

when he points out, quite rightly, that we have to examine to what extent the actual developments corresponded to Stalin's "intentions."

To do so, however, we must also attempt to establish the degree to which Stalin's intentions were reflected directly in official Soviet statements from published and unpublished sources. This is, after all, what Getty, Tucker, and I have been trying to do, albeit with different results.

The problem, as I see it, is that Getty seems to be quite convinced that the official Soviet sources used by him for the 1933–36 period should be taken as pure and simple expression of "central motives." The reason he gives (in his reply to Tucker and me) is that this material consists of internal documents: secret letters, circulars, and closed reports, as distinguished from published ones.

This argument is only valid to a very limited extent. Much of the material referred to by Getty was published at the time. Furthermore, the unpublished sources do not appear to be at variance with published evidence, which is hardly surprising considering the fact that quite ordinary party correspondence intended for a large number of party officials was classified as secret or even top secret in Stalinist Russia. Of course these secret letters contained some information that the party leadership did not want the whole party or the general public to see. But are we really justified in assuming that Stalin wanted to conceal nothing of importance from those party officials to whom the internal correspondence was sent and who were, after all, among his main targets of criticism in the mid-1930s? The fact that Getty seems to believe that any secret letter can be considered, as a matter of course, to reflect fully and directly the real motives of Stalin himself or his closest aides is and will remain incomprehensible to me.

As stated elsewhere, we have to distinguish between the question of *how* Stalin wanted the party officials to act and *why* he wanted them to act in this way. The first question can be answered, more or less satisfactorily, by compiling official, published and unpublished, party directives and by taking their message at face value, i.e. by adopting the primitive method of the "scissors-and-paste historian." (But it should be noted that Getty's idea of the nonpolitical character of the 1933–36 purges even in this respect seems rather doubtful). The second question, on the other hand, needs a far more refined approach to all available sources, published and unpublished, Soviet and non-Soviet.

It is a fact that the essence and degree of Stalin's control over Soviet society has not yet been fully elucidated and that the problem of Stalin's intentions is extremely complex and blurred. It is my hope that future research by my fellow participants in the *Slavic Review* discussion and myself will be able to throw more light on the subject. I have read Getty's article and his dissertation with keen interest, and I shall welcome any attempt to provide new evidence and fresh interpretations. I feel strongly, however, that such attempts should not be made at the expense of a careful, systematic, and critical evaluation of the sources.

#### PROFESSOR TUCKER REPLIES:

The letter's key sentence reads: "The idea that Stalin had planned his policies over the course of many years and then actually implemented them is under such circumstances simply grotesque." What seems odd to me is that Hans-Henning Schröder thinks this idea grotesque on the strength of the considerations adduced in his letter.

#### TO THE EDITOR:

Thank you very much for devoting space in your Winter 1983 issue to Robert Owen Crumney's thoughtful review of my *Slavery in Russia 1450–1725*. In these days when the

standard 500-word review rarely rises above a few remarks of the praise/blame variety, Crummey is to be congratulated for incisively raising substantive objections to my methods and conclusions.

Crummey raises a few issues that may be of considerable interest to *Slavic Review* readers. One is the nature of the early modern Russian demographic regime. Crummey was one of the few reviewers even to mention the book's demographic findings, which are based on the by far earliest available body of relatively solid evidence. Regrettably, however, nearly every remark he makes in two rather lengthy paragraphs is either irrelevant or mistaken.

When I began to study Muscovite slavery, I certainly did not anticipate that the evidence would force me to conclude that the well-known demographic regime of the 1700–1861 era (large extended households, low age at marriage, and absence of solitaries) did not exist in 1600 and was created by the Muscovite tax system and serfdom. (Recent work by Steven Hoch helps to show how the communal system and particularly the interests of the household head perpetuated the serf demographic regime after the replacement of the household tax by the soul tax in 1719–23.) The fundamental issue is not whether the marriage age in Muscovy precisely = (the parent's age) – (the child's age + 1), or = (the parent's age) – (the child's age + 1 + some unknown factor for miscarriages, infant and child mortality, amenorrhea, etc.), but whether the Muscovite demographic regime resembled that of the 1700–1861 period, or whether it more closely resembled that of the European pattern described by Sir John Hajnal in his classic article "European Marriage Patterns in Perspective" (in D. V. Glass and D. E. C. Eversley, eds., *Population in History. Essays in Historical Demography* [London, 1965]). Hajnal himself did not even go into foetal wastage, child mortality, remarriage, and other such issues in arriving at his conclusion that the European pattern was one of high ages at marriage, single family households, and a significant portion of solitaries.

Crummey insists that my procedure for generalizing about the average age of marriage in Muscovite society as a whole is "quite simply, indefensible." He can only make that statement because he claims that the findings are "on slave marriages"—which is simply false. Apparently I neglected to repeat it often enough, but the statistics I compiled are about people who *sold themselves into slavery*, most of whom had not been slaves before. Most were not slaves when they were born, when they married, when they had children, or until the moment when they sold themselves. Table 11.3 in my book gives the occupation or status of those who sold themselves into slavery, table 11.6 presents their age characteristics, and table 13.11 differentiates the ages at marriage of those who had been free from those who had been enslaved. I fail to see why using the data about those who always had been free as a basis for discussion about society as a whole is "indefensible." Moreover, it is possible that "high noble families often married off their sons and daughters in their late teens" (all indicative of compulsion and a certain future—both characteristic of the serf regime as well), but my study of the pattern of slave acquisition by provincial landholders seems to indicate marriage patterns similar to those who sold themselves into slavery. The main point Crummey ignores, however, is that the Muscovite practice of enslaving their own Muscovites would have been largely superfluous had the demographic regime of the 1700–1861 period prevailed earlier.

