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Interactive comment on "The contribution of land-use change versus climate variability to the 1940s CO_2 plateau: Former Soviet Union as a test case" *by* Ana Bastos et al.

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The authors thank the referee for accepting to review our manuscript. In response to the referee, we would like to stress some points mentioned in the manuscript that the referee might have overlooked (we acknowledge we can and will make them clearer in a future revision), and to make some notes regarding what is possible to evaluate with the model, and what is not. Please find below a point-by-point reply to each of the comments raised by the referee (attached a PDF with more legible text and the figures organized along the text):

RC1: Some confusion caused by the statement of the research prob-

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lem. Right, CO_2 concentration was not increasing during 1941-1945(46), however since 1947 steady constant growth already started (see NASA data https://data.giss.nasa.gov/modelforce/ghgases/Fig1A.ext.txt). But within the same period and up to the 1950-1960s the global average temperature didn't grow either. So, there is no any contradiction here. It is known that at cooler conditions CO_2 dissolution in the ocean is increasing. AC1: We would like to call the referee attention to the fact that the research problem in this paper is not to consolidate that there is a missing sink. This has been thoroughly addressed in the companion paper Bastos et al. (2016), which is cited in the second sentence of the abstract (and profusely throughout the manuscript). The goal of this work is to evaluate the contribution of two of the processes hypothesized by Bastos et al. (2016) to explain the missing sink. Nonetheless, we would like to stress that the issue is more complex than a simple increased dissolution effect as the referee implies, as discussed deeply in the companion paper, and summarized below. In fact, to the best of our knowledge, no work has proposed a global-temperature effect on the atmospheric CO_2 stabilization during this period. On the contrary, several works (as discussed in Bastos et al., 2016) have tried to evaluate the robustness of this stabilization and attribute it to the ocean and terrestrial sinks, but they reach very different conclusions depending on the methods and assumptions used to calculate atmospheric CO_2 growth rate (AGR) from CO_2 concentration measurements (Joos and Bruno, 1998; Joos et al., 1999; Trudinger et al., 2002). Secondly, air trapped in the bubbles within an ice-core core corresponds to a mix of air with different ages, and therefore CO_2 concentration in a giver year will be a mix of somewhat older and somewhat younger air, with the age mix being defined by the rate of accumulation of snow in a given place. Since measurements from different ice-cores are scattered in time (and space) and in order to produce one single dataset as the one that the referee indicates (which is consistent with the one in Bastos et al. (2016)) a spline needs to be fit and therefore, assumptions need to be made about the age effects and the uncertainty of the measurements (for a thorough discussion see Trudinger et al., 2002). Actually, Bastos et al. (2016) and many previous works (e.g.

Enting et al., (2006)) have addressed this problem thoroughly, for example as shown in Fig. S1 in Bastos et al. (2016), shown in Fig. 1. Because of age-mixing effects, it is not fully correct to assume (as the referee proposes) that the end of the stabilization period is in 1947, since the difference between 1946 and 1947 is only 0.1ppm (310.3 to 310.4). In fact, Bastos et al. (2016) have addressed the issue of the robust detection of the plateau timing (and you may note that they also discuss the timing proposed by previous works) by performing a structural-change analysis of the CO2 concentration trends (to exclude non-significant variations), and identify 1940 and 1950 as significant trend-break points (figure 2 here, figure S2 in Bastos et al. (2016)). Finally, as Bastos et al. (2016) demonstrate using simulations from Earth System Models, the ocean response only is not likely to produce a sink that equals CO2 emissions in that period. The referee may note that this is mentioned right in the abstract: “Their study indicates that even considering an enhancement of the ocean sink, still a gap sink of 0.4-1.5PgC.yr-1 in terrestrial ecosystems is needed to explain the CO2 stabilization.”

