

Shadow Science in the Soviet Union

Valentine A. Bazhanov

Science in the Soviet Union has been corrupted by the regular intervention of bureaucracies in decisions about publications and awarding of advanced degrees. Advancement as a Soviet "scientist" is more a means for career benefits than the discovery of truth. The processes by which decisions in science take place for non-scholarly reasons can be called "shadow science".

"In order to get ahead in science one needs not only a lively mind but also a death grip." — Mr Bepalchik

Many Soviet television viewers remember Mr Bepalchik, the hero of a very popular programme a couple of years ago, for his wit and keenness of observation. The quote above belongs to a different Mr Bepalchik who, like his television counterpart, has wit, but in his professional work more deeply reflects on the realities of scientific life, some of which are raised in the quote. Alas, these realities, up to now, have never been discussed by those scholars (at least in the Soviet Union) who do research in philosophy, methodology and sociology of science.

Recently, M. A. Rozov ventured to name these realities explicitly, but only in general terms and in relation to philosophy. As he put it, "for many years there have been factors here which steadfastly wreck our philosophical and scientific communities", resulting in "science that is a sort of imitation of science, philosophy that is a sort of imitation of philosophy".¹ The factors themselves are not analysed in the article, but a rather detailed panorama of these factors and their operative mechanisms might be drawn. This will produce a clearer picture of the means which force science to become an imitation of science.

It is very probable that the core of these factors is universal for scientific activity everywhere, independent of whether it is carried out in Spain, the United States, China or the Soviet Union. Certainly, in every country these core factors are mediated by numerous conditions (national, social and political, etc.), which means that case studies are extremely important.

The factors which promoted the transition of science, considered as a human enterprise, into an imitation of science, have specific features in the Soviet Union. I am going to analyse some of them by describing some case studies which make these features explicit. By realising the sources of shadow science, it may be possible to change the intellectual climate which currently promotes the prosperity of shadow science enterprises.

The shadow science phenomenon

In general terms, the term "shadow science" means those details and sides of scientific activity which exude the image of science without its substance. But this formulation is too general and doesn't reflect the differentiated specifics of shadow science.

Shadow economics means illegal economic activity. Similarly, shadow science might be defined as scientific communities, representatives or activity based on a violation or deformation of ideals, norms and values commonly cultivated in this community.

In the Soviet Union, the academic occupation can provide a comparatively good income, prestige and standard of living, even though all these parameters have rapidly worsened during the past decade. That is why some vigorous people treat academia — both research and teaching — as a comparatively profitable employment. Special social mechanisms are invented to make a scientific career not only possible in principle but to provide quick promotion. An academic degree or rank may serve as a solid basis or even springboard (in the case, for instance, of party officials) for personal aggrandisement elsewhere, such as industry and Communist Party institutions. That is why pursuit of academic degrees is considered to be a very important enterprise, able to ensure quite a good standard of living or as a suitable basis for a career in some other field.

I will now start a concrete assessment of the shadow science phenomenon in the Soviet Union. I would like to stress that it should be apprehended as a critical step or desire to understand, figuratively speaking, "the ideal false world where all is true" (S. E. Lec). The proportions of this world are determined by the culture and traditions of scientific schools, communities, status and their social, political and economic features.

The empirical ground of my reasoning and description of shadow science was formed by my personal experiences during my work in the Higher Education Ministry and the Academy of Sciences, as well as experiences of my colleagues, readily shared with me. Although I have disguised the names of individuals, all case histories and characters are real. As far as they represent typical situations, I will not mention inessential details.

I begin with the situation where, for the first time, I noticed the shadow science phenomenon in full bloom. In the early 1980s I was the Research-Visitor Associate at one of the leading Academy of Sciences Institutes in Moscow. In the Academy living quarters I became acquainted with Mr D from the eastern part of the country. Soon we found that the problems we worked on were similar. Certainly, I tried to determine what ideas and authors were of most interest to D. However, although D was on his one-year probation period, he was not even able to name the titles of works of eminent scholars. He could not even name correctly the title of the book he was reading at the moment. I was surprised by his unawareness of information that he ought to know.

We continued the conversation and I asked about D's personal publications. The answer transformed my surprise into astonishment: among D's papers was a paper in the most prestigious Soviet journal and one more was about to be published! This latter article actually appeared very soon after our conversation. It was not original — it contained no fresh ideas — but was quite professional. Hence, its content permitted me to judge the professional level and skills of the author, and his academic erudition and mental outlook as well.