Following the same train of thought, Crummey suggests that Muscovite slavery had that peculiar character because "the rulers and the upper classes of Muscovy saw slavery as the appropriate legal status for those who performed certain functions in society." That is both obvious—after all, they wrote and enforced the laws—and ultimately unhelpful: Moses I. Finley's "social scientific law" that generally prevails throughout the human community in both time and space is that no society could withstand the tension inherent in enslaving its own ethnoses, and attributing Muscovy's violation of this "law" to the "vision" of its rulers explains little.

Crummey suggests that much of my demographic evidence may not be typical because it comes from the era of the Time of Troubles, “a time of exceptional economic and social dislocation.” Definitions of the Time of Troubles vary, but it is generally agreed that those events which affected the populace at large occurred only after 1600. In fact most of my data come from the 1590s, by consensus the “best times” in Muscovy between the 1560s and the 1630s, and thus I might be faulted for generalizing on the basis of data from perhaps better than average times. Moreover, Crummey failed to note, apparently, that I took care of this problem in table 13.8 by comparing the data for 1602/03 with the data for all other times, and found that there was little variability. Methodologically, that should be convincing. (I also generated dozens of other tables for the demographic data by decades, reigns, eras, geographical areas, etc., but few differences emerged there, either, and I did not burden my volume with them.)

The “dependency syndrome” created by Muscovite slavery afflicted not only those manumitted without property mentioned by Crummey, but even some of those who fled with vast quantities of stolen property—and resold themselves to someone else, or returned to their old owner, several months later. In this context it should be observed that in practice Muscovite slaves could and did accumulate property, although it is unknown what happened to such property when their owners died or how dependent such people ordinarily were vis-à-vis propertyless freedmen.

Crummey’s other major critique is about the nature of *Slavery in Russia*. He seems to have forgotten that there are fundamentally two types of books: extended essays and presentations of research findings. Americanists, for example, often write extended book-length essays in which they assume that the reader has some background in the subject and can refer in notes to large numbers of other monographs and even easily accessible primary sources. My *Slavery* volume, on the other hand, is a presentation of research findings (with a six-page summary for those who do not have the patience to read it all); there are no previous articles of research findings or appendixes to cite. Moreover, much of the data base (particularly that about the slaveowners) was assembled for this occasion, cannot be found anywhere else, and probably never will be put together again. Crummey thinks that some of the tables are “excessively detailed,” but I felt an obligation to present some of the data in detail before aggregating it. A very few of the tables present the sparse data that are available on some interesting issues to tell the reader what there is with the hope that perhaps someone else with more powerful techniques will be able to make something of them.

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#### PROFESSOR CRUMMEY REPLIES:

Richard Hellie’s courteous response to my review of his *Slavery in Russia 1450–1725* deserves a brief reply. Rather than drowning the reader in the minutiae of demographic calculations, let me stick to the main points of our discussion.

First of all, Hellie has advanced a fascinating hypothesis, namely that “the well-known demographic regime of the 1700–1861 era . . . did not exist in 1600 and was created by the Muscovite tax system and serfdom.” It seems to me, however, that data on slaves or future slaves will not prove or disprove it. Whether we speak of slaves or people selling themselves into slavery makes little difference. Such people do not constitute a cross section or representative sample of the population as a whole.

Second, I stick to my point that Hellie’s data come mainly from one period—the demographic catastrophe of 1602 and 1603—and one particularly hard-hit region, the Novgorodian lands. To say so is not to blame him. A historian must use whatever data

are available. At the same time, the unrepresentativeness of the surviving data makes Hellie's interpretation of Muscovite slavery as a system of "welfare" more plausible than it might otherwise be. (The data on slaves' ages in table 13.8 really do not address my main concern.)

Third, I am still unconvinced by the reasons which Hellie gives to explain why Muscovite Russians enslaved members of their own ethnos. If Finley's "social scientific law" is generally valid, Muscovites clearly did not perceive slaves in the way in which they were viewed in other slave-owning societies. I would like to suggest two alternative—and admittedly vague—hypotheses to explain this apparent anomaly. Before serfdom, the rulers of Russia found slavery the most convenient legal category for retainers and domestics. Indeed, it is hard to see, in functional or occupational terms, how Muscovite slaves differed from later household serfs or from poorly paid "free" domestics in other societies. For whatever reason, there is nothing strange about having serfs or hiring domestics of the same ethnos as oneself. Perhaps, moreover, enslaving their domestics was easy for Muscovite nobles because they did not regard lower class people as full members of the same human community.

Finally, I agree with Hellie that the length of a book and the amount of detail with which the author supports his argument are matters of personal taste.

TO THE EDITOR:

Robert O. Crummey's review of Richard Hellie's *Slavery in Russia* is characteristically well done. In one aside, however, which is actually a paraphrase of Hellie's position, Crummey comments that American Negro slavery was "a case of extreme harshness on the spectrum of slave societies." This view was standard until the mid-1960s among scholars of this subject, but today historians have concluded that American slavery, despite its indubitably negative qualities, was nowhere near as harsh as Caribbean and other Spanish American slaveries.

Readers of the *Slavic Review* who would like to confirm that Hellie's and Crummey's view is outdated may wish to dip into the immense literature on the subject with the following: C. Vann Woodward, "Southern Slaves in the World of Thomas Malthus," in Woodward's *American Counterpoint*, pp. 78–106; or David Brion Davis, "Slavery and the Post-World War II Historians," in Sidney W. Mintz, ed., *Slavery, Colonialism, and Racism*, pp. 1–16. An excellent brief summary of the present state of slavery scholarship in the United States is in John Boles, *Black Southerners 1619–1869* (University of Kentucky Press, 1984).

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