The contribution of land-use change versus climate variability to the 1940s CO2 plateau: Former Soviet Union as a test case
Bastos et al. *Biogeosciences*

Response to Referee #2 (Editor's comment #2)

Thanks for your heavy elaboration and clarification of the certain parts in the manuscript. This helped me a lot to understand your paper. Let me shortly go over your major answers and point out the remaining critical aspects of your analysis and data you used, which I suggest improving and clarifying if you plan resubmit your paper to Biogesciences or to elsewhere. I have been noticed, your manuscript got already rejected for right now, but I still feel my second comments could help improving the manuscript.

The manuscript has been proposed for reconsideration after major revisions, to be submitted by December 4, which we are carefully preparing, addressing the previous reviewers' comments. We thank the reviewer for taking the time to provide additional comments, even if we find it rather surprising to receive a second review, without having been given the opportunity to submit a revision of the manuscript. Nevertheless, in the revision we will submit, we have addressed the main points raised also here, to which we provide a point-by-point reply below.

RC1: 1. You state, you are using state-of-the-art models and dataset to evaluate contribution of abandonment during WWII into CO2 plateau. I am more on the applied side of DVGM and other methods to estimate SOC dynamic due to LULCC, and this reflects, why I am so picky about utilized LULCC datasets. But let me add five cents on global DVGM and utilized datasets, such as HYDE 3.1. for LULCC modeling and carbon dynamic. Few words about HYDE. As you correctly stated in your manuscript, HYDE 3.1 FAOSAT data (cropland area) at the national level is implemented at country level. Cropland area does not track fodder crops, which is relevant for FSU. Thus, it does not match up data from Lyuri et al.,they used sown areas, which include fodder crops. It would be best to use here consistently the same source of information, such as stats from GOSKOMSTAT/ ROSSTAT, other official sources of FSU statistics on sown data.

AR: As mentioned in the previous reply to (AR16), we do not rely on Lyuri et al. (2010) to produce our dataset, because Lyuri et al. (2010) does not provide any values. The book includes though one figure (2.28) showing the evolution of cropland in the FSU that allows estimating (visually) a decrease in cropland extent of ca. 25Mha. As mentioned before, our data was collected from the official national statistics mentioned by the reviewer and has been organized in a consistent way by aggregating the different crop type categories reported in GOSKOMSTAT reports. Indeed, as written in lines 6-7, page 6, we do mention that our data includes fodder crops:

> "*The total agricultural area is divided into regional values when available and includes winter and spring crops, industrial crops and sown area for fodder.*"

At the same time, we compare our data with LUH/HYDE, which are (as any data) subject to uncertainty, but they are among the state-of-the-art datasets used in carbon and LUC modelling. Therefore, to the best of our knowledge, these are the most reliable and consistent sources of information about cropland evolution in fSU. We should note that FAO records are also based on the same national statistics (but they only compile them from 1961 onwards). In the revised version of the manuscript we improved the data description.

RC2: So, even if it is used widely for global studies (HYDE 3.1, K11), it does not necessarily mean the data is correct particularly for the regional studies, such as parts or entire FSU. For this reason, there were several studies, which tracked recently SOC due to LULCC for different parts of FSU, and then utilized, when it is possible, the official statistics at province level across different parts of FSU (Vuichard et al. 2008 http://doi.wiley.com/10.1029/2008GB003212, Kuemmerle et al. 2015 http://doi.wiley.com/10.1111/gcb.12897, Schierhorn et al. 2013 http://dx.doi.org/10.1002/2013GB004654). Regarding, your modeling efforts, I did not find any contrasts with regional studies, which utilized DVGMs for your study area.

