

Academic Authority in Post-Soviet Russia

This Forum focuses on an article by Mikhail Sokolov, ‘The Failed Consolidation of Academic Authority in Post-Soviet Scholarship’. While Sokolov himself offers a case study of one particular discipline — sociology, the problems that he discusses (lack of international recognition, a chronic absence of universally accepted ‘indicators of peer esteem’) are endemic in the Russian academic world generally. Sokolov himself diagnoses what he sees as a set of chronic problems, and poses questions at the end of the article about how Russian academia can overcome its isolation and adapt better to the changes that have taken place internationally over the last few decades. He does not attempt to answer these questions himself, but the participants in the discussion were invited to consider both the case made by Sokolov, and possible solutions to the problems that he identifies.

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The Failed Consolidation of Academic Authority in Post-Soviet Scholarship: The Case of Sociology

Imagine that the Ministry of Science and Education of the Russian Federation were headed by an official who saw the chief aim of his earthly existence as being to secure a leading role for national scholarship across the world. When he compared ambition with reality he would inevitably conclude that thoroughgoing reforms were needed. Russian scholarship today is not only failing to gain in stature, it is in the process of losing the stature it once had.¹ If our official's thoughts were to run along the same lines as those of most of his counterparts around the world, he would come to the conclusion that the essential problem lay in the inefficiency of the system by which resources are allocated among scholars. He would probably tell himself that the greatest result could be achieved if maximum opportunities were concentrated in the hands of the most gifted, and conversely, if talent and achievement (and only talent and achievement) were always to receive a timely reward. The job that would be left to be done seems to be a purely technical one: how can the most gifted be distinguished, and the most important achievements identified?

It is precisely here that the essential problems arise. The hero of our story would probably suppose — like most representatives of Western societies, excepting the few admirers of Feyerabend and Latour — that it is only scholars themselves who can give a just assessment of what their colleagues have achieved. It is they who must indicate who amounts to what in the world of academia. Our official grasps with relief at the well-known fact that scholars are continually distributing all kinds of status symbols

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¹ This fact has been raised more than once in reports by the real, not fictional, Ministry of Education and Science, with reference, for instance, to the continuing decline in the quantity of publications by Russian authors in the leading scholarly journals (according to ISI): from 2.9% in 2000 to 2.1% in 2005 [Ministerstvo... 2007: 44].

among themselves, which must — isn't that so? — serve as such indicators for the uninitiated. But were he to take a more careful look at these symbols, and discover what academics really thought of them, he would certainly be disappointed.

There are academic degrees that divide the world of scholarship into Doctors of Sciences, Candidates of Sciences, and the rest. Sadly, a study of what has been termed the 'market for dissertation services' [Kalimullin 2006] or a single visit to a site offering 'ready-to-go dissertations' can permanently undermine one's faith that the possession of a higher degree necessarily stands for any scholarly merit at all. And such doubts can only be strengthened by conversations with scholars. Right from the mouths of those who possess these degrees our official might hear things like this: 'I feel a bit uncomfortable about the fact I did [a doctoral dissertation]. These days people reckon you need to get a Cand.Sc., but the only ones who do doctorates are administrators who have cosy relationships with academic councils, not real scholars.' The same is true of all the other symbols of academic rank, from undergraduate degrees (although the official is unlikely to start with any illusions as to those) to membership of the Academy of Sciences (concerning which he will inevitably be told, for instance, that 'most of those who've been elected to it in sociology aren't sociologists at all, they're... God knows who, really').¹

The official might try approaching the problem from the other side — by looking for recognised scholars among the boards and presidiums of professional associations. Members of these bodies are elected, and large numbers of academics take part in the voting; so one might assume that those to be elected would be those who enjoyed a general reputation. But there are reasons for doubt here too. It turns out that in many disciplines there are several professional associations, sometimes openly fighting among themselves, and that even so none of them succeeds in including the majority of scholars. Thus, there are numerous associations of sociologists, with various political and intellectual loyalties: the Russian Society of Sociologists, the Russian Sociological Association, the Community of Professional Sociologists, the Union of Sociologists of Russia, M. M. Kovalevsky's

