacts*" does not reflect the main aim of this work. Our intention is to provide technical (databases and models) and scientific (outcomes) support for policies. With this work, we are able to provide, with an acceptable degree of uncertainty, the evolution of the agro-ecosystems in the near future (e.g. until 2030) to observe changes in biogeochemical cycles (i.e. GHG emissions or SOC changes). Projection at the horizon 2100 provides, with another degree of uncertainty, the impacts of future climate and business-as-usual management on the same variables. To reflect that, the sentence will be amended as *"This study provides near and long-term projections of key agro-ecosystems variables to support and help identify possible actions to maintain productivity and reduce environmental impacts.*". (Lines 73-74 of the new version of the manuscript)

This represent a sound remark and we're intended to point out the relevance and the differences between the near and long-term estimations at the beginning of the section (§2, Lines 81-87 of the new version of the manuscript) to better guide the interpretation of the results.

2. Modelling works do need appropriate model parametrization such as climate variables, soil properties, vegetation parameters and management. Whereas climate and soil properties are clearly reported, several information in the other components are missing or shows very low confidence. For instance, for the management characteristics, I appreciate the effort of the authors to obtain all these information. However, some parameters need to be better explained and discussed. For instance, sowing dates and fertilization were imposed but, for sure, these cannot reflect all the possible variability observed in the whole EU. I understand the need to impose fixed parameters for running the model, however it is also necessary to indicate the results can be affected by uncertainties due to the application of these fixed parameters. My main concern is about crop parametrization. In line 175-176 authors indicate a crops parametrization based on those applied in previous/other works. However, no reference has been reported. Also, I was wondering how authors were able to retrieve and summarize all these crop/grassland information since modelling study are often carried out at single point/area, with different parametrization for the same crop over different areas, and do not report these data. This is also true especially for grasslands where, as the same authors says, very low information are presents. There is no explanation about these parametrization (i.e. water efficiency, radiation use efficiency,

maximum and minimum productivity, etc.) were found, from which studies, and how these parameters were summarized to find the most representative/suitable for each of the applied crops.

First part of the comment, model parameterisation. Although both models (CERES-EGC and PaSim) have been calibrated and verified with direct observations under various pedoclimatic and management conditions at the field-scale (see all the references below, and the review of Brilli et al., 2017), comprehensive studies aimed to calibrate model performances with spatially extensive time series and variables, as well as model sensitivities with climate perturbation, are scarce (Lehuger et al., 2010, Vuichard et al., 2007). This is valid for the model we used here and other biogeochemical models (e.g. Balkovic et al., 2013, Lugato et al., 2010). Calibrating biogeochemical models over large regions is challenging. Wide-ranging experimental dataset that allow testing the interactions of multiple variables (GHG, soil, water, ..) are scarce and fragmented. Aggregated data from regional statistics (Eurostat, FAOSTAT, ...) are insufficient to parameterise the models, as they do not represent the field-scale conditions for which the models were originally developed (Therond et al., 2011). Furthermore, as Reviewer#1 specified, soil and daily climate input data are often available from existing databases at different spatial resolution. On the other hands, spatially and temporally information on crop type, parameters and management are less readily available (e.g. Leenhardt et al., 2010). Dealing with scarce input data and spatialised measurement requires different procedure of downscaling and upscaling for the different data types. This will feed the uncertainty of the outcomes. These aspects will be presented and discussed in a specific chapter (§4.5 “Uncertainty and limitations of this study”) since, with regards to model parameters, knowing the source of uncertainty is the key to understand and exploit our outcomes.

Second part of the comment, input data. Regarding crop and grassland input management in our simulations, certain parameters such as sowing dates, fertilisation schedule as well as cutting dates were not prescribed. They were dynamically calculated per year and for each simulation unit as a function of the specific climatic conditions. To avoid e.g. issues related to shorter crop cycles due to early sowing dates in warmer climates we opted for different crop varieties following a latitudinal gradient to fulfil the thermal units needed. The selection of varieties has been done following the published studies for winter soft wheat, pulses, maize, barley, rapeseed (see references on the following paragraph) and, for the other crops, based on the modellers’ experience by using the CERES-EGC varieties database. Moreover, as described also by Minoli et al. (2019) crop phenological development constitutes a relevant source of model uncertainties in regional assessment, which will be cleared and discussed as well in the above-mentioned chapter regarding uncertainty (§4.5). PaSim model did not use a specific calibration of grasslands over Europe, but was run with a parameter set resulting from a mullti-site calibration for a network of EU grasslands (i.e. flux tower network, see Ma et al. 2015). Likewise, PaSim follows an adaptive management based on climate.

In relation to previous work used for crop parameterisation in Europe - and more specifically in France, Germany, Italy, Sweden, Denmark, UK - we can mention Drouet et al. (2011), Ferrara et al. (2021), Goglio et al. (2013), Lehuger et al. (2009), Lehuger et al. (2011), Rolland et al. (2008), Wattenbach et al. (2010), Haas et al. (2021) that all used, parameterised and tested the CERES-EGC model. Concerning grassland, PaSim has been calibrated in UK, Ireland, Hungary, France, Germany, Italy, Portugal, Spain, The Netherlands by different studies, e.g. Lawton et al. (2006), Calanca et al. (2007), Gottschalk et al. (2007), Vuichard et al. (2007), Ma et al. (2015), Sandor et al. (2016). These references will be reported in the text of the manuscript as suggested by the Reviewer#1. Lines 130-135 of the new version of the manuscript.

