Biogeosciences

Discussions

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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 paper dedicated to the possible explanation of CO2 stabilization during the first half of 1940s despite of maintained anthropogenic emissions. That could be a part of more complex scientific problem: relative stabilization of the global average temperature in 1940-1960s in spite of increased CO2 atmospheric concentrations. Authors suggested original explanation for the problem: increased CO2 sink on the territory of the Former Soviet Union (FSU) during the Second World War (WW2) in the result of cropland abandonment. Obviously that process has taken a place and contributed somehow to the global C balance during that period.

However, it is difficult to agree with authors on the methodology and some activity data used for their analysis.

1.   Some confusion caused by the statement of the research problem. 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. 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. 2. 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. 3. 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 CO_2 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 CO_2 emissions in the research. 4. 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 CO2 emissions. 5. 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. 6. 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. 7. 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.

That is extremely interesting paper and I wish to authors to continue and improve their investigation. More efforts require historical data and verification of the model.

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