
Communications

A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editor's discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

ARTICLES

TO THE EDITOR:

In "Victims of the Soviet Penal System in the Pre-war Years: A First Approach on the Basis of Archival Evidence" [AHR, 98 (October 1993): 1017-49], J. Arch Getty and Gábor T. Rittersporn have in effect extended and commented on figures Viktor Zemskov has published over the past few years. There is little here that is new, for these tables have of course been known for some time to, and considered by, a number of Western and Russian scholars, whose general conclusion is that their gross totals per annum may well be correct, but they are certainly incomplete in important ways. In fact, as has been pointed out, full information cannot be deduced from them: such assessments (by, for example, E. Bacon in *Revue d'études comparatives Est-Ouest*, nos. 2-3, 1992) are here simply not taken into account.

Getty and Rittersporn emerge as more moderate and more responsible than in their previous writings on Stalinism. Their (not wholly accurate) table of excessive figures by myself and others might indeed have included Getty's own estimate (1985) that Stalin killed "thousands" and imprisoned "many thousands"—an underestimate by a factor of several thousand being surely more striking than a supposed overestimate by a factor of five or six! And there are others, not quite so minimalist, that are also illustrative: Jerry Hough's, for example.

This table does indeed genuinely indicate one exaggeration made earlier: this was our acceptance of a figure of *circa* 5 million in NKVD custody at the beginning of 1937, a fairly rough guess, perhaps deriving from registration figures into which, the 1937 Census Board complained, the NKVD had

omitted to report 1 to 1.5 million dead in custody. A correction of some 2 to 2.5 million on our gross totals over the 1937-1938 period is thus needed (in fact, I have long since made it—*Soviet Studies*, no. 5, 1991). Not unimportant but also not much affecting the substantial "high" figure: our estimate for arrests in 1937-1938. (And while the "table" quotes me for an estimate of 12 million in camps etc. in 1952, in fact I merely refer to it as a figure "now given in Moscow." The real total of GULAG and labor settlements was in fact 5 million; and the Moscow figure includes the perhaps 3 to 4 million prisoners of war and exiles in general.)

On a crucial point, though, the table truly shows major gaps in your contributors' reasoning. Roy Medvedev's and my figures were indeed outsiders' "estimates," but the application of this term to those of Olga Shatunovskaia and Dmitri Volkogonov is untenable. Shatunovskaia was a member of the Party Control Commission and, in that capacity, of Khrushchev's Rehabilitation Commission, and reports her high figures as those then given to the Politburo by the KGB (these figures are, moreover, independently supported by others, including Sergo Mikoyan, who obtained them in his Politburo father's papers). That is, they are from a responsible official, on a documentary basis. It is possible to argue that, for example, the dates are misplaced (the table, in fact, in any case mistates these), but simple dismissal is excluded. Similarly, Dmitri Volkogonov was head of Moscow's governmental commission on rehabilitation and also bases his conclusions on documents—he has lately noted that these include *not* merely NKVD archives but also other material (including Stalin's own archive). He has also advanced a figure not quoted in your article, "from 1929 to 1953 . . . 21.5 million people were repressed. Of these a third were shot, the rest sentenced to imprisonment, where many also died" (*Kuranty*, May 9, 1991). In this connection, your contributors might also have cited Colonel Nikolai Grashoven, head of the Russian Security Ministry's own rehabilitation team, who similarly tells us that between 1935 and 1945, 18 million were arrested and 7 million shot (Radio Liberty/Radio Free Europe Press Report, vol. 1, no. 18, May 1, 1992).

From their title, your contributors' main concern is

with the pre-war purges. Here they cite in their table as excessive my own early estimate of 7 million arrests in 1937–1938, mentioned above. The evidence for a figure in this general range was good. Such a total is also suggested by the provincial archives of Kursk (*Agitator*, no. 18, 1988), which proportionately imply just over 4 million arrests in 1934–1937—that is, with 1938 still to come. Volkogonov, more recently than his estimate in your article, and again on the basis of NKVD and other documents, writes (*Trotskii*, vol. 2, p. 323) of 5 to 5.5 million repressed “in 1937 (incomplete) to 1939, of whom a third were shot”—that is to say, 1.75 million executions (*higher* than my figure). A security general, recently giving a lower figure (of 3.5 million) over a shorter period than cited above for executions as a whole, also says that half of these took place in 1937–1938 (General A. Karbainov, quoted in *Report on the USSR*, vol. 2, no. 18, May 1990). Generally speaking, over the whole period, Western “high” estimates overestimated camp populations partly because we underestimated executions and other deaths. In part, this may be because under the NKVD’s system of double bookkeeping, most executions were officially communicated to relatives as “ten years without the right of correspondence.”