3. My main concern is the point 3.1.1., model validation. Model validation should provide a confirmation about the model capability to represent the crop growth, development, and production in different environment. However, looking at picture 1a, does not seem that models are able to reproduce the correct behavior of each single crop. Putting all crops together may create an inthrinsic error and does not indicate if each single crop is well reproduced. For instance, looking at sugar beet or potato, I'm not able to see a proper correlation between modelled and observed crops. In order to provide a robust validation, each crop should be singularly validated and then reported, so as readers are able to see the model confidence and robustness at reproducing the crops. This is needed also to provide robust information about the expected changes due to climate change. Therefore, I suggest authors to provide single validation for each crop and then summarize them in scatterplot or table with the relative statistics. Concerning grasslands, results are very poor for all areas. For instance, for the sole Mediterranean data (L272) the overestimation of 55% do not allow to indicate these data as robust enough to be accepted. This overestimation led to unplausible results under future conditions, resulting not useful and misunderstandable for readers and policymakers. I understand the lack of data, however more recent and affordable information on productivity could be taken by remote sensing and new analysis may be done to provide more robust results. I understand the effort of the authors, but grasslands systems are very complex and sensitive to climate change, especially considering the dynamics involving water reduction and species changes. These results do not take in account changes in composition that, in turn, also affect productivity, neither provide robust statistics. Finally, as indicated for crops, at least model calibration/validation for single areas (if not for pasture composition) should be carried out, to make results more robust.

First part of the comment, validation of cropland. In the light of the previous comment, due to the lack of available data, carrying out a spatialised validation considering cropland development is not feasible at the moment. Furthermore, carrying out a validation by regions (NUTS2 or NUTS0), given the quality and discontinuity of the data available for some regions, is difficult, although this has been presented by some Authors (e.g. Lugato et al., 2017). The comparison we proposed here considers the average annual yield at European level for each crop under analysis, and during the historical period (1978-2004). As pointed out, when comparing all crop yields together, a good regression comes out due to the fact that the crops have different production levels between them. However, when analysing the yearly regression by crop over the time period explored, scores are less satisfying, with a more than likely “cloud” representation (as expected). Since in this work we are dealing with trends over EU, looking e.g. at the differences between the mean of the statistics and our simulation for each crop in the considered period, produced quite satisfactory results (14.22% on average, ranging between 1.57% for sugar beet and 28.60% for grain maize). We decided to add this figure anyway by removing the regression line and presenting the metrics per crop in the text (range, average, RMSE, MAE) and adding the table below in the supplementary material (Tab. S3). A representation of our results is also reported here below as boxplot (Figure R1) and, as suggested by the Reviewer, by single area, or NUT0 (most complete data available in Eurostat) in Figure S2 (See Line 270-271 of the new version of the manuscript).

Tab. S3. Statistics of the simulated crop yields compared with Eurostat in the period 1978-2004. Yields are reported as standard humidity.

crop	**MAE**	**RMSE**	**RRMSE**	**Mean** t DM ha^{-1}		**Minimum** t DM ha^{-1}		**Maximum** t DM ha^{-1}	
	t DM ha^{-1}	t DM ha^{-1}	%	**Simulated**	**Statistics**	**Simulated**	**Statistics**	**Simulated**	**Statistics**
Fodder Maize	2.21	2.97	27.1	10.66	10.95	5.41	5.54	15.37	15.09
Grain Maize	1.58	1.77	31.8	7.15	5.56	5.44	4.61	8.76	6.66
Oats	0.496	0.648	22.1	3.24	2.93	1.94	2.57	4.35	3.30
Potato	3.3	4.04	19.2	17.93	21.06	13.14	18.59	21.10	24.00
Pulses	0.387	0.462	18.2	2.21	2.54	1.90	2.18	2.58	3.01
Rapeseed	0.423	0.495	23.4	1.89	2.12	1.38	1.85	2.89	2.56
Barley	0.543	0.625	17.6	3.10	3.55	2.38	3.09	3.71	4.09
Soya	0.467	0.621	35	2.05	1.79	1.37	1.52	2.97	2.16
Sugar Beet	4.35	5.29	12.8	40.74	41.39	30.94	36.99	48.03	48.93
Sunflower	0.348	0.405	24.9	1.89	1.63	1.38	1.42	2.24	1.96
Rye	0.852	1.09	38.6	3.57	2.83	2.06	2.46	5.27	3.41
Wheat	0.894	1.05	25	5.02	4.21	3.59	3.47	6.16	4.83