A question immediately arises: was D able himself, without any assistance, to write these articles? If yes, why did the journal, which firmly insisted on originality of publication, in D's case publish a lengthy article void of any new idea?

I suspected in this and similar cases that some extra-academic factors, having nothing to do with reaching the truth, were involved. Since then, I have paid special attention to results of scientific activity such as books, dissertations and so forth which enable their authors to make quick personal advancement. The D case was intriguing, for the articles that appeared under D's name were at least professionally competent. The results of scientific activity have been described as a "miserable profusion of mishmash and even muddle-headedness, common among academics".² The D case, though, manifests something more than only "mishmash and muddle-headedness".

To sustain this judgement, I'll say that soon after our conversation D successfully passed his Doctor of Science defence. (This is the highest academic degree in the Soviet Union. The second highest is the Candidate of Science or PhD.) But — oh, miracle! — his doctoral dissertation was not approved by the High Testimonial Commission, which has special expertise and the right to reject a dissertation that doesn't fulfil the requirements. After a second defence of the dissertation (even its title remained unchanged) it smoothly passed through the High Testimonial Commission. We see here a sample of the pseudo-academic industry of making PhDs and DScs.

After obtaining a PhD, a scholar acquires a certain weight in the official academic structures. A PhD has for many decades been considered to give its holder success through financial independence, necessary for any adult person.

That is why the criteria for providing people with PhDs are artificially lowered. They are entirely determined by considerations of minimal economic welfare. Besides, the PhD opens the way to further growth (though limited) of salaries and career opportunities.

The DSc degree requires much more skill and effort, and is a more complex process. Nevertheless, as shown by the D case, this complexity can be overcome with some degree of desire and stubbornness. The DSc gives to its holder an appreciable academic weight, affords a sharp rise in salary, public prestige and, more importantly in Soviet conditions, progress up the administrative ranks (in academic or state hierarchies).

The total number of DSc holders in the Soviet Union is 50,000, and there are ten times as many PhDs (Candidates of Science). The traditions of academic activity in the Soviet Union, which are persistently maintained, treat the process of obtaining PhDs and DScs as the cornerstone of scientific life.

There are even special social institutions of postgraduate studies. During the past few years a great number of doctoral "schools" have appeared. What do they teach about how to "make" dissertations? How to write a thesis? Or how to conduct oneself during the defence?

On the basis of my personal experience of obtaining a PhD and DSc, as well as experiences of my close colleagues, I would like to stress that the goal of creating fundamental academic work and the goal of "creating" dissertations are, if not entirely contrary to each other, on different planes of scientific activity.

The orientation of academic activity towards obtaining a degree is one of the conditions favourable towards turning science into an imitation of science. Extra-scientific direction for a scholar's work enters in with the need to obtain the approval of a comparatively small group of academics (perhaps 30 to 40). If real science requires a "lively mind", the dissertation enterprise requires a "death grip", namely an ability to produce a good presentation that satisfies the tastes and psychological preferences of this group. (This doesn't mean that the requirement of public defence of dissertations is totally worthless or harmful.)

The rate of solicitation of academic degrees illustrates the deformation of the self-consciousness of the Soviet scientific community. The public defence of dissertations, to which exaggerated attention has been paid during recent decades, should be only the by-product of academic activity and not its main goal. At the present time, and indeed for many years, a tradition has been cultivated which

makes an opposite emphasis: first of all the dissertation and only then real research activity. Are three years of postgraduate work and three years of doctoral work justified only by the pursuit, by hook or by crook, of an academic degree, when long-term research targets are relegated to the background? Not to my mind. The sources of change for this situation lie mainly within the scientific community. Without such change, the disintegration of scientific communities will go ahead.

Even the "softer" approach to dissertations in the Soviet Union cannot be fully justified. As a matter of fact, the laws governing degrees declare that doctoral dissertations should contain the resolution or generalisation of important, large-scale scientific problems or the creation of a new academic direction of research. In both cases, a dissertation presupposes the systematic account of somebody's standpoint on a particular problem. This demands prolonged concentration and stubborn work, free of any distraction by other problems, even those close to the topic. The scholar must be willing to "cultivate" the topic. A person who, after producing original ideas, loses interest in them and begins to move into new areas to produce further new ideas, has little chance of finishing the strictly systematic research work. Such a rebellious nature has to be restrained. Mastering the dissertation genre is a difficult task for such academics.

