

Prof. Tauger replies:

Robert Conquest's second reply to my article does not settle in his favor the controversy between us over the causes of the 1933 famine. On his initial points, I noted that the famine was worse in Ukraine and Kuban' than elsewhere, in great part because those regions' harvests were much smaller than previously known. I rejected his evidence not because it was not "official" but because my research showed that it was incorrect.

Conquest cites the Stalin decree of January 1933 in an attempt to validate Ukrainian memoir accounts, to discredit the archival sources I cited and to prove that the Soviet leadership focused the famine on Ukraine and Kuban'. The decree's sanctions, however, do not match memoir accounts, none of which described peasants being returned to their villages by OGPU forces. The experiences described in those accounts instead reflect enforcement of a September 1932 secret OGPU directive ordering confiscation of grain and flour to stop illegal trade (RTsKhIDNI f.17, o.3, d.900, prilozhenie). Since this was applied throughout the country, the Ukrainian memoir accounts reflect general policy and not a focus on Ukraine.

Several new studies confirm my point that hundreds of thousands of peasants fled famine not only in Ukraine and Kuban', but also in Siberia, the Urals, the Volga basin, and elsewhere in 1932–1933. Regional authorities tried to stop them and in November 1932 the Politburo began to prepare the passport system that soon imposed constraints on mobility nationwide (RTsKhIDNI f.17, o.3, d.907, no. 123). The January decree was thus one of several measures taken at this time to control labor mobility, in this case to retain labor in the grain regions lest the 1933 harvest be even worse. Its reference to northern regions suggests that it may even have been used to send peasants from those areas south to provide labor. Neither the decree itself nor the scale of its enforcement are sufficient to prove that the famine was artificially imposed on Ukraine.

The sources I used to estimate the 1932 harvest were operational documents—*kolkhoz* annual reports—which, like the barn yields published in the 1950s, show the truth behind the official data that Conquest and other scholars had to use until now. Ukrainian eyewitness accounts, on the other hand, are misleading because very few peasants from other regions had the opportunity to escape from the USSR after World War II. The Russian historian V. V. Kondrashin interviewed 617 famine survivors in the Volga region and explicitly refuted Conquest's argument regarding the famine's nationality focus. According to these eyewitnesses, the famine was most severe in wheat and rye regions, in other words, in part a result of the small harvest.

The source that V. P. Danilov cited (RGAE f.8040, o.8s, d.7, 1.213–216) does not evidence large reserves. It is a projection by the grain procurement agency in June 1933, based on optimistic procurement plans, with a report explaining that these plans would not be fulfilled. The report noted severe food shortages throughout the country that the regime could not alleviate, as well as new, unplanned demands from the military which the regime also could not fully satisfy. I acknowledged that grain exported could have saved many lives but the small harvests of 1930–1932 made it unlikely that those exports could have prevented famine.

Both Russian and western scholars such as Kondrashin (*Golod 1932–1933 godov v derevne Povolzh'ia* [Soviet dissertation, Institut Istorii SSSR, Moscow, 1991, 38–55]), E. N. Oskolkov (*Golod 1932/1933* [Rostov-na-Donu, 1991], 21), and Alec Nove (*An Economic History of the USSR* [Penguin, 1993], 178), now acknowledge that the 1932 harvest was much smaller than assumed and was an important factor in the famine.

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To the Editor:

Thomas Prymak's review of M. Hrushevs'kyi's *Na porozh novoi Ukraïny*, (*Slavic Review* 52 no. 2) presents a rather inadequate discussion of the contents of the work under review, leaving the reader in the dark regarding the material it includes. He also ignores the importance of *Na porozh novoi Ukraïny*, published by Hrushevs'kyi in Kiev in 1918, to the establishment of the Ukrainian National Republic in 1917–1918. Prymak notes that this work emphasizes Hrushevs'kyi's commitment to "building an independent Ukrainian state" and not on his "federal principle." It suffices to state that objective of this publication is to focus on Hrushevs'kyi's views on an independent Ukrainian state in the context of the Ukrainian National Republic.

Prymak leaves the impression that "it was only after the publication of a major English-language biography and other works in the later 1980's that Hrushevsky's true profile began to emerge." I assume he refers to his own publication. I take issue on

two points. First, it is difficult to accept what I consider a parochial and ethnocentric viewpoint that publications have to appear in English in order to have “impact upon the wider world.” Serious historians have a reading knowledge of the language of the nation which they are studying so they can analyze primary sources and published literature. Second, a systematic research on Hrushevs’kyi’s activities started in the 1960s in the US with the introduction of “Hrushevs’koznavstvo” (Hrushevsky studies) by the Ukrainian Historical Association, resulting in publication of numerous materials which were abstracted in *Historical Abstracts* and other reference publications. In fact, Dr. Prymak notes his reliance on these works in the preparation of his own English language biography. The materials published by the Ukrainian Historical Association from the 1960s to the present are well known by serious scholars of Ukrainian history and Hrushevs’kyi.

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Prof. Prymak chooses not to respond.

To the Editor:

Two letters to the editor in *Slavic Review* 52, no. 3 took exception to James Cracraft’s review of John Alexander’s *Catherine the Great: Life and Legend* (“Great Catherine,” *SR* 52, no. 1). Their main objection is that Cracraft criticized the book’s emphasis on Catherine’s sex life.

The letters’ use of rhetorical phrases, such as “political correctness” and “the suspicious odor of censorship,” obscures the real issue raised by Cracraft’s review, namely scholarly standards in relation to the selection of historical data. No one would object to publishing detailed evidence of Catherine’s sensuality, if it had an impact on her policies and rule or if the book were a psychohistory or a study of eighteenth century monarchical mores. However, Alexander’s obsessive chronicling of every rumor and innuendo concerning Catherine’s favorites and her sex life is misplaced in a broad biographical study, which should include but not highlight such information. Instead, there is explicit sex on roughly 100 of the 350 pages of the book, whereas serfdom and the Pugachev revolt are treated in less than a dozen pages and the empress’s deep involvement in cultural development is virtually ignored.

Since it is widely known that Catherine kept her love life separate from her public life, the only reason to have this theme dominate the book is to sell more copies or, as Marc Raeff put it in another recent review, “to satisfy the prurient curiosity of adolescent college students.” This unwarranted emphasis on sex distorts the architecture and importance of the life of the empress and comes at the expense of serious scholarly analysis. It is thus a normal, valid ground for criticism and not at all a question of censorship or “political correctness.”

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