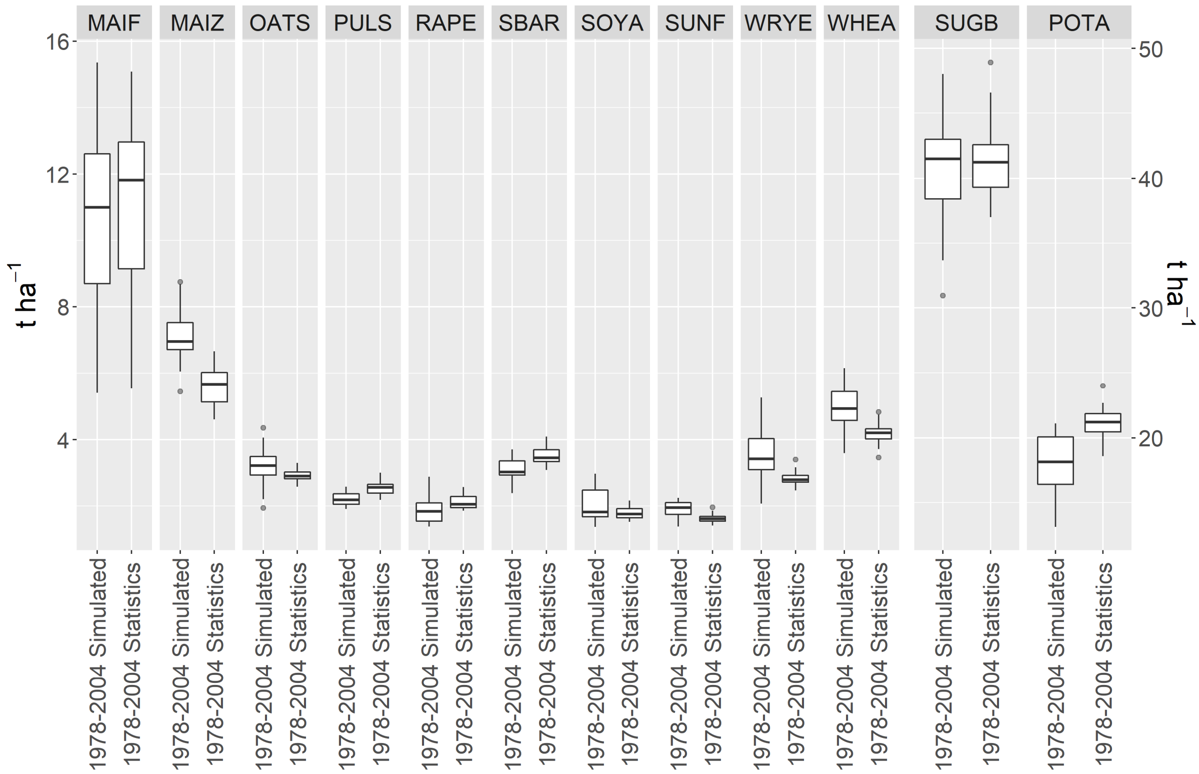

Figure R1. Difference between simulated and statistics (Eurostat) yields for the simulated *crops in the 1978-2004 period. Crop names are reported as code names. This figure is reported only in this document.*

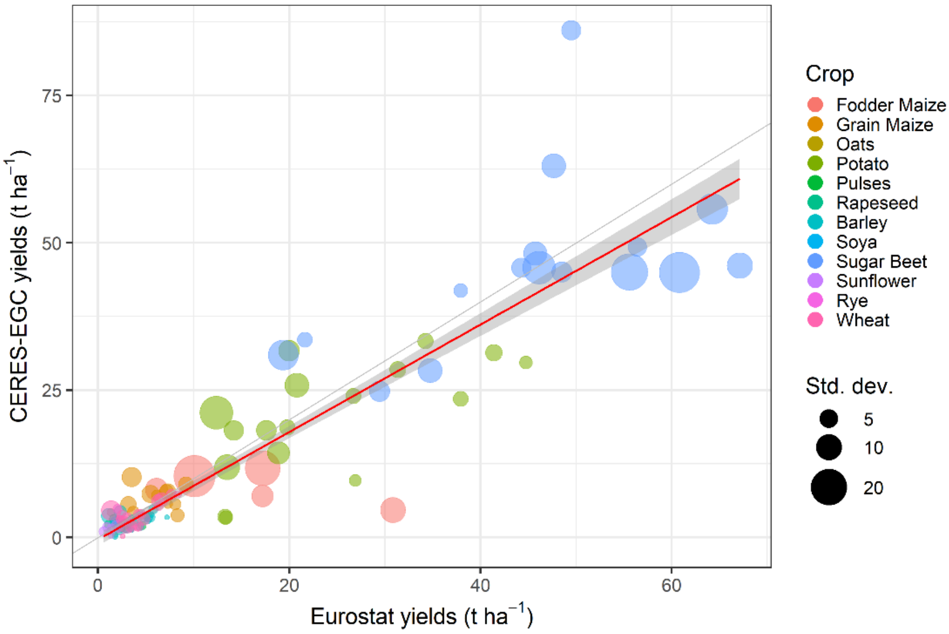

Figure S2. Difference between simulated and statistics (Eurostat) yields for the simulated crops in the 1978-2004 period. Each point represents the average 1978-2004 aggregated by country (NUTS0 level). Red line is the regression over the plotted points.

Second part of the comment, validation of grasslands. The results presented by this study, as well reported in all the studies concerning grasslands in Europe (e.g. van Oijen et al., 2014, Chang et al., 2015a; Chang et al., 2015b, Chang et al., 2017; Blanke et al., 2018), suffer from the same problem of the overestimation of the Mediterranean area (Spain, Italy, Greece). This stems from the fact that the observed data over EU are scarce, and the existing databases (e.g. Eurostat) are incomplete. We decided to use the spatial analysis of Smit et al. (2008) based on regional, national and international census statistics for Europe, which are currently used as a reference also with the above-mentioned modelling estimations and other literature. For example, Chang et al. (2015a) and Blanke et al. (2018) simulated higher potential productivity with, respectively, ORCHIDEE-GM and LPJ-GUESS-LUI than the productivity reported by Smit et al. (2008) and clearly declared that their estimations were decidedly far (above) compared to the Mediterranean area (Mediterranean Mountains, Mediterranean North, Mediterranean South). Although these results are not encouraging, they are in line with our outcomes. This overestimation is partly due to the fact that the models simulate the potential (maximum) productivity of grassland, whereas Eurostat productivities are based on actual harvest data (see Lines 284-286 of the new version of the manuscript). Although these estimations biased, some study presented also projected scenarios into the future (Chang et al., 2017; Blanke et al., 2018). Finally, to strength our findings, in our study we tried to compare the results with the available measured data over EU (Hörtnagl et al., 2018; Viuchard et al., 2007; Soussana et al., 2007; Merbold et al., 2017) with reliable results.

Aware that the collection of data across European countries need to be more consistent and standardised to improve the quality of European grassland productivity and land use data, we decided to mention this point in the §4.5 chapter and Lines 284-287 (of the new version of the manuscript) concerning the uncertainty and the limits of this work, to avoid any misinterpretation. Since we do not dispose of the Smit et al. (2008) database, we based our comparison with the results present in their publication (average per area in the period considered), thus we are in measure to provide the evaluation which is already been provided in the manuscript and in Figure 1.