¹ Quotations from interviews are given in italics. Readers who are familiar with Russian sociology will themselves be able to check how far the list of members and corresponding members of the Russian Academy of Sciences corresponds to their conception of academic achievement by first compiling their own list of 12 names that deserve the highest symbol of honour and then comparing it with the following: Iu. V. Arutyunyan, V. I. Boiko, M. K. Gorshkov, A. V. Dmitriev, V. I. Zhukov, T. I. Zaslavskaya, V. I. Ivanov, V. N. Kuznetsov, N. I. Lapin, G. V. Osipov, M. N. Rutkevich, Zh. T. Toshchenko (academicians and corresponding members as of May 2008 in the Philosophy, Sociology, Psychology and Law section and the Economics section (Zaslavskaya), listing sociology as one of their areas of specialisation). Ill-wishers could also tell our official a number of anecdotes tending to harm the Academy's reputation, for instance, the story of how easily major politicians have become academicians in recent decades — sometimes only five years after receiving an extra-mural degree.

Russian Sociological Society, and others.¹ Depending on which association the official picked, he would receive a different selection of leading scholars.

Or the official might turn to Western experience and try relying on citation counts, which are supposed to give the most precise assessment of real intellectual influence. But the same problem arises here as with the professional bodies: the result will be entirely predetermined by the initial choice of journals to study citation in. To return to the example of sociology: articles by people who call themselves sociologists appear in several hundred periodicals, most of which are published by specific institutions and have their own circles of writers, to a great extent drawn from people working at the institution concerned. Their staff offer articles to the editors of ‘their’ journals, read only these same journals, and, as a result, refer to other scholars who fall within the same ‘attention space’ (to borrow the well-known phrase of Randall Collins). To choose the *Papers of the University of X* as a publication in which a citation will ‘count’ would automatically guarantee academics at that university a favourable index, while its exclusion would, equally automatically, reduce their ratings — sometimes to zero.² It would be possible to include all the sociological titles, but this would immediately raise two troublesome problems:

- (a) it is difficult to distinguish sociological journals from non-sociological, and even entirely non-academic (should citations in *Neprikosnovennyy zapas* or *Ekspert* be included?), and
- (b) an obvious counter-strategy on scholars’ part would be the formation of ‘citation cartels’, whose members would refer to one another; and the victors in such a competition would be the largest

¹ Probably the highest-profile clash between Russian scholarly associations was provoked by the continuing conflict around the dean of the Sociology Faculty at Moscow State University, Dobrenkov, who is known for his extremely conservative political views. He was accused of plagiarism and of excluding students for political reasons. The Russian Society of Sociologists adopted a statement expressing concern at the situation and calling on the university’s rector, Sadovnichy, to take measures to ‘normalise’ it; and members of the Society’s board publicly called for Dobrenkov to be dismissed. At the same time the Russian Sociological Association, of which Dobrenkov was president, spoke up even more decidedly in his defence. Each association accused the other of serving the political interests of its leadership.

² A few figures, characterising sociology once again. The proportion of authors who represent the institution that publishes the journal exceeds 90% for the majority of these ‘Papers’ (a category to which 25 of the 58 journals entered in the VAK [Higher Academic Commission] listing for sociology belong), comes to 35% or 40% for some major publications (the *Sotsiologicheskii zhurnal*, the *Zhurnal sotsiologii i sotsial’noi antropologii*), and falls to 10% or 15% only in the veteran *SOTSIS* — and even in *SOTSIS* a full 47% of authors are from Moscow (the figure is also 47% for the *Sotsiologicheskii zhurnal*, while 61% of *ZhSSA* authors are from Petersburg, where the periodical is published). Meanwhile, 65% of all references to journal articles appearing in *SOTSIS* over recent years are references to articles in *SOTSIS*, and no more than 4% represent references to any other single publication. A similar picture is visible with other journals. These figures have been kindly made available to the author by Katerina Guba (European University in St Petersburg), who is researching the journal system in Russian sociology.

and most cohesive groups — which by no means necessarily means those with the greatest creative potential.