In addition, it is worth noting that many executions took place without even the minimal forms of the NKVD troikas. Even individuals as important as the deputy head of GULAG, two army commissars, two former members of the Central Committee, and the deputy chairman of the NKVD are now listed as shot by administrative decision. Others died under interrogation, such as Marshal Bliukher, various Georgians, and others. Others were simply murdered in their offices, such as the Armenian first secretary, or in the streets, as was the leading Yiddish actor manager, Solomon Mikhoels.

A key figure for the crucial period not given by Zemskov and his sponsors is of the number in prison in 1937–1938. They note, indeed (of Turkmenistan), that there was an unprecedentedly large intake. There are in fact scores of accounts of overcrowding by from six to sixteen or more times the cells’ capacities and, in addition, of the taking over of children’s homes, transport garages, barges, and so on, and even the construction of roofed pits to cope with the intake. If my (possibly too low) estimate of about 1 million as an average in jail over most of 1937 and 1938 is correct, and the average time spent there—as reported—was about three months, we have *circa* 6 million to account for.

A further major note on 1937–1938 is the particularly refractory problem of the 1939 census. The figures published at the time, of 170 million plus, though credited by “revisionists” until quite recently, are now known to be about 3 million higher than those sent in by the Census Board. However, this does not mean that even the census figure (of *circa* 167 million plus) is veridical. The previous Census Board, of 1937, had been shot (and their census suppressed)

for having, as a “serpents’ nest of spies,” worked to lower the population; there was therefore a strong incentive for their successors to procure results as high as was feasible. We have evidence from one *krai* that double counting was implicitly encouraged, to the degree of 3 to 3.5 percent. At any rate, the 167 million figure cannot be relied on.

If taken in proper perspective, the Zemskov *et al.* GULAG grand totals are not incompatible with the Shatunovskaia-Volkogonov-Grashoven figures just discussed. After all, they record around 19 million entries into GULAG camps from other places of detention, and the figure for labor colonies, though not directly deducible, seems to run to another 4 or 5 million.

But important reservations remain. First, GULAG did not cover the whole penal system—as can be seen in this very article from the millions listed as entering it from, or leaving it for, other “NKVD camps.” Some 4.5 million leave in this manner. It has been suggested (Bacon above) that these movements were, in general, to camps of the First Special Department of the NKVD: that is, execution camps.

Nor should we merely accept the various subcategorizations such as “escaped” or “freed.” Robert Davies and Stephen Wheatcroft (both very restrained on such issues), in their forthcoming work on Soviet population, note that “escape” from NKVD “settlements” may well be a euphemism for death. More definitely, Volkogonov (*Trotskii*, vol. 2, p. 371 n) quotes a document given the Politburo by then MVD chief Kruglov in March 1947 on camp statistics, with one category “losses through discharge,” and he states that this in fact meant “deaths” (100,000 in the first quarter of 1947).

The other set of figures quoted in the article is that given by Kruglov to the Politburo on another occasion of the total sentenced to imprisonment or to death over the whole postrevolutionary period (this latter even appearing in your table in “documented”). The head of the Archival Administration of the Security Ministry, Major-General Anatolii Kraiushkin, stated clearly (*Rossiiskaia gazeta*, April 17, 1993: 13) of these figures that they were only “true” under the most limited definition, and the real figures were “far bigger.”

General Kraiushkin also notes that the word “political” of an offense or offender is used far too formally. Even in Cheka times, the police were instructed to arrest Mensheviks and others as “speculators, counter-revolutionaries, persons misusing their authority,” etc. In 1937, too, the categories are to some extent random, although those charged under the “political” rubric were less likely to survive and go to camp. Again, Kraiushkin implies that the charge “Wife of an Enemy of the People,” a large category, did not formally count as “political.” In fact, the whole operation was more random than the tables imply.

