Ref.: Ms. bg-2021-241 Biogeosciences

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

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.

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 "*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*".

- 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 in chapter "2.2 Input data set" to define the boundary and the characteristics of these periods. Finally, this separation is discussed 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. S4). 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.

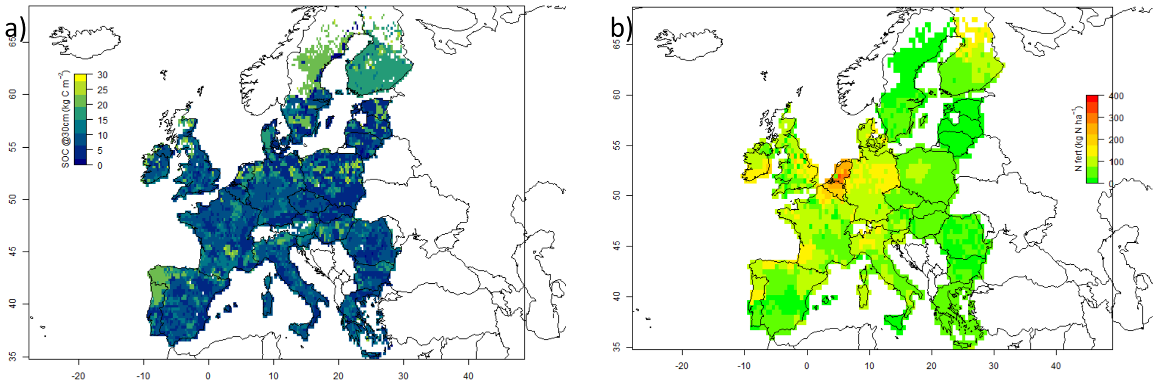
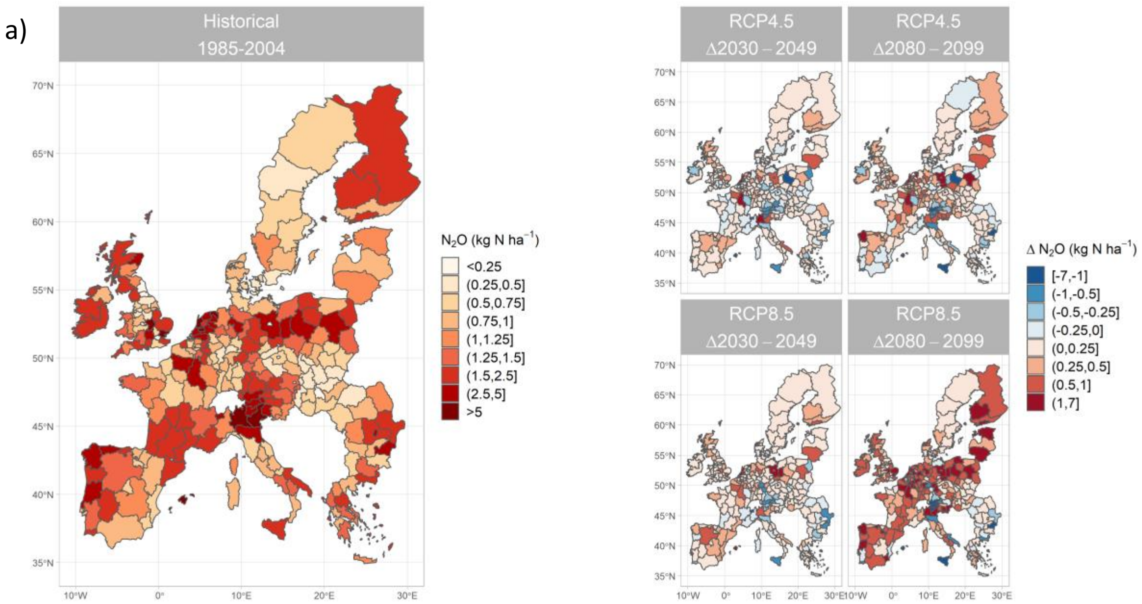


Figure R1. a) Soil organic carbon (SOC; kg C m^{-2} in the first 30 cm topsoil). b) Nitrogen application as fertiliser (organic + mineral) (kg N ha^{-1}) reported as the average 1980-2004.



b)

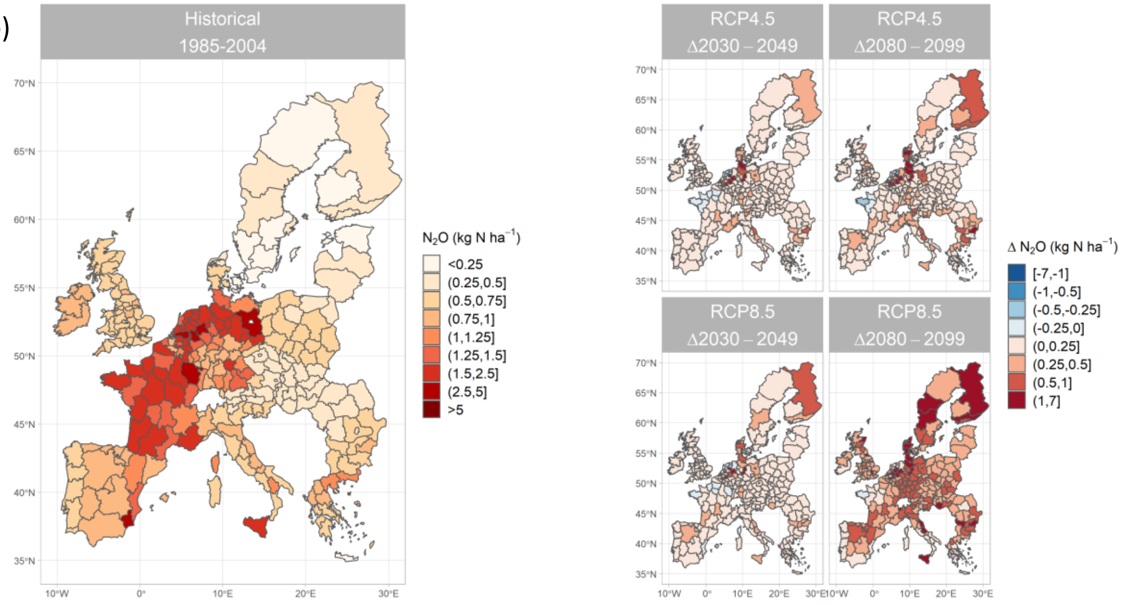


Fig S4. N_2O (kg N ha^{-1} y^{-1}) for croplands (a) and grasslands (b) in the European administrative borders (NUTS2). Emissions are reported for the historical period (1985-2004), and the difference "Δ" with the middle (2030-2049) and the end of the century (2075-2094) for the two climatic scenarios 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 1978 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. 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 generic starting point for each simulation unit, then run the model 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).

- 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). This sentence will be revised as follows "Residue 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".

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 paper, line 562-565 ["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"].

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 multiple models.

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?

Yes. As mentioned before, the initial carbon value for grasslands was calculated starting from 1901.

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. S7, 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.

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 in the figures reported here above).

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