In total despair our official might look to Western scholarship, which has presumably proved its ability to cope with all these problems. Why not take recognition in Europe and the USA as the basic indicator of achievement — a simple extension of the field within which their status system operates? A certain proportion of sociology scholars in Russia, including the majority of likely readers of this article, would undoubtedly welcome such a decision. But in itself this approach is pregnant with a number of problems. Firstly, ‘international recognition’ is a much more diffuse category than might appear at first sight. The systems of status symbols employed in Anglo-American, French, and German scholarship (to take only the three most important instances) are not at all equivalent, and it is unclear how a decision should be reached to focus on one or another of them. It is unclear, further, exactly which symbols of recognition should be preferred: citation indexes? The total budget of joint projects in research and teaching? Regular participation in international conferences and long service on the boards of international associations? Unexpectedly for many, these criteria — at least in the case of sociology — yield different lists of ‘recognised scholars’.¹

Secondly, the general level of recognition of Russian scholars in the social sciences is too low for it to be possible to extract any guidance from it that would be useful within the framework of existing institutions. You couldn’t put together even one dissertation council (in Russia, this is supposed to consist of at least twenty members) from people who can be more or less described as ‘internationally recognised’, whatever criterion to justify the term might be chosen. Thirdly, even scholars who are very far from being nationalistic might see certain theoretical defects in such a decision. If we recognise that social theories owe their emergence to a social context, and that the aim of their creation is to enlighten those who are embedded in that context — as the majority of modern sociologists would probably accept — then the idea of lending the character of a decisive verdict to assessments arising from a completely different context becomes less convincing.²

¹ The various types of international cooperation pose different minimal requirements for participants, and utterly different people are correspondingly drawn into them. Teams carrying out comparative studies need a smooth-running machine for conducting surveys, and hence here directors of research centres with a good network of interviewers are placed *en valeur*; in teaching institutions, it is the deans of big and stable faculties who appear to advantage; editors of journals and at publishing houses prefer authors who write on topics that interest potential readers; and the boards of professional associations are the domain of energetic individuals with great communication skills who are also tireless correspondents. Individual scholars and organisations combining all these skills and qualities are, however, practically non-existent; and all the symbols of recognition are never conjoined in the same hands.

² The extensive literature on ‘academic dependence’ and ‘cultural imperialism’ provides a significant number of arguments in support of this thesis (e.g. [Alatas 2003]).

Having failed to find any ready-made indicators from which it would be possible to compute scholars' level of achievement, the official might have a go at creating new ones. In fact, the Ministry of Education and the Russian Academy of Sciences have carried out masses of work in this line, publishing all manner of ratings of universities and institutes based on indexes that government experts think might compensate for the weak points in the most familiar means of assessing academic achievement.¹ But the feverish intensity of these searches itself betrays the perplexity that the bureaucrats of scholarship feel in the presence of the problem that has arisen.

What follows is an attempt to present our official's problem formally, which will make it possible to consider it in a broader comparative context and may even allow certain practical conclusions to be drawn.

2. Crystallised symbols and curator groups

Any form of social activity, from the viewpoint of the observer, can be described in terms of certain characteristics. Firstly, it can be more or less *open*, when openness means the absence of obstacles to observing it. Secondly, it can be more or less *transparent*, where transparency means an understanding of the principles of organisation that allows what is taking place to be interpreted without difficulty. A viewer who does not know the rules of football, but who happens to be at a match, will be faced with an activity that is open but that to him is not at all transparent; a coach, trying to work out what strategy his rival is developing for his team, is trying to penetrate an activity that is entirely transparent for him, but almost completely closed. Thirdly and lastly, the activity can be more or less *relevant* — engaging some or other interests of the observer.