AR: The two studies pointed by the referee are an excellent terms of comparison with our study, since they both used DGVMs to track changes in C-stocks following abandonment. Like in our work, both studies cited rely on national statistics to create a spatially-explicit dataset to force their models. In order to do so, they rely on prior information about geographical distribution of cropland, in Vuichard et al. using the maps from from Hurtt et al. (2006) ("*The shrinking total cropland area is distributed using the spatial land use pattern calculated by the global modeling study of Hurtt et al. [2006] which accounts for the marginality of land in the abandonment process.*"), and in Schierhorn et al, by combining land-cover and satellite maps, with statistics of sown area ("*This procedure is a combination of satellite-based global land cover data sets, namely, Global Land Cover 2000 (GLC2000) [Bartholomé and Belward, 2005], MODIS Land Cover [Friedl et al., 2002], and GlobCover [Bicheron et al., 2008], and subnational statistics on sown area […]*"). In fact, our method is analogous to the method used in Vuichard et al., only an updated version of spatial land use pattern was used, i.e. based on Hurtt et al. (2011), instead of Hurtt et al., (2006). Therefore, we are confident that our production of the spatially-explicit maps from the national statistics and our model simulations follows commonly accepted procedures in comparable land-use modelling studies. We will, nevertheless, introduce a more detailed description of the methods used to produce the land-cover map used to force ORCHIDEE-MICT simulations.

RC3: Unfortunately, often validation of the outputs of DVGMs is neglected produced numbers on SOC sequestration or release may vary quite a lot. As reviewer #1 also pointed out, you did not present and did not elaborate enough on validation of your model for such large region as FSU.

AR: We would like to point out that an earlier version of the ORCHIDEE model has been used by Vuichard et al. (2008) in their modelling exercise for C-stock changes in Russia following the collapse of the Soviet Union. We agree that it is worth introducing a comparison of our results and other estimates of C-stock changes following land abandonment. Therefore, we will add a Table comparing post-abandonment changes in C sequestration rates in the 1990s-2000s in the territory of the fSU provided by the review of Kurganova et al. (2014), which is reproduced below:

Table 3 Estimations of total carbon sequestration in former arable lands of Russia

Period	Area (м ha)	Approach	Total C sequestration (Tg C)	Average rate of C sequestration (Mg C ha^{-1} yr^{-1})	Reference
1990–2011	45.5*	Soil-GIS	870 (254)*	0.92 (0.28)	Present study
1990–2011	45.5*	Approximation	861 (646)*	0.96 (0.72)	Present study
1990–2006	30.2	Soil GIS	648 (47)	1.26 (0.09)	Kurganova *et al.*, 2010;
1990–2006	30.2	Approximation	585 (33)	1.14 (0.06)	Kurganova *et al.*, 2010;
1990–2005	27.9	RothC model	248 (37)	0.55 (0.08)	Romanovskaya, 2008;
1991–2000	20.0	Orchidee model	64	0.47	Vuichard *et al.*, 2008; **
1990–2004	34.0	Approximation	660	1.29	Larionova *et al.*, 2003

We would further like to note that Table 2 of the manuscript does provide a comparison of modelled C-stocks and fluxes with observation based datasets for the late 20th century, which is intended to evaluate the results of ORCHIDEE-MICT simulations.

RC4: 2. Now let me reflect on very important dataset you used to reconstruct cropland decline during WWII across FSU, namely reconstruction of sown area statistics. Thanks for clarification that you utilized Nove, Linz, Sapir, Davies for cross-reference and Maddison. I personally feel, you can delete this references and unnecessary text for few reasons. With the exception of few people, very few could easily access the stats from these books (I managed to download only work of Linz and just preordered the book of Nove). You could provide some permanent links to few key pages with data and key references to this data from Nove et al. I do not think, in this case, Nove publishing house would object. More importantly though, the sources of statistics, Nove and you utilized maybe the same, but not necessarily absolutely correct. There is no reason to argue that sown statistics from Goskomstat is better compared to FAOSTAT, so I would downplay spending so much time on data from other questionable sources (here I am asking why not then to use entirely stats on Gross Regional product and population from Soviet stats consistently?).