There are other matters on which your contribu-

tors lay themselves open to criticism: their treatment of "exile," for example—a major matter when one recalls that this was the main penalty inflicted on Lenin, Stalin, and others in tyrannical tsarist times. It appears to have been common as a sentence in the early and mid-1930s, but thereafter mainly in that most sentences to imprisonment were to eight, ten, or twenty years' imprisonment "followed by five years exile," so that survivors of GULAG mostly became exiles. Exiles under penal control were in most cases ill paid, given menial work, treated as criminals. This on a mass scale is yet to be properly investigated.

In general, the material presented in the article is of interest and a partial contribution to our knowledge. But there is much to consider outside the contributors' limited documentation, and their work does not warrant the claims implied.

ROBERT CONQUEST

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THE AUTHORS REPLY:

Robert Conquest's remarks signal an acceptance of some of the new evidence and an attempt on his part to discover numerous additional prisoners and places of detention and execution outside our documentation. Although Conquest is not familiar with the Soviet-era archival sources in question (which has not prevented him from lecturing the field on their proper uses in this and other forums), he has presented a familiar menu of press articles with sensational assertions from unverifiable sources. We would be glad to see a single exact citation from such sources, whose nature we apparently should trust because a small number of post-Soviet officials claim to have seen them. Nothing should prevent the Russian government from putting such data—if they exist—at the disposal of researchers. Still, the only precise figures quoted from archives that are still secret are in the range of what we have established on victims of political repression in the Soviet sense of the term (see *Izvestiia*, August 3, 1992; *Rossiiskaia gazeta*, April 13, 1993). Similarly, the number of people sentenced in cases initiated or investigated by the secret police that are quoted from closed archives of post-Soviet security agencies is exactly the same as that we found in the archives and reproduced in our article (*Istochnik*, no. 4 [1993]: 62).

We can say little about supposedly concealed places of detention or execution not counted in our sources, especially in the proportions Conquest proposes, except to advise him to study the documents in question. Thus, for instance, he would discover immediately that the administration he believes operated mysterious "execution camps," the First Special Department of the NKVD/MVD, actually dealt directly with accounting; most of the reports we cited came from there. In the same way, the provenance and texts of these documents make it highly unlikely that the NKVD would have concealed executed exiles

under the category of escapees. Aside from the fact that it compiled reports for its own and the Kremlin's use and not for the public, it was itself responsible for capturing and returning escapees, and such a system of accounting based on lying to itself would make this work impossible.

One must seriously doubt Conquest's interpretation of the data when he notices that Shatunovskaia, Volkogonov, and Grashoven refer to different dates in their estimates but misses the fact that if any one of them is correct, then we must inevitably dismiss the others. Their estimates, which Conquest presents as a unified revelation, are manifestly incompatible with one another.

Conquest cites post-Soviet writers who had access to Stalin's own archive and who claim to have found there material on the scale of repression in documents other than those of the NKVD. But it was the NKVD that was in charge of penal repression (and of accounting for it), and it would be interesting to know which agencies might produce more reliable data for Stalin. Sadly but predictably, we are not told. It is similarly curious that Conquest relies on Volkogonov for the assertion that there were camps not controlled by the NKVD, since Volkogonov quotes word for word a document showing that the so-called special camps were under the control of the security agencies whose records we used. (See Dmitri Volkogonov, *Trotskii*, vol. 2 [Moscow, 1992], 204. For the fact that the populations of such camps were indeed included in the GULAG materials we used, see State Archive of the Russian Federation [GARF], f. 9414, op. 1, d. 1319, ll. 1–20). Conquest's search for hidden camps is not supported by a shred of evidence.

His quest for hidden inmates is also impossible to document. The relevant archival materials (in GARF, f. 9414, op. 1) make it crystal clear that people entering and leaving the GULAG system, on whom Conquest depends so much as inmates of phantom camps, were simply being transferred to labor colonies or work sites (and back) and are thus included within our totals. These documents, which were kept secret for so many years, were not compiled to satisfy the curiosity of historians or to confuse them decades later. They were not prepared by secret police officials for the purpose of lying to themselves or deceiving Stalin and the Kremlin leadership. They were not deliberately made incomplete. They were written to keep track of a penal population whose precise size had to be known if it were to be transported, housed, fed, encircled by precise amounts of barbed wire and guarded by exact numbers of security troops. Invoices and accounting documents on all these questions are among the sources we used, and it is a desperate and silly conjecture to imagine that their compilers sought to hide data from themselves.