The history of scholarship is the history of how its world has become ever less transparent to external observers, but, at the same time, ever more relevant. The more important scholarship has become, the less comprehensible it has been to non-scholars. This process has been accompanied by a transformation in the interaction between the academic world and its social surroundings. One side of this transformation has been the erection of barriers that sharply divide 'scholarship' from 'everything else'.² The other side, which logically completes the first, has been the development of a signalling system that allows scholars to inform non-scholars of such things as they

¹ Three well-known examples are the university rating compiled by RosObrNadzor, the system of 'indicators of the results of scholarly activity' (IRSA), introduced by the Academy of Sciences, and work on the Russian Index of Scholarly Citation (RISC). The two first are based on adding up points awarded for a long list of varied indicators, which are meant to even out the imperfections of each indicator in isolation; a little more will be said below about the third.

² On boundary work see [Gieryn 1983].

have thought it necessary for them to know. Academic status — colleagues' recognition of a concrete individual's achievements — has been something needing particularly effective translation. Scholars' demand that non-scholars allocate rewards and resources coming from without in accordance with the hierarchy of academic merit can only be realised if non-scholars are given comprehensible and unequivocal indications of what this hierarchy is. Academic autonomy presupposes a monopoly on the part of scholars in presenting information as to the relative ranking of representatives of their discipline.

The boundary that separates the senders from the receivers of such signals does not, of course, run only between those who have chosen an academic career and those who have not. Even within particular disciplines we encounter a chaos of research fields and specialisations, whose representatives tend to be thoroughly ignorant concerning the character of one another's work and therefore need hints to help them assess whom it makes sense to listen to, to read, to cite, to invite to lecture, or to lure towards a vacancy on the faculty.

To be effective, academic status needs symbols that can easily be transported into another social or institutional context, symbols whose vocabulary can be understood even by those who will never find out what merits led to their possession. The standard academic CV is a catalogue of such symbols. The degree and the institution that awarded it, the professional positions that the individual has occupied and occupies now, publications in certain journals and publishing houses, membership of associations and posts on their boards or on the editorial teams of journals, research grants received (sometimes including their size in dollars), conferences and seminars in which the individual has taken part — these make it possible to grasp how an individual has been assessed by those who have had the chance to observe him/her at close quarters. These symbols have various levels of transparency for different audiences — greatest of all for representatives of adjacent specialisations, and progressively less for a progressively broader public. As a minimum, some of them are meaningful to the majority of adult members of the given society (for instance, a Ph.D from Harvard). An academic career represents a story of the acquisition of similar symbols,¹ of which the successful accumulation guarantees a significant financial and symbolic yield in the future.²

¹ One of the first pieces of advice I received in my own academic life was *'Before doing something, think how it will look on your CV'*. The life of the scholar is arranged to be as profitably accountable as possible in this literary form. This part of my text is obviously much indebted to the work of Goffman, Garfinkel, and Latour.

² Numerous studies of the economics of scholarship have been devoted to the question, of desperate importance to scholars, of just how significant this yield is (see the survey in [Diamond 1993]). In the 1970s a publication in a leading economics journal raised likely earnings by an average of \$100 per

Symbols, however, do not arise all by themselves. To turn the spontaneous recognition of the value of another person's intellectual contributions into unequivocal and portable symbols, significant work of crystallisation of the former from the latter is necessary. Following Goffman, we can dub the individuals who undertake this work as the *curator groups* of the given symbol [Goffman 1951a]. A curator group can consist of a single person (each of us curates the citations we make in our own articles), of several people who cooperate on a regular basis for a fixed or for an unlimited period (an example of the first would be the board of a professional association, of the second would be the editorial group of a journal), of several people selected *ad hoc* from a certain pool (dissertation committees in the USA), or it can be made up of multiple groups that are independent of one another and are not connected (dissertation councils in Russia). And the processes by which someone becomes the possessor of a status symbol are just as varied. Here there are two poles, one of which being *organic* procedures that sometimes merge completely with other interactions and are not even perceived as independent events (an invitation to participate in a research project) and the other being *autonomous* procedures that tend to be provided with special ceremonies (defending a dissertation or being elected to the Academy).¹

Despite these variations, a general logic can be discerned in the processes by which individuals become legitimate possessors of academic status symbols. Individuals invest their efforts and resources to acquire symbols that will be able to serve them with the greatest success in the future, by ensuring profitable offers on the labour market and colleagues' attention toward their work. In their turn, curator groups select those individuals who are most likely to help raise the value of the symbols they confer.