AR: We have restructured the text in order to be clear that we do not use these data to construct our cropland area dataset. They are only now mainly used to provide a rationale about the reasons to focus on the FSU and to complement the discussion. Although we understand the reviewers concern with the fact that we use data from books, many other studies rely on statistics that are only available in books (e.g. Klein Goldewijk et al. (2011)). To the best of our knowledge, it is not common practice to provide reproductions of book pages as suggested by the reviewer, most likely because it is not compatible with copy rights. We would like to point out that many public and university libraries may have copies of the books for open consultation.

We do not find in the manuscript any claim that Goskomstat is better than FAOSTAT. This is actually impossible, because FAOSTAT is available since 1961 onwards (as mentioned in the manuscript) and we use data up to 1961. In fact, this is precisely why from 1961 onwards we keep LUH/HYDE values (themselves partly based on FAOSTAT). What we compare are the values prior to 1961 – which do not rely on FAO data, but on an extrapolation of total population for each country – with the data collected in this work.

RC5: But let me now shortly write down why statistics you utilized on abandoned land (declined crops) in your study requires additional elaboration or modification. I managed to find and download the book you used. I am providing the link.
https://drive.google.com/drive/folders/0Bzi0kSKsuOdgeFUzSFhraGFabWM?usp=sharing You utilized the following statistics Goskomstat SSSR." These numbers were collected from: National economy of the USSR in the World War II (1941-1945). (Statistical Digest), Chapter 13: Agriculture (pp. 83-92). Goskomstat USSR, Information and Publishing Center, Moscow, 235 pp., 1990. (Narodnoe xozyajstvo SSSR v Velikoj

