Toward a Post-Cold War Historiography (A Reply to David Joravsky)

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Today, when seriously engaged criticism is becoming a rarity, David Joravsky's penetrating and challenging remarks on my account of the games of Stalinist democracy and post-World War II ideological campaigns in Soviet science are a genuine gift.[1] I am especially grateful to Joravsky for underscoring two features—an anthropological approach and an implicitly comparative agenda—both of which, indeed, were central to my effort. In the following, I group my responses to his criticisms under these two rubrics. It is my feeling that we do not disagree too much about the former, but have more serious differences with regard to the latter.

Joravsky praises the shifting of analysis of Lysenkoism and similar phenomena "from melodrama to anthropology, that is, from tales of good guys vs. bad guys to functional analysis of complex relationship." Yet he still has a reservation that the study of cultural rituals is not paying sufficient attention to moral differences among individual actors and to the development of substantial agendas in time. He points out that some principled Soviet scientists were not happy to pursue their goals by playing according to the rules of party games, but dared in one or another form to protest against deviations from the "norms of world science." In Joravsky's view, there was also a substantial policy change in 1950: a certain recognition of self-defeat and a retreat from the system of *partiinost'* toward normalcy, initiated by Stalin's intervention in linguistics.

Indeed, the story told in my paper is not a melodrama, nor is it a medieval morality play—a cosmic battle between Good and Evil. It is, if we carry the genre definition forward, a perfect tragicomedy, that ancient genre where characters are put to play roles that are often not suited for them. This leads to a sequence of alternately comic and tragic situations, sometimes to be resolved in an unexpected way by the *deus ex machina*, personified in this particular case by Stalin. Like in most situations in life, both good and bad, moral and irresponsible personalities appeared in these "plays." My analysis does not deny the moral judgment, but it does defer it because at first I needed to analyze the roles characters were supposed to play, which were different from the characters themselves.[2] The reverse order is less advisable: some observers who are not as perceptive as Joravsky can easily go astray when judging the players without knowing the rules of such unfamiliar games as *kritika i samokritika.* It was even possible—in one of the first western reactions to the philosophical dispute of 1947—to mistake G. F. Aleksandrov, a quintessential party hack, for a principled

[1]See David Joravsky, "The Perpetual Province: 'Ever Climbing up the Climbing Wave,'" and Alexei Kojevnikov, "Rituals of Stalinist Culture at Work: Science and the Games of Intraparty Democracy circa 1948," *Russian Review* 57 (January 1998): 1–9, and 25–52, respectively.

[2]The titles of my related studies show that I am not averse to considering dilemmas and choices of honorable individuals who tried to live moral lives under Stalinism. See my "Piotr Kapitza and Stalin's Government: A Study in Moral Choice," *Historical Studies in the Physical and Biological Sciences* 22:1 (1991): 131–64, and my "President of Stalin's Academy: The Mask and Responsibility of Sergei Vavilov," *Isis* 87 (March 1996): 18–50.

and courageous defender of liberal objective scholarship.[3]

Those scholars who had to play in the games of intraparty democracy could have—and indeed had—a variety of motives. Some mainly served science, either as an ideal principle or as a community; others cared mostly about careers, or the country, or the ideology, or simply pleasing the authorities. Few openly questioned the rules (both Orbeli and Rapoport were mentioned in my article), and there was, of course, the chorus, the majority who did not play actively but watched and occasionally commented. In another study, say, biographical, Orbeli's protest would have to be noticed as demonstrating his moral qualities. But here the genre was different: I needed the example primarily to illustrate what the actual ritual was and how strongly it was enforced.

Is, then, the anthropological approach in conflict with history? In some sense, yes, but the sense needs to be specified. Not because it treats culture as static, ignoring temporal development. Rather too much, then too little, happened and changed during the short time, 1947 to 1952, that I covered in my story. And, after all, a study of just one episode often can demonstrate more respect to the idea of the past—and therefore more historical consciousness—than a panoramic exposition from Plato to NATO. The conflict has more to do with neo-Kantian distinction between nomothetic and ideographic explanations, or between universalizing and particularizing tendencies. I was aware of this tension and am myself only partially satisfied with the compromise achieved. Insofar as I was pursuing the goal of understanding rules and roles—mainly in the first two sections of the article—to this extent personal differences and a variety of substantial policy considerations have not been given full attention. But history takes back hold in later sections, where the story follows individual players mastering the rules, flexibly applying and interpreting them, and even getting around them to unexpected consequences. At this stage, history profits from the preceding anthropological analysis, which helps to show that a retreat from *partiinost'* in science occurred neither in 1950 nor any time before Stalin's death and Yuri Zhdanov's expulsion from the Central Committee apparatus. The linguistic discussion of 1950 was conducted as a game of intraparty democracy, just like the biological discussion of 1948. It produced the result which was more acceptable from the point of view of modern science, but the difference in outcomes should not be mistaken for fundamental changes in game rules.