4. I appreciate the wide analysis done by authors; however, the above-described issues make the level of confidence of these results very low. Discussion section do not address this high level of uncertainty and only report the agreement in the impacts with other studies. This information is quite negligible if not accompanied by a strong model performance in the magnitude of the results. Simulated dynamics are found expected since driven by common algorithms (i.e., GDD for crop growth), but robust information about the expected change need to be provided. Whilst crop data may be improved, I'm quite concerned about pasture dynamics which need to be adequately addressed through a more consistent approach since, at this time they cannot be accepted as here proposed.

Based on the literature produced and the data available, we find that our work, although different in terms of input data and less extensive in terms of management possibilities, has a level of confidence comparable to the previous cited publications on grassland modelling (e.g. Chang et a., 2015a, 2015b, 2017; Blanke et al., 2018). We aimed to show tendencies related to future climate change in order to improve our understanding. Results are discussed and an overall uncertainty of the work is addressed (new chapter §4.5), starting from the input data and ending with their interpretation. As described above, this also applies to crops.

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Reviewer#2

1. The manuscript describes the set-up for spatial runs of crop productivity and emissions from croplands and grasslands in Europe. This is analysed on simulation model results provided by two model simulation approaches for a historic (1985-2004) and a future period (2005-2999). The future scenario simulations include two scenarios, representing a moderate and an extreme climate development.

We thank the Reviewer#2 for the careful analysis of our work and the detailed comments which will hopefully allow us to correct and improve the manuscript. All the critical points raised have been taken into consideration and addressed/discussed, as detailed in this document (in blue).

2. Spatial data about emissions of croplands and grasslands are very important and model approaches are most suitable to provide these spatial data. The data demand for models is high and the errors and uncertainties of used data as well as of the model results is high. This combination makes the judgement of the quality of spatial model studies on large scales, like this one here, very difficult. There is no doubt that the technical set up of the model is up-to-date and the used data represent the state of the art quality of available data. However, there are still a couple of genera aspects that let this manuscript down:

In agreement with the Reviewer#2's remark, we are aware of the gap existing between direct measurements (or estimations) at field scale and the results of modelling at regional scale, despite being based on the most accurate possible spatial input data. Nevertheless, accurately estimating of e.g. GHG emissions from agricultural soils at territorial or regional scales is challenging, as observations are sparse, and the effect of management practices may take years to realize (e.g. on soil carbon stocks). For these reasons, process-based biogeochemical models as we use here, represent a suitable tool to estimate emissions from agriculture and evaluate the effects of e.g. management or climate on production, soil carbon and GHG emissions.

- I mentioned the uncertainty and error affected data. I appreciate the open communication about the used data and the limitations in the approach. I see the extrapolation of the data for fertilizer application rates and crop rotation difficult. This extension introduces a different error and shifts the uncertainty compared to the period these data sets were originally aggregated for. Changed climate will affect the rotations and the fertilizer application rates and it is difficult to predict these changes. I do not mind that these data are used, but I would expect a more critical discussion on these points and a more careful conclusion.

This comment prompts us to add some element to the discussion regarding the hypothesis behind the dataset and the extended degree of uncertainty in the results.
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As known, nitrogen fertilisers represent one of the most dominant sources of anthropogenic N_2O emissions from agricultural sector. Nonetheless, fertilisers are one of the more difficult information to obtain in order to establish a reliable dataset at a high spatial resolution. This is the reason why we used a large spatial unit (i.e. 25° side) since the original information regarding crop management data were only available at coarse resolution (downscaled to 1km from NUTS2 region, then aggregated to our spatial unit).

Second, the doses of nitrogen fertilizers, as well as crop rotations, were kept constant during the climatic projections. This is because the variation of nitrogen rates according to the needs of the crops, as mentioned, would have added a further source of uncertainty and would not have respected the diversity of N rates between the simulation units over EU (e.g. regions with more leguminous crops have lower N doses, regions with more livestock have lower mineral nitrogen). This can be seen as a bias with respect to an “adaptive management” based on specific crop needs but, at the same time, our choice to keep management constant provides key information regarding the effects of climate under business-as-usual management, which is one of the aims of this paper. As described in materials and method, nitrogen fractionation follows the crop sowing dates. Similarly, for grasslands, the management remained unvaried in the future projections and the expected nitrogen rates were not applied if the thermal sums for a specific year do not allow it.

Finally, changing the fertilizer doses for future scenarios would mean considering some hypotheses that are not included in our models (e.g. dynamics of animal production over time on a local scale, change of human diet, modification of EU policies). Regarding the effects of climate change on crop rotation composition, as well identified by the Reviewer#2, it is noticeable that it generates another bias compared to our data as it is difficult to translate agronomic choices driven by climate perturbation. This would require an in-depth study about possible choices of farmers as a response to disturbing agents (e.g. introduction of new varieties, change of species in the rotations, increase of diversification, ...) which, in addition to being far from our target, would need to dispose of data not easy to obtain (e.g. CAP declarations).

Following this and the following remarks we decided to add a further chapter in the Discussions “4.5 Uncertainty and limitations of this study” regarding the uncertainty of the input data provided for the “management driven change period” (historical period) and the uncertainty produced in the estimation of the “climate driven change period” (future scenarios).

- I highly appreciate the detailed comparison of the different results with literature, but I see two problems with this. One is that some of the studies that are used for comparison are model approaches that used similar data and use similar model approaches (still worth to compare, but not necessarily completely independent). More relevant, I do not see the new or innovative contribution of the here presented data. There is a lot going on in the manuscript and it is not all linked with each other. Even though, croplands and grasslands are analysed, both are analysed parallel and not integrated. The most obvious connection (using manure from livestock production) is not included (I understand why and I agree to this point) and spatial emission budgets are not provided (as land use data are not used). Overall, the data are nicely presented, but more moderated than discussed. This is also reflected by the conclusion, which is more a summary of the manuscript and the conclusive sentences are not really convincing (lines 773-774: I do not think that the study proves this, even though it adds a strong indication and to conclude that C stock changes are variable over time does not require a complex study). I think that the errors and uncertainty and their impacts on the results should discussed stronger.