Otechestvennoj vojne 1941-1945 gg. (Statisticheskij sbornik)., Goskomstat SSSR., Glava 13: Selskoe xozyajstvo (str. 83-92), Informacionno-Izdatelskij Centr, Moskva, 235 s., 1990), (in Russian). Even though there is a gap of two years in 1941 and 1942, it is evident that the decrease in cropland area was not all located in the occupied territory." If to look at the publication, this is minor, you provided wrong set of pages, this is surely minor (pp. 83-103). The most important though, the years for which there has been reported sown area statistics for the occupied area, namely 1940, 1943, 1944, 1945. By 1943 (and here we do not know if it was assessed for January 1st or by December 1943) there has been reported 23.1 Mha. However, by this time only a fraction of occupied area has been freed from Nazis. This means, Soviets could report only for those lands, which were able to control. I fully agree, a portion of abandoned lands on liberated territories could be abandoned (we also do not when abandonment actually started in 1941 or 1942). So only by 1945 the largest territory of FSU (including the Baltics) has been freed and here we confidentially by taking a difference between 1941 and 1945 to be on conservative safe side. Here we have then 19 Mha. It is much less than 64 Mha, but you can ensure avoiding issues with a lack of information for uncontrolled territories. Also a large portion of abandoned lands was actually abandoned for a year or two. This was also a reason, why I suggested to spatially differentiate and account in your models, where and when abandonment occurred. But I did not find any such spatial adjustment for your regional study. I became further intrigued how Soviets could collect stats on occupied territories and decided to look at the definitions (p.4 National economy of the USSR in the World War II (1941-1945).). Here in Predislovie in Russian it says: Â´nÐš Ñ ¸AÐ´sÐ¿ÑĂÐ¡ÐÿÐžÐ¸tÐ£Ñ˘AÐÿÐšÐ¸tÐt'Ð¸tÐ¡Ñ´N Ðt'Ð˘rÐ¡Ð¡Ñ´NÐ¸t Ð ˚uÐ˘r 1940 Ðÿ 1943-1945 Ð ¸sÐ ¸s.Ð£Ð¿Ñ˘AÐ˘rÐ ´zÐ¿Ð¡Ð˘rÐij,Ð£Ð¿Ðt'ÐšÐ¸tÑ˘AÐ ¸sÑ´LÐÿÐijÑ ¸AÑ´RÐ¿ÐžÐžÑ˘CÐ£Ð˘rÑ ¸EÐÿÐÿ.Ð §Ñ˘AÐÿÑÑ´CÐ¿ÐijÐ ˚uÐ˘rÐžÐ˘rÐ ˝uÐt'Ñ´NÐ ´zÐÿÐ ˚uÐšÐ¿Ð¸tÐ¡Ð¡Ñ´NÑ˘EÐ˙zÐ¸tÑ ´C (1943-1945 Ð ¸sÐ ¸s.)Ð£Ñ ˘AÐÿÐšÐ¸tÐt'Ð¸tÐ¡Ñ ´N Ðt'Ð˘rÐ¡Ð¡Ñ´NÐ¸t Ð£Ð¿Ð¿Ð´sÐ˙zÐ˘rÑ ¸AÑ´CÑ´RÐijÐÿÑ˘AÐ˘rÐ ´zÐ¿Ð¡Ð˘rÐij,ÐžÐ¿Ñ´CÐ¿Ñ˘AÑ´NÐ¸tÐšÑ´CÐ¸tÐžÑ˘CÑL'Ð¸tÐi j ÐÿÐ˙zÐÿ Ð£Ñ ˘AÐ¸tÐt'Ñ´NÐt'Ñ˘CÑL'ÐÿÑ˘E Ð ¸sÐ¿Ðt'Ð˘rÑ˘E Ð´sÑ´NÐ˙zÐÿ Ð¿Ñ ¸AÐšÐ¿Ð´sÐ¿Ð ˝uÐt'Ð¸tÐ¡Ñ´N Ð¿Ñ´C Ð¿ÐžÐžÑ˘CÐ£Ð˘rÑ ¸EÐÿÐÿ.Ð˚UÐ˘r 1940Ð ¸sÐ¿Ðt' Ðš ÑÑ´CÐÿÑ˘E Ñ´CÐ˘rÐ´sÐ˙zÐÿÑ ¸EÐ˘rÑ˘E Ð´sÑ´NÐ˙zÐÿ Ð£Ñ˘AÐÿÐšÐ¸tÐt'Ð¸tÐ¡Ñ´NÐt'Ð¿ÐšÐ¿Ð¸tÐ¡Ð¡Ñ´NÐ¸t Ðt'Ð˘rÐ¡Ð¡Ñ´NÐ¸t Ð£Ð¿ Ñ ´CÐ¸tÑ ˘AÑ ˘AÐÿÑ ´CÐ¿Ñ˘AÐÿÐÿ Ð¡Ð˘rÐijÐ¿ÐijÐ¸tÐ¡Ñ´C Ð¸tÐ¸t ÐijÐ˘rÐžÑ ¸AÐÿÐijÐ˘rÐ˙zÑ˘NÐ¡Ð¿Ð ´z Ð¿ÐžÐžÑ˘CÐ£Ð˘rÑ ¸EÐÿÐÿ Ðš1941- 1942 Ð ¸sÐ ¸s.)Â˙z. I am translating this. Â´nIn the compendium there have been provided numbers for 1940 and 1943-1945 for the areas(can be also interpreted-districts or rayons), which suffered from the occupation. At the same time, for each of the war years (1943-1945) there have been provided the numbers for oblasts and areas (can be also interpreted-districts or rayons), which in the current and preceeding years were liberated from the occupation. For 1940 in tables there have been provided prior the war data for the territory of the maximum occupation in 1941- 1942Â˙z. This confirms my assumption that stats you used is representative for freed territory by 1943 but not for entire occupied area, however, we do not know exactly if this was reported by jan 1, 1943 or December 31, 1943 Again, to regionally fine tune your study, I recommend to account for the spatial location of the occupation and a fraction of the potentially occupied area. You may consider to reconstruct land use at province (oblast) level (this stats is available), and then make plausible projections on abandonment for occupied provinces for eahc specific year. Solely relying on country level data complemented with global data, is not sufficient to disentangle regional spatially differentied processes. Here are two examples of the maps on the advancement and retreat of Nazis http://press.princeton.edu/chapters/haywood/s5_9519.pdf https://en.wikipedia.org/wiki/Eastern_Front_(World_War_II)#/media/File:Eastern_Front_1943-08_to_1944-12.png

AR: We agree with the referee that there is uncertainty regarding the numbers referring to occupied area, which we will mention in the revised version of the manuscript. However, to the best of our knowledge the cropland area for the regions occupied between 1941-42 was not collected (or at least not reported) at the time. Any projection of hypothetical abandonment scenarios will, in our view, only contribute to increase uncertainty rather than reducing it.