RC2: As for the average temperature – please, see attached Fig.1 Upper line – average temperature of Russia, lower – world temperature. As we could see, during 1940-1950s in Russia the decrease of the average temperature was more than for the world. AC2: The authors would be pleased to see the axis on the figures, otherwise it is not easy to identify the periods of warming and cooling. Nonetheless, the authors believe the referee may be mixing the concepts of temperature anomaly (warmer/cooler than reference conditions) as discussed in our manuscript and temperature trends. The period 1940-1950 was characterized by a warm anomaly relative to the previous decades (1901-1930) both globally and in the Northern Hemisphere, as can be seen in the HadCRUT4 dataset (see figure 3)

Indeed, in our manuscript, we also show that warm temperature anomalies at decadal time-scale were observed over the FSU territory, which justifies our study focused on this region of the Northern Hemisphere. Please note the color shading in the background of Figure 1a (reproduced here as Figure 4), showing temperature anomalies

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relative to the period 1901-1930 (red/orange warm anomalies, blue cold anomalies):

Indeed, the peak of the temperature anomaly seems to occur around 1938, with temperature decreasing over the following years, but nevertheless, the decadal temperature anomaly is indeed positive. This is relevant because vegetation will respond to the temperature anomalies more than the sign of the trend: e.g. the onset of the growing season is more likely to depend on whether the spring temperature is above a given threshold, than on whether one year is slightly cooler than the previous (provided T is still above the threshold). We would, therefore, like to stress that in our manuscript we analyze temperature anomalies, and not temperature trends.

RC3: Looks strange that as sources for national soviet statistics authors used foreign publications of (Nove, 1982; Sapir, 1989; Davies et al, 1994) instead of original statistical data. Such data should be available in the State archive of the Russian Federation. It might be recommended to the authors to check original data of the Central statistical office of the USSR and Central administration of national economic statistics of Gosplan of USSR. Without that it is difficult to judge on the accuracy of the activity data. AC3: As mentioned in Section 2.1.1 (Russian and Soviet crop area), our new dataset of FSU agriculture area is collected from official national FSU statistics from Lyuri et al. (2010). National statistics were provided for the Former Soviet Union (FSU) during the period 1917-1961, and the Russian Empire during the pre-Soviet period starting from 1913 (dataset and full list of references may be found in Supplementary Data). The referee may revisit the Supplementary Material, where we present the full list of references (22 total) from where the data was collected, all from Soviet and Russian Empire statistics, including the Gosplan of USSR. The referee may also note that the international works cited are important references in the study of Soviet economy and that their studies were also based on soviet statistics (as the referee may find in their manuscripts). The reason to include such studies is precisely to show that, even if their values are somewhat different, they agree better with our newly collected dataset than with the LUH/HYDE (which is also based on international data). Given the high uncertainty in evaluating changes in land-use in the past (which is discussed in lines 18-29 in Page 3 of our manuscript), our perspective is that having alternative sources of data, rather than being a weakness, adds robustness to our study.

RC4: One of the crucial omissions of the authors could be their assumption on fire emissions. On the page 6 it is stated that "fire occurrence is simulated using the SPITFIRE fire model ... which is well calibrated to simulate boreal fires". ....???? Authors have not estimated any CO2 emissions from other burning processes during the WW2. It is well known that annual burned area of abandoned fields, forests, even villages and whole cities on the occupied territory should be large. Fire occurrence and burning of biomass within the front line should be even more essential. Disregard of the other sources for biomass burning leads to potential significant underestimation of CO2 emissions in the research. AC4: Regarding the ability of the model to simulate boreal fires, the referee may revisit the reference provided in the same sentence (Yue et al., 2014) and the model evaluation paper (Guimberteau et al., 2017). Here with the prognostic fire model our focus is to include the open biomass burning in the carbon balance accounting. Yue et al (2014) Figure 10 showed the simulated burned area agrees well with another data source based on national statistics on a decadal basis over the 20th century (see Figure 5). Guimberteau et al (2017) showed that our simulated fire carbon emissions over the boreal Asia agree well with the GFED4s data set based on satellite-derived burned area over 1997-2009, confirming well the model capacity to simulate open biomass burning. We agree with the referee that fire emissions in our work are likely underestimated because of human-made fires during war conditions not being modeled. However, given the difficulty of making studies so back to the past with so little information, we try to keep our work the least speculative possible and therefore fires are included in a "baseline" scenario. However, if the referee could point us to referenced datasets on annual burned area in the war front, we would gladly attempt to incorporate this in our analysis. Nevertheless, we acknowledge that further discussion should be added in the revised version of the manuscript about the possible underestimation of fire emissions in the current setup of the simulations.