We thank the Reviewer#2 for the appreciation about our work. Here some element to address this remark.

First part of the comment, the comparison of our results with independent and innovative data. The validation of the results provided by a biogeochemical model applied to regional scale is confronted with several challenges. Primarily, a comprehensive model calibration (which is a prerequisite for its reliable application) is prevented by the fact that there are no comprehensive and systematic experimental data available (at EU extend) that allow testing of the entire set of variables and their interactions. Consequently, validation of models at the regional scales is far to be achieved, even if literature report some efforts (Balkovič et al., 2013, Lugato et al., 2010; Faivre et al., 2004; Challinor et al., 2009; Niu et al., 2009). Secondly, aggregated data over the same extent (regional statistics) can be used for evaluating (compare) model representations, even if they do not represent field-scale conditions for which the models have been originally calibrated (van der Velde et al., 2009, Lugato et al., 2017; Therond et al., 2011). We are aware that running the model for a large spatial scale as EU exposes the model to a broader range of conditions (e.g. weather and soil characteristics) for which it may not have calibrated and evaluated, potentially increasing the uncertainty. Anyway, in the two models we used, this source of uncertainty has been reduced through specific parameterisation resulting from a mullti-site calibration for (i) a network of EU grasslands (i.e. flux tower network, see Ma et al. 2015) for PaSim, and (ii) a multi-site calibration in Western EU for croplands (Lehuger et al., 2010; Lehuger et al., 2011).

The advantage of using statistical data aggregated at regional scale (Eurostat crop yields) gives a clear indication about the magnitude of our estimation in that area. In our work we also compared the outputs with measured data on the European territory (especially for grassland) to obtain information concerning the magnitude of our estimation and, finally, we compared with other modelling interpretations associated to other dataset and hypothesis to assess the goodness of our estimate.

In particular, the comparison with other modelling studies has shown that our estimates are for some traits in line with them and, for other traits, differing (e.g. Lugato et al., 2017) and more aligned with the scientific literature (Wells et al., 2018) and experimental findings (Stehfest and Bowman, 2006). In addition, with regards to grassland productions, we tried to compare our estimation with existing databases (Eurostat) but data are still scarce and incomplete. Thus, we used the spatial analysis of Smit et al. (2008) based on regional, national and international census statistics for Europe, which is currently used as a reference also with the existing modelling estimations over EU (e.g. Chang et al., 2015; Chang et al., 2017; Blanke et al., 2018). Compared to these studies, our findings provide a comparable and improved data representation. Finally, compared to similar modelling studies dealing with both cropland and grasslands (e.g. Lugato et al., 2017; Lugato et al., 2018) the work we’re proposing grounds in different data sources and proposes an analysis with two specific biogeochemical models, one for cropland and one for grasslands and pasture. This latter makes a point of novelty compared to other modelling estimations (which use only one, often “broad”, model). Moreover, regarding innovations, our work is based on specific crop rotations (13 crops) and not based on one or a few specific crops as in other similar studies (Sansoulet et al., 2014, Yan et al., 2019). A further point regarding comparison with existing data is that our study was not designed in making exact estimates (e.g. yield) at a precise time and place in Europe, but rather in having a robust estimation system (i.e. producing results in reliable orders of magnitude) to track and study trends and dynamics in the near and long term.

Second part of the comment, croplands and grasslands. Croplands and grasslands outcomes are reported both separately and jointly. Due to the diversity of the two systems, separated results are provided to discuss the key factors for each of the two production systems (NEP, N_2O, CH_4, productivity) and, not least, to compare them to the literature. Furthermore, the results are presented jointly (N_2O, EF, NEP), to give an overview at EU scale and to compare it, of course, to the present literature and datasets. In order not to burden the paper, most of the results on individual systems are already reported in the supplementary material. We found very interesting the idea proposed by Reviewer#2 concerning the connection between the two systems (manure, straws) with the aim to study dynamically the two systems on a territorial scale. Besides, in this study, the two production systems are based on independent inputs data provided by measured/statistic data and not (at present) producing and using circularly fluxes of matter.

Third part of the comment, discussions and conclusions. In light of this comment, we agree to add a further chapter in the discussion section (§4.5) regarding the uncertainties of the input data and their effect on the actual and projected outputs. In addition, we propose to modify the conclusions to be more effective. Regarding lines 773 and 774 (of the previous verions of rhe manuscript) "*Our findings prove that productivity, GHG emissions and soil C stock changes have a heterogeneous trend over time and space*" our intention was to state that these variables have a heterogeneous trend over time and space with different dynamics (i.e. productivity increases, then stabilises or decreases, while emissions increase, as C stocks do) and not to necessarily quantify their absolute values. In agreement with this remark, modified the sentence as follows: "*Our findings show that productivity, GHG emissions and changes in soil C-stock have a heterogeneous spatial distribution*”. Line 887 of the new version of the manuscript.

- The presentation of the results for the historic period sounds partly like observed data or robust baseline. Considering the uncertainty of the use data (the crop rotation and fertilizer data are also simulation results, soil maps are developed over larger time periods and climate realisation are partly weak in the representation of extreme events) this is not a robust baseline. As mentioned above, the data that are used for the historic period are aggregated in a different way than the data for the future scenarios. While the trends, productivity and emissions during the historic period are driven by management changes (the farmers action is considered, as these are statistical data sets) the future scenario results are driven by climate impacts. Both can be presented and in the end they can be compared by a direct comparison including conclusions is difficult. Figure 5 shows the spatial distribution of the fluxes, but it would be great to see the emissions separated for croplands and grasslands. There are a couple of places where the N emissions surprise me. I would expect higher N emissions around Paris, less low emission spots (and more extreme emissions) in the Netherlands and I cannot follow the logic that North-West Germany shows lower emissions than the East of Germany (historically simulations). I am also critical about the results for Eastern Europe and, again, using these data for future extrapolation. Eastern Europe make these data doubtful as baseline for the future simulations. While management in historic data changed due to political changes or to adapt changing conditions, the management for the future simulations remains constant. I think simulations on the historic data are a good point to prove the functionality of the models, but I would only compare the future simulation scenarios with each other. Obviously, you can show general changes and trends, but I wouldn't use the historic simulations or observations as baseline or reference. Errors and uncertainties are different in both data sets, which affects the results and the differences.