The career of the symbols demands special commentary. In Goffman's terms, symbols of academic status are poor tests of this [Goffman 1951].² The value of a degree from N. university is determined

annum (while the first such publication meant a rise of the order of \$700) at a time when the average salary of an economics lecturer was somewhat less than \$16,000 [Katz 1973]; [Tuckman, Leahey 1975]. Studies of the 'Matthew effect' [Merton, 1968] have demonstrated the equally blatant operation of the symbolic yield: assessment of what has been said depends significantly, in the academic world, on the status of the person who has said it.

- ¹ Citation indexes occupy a peculiar position: exceptionally for the academic world, they hand the central operations and techniques of their preparation to non-specialists in the field they describe, and thereby create an extra-disciplinary curator group.
- ² Goffman introduces the distinction between 'symbols' and 'tests' in his first published article in order to indicate the distinction, which he frequently drew afterwards, between the 'categorical' and 'expressive' meaning of symbols — between what symbols convey, and what they express, or between what they communicate, and what they exude ([Goffman 1959]; [Goffman 1969]). A mobile phone costing \$2,000 and a diamond ring costing \$800,000 are symbols of belonging to the upper class, but, although they are equally symbols of it, they are not of equal quality as tests of it. The telephone is a poor test, because almost anyone could have bought it on credit, and it only shows that the individual wants

solely by what is known from all other sources as to the other achievements of individuals who have received that degree. Continual migration of academic personnel between universities X, Y, and Z, or the appearance of articles by the same authors in journals A, B, and C lead to the supposition that the universities and publications in question are on roughly the same level, so if we know the status of any part of this equation we can always determine the significance of the others. It is such correlations that allow us to find out what each concrete symbol is worth.

Conferring a symbol always represents an exchange in which the individual and the symbol are acknowledged to be of comparable merit. Symbols, through the groups that curate them, place a stake on individuals just as individuals place a stake on symbols.¹ An unhappy choice of bearers of a particular symbol can discredit the symbol; an unhappy choice of symbols can discredit their possessor. Curator groups here face a decision that is analogous to the one faced by those who aspire to possess their symbols. At the same time, membership of a curator group is itself a symbol of academic status. The higher the status of the symbol, the higher the status of group members (it is incomparably more prestigious to be a member of the editorial team of the chief journal in a discipline than to hold the same position on a journal that is rarely cited, and it is incomparably better to work at a faculty that has turned out a whole *pléiade* of young geniuses in recent years than to work at a faculty that cannot boast of any such thing).² The permanence of the value of academic status symbols depends on the system of balances that this symmetry creates.

3. Equilibrium in symbolic systems

The simplest way to create a model of the exchanges between a curator group and candidates for its symbols is to begin from an elementary case and then to add various complicating factors. To begin with, let us imagine a disciplinary community in which:

to demonstrate that he belongs to a certain group. As a symbol, it is not immune to unscrupulous use in order to mislead. The ring not only conveys an intention, it also offers obvious proof of its owner's class adherence.

- ¹ Such exchanges are not, of course, confined to academia. The procedure by which a man exchanges the right to call a particular woman his wife for an analogous right on her part to call him her husband does not differ in any respect from the procedure by which one co-author exchanges the right to use someone else's surname in a list of publications for the right to use his/her name in someone else's.
- ² It is characteristic that many curator groups include only those who themselves possess the corresponding symbol. Those who decide to co-opt a new member to the Academy or to a professional association usually need to be members themselves, just as those who decide to invite a lecturer onto a faculty must themselves be lecturers there. This configuration creates an obvious stimulus to select candidates who are capable of being an adornment to the symbol. On the whole, however, the relationship between individual members of a curator group and the symbol they confer varies widely, and a theory of the behaviour of curator groups would have to include an analysis of these variations.