RC5: There is no any assumption in the research on the disturbance of the natural vegetation during the war, as well as for abandoned fields. For sure, we should assume not only its regular burning, but also losing of the soil layer totally with craters from the bombs and trenches. As a difference between the war on the FSU territory and Europe – most territory left occupied after fighting on the front line in FSU was pitted with holes. These are additional omitted CO_2 emissions. AC5: The authors agree with the referee that is it very likely that war resulted in relevant damages to forests, grasslands and agricultural fields. However, as in the previous point, we do not have any reliable data that could allow us simulating such effects. Therefore, our analysis is focused on: (1) the difference in CO_2 emissions due to land-use change estimated resulting from the use of the LUH/HYDE dataset (a reference in LUC studies, e.g., Le Quéré et al. (2015), Hansis et al. (2015), Stocker et al. (2014)) and our newly collected dataset and; (2) the influence of the climate anomaly on ecosystem CO_2 uptake and soil carbon stocks. Please note that even if such information would be available, the correct way to assess the impacts of war on ecosystems would be to perform two factorial simulations (one with war effects, one without). Because we focus on differences between pairs of simulations, both excluding such potential effects, they do not influence the results of our factorial simulations. We will introduce a discussion on this issue in the revised version of the manuscript.

RC6: It is questionable the correctness of the modeling of revegetation process by the model. If there is any verification of the model ORCHIDEE-MICT for abandoned croplands on the territory of Russia – such literature sources should be included and the accuracy of such modeling should be discussed. Overwise the model could not be applied for such kind of research. AC6: The authors acknowledge that the manuscript should include a reference to previous estimates of the impact of land-abandonment in the territory of Russia, even if performed for different times in history. Indeed, Kurganova et al. (2014) has performed an intercomparison of estimates of changes in carbon stocks after land-abandonment following the collapse of collective farming in Russia. In their study (table in Figure 6) they include the results from Vuichard et

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al. (2008), which used a previous version of the ORCHIDEE model. The referee may note that this version of ORCHIDEE underestimated the rates of C sequestration, likely because the version of the model used lacked important soil processes which are included now in ORCHIDEE-MICT (a thorough description of the improvements to the model may be found in Guimbertau et al. (2017). Furthermore, as discussed in lines 7-12 in Page 12, our results for CO2 rates from vegetation regrowth in areas registering cropland decrease are consistent with values from literature. The authors will introduce some sentences regarding this point and the complete references in a revised version of the manuscript.

RC7: There are no data on the actual land use changes, only net cropland area change is available. However, the actual distribution of abandoned fields is crucial. Recent investigations of the process of revegetation on abandoned croplands in Russia (Romanovskaya, 2008 (in Russian)) show that croplands on the south (Rostovskaya obl., Krasnodarsky krai, Stavropolsky krai, the corresponding area of Ukraine) are losing soil C during first 5-7 years of the abandonment. During the WW2 all these territories were occupied and abandoned. Thus we should estimate emissions there, no sink. AC7: Indeed, our analysis starts from total cropland area change, however, as discussed in the Methods section 3.1, we produce spatially explicit annual maps, which are then used to force the model for changes in C pools and fluxes. As we explain in lines 24-28 in Page 6, in order to convert national total changes to pixel-level changes, we subtract the difference between FSU-REF and FSU-NEW proportionally to the fraction of cropland in each pixel. As an example, the map of the difference between the two datasets for the year 1940 is shown in the Figure 7 (from Reis, 2017), with number corresponding to fraction of pixel area:

“Indeed, most cropland change is located in the south-west. Nevertheless, the authors would like to point out that this issue is discussed in Section 5 (line 11-16, Page 11):

The method used here to update LUC maps does not account explicitly regional dynamics, such as the displacement of farmland from the front and occupied regions during WWII to the eastern countries (Linz, 1984). We deliberately did not account for this displacement, as it would imply changing also natural vegetation fractions in other regions of the FSU (e.g. require forest/grassland removal), and increase the possible inconsistencies between the datasets. Even though such differences may be relevant at local scale, they are unlikely to significantly change our results for the aggregated FSU. Our data still provides a better match to country-level estimates of crop area (Figure A3) than previous reconstructions."