Historians study primary sources in order to make their understanding of the past as independent as possible from collective representations of their own times. Although the unsubstantiated press statements

Conquest cites reveal a lot about the imagery today's Russian citizens have of their own country's past, they constitute sources on the history of mentalities and indicate nothing about penal repression in the USSR beyond its impact on people's minds. Times are changing, but the nature of Conquest's sources and the way he employs them make him a prisoner of the self-image of the society he seeks to describe. Indeed, one scholar has recently shown how Conquest's discourse incorporates essential elements of Soviet official mythology (Dina Khapaeva, "La mythologie commune des Soviétiques et des soviétologues," *Revue des études slaves*, no. 4 [1993]: 707-14).

It is astonishing that at the moment when we finally have massive internal documentation—more detailed than anything the Nazis left—scholars would continue to speculate on alternative realities and not occupy themselves with the existing voluminous records. Specialists of the French Revolution waste little time arguing with writers who limit themselves to quoting what respectable politicians and journalists pretend to know about the subject. It is testimony to the sad state of their trade that students of Soviet history are not in a position to follow the example of their colleagues in other fields.

GÁBOR T. RITTERSPORN

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J. ARCH GETTY

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(It was impossible to solicit a response from Dr. Viktor Zemskov in the time available.)

REVIEWS OF BOOKS

TO THE EDITOR:

I refer to Richard Wolin's review [of *Martin Heidegger*] in the *AHR*, 98 (October 1993): 1277 and following.

Wolin accuses me of "justifying" not only Heidegger but National Socialism, not least by "relativizing" the significance of Nazi atrocities. My principal aim in this book as in my former works was, indeed, not to accuse but to comprehend. Yet the attempt to comprehend a complex reality includes many conceptual differentiations, and it does not exclude moral judgments, which should, however, not take a prominent place, being self-understood. If Wolin were willing to think about distinctions such as "verstehbar," "verständlich," and "gerechtfertigt" (comprehensible, understandable, justified) or "nationaler Sozialismus," "Sozialnationalismus," and "Radikalfaschismus," he would probably avoid vituperative clichés such as "cynical exercise in German neonational exoneration." An accusation resulting from the artificial isolation of a single phenomenon

from the epochal connection seems to me unworthy of a genuine historian.

ERNST NOLTE

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RICHARD WOLIN REPLIES:

Ernst Nolte claims (somewhat disingenuously) that his primary aim in his book on Heidegger, as well as in his other work pertaining to the ideological origins of National Socialism, is not to "relativize" but merely to "comprehend." But to comprehend toward what end? Nolte would like to steer clear of the problem of so-called revisionist interpretations of the Third Reich, with which his work bears distinct affinities. By seeking to discount the extent and reality of Nazi atrocities, these, too, presumably merely seek to "comprehend." He would, moreover, like to deny (also disingenuously) that there might in any respect be a political agenda behind some of his more controversial claims—for example, his ceaseless polemics against what he calls "the liberal system" and his tenuous justification of Christian anti-Semitism. Then there is his tasteless thought experiment in "Between Myth and Revisionism" (in H. W. Koch, *Aspects of the Third Reich*, 1985), in which he contemplates what the history of the State of Israel would look like today were it written by victorious PLO conquerors. The analogy Nolte intends here pertains to the purported rewriting of German history (to the detriment of the national honor) by the Allied conquerors. But do such specious comparisons constitute sound historical argumentation? If Nolte is telling us the truth about his desire merely to "comprehend" (and I have my doubts), I fear that he can no longer hear the ideological resonances of his own rhetoric. And, in this respect, all I tried to do in my review was to point these out to a more credulous readership.

RICHARD WOLIN

Rice University

TO THE EDITOR:

Courtney Vaughn's review of my book, *Schoolwomen of the Prairies and Plains: Personal Narratives from Iowa, Kansas, and Nebraska, 1860s to 1920s* (1992), is inadequate [*AHR*, 98 (December 1993): 1691-92].

Vaughn contends that I ignored the literature on frontier women. Because the lives of the schoolwomen of the early settlement days intersect with the history of education in Iowa, Kansas, and Nebraska, I used research in the history of education as well as the literature on regional history, western women's history, and on perceiving women's lives and documents. My fourteen-page bibliography and 726 footnotes indicate the application of the narratives and primary documents of ninety-six schoolwomen, numerous other archival documents including photographs, and works on women's history and lives

by Armitage, Bateson, Bernard, Degler, Griswold, Grumet, Hampsten, Hoffman, Jensen, Kaufman, Schissel, Personal Narratives Group, Riley, West, and others.