Furthermore, the reviewer mentions that GOSKOMSTAT and the book cited provide cropland area at oblast level for this period. However, we were not able to find any reference to such data in GOSKOMSTAT. In the book mentioned by the referee, a list of oblasts is indeed provided, but no values of cropland per oblast are mentioned. Regarding the data for 1943, the data provided is for the full year, which usually is referred to growing season period in the case of FSU. In any case, for the point of this study, we would like to point out that FSU-REF (LUH/HYDE) also does not include any kind of information about occupied versus non-occupied regions.

Even though there are illustrative figures about the area roughly occupied, we could not find (we did search in the preliminary stages of this work) a geo-referenced map of the areas under occupation for different years of WWII. Even if we chose to present the results with the more simplistic rule of removing cropland proportionally to the fraction of crop in each pixel (as described now in more detail in the revised paper), this does not mean we did not carefully evaluate whether our resulting land-abandonment during the war period was consistent with the changes reported in the cited literature (namely the strongest reduction in occupied/front regions). We show the per-pixel reduction in cropland fraction between 1940 and 1942 in the following figure, and compare with one of the figures suggested by the reviewer:

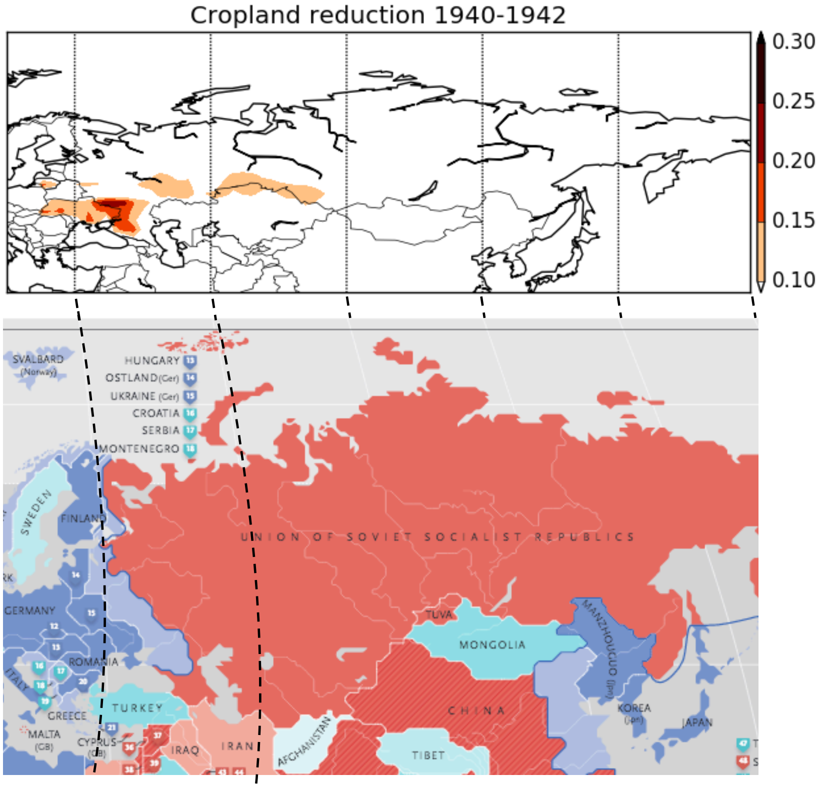

Figure: Geographical distribution of changes in pixel fraction of cropland (positive means cropland loss) between 1940 and 1942.