I would thus dare to say that the return from anthropology back to history was achieved; however, only a portion of historically relevant questions have been addressed. As an article only, the study had necessarily to have a very restricted goal, which was to demonstrate that the ideological discussions in the sciences were connected by common genre rules which belonged to the repertoire of intraparty democracy, and to analyze this genre at the stage when it was most fully developed and widely deployed. This conclusion opens up many further questions, which were at best hinted at in the paper. How did the games of intraparty democracy, in particular *kritika i samokritika*, originally develop in Bolshevik culture? If they were borrowed or adapted, where from? How did they first spread outside the party world into the world of academia, and in what sense did politico-academic disputes around 1930 differ from those of the late 1940s? How did the rituals gradually die out in post-Stalin times? And how were they adopted and transformed in other Communist cultures, especially the Asian, where they apparently came to play an even greater role? I cannot agree with Joravsky more that these important questions have yet to be answered,

[3]V.D. Esakov, "K istorii filosofskoi diskussii 1947 g.," *Voprosy filosofii*, 1993, no. 2:102.

hopefully, in some future studies. So much for the anthropological approach: I believe that here we are in basic agreement about desirable historiographical goals.[4]

An implicit comparative agenda, indeed, was hidden at the background of my study. It has become unavoidable for me to check my statements against several different perspectives—Soviet, Russian, some European, American—which have become parts of my cultural experience, if not so much by way of conscious choice than by way of life's hard realities. And here my main disagreement with Joravsky begins: the notion of cosmopolitan scientific norms is not just by chance missing from my paper. Sociologists of science who introduced and fashioned this concept in the middle decades of this century have by now largely abandoned it. A critique has been offered that exposed declaring norms as an ideology: often proclaimed the universal practice, they could not be found actually operating in any concrete location.[5] Pondering this disappearance of a theoretical yardstick against which Soviet experience could be compared, I was led few years ago to a suggestion that, instead of throwing the notion of norms away entirely, we reconsider them as rituals.

The first advantage of this change is that while norms are supposed to represent the actual practice, rituals belong to its public façade. Therefore, big, even fundamental, disparities between them and practice are no longer detrimental; on the contrary, they should rather be expected. Neither do such disparities make rituals irrelevant and unimportant for the practice: rituals do work and produce real effects that have to be studied. This means, in simplified psychological terms, that instead of being persistently frustrated in our theoretical attempts to close the gap between how things are ideally supposed to be (for instance, *kritika i samokritika* as a grass-roots check on the bureaucracy) and how they usually turn out to be (tragicomedy), we can make this very gap an object of investigation.

Rituals of a particular culture are often not perceived as such by insiders who take them for granted. Their conventionality can be revealed, however, with a help from an outsider's perspective. Those who sincerely believed in *kritika i samokritika* did not think of it as a peculiar Soviet idiosyncrasy. They thought it corresponded to the very nature of scholarship, just as how others may think about peer review and letters of recommendation. A real comparative study should compare Soviet experience not with supposedly universal but empirically unrealized "world norms" but with rituals of other contemporary academic cultures, for instance, the United States's during the Cold War fever, or Germany's during de-Nazification. Thus, the further advantage of switching from norms to rituals is the possibility for more genuine comparisons: cultures can share certain rituals, borrow or exchange them, but also develop unique ones.

In a similar way, one can compare and contrast the ideological work that justifies rituals as "the natural order of things." Those who developed the theory of *kritika i samokritika* as a "general law of the development of science," had they only known of their American contemporary Robert Merton, certainly would have recognized some similarity with one of

[4]Perhaps it is worth mentioning that I also agree with Joravsky that chaos theory does not offer a model of historical explanation. I employed the term as a metaphor, and also as a warning against too narrow understandings of what can be accepted as a historical explanation. One critic, for example, suggested that calling events chaotic amounts to giving up explaining them. But even natural sciences do not narrow their spectrum of acceptable explanations to a degree that would satisfy such strict requirements, which, nevertheless, occasionally still are rendered about history.