Vaughn contends through citing Julie Jeffrey (1979) that coeducation, suffrage, and higher education for women were "ideological conservative ventures" favoring male political control. I maintain that, in Iowa, Kansas, and Nebraska, these "ventures" were seen as progressive accomplishments that brought women expanded education, employment, and selfhood. My research and that of Tyack and Hansot indicate that there is little evidence that instruction and discipline were gender-specific in the one-room schools (*Learning Together: A History of Coeducation in American Public Schools*, 1990). The schoolwomen's documents attest to the positive value and purpose of their education and employment (pp. 12-17, 27-39, Chapter 2, "Educating the Schoolwomen," and chaps. 5-9).

Vaughn's comparison of suffrage in Idaho with my study is inappropriate because of the differing populations and histories. Sandra Myres observed that the Western suffrage movement is yet to be fully documented and understood (*Westering Women and the Frontier Experience, 1865-1895*, 1982). Western suffrage was interwoven with temperance, economics, legal reform and was strongly involved in local politics, including school elections. (See my pages 96-102, 179, 196, and chaps. 5-9 on schoolwomen's involvement in suffrage and school politics.)

I extensively applied research on the history of education such as the American Educational Research Association's *American Teachers: Histories of a Profession at Work*, Donald Warren, editor (1989), especially in Part I, "The Educational and Historical Setting." Vaughn neglects to indicate that my book is organized into two parts, with Part II containing the narratives of five schoolwomen whose lives illustrate various conditions of life cycles, teaching, education, and the social and environmental milieu.

Vaughn found my inclusion of schoolwomen's narratives about the physical environment to be purposeless. As rural working women, their lives and work were profoundly affected by weather and the seasons (pp. 83-96). They observed that "the wind was part of being home" and found a sense of pleasure and personal freedom in the spaciousness of the open land (pp. 83-84). The biographical chapters, 5, 6, and 8, also include this theme emphasizing that the schoolwomen were at home in this region; they did not find it a foreign, malevolent place. These assessments from the period of community development differ from the accounts of white women's struggles on the westward journey and the frontier. I applied

recent research and literature on this theme found in numerous articles in *The Great Plains Quarterly* from the 1980s; Michael Malone and Richard Etulain, *The American West: A Twentieth-Century History* (1989); James H. Madison, ed., *Heartland: Comparative Histories of the Midwestern States* (1988); Elliott West, *Growing Up with the Country: Childhood on the Far Western Frontier* (1989); Elizabeth Hampsten, *Settlers' Children: Growing Up on the Great Plains* (1991); and others.

Vaughn erroneously applies Kathryn Kish Sklar's term "invalidism" to schoolwoman Sarah Gillespie Huftalen after she quit teaching to care for her bedridden dying mother and became exhausted. Since Huftalen was the principal caretaker, her exhaustion was not based on a dichotomy of selflessness and ambition. Huftalen was a teacher, administrator, and teacher-educator for another thirty-two years after her mother died.

Vaughn concludes that historians "rewrite the past in their own image." Through the schoolwomen's documents, I present a unique historical perspective of schoolwomen's lives *from their viewpoint*, combined with my endeavors at reasoned interpretations based on their testimony.

MARY HURLBUT CORDIER
Western Michigan University

Courtney Vaughn does not wish to reply.

THE EDITOR

TO THE EDITOR:

In her otherwise most perceptive review of Carol K. Coburn's *Life at Four Corners: Religion, Gender, and Education in a German-Lutheran Community, 1868-1945* (*AHR*, 99 [February 1994]: 306-07), Betty A. DeBerg makes a small but meaningful error. DeBerg writes, "These German Lutherans were shunned even by other Germans and other Lutherans." Coburn's narrative makes it clear (p. 6) that the Missouri Synod group itself did the shunning. The synod's hierarchy actively discouraged contact with people outside the synod.

ROBERT W. FRIZZELL
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Betty A. DeBerg wrote to say that she had meant to write, "These German Lutherans shunned even other Germans and other Lutherans" but actually wrote, "these German Lutherans were shun even other . . ." Our editors understood her to mean "were shunned by . . ." and so altered her text. DeBerg, unfortunately, did not catch the change in the galley proofs.

THE EDITOR