The figure shows that the strong cropland area decrease in our dataset occurs mainly in occupied regions, but especially in the regions corresponding to the war front. Since the resulting pattern was consistent with the literature cited and provided a better fit to region-level statistics (Figure A2), this dataset was found appropriate for the purpose of our study (again, keep in mind we want to compare with LUH/HYDE, which does not have any wartime LUC signal). We did, however, make some preliminary tests in which the reduction in crop area occurred mainly in the occupied/front region, as shown in the figure below.

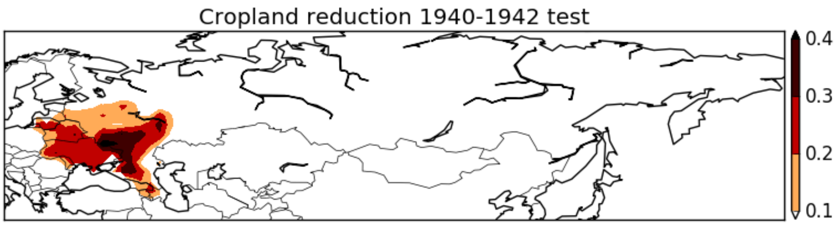

Figure: Cropland reduction between 1940-1942

The simulation in which abandoned areas area replaced by grassland PFTs (using the same procedure as for the other simulations) resulted in an additional sink from ELUC of 0.08PgC/yr, instead of 0.07 PgC/yr in S_{GRA}. This relatively small difference does not hamper our conclusion: that land-abandonment during WWII could contribute only a small fraction of the gap sink required to explain the plateau, as compared to natural climate variability.

RC6: 3. 13 Mha of cropland expansion for USA it is a large number as for any country too, including FSU. Taking into account large uncertainties with abandonment during WWII and more realistic 19Mha, this number has to be taken into account. I would still heavily elaborate and contrast with other contributions to CO2 emissions, since AFOLU represents roughly 21 from total anthropogenic emissions of CO2. Reviewer 1, correctly pointed out, the importance to account for fires, burning, heavy extraction or forests by Nazis on occupied areas (e.g., Smolensk region). You take a hard task –to deal with large uncertainties with the numbers/ data you use and modeling approach, and you need to account for factors, which may balance out CO2 sink, in order to trust your numbers.
AR: We understand the reviewer's point about the fact that other regions might present different LUC trajectories that might offset/reinforce the responses we find in the FSU. However, the goal of this paper is not to discuss global patterns of LUC during the WWII, but to compare – using FSU as a test region – the potential contribution of the two

processes proposed by Bastos et al. (2016) to an enhanced terrestrial sink during the 1940s. The other contributions of CO_2 emissions (and sinks) at global scale have been thoroughly discussed in Bastos et al. (2016) and are therefore out of the scope of this paper. Still, we would like to note that in the 1940s, LUC contributed about as much as fossil fuel burning to anthropogenic CO_2 emissions, and with much larger uncertainty (about 1.5PgC/yr, please see figure 2 in Bastos et al. (2016)). The large uncertainty in LUC reconstructions is thus, not particular to our study, but a problem inherent to LUC data collection, terminology and assumptions (see e.g. Gasser and Ciais (2013) or Pongratz et al. (2014)). Our perspective is that it is best to base our analysis on the reliable and traceable data we can collect (as we did in this study), rather than adding further assumptions about village burning, bombing, or forest extraction by Nazis.

RC7: 4. As you pointed out, and I reread your modeling approach, it would be best to avoid wording such as immediate forest regrowth, afforestation, rather establishment of seeds, shrubs regrowth. If you will spatially differentiate occupied lands, where occupation occurred, you will notice, a large portion of lands experienced SOC loss (thanks of for your figure). Even it has been occupied a large portion of temperate and northern regions, contribute probably not that much regarding cropland extent, compared to forest-steppe and southern regions (thanks for explanatory figure on spatially differentiated C pools and sinks).

AR: We have corrected the manuscript accordingly to explain how post-abandonment succession is modelled in ORCHIDEE-MICT. We now also include also a comparison with reference works on C-stock changes in Russia or FSU territory (see reply to RC3).

Additional remarks.