First part of the remark, historical and future periods. We agree with Reviewer#2's point of view. The so called "historical period" was not targeted to represent a baseline in the paper, although it looks like it. We take this suggestion and we re-oriented the paper's discussion by presenting (i) the historical period, or "management driven changes period", to define the performance of the models, and (ii) the comparisons between the "climate driven changes period" or the climate scenarios. Based on this remark, we introduced a note at the beginning of the chapter "2. Materials and Methods" (L 81-87 of the new version of the manuscript) to define the boundary and the characteristics of these periods. Finally, this separation is reported in the chapter 4.5 "Uncertainty and limitations of this study".

Second part of the remark, Figure 5 and N emissions separated. We report here below and in the supplementary material the N_2O emissions separated from croplands and grasslands (Fig. S5). Note that the separated maps report emissions per hectare and they not consider the presence and share of cropland and grassland for each simulation units, as the case of the joint map. This is clarified in the figure caption.

Regarding N_2O emissions in the mentioned areas, the results around Paris are indeed low, as also reported by Lugato et al., 2017, and are related to the N doses applied (see below). Regarding the Netherlands, we checked again carefully and, indeed, there was an error in the name of one NUTS2 region (NL11) for grasslands and one region was not reportd (NL31). We corrected the issue and the emissions now look less spotty. For Eastern Europe, there are important emissions between southern Romania and northern Bulgaria due to the fact that there is a very high arable land use intensity in this area. In the north of Eastern Europe, emissions are triggered by high levels of soil organic carbon in the topsoil (e.g. Guenet et al., 2021) as reported in Figure R1a (here below). Finally, as mentioned by the Reviewer#2, the estimated emissions in Germany are expected to be higher in the north-west than in the north-east, as also described by Dechow and Freibauer (2011) and Mathivanan et al. (2021). In other hands, our simulations indicated strong emissions in the north-east due to the higher N fertilisation amounts, as can be seen from Figure R1b which shows the map of the averaged fertilisation over the historical period. Apart from north-east Germany, comparing the emissions reported by Dechow and Freibauer (2011), we can visually see a good spatial fit regarding the emissions for the grasslands (north-west, north and south), as well as for croplands in the south-eastern and north-western sides of the country.

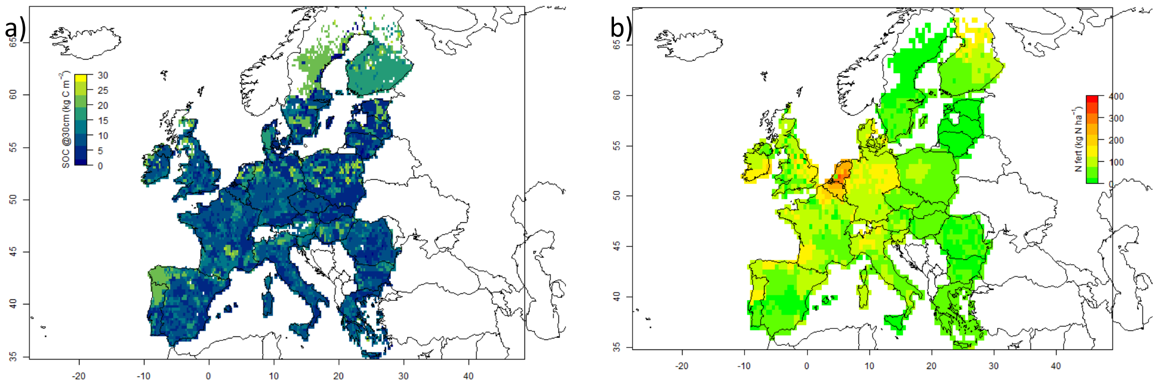
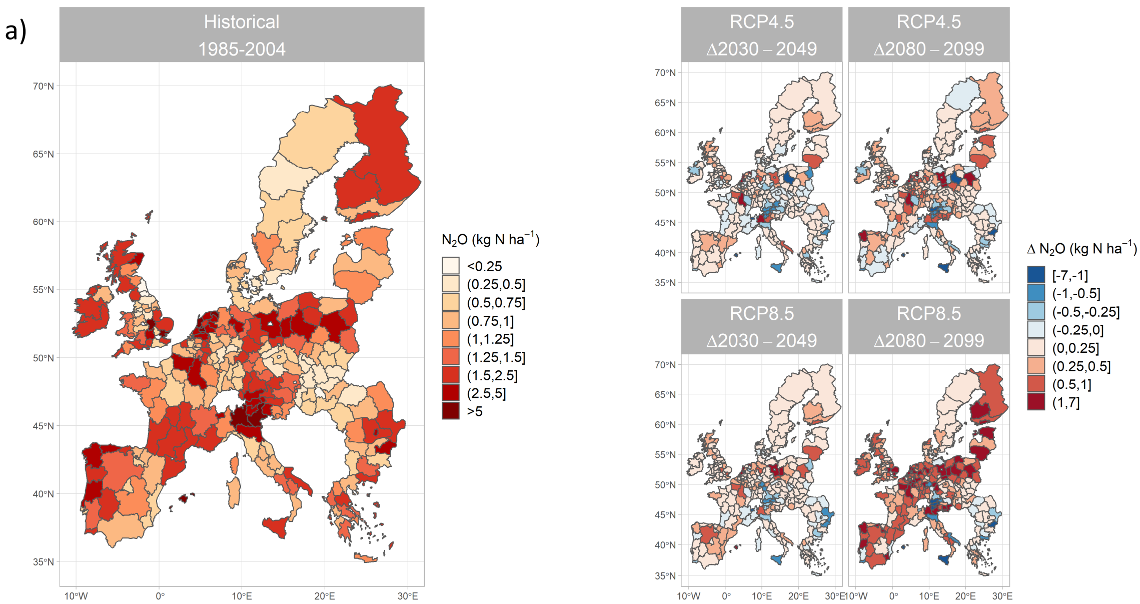