As for the resulting sinks from abandoned croplands, Kurganova et al. (2014) have made an review of different estimates of post-abandonment CO2 exchanges in the Russian territory, and all studies indicate a total strong sink in the first 10-20 years after land-abandonment. This does not mean that in some regions, cropland abandonment might have led to a source. Indeed, our simulations indicate a small source resulting from land-abandonment in some southern regions (as shown in Figure 8), however this is largely offset by increased uptake in other regions.

Figure: Difference in NBP in 1940-1950 between the two factorial simulations SFOR and SRef. Values correspond to g.m2.decade-1.

RC8: It is not clear how revegetation into the forest was assumed in the modeling. During 5-7-10 even 15 years could not appear any forests on the abandoned fields. If we add to the assumptions regular burning of abandoned fields we could not estimate any C sink for young growing trees on that area. Thus, it could be potential overestimation of the C sink for that simulation as well. AC8: The assumptions and methods used to define each of the two scenarios (forest and grassland regrowth) are explained in Section 3.1 and 3.2, line 29 Page 6 to line 7 Page 7:

"Given that we have no additional information about forest or grassland changes in FSU during this period (apart from FSU–REF), we define two scenarios for the natural vegetation replacing crop area after abandonment. In the first one, crop area in each pixel is replaced by forest cover if forest is already present otherwise it is replaced by grassland (FOR). The second scenario (GRA) is similar, but with grasslands replacing abandoned cropland, if grassland is already present, and otherwise allocated to forests. It should be noted that these two cases correspond to the two extremes of the possible range of forest vs. grassland trajectories in regions where agricultural area was abandoned. The resulting total forest and grassland areas over FSU corresponding to each case are shown in Figures A2b, c.

3.2 Model simulations The information about forest and crop and grassland fractional cover and transitions from LUH1 was converted to 2x2 degree lat/lon maps of the 13 PFTs in ORCHIDEE-MICT consisting of bare soil, 8 forest PFTs, 2 crop PFTs (C3 and C4 crops) and 2 grass PFTs (C3 and C4 grass). We consider only the region corresponding to the FSU, as highlighted in the shaded areas in Figure A1. The average PFT distribution over the 20th century in the FSU region, is shown in Table A1"

After a decrease in crop fraction in a given pixel, the resulting change is attributed to a forest PFT already present in the pixel (or a grassland PFT in the grassland scenario). This does not mean that a fully grown forest immediately follows land-abandonment, but that trees slowly start to regrow in this area, following a normal growth curve as in most land-surface models. As in our responses to RC4 and RC5, we try to avoid as much as possible assumptions not able to be validated by referenced data, therefore we do not think it is advisable to introduce the assumption proposed by the referee. As for the fire occurrence over the young forests generated from land abandonment, the referee added the regular burning assumption and argued that recovery carbon sink might not be that much considering fire emissions. However as far as we know there is little evidence showing that young reclaimed forests are more likely to burn than naturally regenerating forests with similar ages. On the contrary, young forests from former agricultural land would have smaller ground fuel than otherwise a naturally regenerating forest, and therefore are less likely to burn. One exception indeed exists however, that is active fuel wood collection over these forests on abandoned land. But as we explained in RC4, we focused on open biomass burning which our model is capable of simulating. In general, both dendrochronological (Conard & Ivanova, 1997) and satellite-based studies (Giglio et al., 2013) indicate that Russian boreal forests are dominated by a fire return interval of 20–50 years up to 100 years, which is well captured by our model (Guimberteau et al., 2017, Figure 19). Based on this, we argue that the risk of underestimating fire emissions in recovery forests is low. We acknowledge, however, that one sentence about this issue may be added in the discussion in a revision of the manuscript.

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Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2017-267/bg-2017-267-AC1-supplement.pdf

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-267, 2017.