Bureaucratic sponsorship of shadow science

The less democratic the society, the fewer reasons to talk about its science in terms of a self-organising, self-governing enterprise. Totalitarian and authoritarian societies possess a feature that can be called "superconductivity": all the subsystems of such societies perceive the commands and instructions produced by party or government officials with little or no resistance. Even verbal requests or indications in the form of cues have the status of commands. This phenomenon is known as the "telephone law" (in the lawless society). In the Soviet Union since 1917, the telephone law conquered vast areas of social life, if not the whole state space, forming a vertical command system.

In such societies, the degree of interdependence is so large that friendship relations and simple acquaintances are often forced to give up all juridical regulations and behave according to the principle, "I am serving you and you are serving me". The implicit horizontal social structures are formal. The society as a whole becomes loaded with numerous ties. In democratic societies we cannot entirely exclude the interdependence of its members but its level is much less.

The following comparison is perhaps possible. The extent of interdependence in democratic societies may be viewed as a minimal

energy phenomenon of quantum mechanics, when the wave function of a certain system is equal to zero but the energy of the system cannot be less than some quantity determined by Planck's Constant. In democratic states the level of interdependence is defined, for instance, by psychological or interpersonal realities, rooted ultimately in human nature.

What about the concrete bureaucratic structures that hold rein over science in the Soviet Union? To begin with, one can name some bureaucratic power structures which represent special units of administration conducting the policy of the Community Party of the Soviet Union (CPSU). The ranks of the CPSU, from the Central Committee to local committees, contain such units, often headed by persons totally ignorant about the organisation of scientific issues. The official departments are powerless. The power — though decreasing — is still concentrated in CPSU committees. Analogous departments are found in the Ministry of Higher Education (now the State Committee of Education) or the State Committee for Science and Technology, but they are of minor importance.

All major decisions — from official journeys to the so-called capitalist countries, to 5-year renewal rules — are taken only in accordance with the opinions of officials with the CPSU committees. Even in cases when agreement by the party committee is not necessary, the committee may still express its "own opinion" and, more likely than not, the decision will reflect it. During the previous two years, party influence on the scientific media has diminished, but the degree of this diminution is not proportional to the decline of the general authority of party officials among the people due to various perestroika processes. The influence on scholars often remains noticeable. Moreover, this influence is effectively used by scholars who know the ins and outs of the party apparatus.

A two-sided movement has taken place. On the one hand, the apparatus has done its best to recruit loyal (more precisely, obedient) representatives among academics, and the latter readily appeal to party power. The by-product of such collaboration is various "coordinative councils", which aim to usurp rights to rule over science, to become some sort of structure on top of academia.

The party and state officials vigorously exploit the means for obtaining academic degrees more easily than everybody else, for they join the category of practical research workers who possess numerous advantages over pure academics. In addition, they fully use their positions as representatives of power.

A rather typical example is Mr X, who was going to enter the doctorate for three years but was offered a party post and immediately accepted it. In no time after becoming a party official, he initiated a campaign to gain his DSc. Moreover, being young, he chose

the type of public defence available only to elderly and eminent academics. He succeeded without the three years required for finishing his doctoral dissertation.

By the way, I should add that the party apparatus represents, so to speak, a non-classical bureaucratic structure: the classical bureaucracy is known by its red tape, procrastination, and formal instead of informal treatment of various affairs. The peculiarity of the CPSU bureaucratic machine is that its actions strongly depend on the concrete situation and the internal interests of the apparatus. The apparatus can and does use procrastination. Meanwhile, when the apparatus has any interest in an issue, all sorts of problems can be solved in no time, without any formalities. The impossible becomes possible; all channels are opened. All laws and norms of the scientific community are abandoned, and no criticism is accepted. This is the style of action used not only by CPSU officials but by almost all closed state social structures, ministries, committees, etc. This is the style of life of the *nomenclature*.

The scholar L tried to publish his book. It was severely criticised by the publishing company reviewer. He appealed to the apparatus for help. The apparatus appointed new reviewers and organised discussion of the book in the local scientific community. The reviewers and the local community appraised the manuscript as incompetent. Mr L didn't lose his temper. He finally found previously unknown supporters in a high party office, who simply ordered the book published. It was actually published in record time. How did he manage to enlist their support? At this point I collide with one of the characteristic features of shadow science, worth describing in more detail.

The roots of shadow science

As a matter of fact, the publishing of the monograph provides a way to obtain a DSc with all its possible consequences (high salaries, posts, etc.). That's why the struggle to publish a book by any means often is a virtually desperate operation. This to a large extent explains Mr L's desperate undertaking and appeal for CPSU support.