Figure R1. a) Soil organic carbon (SOC; kg C m^{-2} in the first 30 cm topsoil). b) Nitrogen application as fertiliser (organic + mineral) (kg N ha^{-1}) reported as the average 1980-2004.



b)

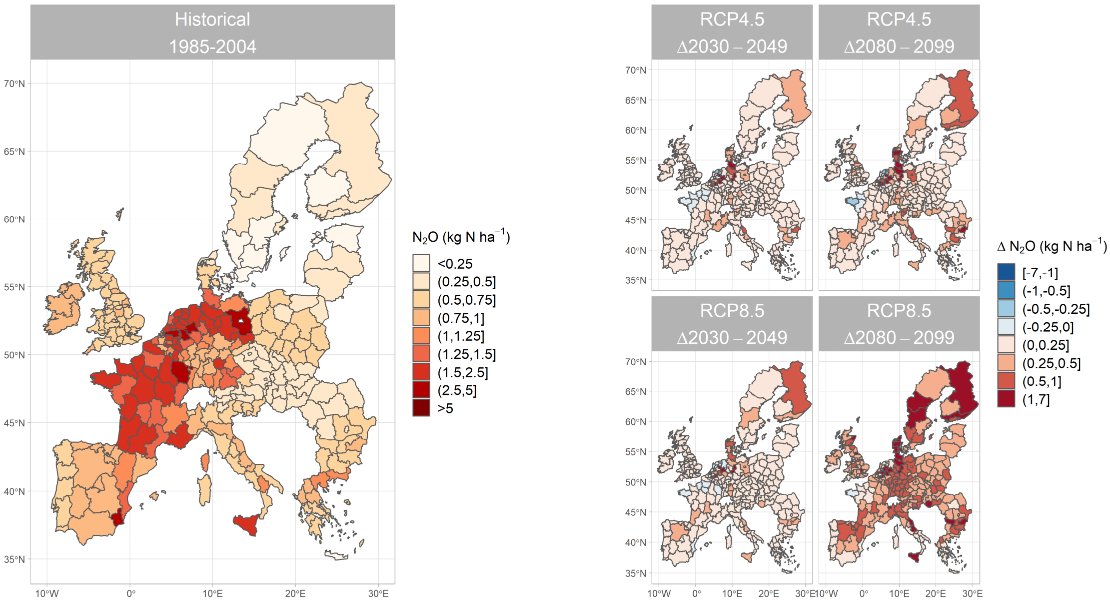


Fig S5. N_2O (kg N ha^{-1} y^{-1}) for croplands (a) and grasslands (b) in the European administrative borders (NUTS2). Emissions are reported for the historical period (1985-2004), and the difference “Δ” with the middle (2030-2049) and the end of the century (2075-2094) for the two climatic scenarios RCP4.5 and RCP8.5. N2O emissions are reported in cropland with the irrigable scenario.

- The spin-up for such a model approach is fairly short. Was the model tested for equilibrium in all grid cells? What were the criteria for equilibrium (no change over 10 years? Only 1 % change over a fixed period?). If the spin-up was only tested for an average, I wonder, if the authors think this is good enough? Where did the authors got the initial SOC values from or did the model used spin-up SOC?

With regards to croplands, CERES-EGC model was spin-up from 1951 to 1977 for each simulation unit, reaching an estimation error lower than 0.1% of the relative variation in the C balance between two cycles of management and climate in 5 years. The first initialisation of SOC was provided for 1951 with the values taken (and allocated to the various pools) from the ESDB for the year 2013, as the most complete spatial dataset found. Equilibrium was reached for the totality of the pixel before 1971. The process used in PaSim to obtain an initial state on soil C pools for each pixel was to set a starting point for each simulation unit as well as for croplands. Then the model was run for pixel from year 1840 based on HadGEM2-ES weather data. Following, the model was let drive following the simulation management from year 1901 as described in the material and method [from 1901-1950, a low intensification management level with no mineral fertilization and cut at 900°C-days; from 1951 to 2010, a gradual management intensification (linear increase of quantities, progressive earlier shift of cutting date)]. Meteorological data were obtained from the same source (HadGEM2-ES, 1861-2005). See Lines 226-235 of the new version of the manuscript.

- I have some concerns about the management and the potential impact of the management options on the results. I find the amount of 80 % residues removed from the field very high, compared to other comparable model approaches. Do the authors think that this is a realistic number for Europe? I didn't find any support for this number and would like to understand how the authors came to this result. Changing this number will change the NEP significantly. I also wonder, if the authors thought about introducing a second growing period. The shorter growing periods are only presented as a problem, but this can be used for double cropping seasons. Again, I do not mind limitation, by they should be discussed and in a best case the relevance (quantitative impact if possible) estimated.

First part of the comment, crop residues. The sentence in the paper is not at all well written and has led to its misinterpretation. Residue management was defined as a function of the crop species. Average amounts of above ground residues removed / remaining on the field are for cereals 50/50%; grain maize, soya, potato, pulses, sugar beet, sunflower, rape seeds 20/80%, silage maize 80/20%. The same percentages have been also reported by Scarlat et al. (2019) and in a research report by some of the Authors of this present paper (Carozzi et al., 2021). This sentence will be revised as follows "Residues were managed based on crop species, exporting half (50%) of the aboveground cereal straws, 80% of the fodder maize and removing 20% of all the other crop types including grain maize" (Lines 195-197 of the new version of the manuscript).