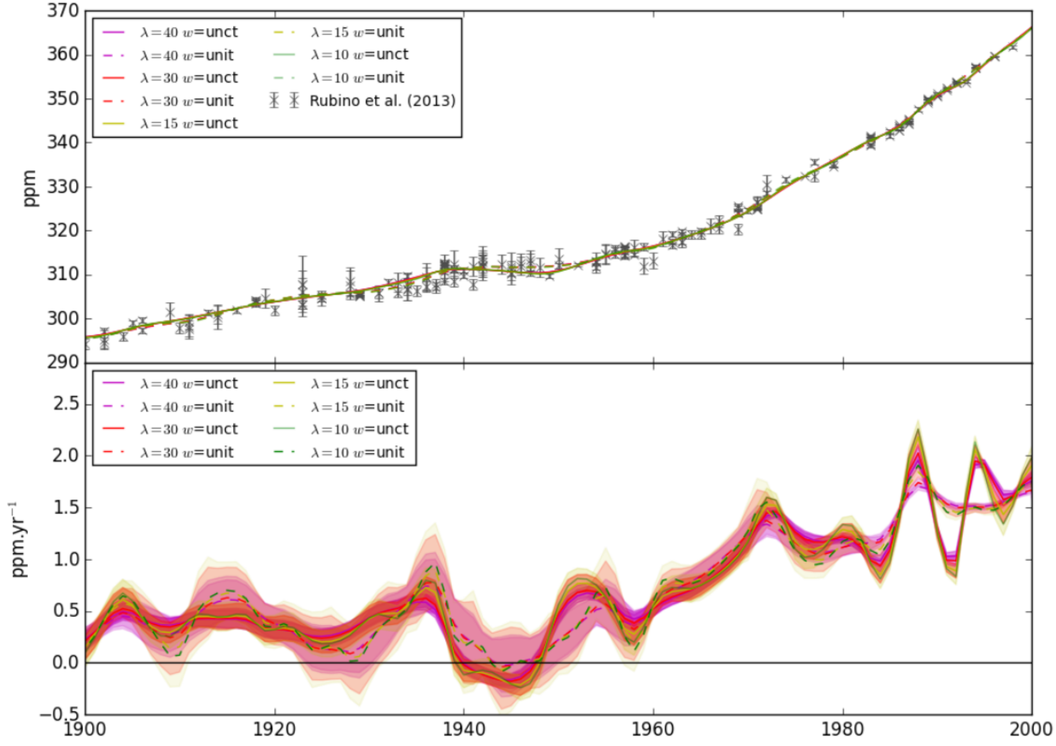

Fig. 1.

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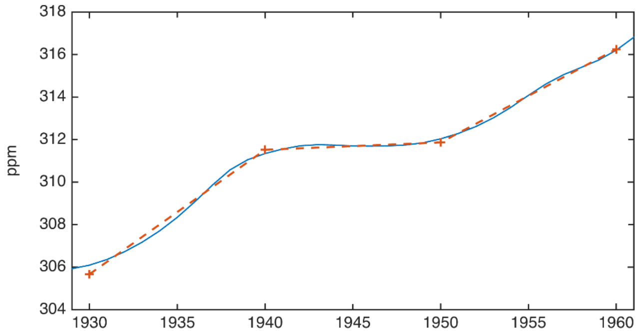

Figure S 2. Piecewise linear regression model fit (orange dashed lines) on the annual values of atmospheric CO_2 between 1930 and 1960 (blue solid line) calculated from the spline-fit shown in Fig. 1. The trend break-points are marked by +, and correspond to the years 1940 and 1950. During this period, atmospheric CO_2 does not present any significant trend.

Fig. 2.

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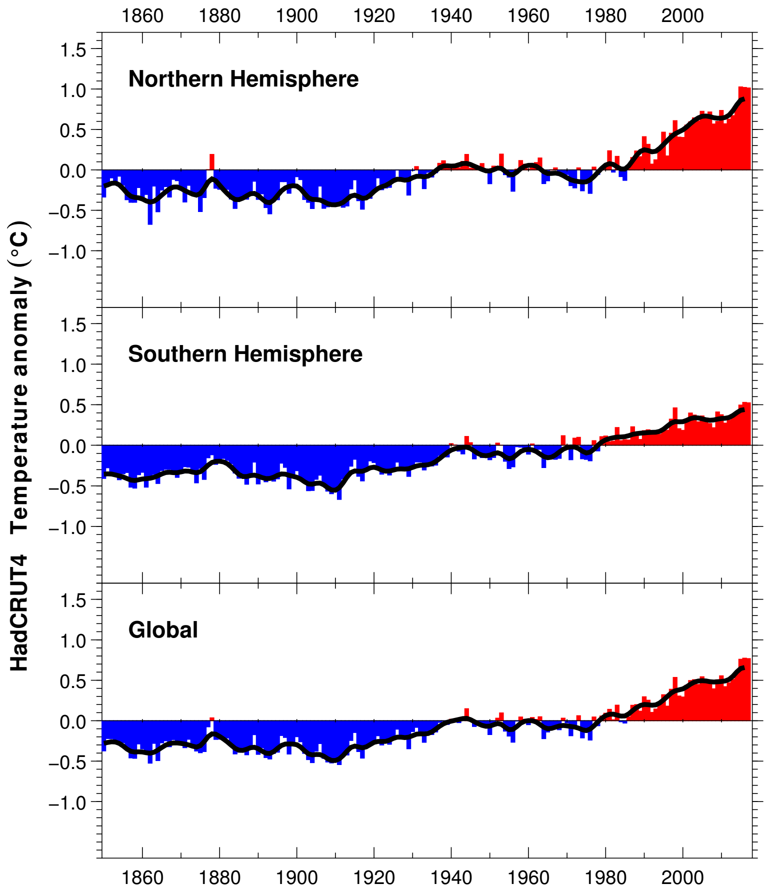
HadCRUT4 Temperature anomaly (°C)
Northern Hemisphere
Southern Hemisphere
Global
1860
1880
1900
1920
1940
1960
1980
2000
1.5
1.0
0.5
0.0
−0.5
−1.0