This support is impossible without several features characteristic of shadow science. The first one, already mentioned, involves a high-ranked person exerting power over particular scientific realities. A second one is the piercing force of a certain set of officially certified reviews by some eminent and, if you are lucky, high-ranking academics.

The mechanism of the first feature is quite obvious: the telephone law may do the trick and often does. The same mechanism works when your close colleague expresses a desire and you are forced to act accordingly. Today I'm doing something for you. Tomorrow I wish your favour. If you neglect the request, there is a risk that all kinds of

relations with higher-ups will be broken off. Only a few can escape the process of prudential acquiescence and adopt the course of opposition rather than accommodation.

The proportion of "apostates" amidst the total number of academics is identical to the proportion of science amidst what is only an imitation of science. Nonconformist scholars officially find themselves in the position of inconvenient persons, especially for the administration.

The second factor — officially certified reviews — is perhaps specific to Soviet society. It makes the operation of the first factor easier, or entirely replaces it. Sometimes it is hard not to acquiesce in the face of reviews sanctified by state seals of noteworthy academic institutions. The reviews effectively work in any kind of situation. If the work has only just started on its path to publication, then such reviews ensure that it will pass without a hitch. If the manuscript was already rejected by reviewers, then new reviews enable it to begin another ascent to the printing house.

On another case, one scholar confided the secret for publishing articles: "You ought to get as many 'positive' reviews as you can, for instance ten laudatory reviews for every critical review. As far as I know, no publishing company would fight against such a proportion."³

Let us imagine the following situations. (1) The manuscript of academic N is discussed by official reviewers (and colleagues in general), who are highly critical of the work and think it should be improved. At a suitable moment, the reviews of eminent scholars, obtained beforehand, are extracted. They cause disarray: "We are not able to oppose such eminent names"; "We will not be correctly understood by these figures"; "It is better not to intervene."

(2) A manuscript by Mr N was negatively appraised by 12 members of his department. Mr N collects other reviews. When a sufficient number of these other reviews are obtained, he demands that the decision be reconsidered, although the work remains unchanged. The department members feel spited. The department refuses to reconsider its decision about the incompetence of the work. The additional reviews serve as a pretext for an appeal to CPSU officials on the ground that local academics are ignorant. The officials pass a resolution to publish the book in a roundabout way in violation of common rules of academic publication.

Several years ago the officials offered no explanation for their actions, since they considered themselves sovereign masters of all possible situations. But in the past one or two years they have tried to support their resolutions with the argument that a plurality of opinions is desirable. One opinion cannot be spoken, though: that a party protege is incompetent. They seem oblivious to the devastating and undermining effects of such actions on the ethical norms of the

intellectual community. The person who stands over the community on the basis of external power feels infallible and authorised to do anything.

It is quite possible for real cases of misevaluation to occur due to challenges made to central beliefs. But works able to undermine a paradigm do not appear every day, and they are not under consideration here. Instead, I focus on unprofessional, incompetent works, written with the prior intention of appeal to forces outside the community. Those who are able to produce something really revolutionary are usually quite capable of showing their mastery over the existing paradigm or, at least, to show elementary knowledge. To my mind, a good indicator of professionalism is an openness by scholars to a wide range of problems, especially those adjacent to their main occupation, and an ability to deal with them. It should suffice, however, to return to officially certified reviews and the circumstances of their creation.

Shadow science scaffolds

It is well known that mathematics, due to its nature, is considered to be a citadel of austerity which leaves no place for nonacademic intrigues. I argue that, contrary to this picture, all the mechanisms mentioned above are smoothly working in mathematics as in the rest of science.

The well known mathematician A. D. Alexandrov, a full member of the USSR Academy of Sciences, was filled with indignation over the following incident.⁴ A mathematics textbook which contained numerous blunders was published in enormous numbers. Such things happen sometimes. The Soviet mathematical community produced many highly critical reviews, for this book was recommended by the Education Ministry as obligatory for teacher-training institutes. Alexandrov complains that the nature of mathematics should prevent such blunders. Far from it! Neither the Education Ministry nor the Academy of Pedagogical Sciences reacted to the negative reviews. The blunders were called "minor instances" which were not essential. This appraisal finds support among some professional mathematicians.

