

to the approaches (favored by journalists and politicians) that emphasize only a single factor. Perhaps she thought that these scientists are also seeking to assign collective responsibility to a single group or factor, while in fact they were offering a ranking of several causal factors. One of the best grounded and complex multiple factor theories is confined to a brief footnote; its author forcefully argues that the search for collective or personal responsibility in general is *not* the task of social science. Her contribution to this volume, in my opinion, fails to do justice to the social science theories that attempt to offer empirically testable explanations of the Yugoslav disintegration.

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To the Editor:

I read with increasing incredulity and disappointment the review of *From Sovietology to Postcoloniality* edited by Janusz Korek and published by Södertörn Academic Studies in 2007 in the Summer 2008 issue of *Slavic Review* (vol. 67, no. 2). Two-thirds of the review consists of admonitions against treating central and eastern Europe as postcolonial territories; only toward the end does the reviewer mention the contents of the volume he was supposed to survey.

The reviewer posits that since Franz Fanon, a black man, came to dislike French and European culture (although he wrote in French), no European nation can be subject to colonialism. But the second thesis does not follow from the first. He further posits that “the ‘Enlightenment Project’” (478) the colonized peoples of Africa and Asia rejected was embraced by intellectuals in eastern Europe, and thus they cannot themselves be colonial subjects. Even if this erroneous generalization about the Enlightenment were true, the second thesis does not follow.

Slavic Review is a periodical published in a country that began as the “thirteen colonies” and fought a war of independence against the colonizing power. White-on-white colonialism was not uncommon in Europe either. It is disturbing that *Slavic Review* has published reviews of books dealing with Russian/Soviet colonialism written by persons who on principle reject the notion that *non-Germanic central Europe* was a Russian/Soviet colony. Such conditions produce a rant rather than a review. Soviet/Russian colonialism often belonged to the white-on-white variety and developed unique features (such as the “surrogate hegemon”) that are presently being theorized by academics in a number of countries. Among the most outstanding is Dariusz Skorczewski of the Catholic University of Lublin. The review penned by Stephen Velychenko shows no familiarity with such research and appears bent on discouraging young scholars from pursuing this line of inquiry.

EWA THOMPSON
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Professor Velychenko responds:

Like most historians who dare to review modern literary scholarship, I am as overwhelmed by “litcrits” exposition of theory as by the ignorance of history most of them share. The imbalance is only partly compensated for by the insights that the best of the “postcolonialists” within this group sometimes provide. Yes, I would not encourage anyone to classify something as “white-on-white colonialism,” to study whether it was like or unlike a “yellow-on-yellow colonialism,” or to investigate whether Shaka Zulu was responsible for “black-on-black colonialism.” Perhaps such students could go the way of Napier, who began by counting angels on pinheads and ended with logarithms. My hunch is they will end up like Francis Bacon. Trying to discover a way to preserve meat, he died of pneumonia caught while stuffing dead chickens with snow.

Ewa Thompson implied that I am among those who “on principle reject the notion that non-Germanic central Europe was a Russian/Soviet colony.” I do not. Just as she wrote, this notion is just that, a notion, not a proven, generally accepted fact. The subject requires more historical study and, in my opinion as a historian, the jury is still out on the issue of whether or not Russian-ruled European lands were “Russian colonies.” “Litcrits” can think otherwise and invent more obscure neologisms like “surrogate hegemon,” to

write about them. They can also claim truth does not exist, and all interpretations are the same. But, if they do and are reviewed by those who do not share their preconceptions, they should expect to be taken to task. Those who disregard such criticism as “rant” are merely locking themselves into their subdisciplinary ghetto.

Finally, Thompson did not correctly summarize my remarks on Fanon or the Enlightenment. Only Bolsheviks, like Fanon but unlike Marx, considered the *lumpenproletariat* “bearers of liberty.” Nor did I write that colonized Africans and Asians rejected the Enlightenment. I wrote that postcolonialist litcrits did. The question of eastern European intellectuals, the Enlightenment, and colonialism does deserve attention but cannot be adequately covered in a letter.

STEPHEN VELYCHENKO
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To the Editor:

In her review of my book, *Russian Monastic Culture: “Josephism” and the Iosifo-Volokolamsk Monastery, 1479–1607* (*Slavic Review*, vol. 67, no. 2), Gail Lenhoff criticizes its social analysis by stating that, “This model, based on criteria singled out as significant for pre-Petrine society in Boris Mironov’s social history of the imperial period, is anachronistic and misleading because the distinguishing characteristics of separate categories . . . overlap” (485). If in order to avoid being “anachronistic and misleading” a historian must find non-overlapping “distinguishing characteristics” for social groups, then no competent historian of any culture at any time can ever avoid this criticism. My book clearly explains its intention to reach broad general conclusions within the limits of the available data: “It is not my intention here to write a social history of sixteenth-century Russia, only to give a general impression of where monks fit into that social environment. . . . I am consciously and deliberately joining that great class of historians who according to Reddy do not attempt to apply ‘great precision’ to the definition of classes” (102–3).

Lenhoff alleges that “references to secondary sources” include “inaccurate claims” and “frequently lack page numbers.” As an example she says that I attribute to Ludwig Steindorff an inaccurate claim that Iosif of Volokolamsk “pioneered” the practice of charging for liturgical commemoration of the dead. The sentence in my text is “Iosif pioneered an especially lucrative monastic enterprise: charging for liturgical commemoration of the dead” (35). The footnote reads: “The standard works on this topic are by Steindorff (1995, 1995a, 1995b, 1999, 2000a, 2000b).” This is not a quotation but a list of references for further inquiry. Nor is “pioneered” here a misstatement of fact. Whether someone prior to Iosif ever charged for commemoration is beside the point; the fact remains that Iosif’s monastery brought this practice into common usage.

I invite readers to compare Lenhoff’s review with that of T. Allan Smith in *Canadian Slavonic Papers* 50, nos. 1–2 (March–June 2008).

TOM DYKSTRA
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Professor Lenhoff chooses not to respond.