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Biogeosciences Discussions

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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Please find below a reply to the issues raised:

RC2: I do not think there is a problem here, thanks. Only the data I would like to show - please, see the figure in the attachment. That is time series of spatially averaged anomalies of mean annual temperature at the earth's surface for Russia (top) and the globe (bottom). Axis: horizontal - years, vertical - deviations from the mean during 1961-1990 (Celsius). Red shows the course of the 11-year average. That is a bit different from the curve for the Northern Hemisphere on your figure. So as for me I do not see for 1940s extra positive anomalies in Russia. Maybe it is even opposite - there

is a strong negative anomaly in 1941

AC2: The figure presented by the referee does not contradict our analyses (nor could it, since it is also based on the HadCRU temperature data, used to produce CRU/NCEP). Indeed there is a negative anomaly in one year (1941), but overall the 1940s decade is warmer than the period 1901-1930. Figure 1 shows the graph of annual and 5-yr mean temperature over the territory of the FSU (not just Russia) from CRU/NCEP (the dataset used to force our model). Again, as the referee points, 1941 is indeed colder than average (see Brönnimann et al., (2004) for an explanation of such cold anomaly), **but the decadal temperature anomaly is higher than any of the previous decades**. Also, the referee may verify the map of the temperature anomaly in the 1940s (Figure 2). Parts of western Russia might have been colder than average, but very large regions in high latitudes registered very warm anomalies (consistent with figures 2 and 5 in our manuscript).

RC4, RC5, R8: In my personal view it is crucial. It is clear underestimation in your results. Unfortunately, I am not historican, and cannot help to find robust datasets for disturbances of ecosystems during the war. There are few data - for example, there are data on the number of burned villages (for example for Belarus it is about 9200 villages). Probable it is possible to find more indirect data in the Arhive of the Russian Federation, Ukraine, Belarus. Another way could be to obtain "expert opinion" - to find historican for that period. I do not think that just adding the discussion on the underestimation in the paper would be enough. We do not know the scale of that underestimation. In my view that could be very high and the results could be potentially misleading.

AC: As mentioned in our previous reply, here we include the open biomass burning in the carbon balance accounting as a baseline estimate of fire emissions. Since our work is focused on the effects of (i) land-use change and (ii) climate variability, we perform factorial simulations to test the relative contribution

of these two processes in the resulting carbon budget. Therefore, unless the fires mentioned by the referee should significantly vary with any of the two processes (unlikely since they are human-caused), they do not affect our results (the underestimation is cancelled out). Furthermore, the referee may note that here we compare our results with the set of models in Bastos et al. (2016), and **none of these models includes war-related fires**. Therefore, even if we do acknowledge that the may some underestimation of the effects of fires, this does not significantly affect (i) the results from our factorial simulations and (ii) the comparison with TRENDY models. As mentioned before, **we avoid introducing processes in our analysis that are not based on reliable evidence**.

RC6: that the second point which is crucial. You have mention that ORCHIDEE model was verified and gave uncorrect results. And you assume that ORCHIDEE-MICT isnow estimate correctly (?) I believe that the standart way of performing any modeling - is a verification against experimental data and assessing of the uncertainty of modelling in the beginning

AC: As mentioned in our previous reply, a thorough description of the improvements to the model may be found in Guimbertau et al. (2017). The referee may note that **in their paper, the authors perform an exhaustive analysis of the ability of the model to simulate carbon exchanges, hydrological processes and soil carbon stocks, by comparing with several observation-based data**. In a revised version of the manuscript, a short summary of the model validation may be included. Nevertheless, the referee may note that in our Table 2, we compare the model simulated CO2 fluxes and C-stocks with observation-based datasets Also, the referee is invited to revisit lines 32 (pg 8) to 9 (page 9) of our manuscript, where the

Bronnimann, S., J. Luterbacher, J. Staehelin, and T. M. Svendby. "Extreme climate of the global troposphere and stratosphere in 1940-42 related to El Niño." Nature 431, no. 7011 (2004): 971.

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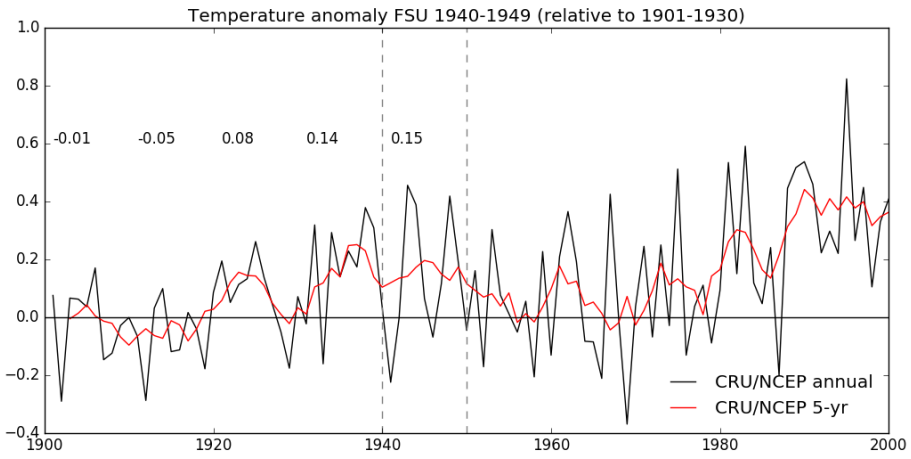
Temperature anomaly FSU 1940-1949 (relative to 1901-1930)
-0.01
-0.05
0.08
0.14
0.15
CRU/NCEP annual
CRU/NCEP 5-yr

Fig. 1.

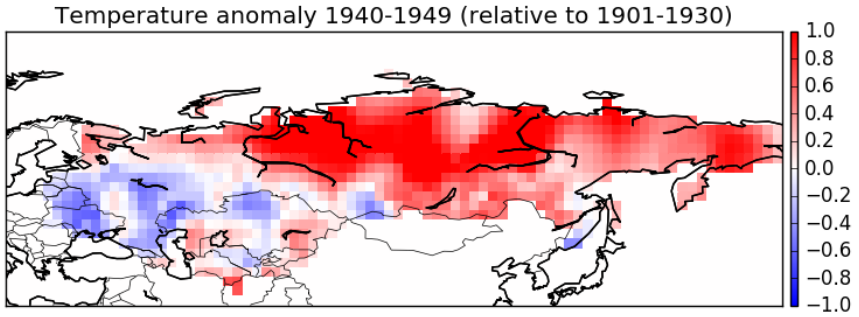
Temperature anomaly 1940-1949 (relative to 1901-1930)

Fig. 2.