(a) there is complete consensus as to what should be considered a scholarly achievement;

(b) agreement automatically arises among the audience for any scholar as to how great his or her achievements are (for simplicity of exposition we will also assume that these achievements can be measured on an interval scale, from, say, 1 being the least to 10 being the most significant);¹

(c) there are many competing symbols of academic status, and there are several symbols that approximate more or less accurately to any level of achievement;

(d) these symbols are binary — that is, they distinguish those who possess the symbol from those who do not, with no gradations in the degree to which a symbol is possessed.²

Each symbol bisects the community into an upper portion — those who have received it or who might have done so — and a lower portion: those whose attainments are not adequate (see Figure 1). Individuals' investments in this system will be orientated towards the acquisition of symbols that stand as high in the hierarchy as seems to be possible for them.³ Let us imagine a researcher, X, whose achievements are assessed by those who collaborate with him at 6.1 points. He has written an article that might be accepted by any of three journals, A, B, and C. Journal A is known not to accept articles submitted by authors of a level lower than 5.2, while the equivalent figure for B is 5.9 and for C is 4.5. Of course, his best choice is journal B — because an article there guarantees that the author's rating is no lower than 5.9. A and C cover a significantly broader field: they do not make the desired distinction between X and those who stand several rungs lower on the academic hierarchy.

A disciplinary community where the above conditions are fulfilled will soon reach a state of *ideal consolidation*, which is characterised graphically by the fact that all the lines that bisect the community are parallel. Two symbols will always describe sets of which one is a subset of the other. If article X can be accepted by journal A but not by journal B, then journal B stands higher in the hierarchy of academic

¹ Scholarship in the English-speaking world may approach fulfilment of these two conditions if the administrative use of citation indexes continues to grow at the rate it has over recent decades and if these indexes become the primary system by which status is calculated for any academic career.

² The majority of academic status symbols are of just this kind: an article is published or is not published in a given journal, a degree is awarded or is not. There are, however, exceptions — symbols that allow individuals to be ranked into several classes ('excellent' degrees; plain degrees; no degrees) or even on interval scales (citation indexes, GPAs). These distinctions, however, do not alter the basic logic of the model.

³ These considerations are based on the assumption that the expense of labour in acquiring each symbol of a given type (a degree, a publication) is roughly equal: someone who is capable of writing an article of level 7 will not save significantly in time by writing one of level 6.