[5]Some of the older interesting polemics over scientific norms have more recently been republished in collections of essays: Michael Mulkay, *Sociology of Science: A Sociological Pilgrimage* (Bloomington, 1991); and Joseph Ben-David, *Scientific Growth: Essays on the Social Organization and Ethos of Science* (Berkeley, 1991).

his four basic norms of science, "organized skepticism." My article's section headings were intended further to suggest how Stalinist democracy in science, in compliance with the genre rules of tragicomedy, parodied notions of the serious philosophy of science such as "consensus" or "paradigm shift." By doing this, I do not mean to elevate scholastic Stalinist Marxism to the level of serious philosophy: it was, indeed, an outstandingly dull ideology. But even such ideologies are worth serious study because they are applied to real problems and have important effects; and, moreover, because duller versions of ideologies—whether Communist, anti-Communist, or others—may offer easier ways to understand how ideologies work.

For this very task of analyzing the work of ideologies as ideologies, it is very useful to compare them to Wittgensteinian language-games. In order to illustrate his theory of meaning, Wittgenstein constructed special situations where communication was possible only through a severely restricted set of linguistic resources. Ideologies offer a close real-life analogue to these artificial language games. They characteristically assign a special, almost magical, importance to a selected set of phrases and apply this linguistic erector set too widely, to a potentially unlimited spectrum of real or imagined problems. Soviet Marxists tried to come to grips with a very serious situation in sciences created by modernist revolutions in such disciplines as physics, biology, and linguistics, and with the grave academic conflicts they produced. The results of the application of ideological language to these problems turned out to be as unpredictable and unstable as they should have been according to the Wittensteinian theory of language. In an ironic version of the indeterminacy principle, the more rigid the ideological formulations had to be, the wider and more undetermined their possible meanings became.

In the case of biology, for instance, Soviet Marxists developed a whole spectrum of incompatible solutions. The list of most important ones, in chronological order, opened with the conclusion around 1930 that Lamarckism was ideologically wrong. That decision de-facto favored genetics, yet without its more radical extension, eugenics, which a short time thereafter also was denounced on ideological grounds. The next major revision (concluded finally by 1948) chose a version of neo-Lamarckism instead of genetics as the representation of Marxism in biology. And, finally, after 1964, physicalist molecular biology was accepted as the ideologically correct life science. That the meaning of ideology is so underdetermined may sound rather trivial, but it has a serious historiographical consequence. As historians we should be aware of the danger of being misled by the very ease of developing ideological rationalizations for any given course of events, whichever way they turned out. Rather than being logical causes, ideologies much more often work as language games or as post-hoc justifications of events. The former assumption has been used by historians too often; latter ones have not been explored sufficiently.

No matter how weird its rituals may seem now, and how tragicomic the discussions were, we should not forget that we are dealing here with a serious phenomenon: science in Stalinist Russia, judged by its contributions, was a science of the first rank. Its mentality was, indeed, influenced very much by the idea "to catch up with and to surpass the West." To make sense of the results, however, we should evaluate them against a measure more realistic than this ideological chimera compounded of Russian inferiority and superiority complexes together with ignorance about cultural differences within "the West." We can consider, for example, the list of Western countries—including Britain, Germany, Italy, Spain, the United States, Canada, and so on—and find that Russian science was among the more provincial of them for most of the nineteenth century. The achievements of Lobachevskii and Butlerov, however great, do not disprove this overall assessment of the

context of their work. For the greater part of the twentieth century, however, imperial Russian and Soviet science would rank close to the top of the list, despite two serious setbacks—one due to the Civil War, the other to the collapse of the Soviet regime.

One can, of course, pick out the proverbial Lysenko disaster and inflate it into a general theory of Stalinist science as an overall failure, but such scholarship simply reflects the persistence of Cold War mentality and cannot be taken seriously. Joravsky, a great scholar who also was among the first to initiate, as early as the 1960s, the departure from this mentality, already has called attention to the fact that, paradoxically, the period of Soviet sciences' greatest achievements approximately coincided with the period of Stalin's rule.[6] Even the Lysenko scandal would not have been important had there not been a world-class science, Soviet genetics, developed before. The Communist regime had nurtured its initially stellar rise about as much as it later contributed to its tragic fate. Historians have yet to come fully to grips with the important paradox of Soviet achievements in science under apparently very unfavorable conditions. The challenge of developing a genuine post-Cold War historiography of Stalinist Russia has become pressing, but we still have quite a long way to go in order to achieve this goal.

During the twentieth century, the role of science and technology in Russian culture was about as central as the role of Russian literature and literary criticism during the previous century, and their respective contributions to world culture are of comparable magnitude. I can thus only agree with Joravsky that the history of Russian science is indispensable for the understanding of Russia, as well as for the understanding—by comparison—of science and technology in Western countries.

[6]David Joravsky, "The Stalinist Mentality and Higher Learning," *Slavic Review* 42 (Winter 1983): 575–600.