Fig. 3.

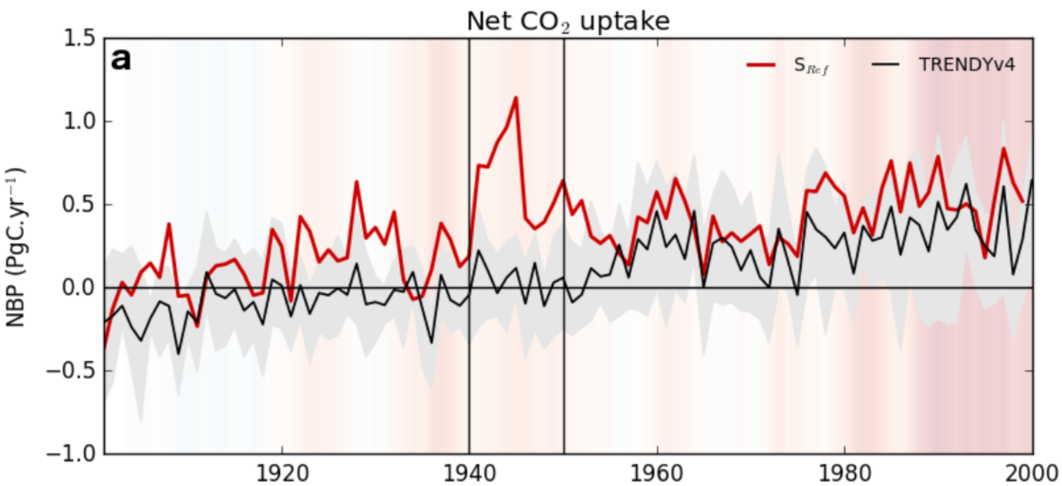

Fig. 4.

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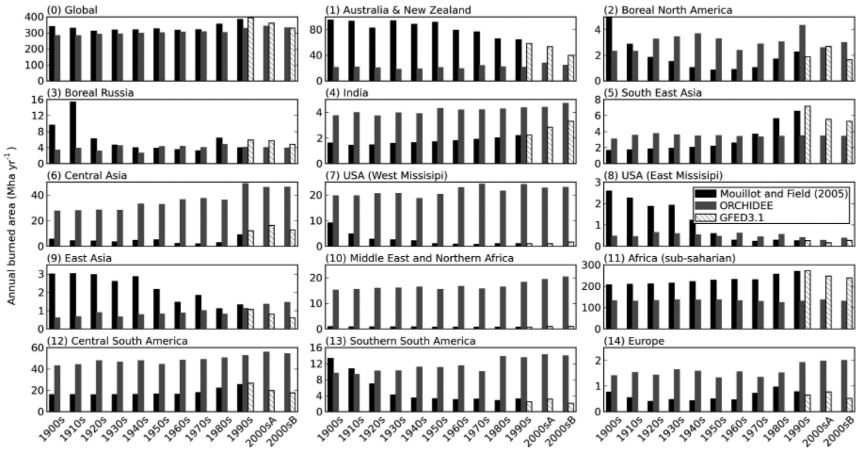

Figure 10. The annual burned area for 1901–2009 as simulated by ORCHIDEE (grey bar), reported by the Mouillot data (Mouillot and Field, 2005, black bar), and by GFED3.1 data (dashed white bar). Data are shown for the mean values over each decade for 1901–2000, and for 2001–2005 (2000 sA) and 2006–2009 (2000 sB). Refer to Sect. 2.4.1 for the correction of the Mouillot data by using GFED3.1 data.