Second part of the comment, introducing a second growing period. We have not introduced a second growing period for two reasons. The first reason is that to our knowledge there is no available data concerning a second crop growing period. The second reason is that the introduction of a second crop is one of the implicit outcomes of the reduction of the growing season as indicated by the Reviewer#2 and as already pointed out in the previous version of the paper "Our findings confirm that climate change will have a regionally distributed impact (Howden et al., 2007; Challinor et al., 2014; Parry et al., 2005; Lobell and Tebaldi, 2014) even in scenario that include mitigation measures to offset climate change (RCP4.5), creating the possibility to the design cropping systems with multiple crops in a year". Lines 580-582 of the new version of the manuscript.

3. In summary, I think this is an interesting study, but I do not see the clear objective. For me all the different points does not come together. There are plenty of options to improve the manuscript (focussing on single aspects; defining key/new aspects of the presented study; include a more critical discussion about the limitations, including estimates about their relevance; etc.). In the actual form I am not convinced by the manuscript, but there is definitely enough potential for improving it,

Thank you very much for the appreciation of our study. The objective of this paper is twofold and is (i) to provide an overview of productivity, GHG emissions and soil carbon stock from the current croplands and grasslands and (ii) to assess the likely effects of climate change on these agro-ecosystems to support actions aimed at reducing environmental impacts and sustaining agricultural production. Following this recommendation, we intend to put more emphasis on the discussions in the paper distinguishing these two objectives, management driven change (historical period) and climate driven change (future scenario). This will help to stick more on our objectives and drive the discussions.

Additionally, the intention of this paper is to present a spatial dataset that can be improved and used to carry out other studies regarding the evaluation of management change (fertilisation, crop residues, introduction of intercropping or double cropping, ...) or with different simulation models. This dataset does not claim to be complete, but represent a characteristic approach which can be the basis for future improvements (e.g. with the improvement of national and European Land Parcel Identification Systems) or with fine spatial scales. In particular, Haas et al. (submitted) and Carozzi et al., (in preparation) used and modified this database to investigate the effects of crop residue management in terms of greenhouse gas balance and feasibility of environmental policies (e.g. 4p1000 initiative) with a multi-model approach.

Line 51: become neutral

Thanks for this remark. It will be corrected in the new version of the manuscript.

Line 179: species

Thanks for this remark. It will be corrected in the new version of the manuscript.

Line 205-209: Was cutting and grazing on all grasslands?

Thanks for this remark. Yes, all grasslands were cut (as a function of thermal sums, i.e. degree-days) and grazed. Grazing is extended to all grassland in function of LSU density, although grazing is not present in a very few simulation units.

Line 218: Do you mean 1901-1977?

As mentioned before and better detailed on the paper, equilibrium for C pools was iterated from 1840, then management intensification was calculated starting from 1901. See Lines 226-235 of the new version of the manuscript.

Line 309: decreased (check writing).

Thanks for this remark. It will be corrected in the new version of the manuscript.

Line 309: What is driving the yield reductions for maize: drought, heat, water or nutrient limitation? A shorter growing period or other weather impacts have a stronger impact for spring crops, as the growing season is short and impacts show a stringer effect.

Concerning maize, yield reductions are mainly related to heat, which shortens the growth phases and reduce the whole growing season. In agreement with the Reviewer's suggestion, we intend to point this out in the discussion part "4.1 Length of crop growing cycle".

Line 360: Higher productions

Thanks for this remark. It will be corrected in the new version of the manuscript.

Line 654: lower case 2

Thanks for this remark. It will be corrected in the new version of the manuscript.

Line 686-689: is this a realistic effect or a model artefact? I would expect increased emissions and leaching, as there is increased soil water content and water transport in the soil. Even though the nutrient up-take by plants is increased, I would still expect an increase of leaching and emissions. Only optimised systems would show a decrease.

In contrast to croplands, in grasslands nitrogen doses are inversely proportional to livestock density, so high nitrogen doses do not necessarily correspond to nitrogen surplus. Therefore, this causes a weak or inverse effect in relation to nitrate leaching and NH_3 emissions. We intend to point this out in the discussion part “4.2 Effect of climate on N_2O and CH_4 emissions” – “Nitrogen Use Efficiency (NUE)”, thanks for this remark.

Lines 707-710: Does the decline of NBP means stringer sink? Is this not affected by increased production due to rising temperature in an area with sufficient water availability?

Yes, the decline of NEP means a potential C storage. In Lines 707-710 we have confused "decrease" in the sense of absolute value, it is actually an “increase”, as can be clearly seen in Figure 10. We will correct the text in this sense. We thank Reviewer#2 for his meticulous reading of our text. Furthermore, Figure 11 reports the NEP over the European NUTS2 regions. In the central and in the north-eastern European, with mild heat impacts with climate perturbation, we have an increase of potential C stock (negative NEP) triggered by high crop productions. Furthermore, the supplementary material, Fig. S9, report the NEP over the EU regions for grasslands and croplands.

Line 779: agro-ecosystem

Thanks for this remark. It will be corrected in the new version of the manuscript.

Figure 1: please mention the spatial unit that is used here. Does each point represent a NUTS2 unit or a country?

Thanks for this remark. Each point is a year over the entire EU. This is corrected in the new version of the manuscript. A figure with the NUTS2 units is present in the Supplementary material (Fig. S2)

Figure 5: Please indicate in the legend delta N2O.

Thanks for this remark. It will be indicated in this and the other figures of the same shape (as you can see also in the figures reported above).

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