Some time later, a geometry textbook for future teachers was published with blunders, indeed a contempt for accuracy, that taxed the imagination and achieved a sort of record in the mathematical literature. The textbook, once again, was recommended as obligatory for teacher training institutes and all the blunders remained.

One can ask: why exactly was this record-setting book of blunders produced in a system consisting of ministries and bureaucratic state departments? For those who know well Soviet social realities, the answer is obvious: the ministries are fully independent in their behaviour.

Now we are at the point of the holy of holies of shadow science and official reviews. As Alexandrov puts it, "All the critics of textbooks were rejected by the ministry's appointed reviewer, who evidently *didn't read it*. Moreover the criticism was judged inappropriate since it undermined the faith of future teachers in textbooks."⁵ Remarkable argumentation by any standard. But I'm not going to devote attention to this argumentation (it is worthy of special analysis) but to the fact that appointed reviewers "didn't read it" — even after severe criticisms from colleagues of the author, even after a scandal caused by these criticisms.

This is the crux of the matter. The works under review are not read, but at most looked through (for example, to search for a reference to the reviewer's articles). Academics have a severe lack of time, especially in the organisation where the flow of manuscripts, dissertations and so forth is heavy. What sense is there in reading numerous works when they are almost all dull and unworthy of the careful attention that must be given to new ideas? What sense is there in giving criticisms when the author could easily be hurt? (In the state of total interdependence and prudential accommodation, it is not profitable to give criticisms.) If the reviewer feels uneasy in making any critical remarks, then what sense is there in reading?

Here is a very typical picture in an academic organisation: feverish leafing through a manuscript, which in an hour (or half an hour, or a few minutes) will be discussed. This may be the reason why the quality of doctoral dissertations from universities is superior to those from the Academy of Sciences sector.

I have personally seen reviews, with all officially necessary attributes, which must be considered fantastic. Their content — including elementary grammatical mistakes typical of non-Russians — simply demonstrates that they were written by inhabitants of Central Asia or the Caucasus, although the signatures belong to eminent scholars in Moscow. Yet all appropriate certifications were available.

On the other hand, when the review is written smoothly and correctly — and only its content is suspect — it is impossible to prove that its author is anyone but the person whose name appears with it.

The administration in its turn is inclined to unconditionally trust the signatures, as officially certified. They say that false signatories are not placed on reviews easily. Alas, the empirical facts show that they are. Once this fact is admitted, it suffices to stress that it has a devastating effect on the scientific community, including morale, welfare and moral climate.

M. A. Rozov wrote that "at the turn of the 1980s, a well known Soviet philosopher recognised that for a long time he had purchased only books by Western authors. He did not purchase the work of Soviet scholars and did not read them. 'Not without exception. I read your

works,' he said, being polite. 'I am also reading your papers,' I answered. Now I should honestly recognise that I never read them."⁶ If published books are not read, what can be said about the unpublished, namely those on the way to the printing house?

I was told by several mathematicians that during a public defence of a candidate dissertation (for PhD), the author Mr A, supposedly a mathematician, was not able to define a hyperbola, which he had repeatedly mentioned shortly before. This and other evidence suggested that he was virtually ignorant of mathematics. After the secret vote — the necessary procedure for every public defence — the result was half in favour of Mr A and half against. As well as asking how Mr A reached the stage of public defence, we should ask why half the Academic Council voted for him. The reasons probably lie deeply rooted in the psychosocial climate of Soviet-style science management.

I have given evidence of the importance of shadow science within Soviet science — or, should we say, imitation of science? In any case, the result is dull science, and the only product of dull science is more dull science. Rather than producing scientists with lively minds, they are produced with a death grip.

NOTES

- 1 M. A. Rozov, "Is philosophy a part of the scientific community?", *Voprosy Filosofii*, 1988, No. 8, pp. 23-36, quotes on pp. 23, 29 (in Russian).
- 2 V. P. Skulachev, introduction to G. N. Gilbert and M. Mulkay, *Opening Pandora's Box* (Cambridge: Cambridge University Press, 1984), Soviet edition, 1987, p. 4 (in Russian).
- 3 Yu. N. Davydov, "Reflections on philosophical culture", *Voprosy Filosofii*, 1988, No. 3, pp. 57-70, quote on p. 70 (in Russian)
- 4 A. D. Alexandrov, "Truth as moral value", *Science and Values*, Novosibirsk, 1987, pp. 27-43, see pp. 36-38 (in Russian).
- 5 *Ibid.*, p. 37.
- 6 Rozov, *op. cit.*, p. 23.