RC8: L.15 p6. Howe sensitive your model to this threshold 0.85. how important this number compared to many other assumptions?

AR: The threshold 0.85 represents the fraction of aboveground NPP that is harvested, and therefore is not transferred to the soil through litter. This value affects crop harvest productivity (together with many other parameters, e.g. Vcmax) and emissions when land-use change occurs. The resulting low C input from crops to soil may lead to overestimates of C soil gain following abandonment if this value is too high. Because the resulting C-stocks and LUC emissions result from a combination of several parameters, including this one, we compare our results with reference values of C-stocks and fluxes in the observational record (Table 2), with crop harvest estimates from economic records (Figure 3) and include in the revised version of the manuscript a Table comparing our estimates to C-sequestration

Table A1. Is it based on field data? How do these numbers vary across the study area? Some additional information on these numbers would be helpful.

AR: We believe the reviewer refers to the Vcmax values. As in most DGVM simulations, Vcmax values do not vary regionally and are PFT-dependent (see Krinner et al., 2005; Zhu et al., 2015). In our simulations, and analogous to Vuichard et al. (2008), we use somewhat modified parameters for crops and grasslands as compared to the standard values used in Zhu et al. (2015) and Guimberteau et al. (2017). This is the reason why, for transparency, we present the values used in Table A1.

Figure 1. I would just stick to your major storyline and will retain only FSU-Ref and FSE-New. Too much unnecessary details, with most likely, repeatable and questionable data.

AR: We agree that Figure 1 has too much information and thus now only present FSU-REF and FSU-New together with the population statistics relevant for the discussion about LUH/HYDE characteristics.

To sum up, the storyline on wars and catastrophes and any socio-economic and environment shocks regarding land use and C dynamic is interesting and hot. However, the data you used (primarily data) and some assumptions right now downplay the validity of your story and claims. This certainly makes for right now feeling contribution of land abandonment to explain the plateau is dubious. However, surely, any large scale abandonment represents a certain C sink, but how other factors may counterbalance such sink, have to be accounted as well. Nevertheless, I pointed the options to improve the manuscript to address raised issues well and to make your findings stronger and more trustful.

AR: We agree that the LUC impacts on C-sticks following major socio-economic events is a hot topic, but we would like to call attention for the other relevant aspect of this work, that relates to the impact of climate variability in high-latitudes in terrestrial CO_2 uptake. As Kurganova et al. (2014) thoroughly summarized for the post-1990s land abandonment in FSU, C-stock change estimates are likely to vary considerably depending on the methods used. Also, as many works show (already cited here), emissions from LUC are subject to multiple sources of uncertainty, being one of the least constrained terms of the global carbon budget. The dataset we collected here is provided by the same sources used in FAOSTAT (and thus LUH/HYDE) after 1961. Our work was to collect and harmonize the data for FSU, to produce an annually-resolved, spatially-explicit, dataset of cropland area variations in FSU over the period prior to FAOSTAT, following standard methods used in works also cited by the reviewer. Here, we discuss how using regional LU data that incorporates information about drastic effects of socio-economic crises might result in significantly different ELUC estimates (up to 70% difference in the 1940s). We chose the FSU because its territory encompasses a very large terrestrial sink, a considerable fraction of soil C-stocks, and because it experienced at the same time the two processes that Bastos et al. (2016) hypothesised could contribute to the plateau – major social and economic changes during the early 20th century, and particularly in WWII, as well as high latitude warming during the late 1930s to late 1940s (Overland et al., 2004). Our intention with this study is not to have a final word about the LU changes in FSU during WWII, nor do we claim that our results indisputably show that LUC explains the plateau. Rather, our goal was to weight the possible contributions of both LU and climate variability to an increased terrestrial sink during the plateau period. Indeed, we find that large-scale high-latitude warming might be a better candidate to explain a potential enhancement of the terrestrial sink. We are confident that the dataset collected is relevant for the LU community and that our approach and model used are among the state-of-the-art methods used by the community and are therefore, scientifically valid.

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