Fig. 5.

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

Period	Area (M 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

The estimations are based on different approaches and were done for various periods and areas of abandoned arable lands due to LUC after 1990. The one-sigma uncertainties of total C sequestration and average C sequestration rate estimated in the present study are shown in parentheses. The two studies at the bottom did not provide any uncertainty of the estimations.
*The rates of C sequestration and areas of abandoned lands were used for calculation of total C accumulation during the first 20 years after LUC (1990–2009). For the period 2010–2011 the total area of abandoned lands was 45.1 M ha and the average rate of C-sequestration was 0.19 Mg C ha^{-1} yr^{-1}.
**This estimation included the whole area of former USSR based on FAO statistics for 2000.

Fig. 6.

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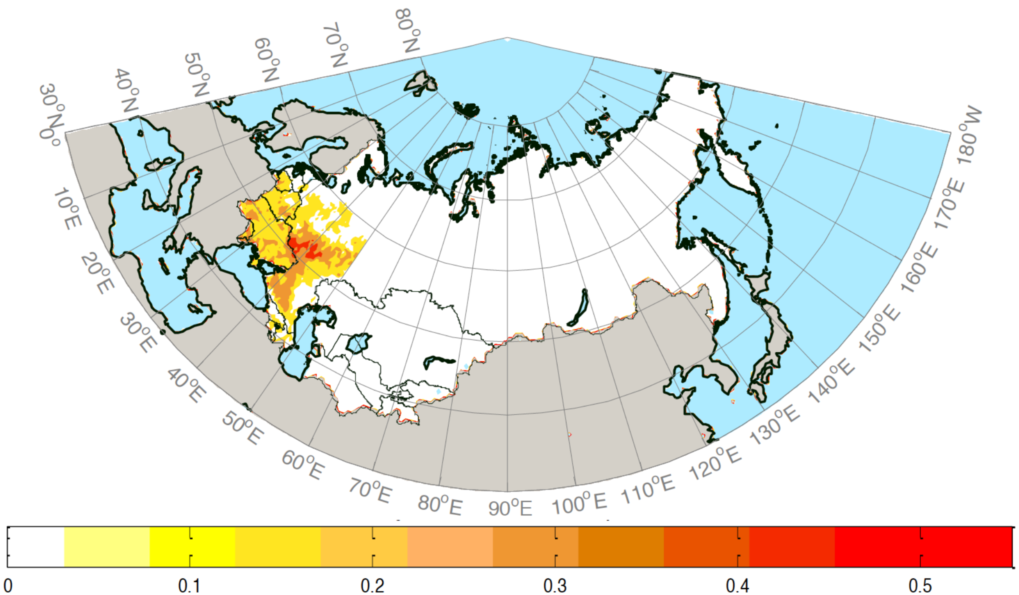

Fig. 7.

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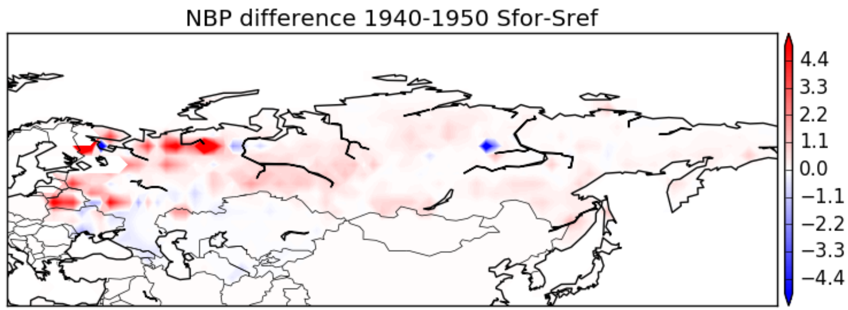

Fig. 8.