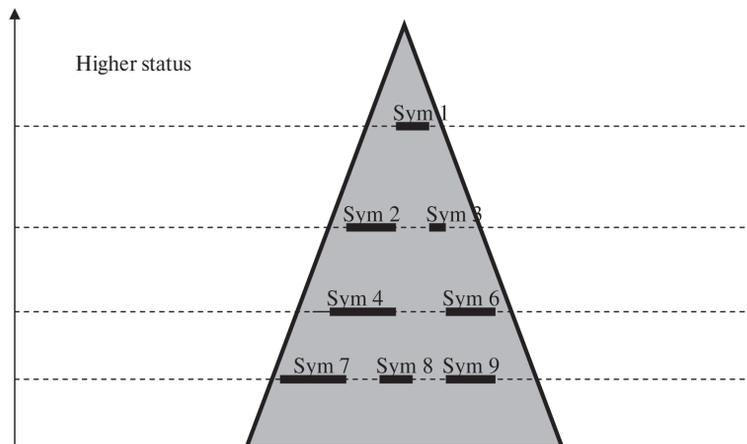


Figure 1. Elementary model of a discipline with ideal consolidation

symbols, and the reverse situation — where an article rejected by A would be accepted by B — cannot arise. A state of ideal consolidation would be a point of balance in the given system. In fact, let us imagine that the editors of one of the journals, whose previous publications place it on level 6, were to accept a number of articles that weren't even worth level 4. As this fact became known to level-6 potential authors, they would probably prefer some other periodical that would more effectively mark them off from their inferiors. A journal that permitted such a failure of judgement would see a fatal decline in the quality of manuscripts submitted to it, since its mistake would be made immediate use of by competitors that had preserved their level. In sum, we can predict the emergence of a strictly stratified system of journals, in which the curator groups of the highest-level symbols receive articles from anyone they want, and all the others from those left over when the higher-ranked have taken their share. The symbols would *de facto* mark their possessors off not only from those below, but also from those above. In the example given above, the appearance of the article in journal A would be unequivocally interpreted as a sign that the author's 'ceiling' was somewhere between 5.9 (the 'value' of the journal in which he failed to publish) and 5.2 (the 'value' of the journal that did accept the article).¹

Publication in a periodical is one symbol that can only be offered within the confines of a pre-established framework: a journal can only print a certain number of articles. The picture does not change

¹ A more complete model would have to take account of the fact that symbols are used not only to assess the marginal level of individual achievements, but also their quantity — otherwise there would be no incentive for someone who had once published in the best extant journal to write anything more at all. This important comment was made to the author by Mariya Yudkevich. Nonetheless, it can be left temporarily on one side for the purposes of the present article.

substantially, however, even when we turn to examine symbols that can be offered to an unlimited extent. Speaking purely technically, a faculty can award as many undergraduate or higher degrees as it likes. But the level of candidates it receives is nonetheless limited by an upper bar. The demand can only be expanded at any given moment by encroaching on inferior universities' territory — but any movement in that direction will lead immediately to a loss of the better students and, therefore, to a drop in the lecturers' status. The only universities that can lower the bar without repercussions, in such a system, are those that already stand at the very bottom of the hierarchy and are known to be willing to hand out degrees to anyone who pays for them. In striving to maximise their status, which can be calculated from the crystallised symbols in their possession, candidates and curator groups reach equilibrium, at which point neither participant in the game can deviate from the strategy 'select the best available symbol' / 'select the best possible candidate' without harming their own position.¹

The elementary model shows only a very distant similarity with the reality we all know so well. The most obvious difference is the presence in the model of consensus as to what constitutes 'good work'. Sociology, which provides the main source of examples for the present article, is often described as a conglomeration of warring 'academic gangs' [Scheff 1995] who feel nothing for each other's work but contemptuous distaste. It would be naive to hope that a positivist of the Lazarsfeld tendency, a devotee of critical theory, and an ethnomethodologist should easily agree on any general assessments.² A more accurate graphic representation of the system of academic status symbols should represent not a triangle bisected by horizontal straight lines, but a set of concentric lines on a surface divided by various rays issuing from a single centre. The division into sectors corresponds to sub-disciplinary fields, research specialities, and theoretical approaches — those zones within the discipline where there are differing ideas as to how good research is distinguished from bad.

¹ These considerations have parallels in the formal theory of social status proposed by Jasso [2001: 99-102], which distinguishes the following varieties: (1) the status of individuals based on quantitative parameters (quantity of money or, in our case, the assessment of an individual's 'professional level' by his/her immediate audience); (2) the status of qualitative characteristics based on average indicators for members of one or another social group (for instance, gender or race, or the group of lecturers at a given university); (3) the status of individuals deduced from the status of categories to which an individual belongs. Since information about an individual's racial identity is much more easily accessible than information about the state of his bank balance (or: information about the university where she works is more accessible than information about her impact factor), assessments of individual status are often made on the basis of what is known about 'people like him/her' in general. These assessments do not lose their significance even when the necessary data for determining status of type (1) become available.

² The fact that they do sometimes manage to do this *in situ* serves as perhaps the strongest argument for continuing to work in the social sciences. The fact that such an improbable event can take place proves there is some sense to what we do.

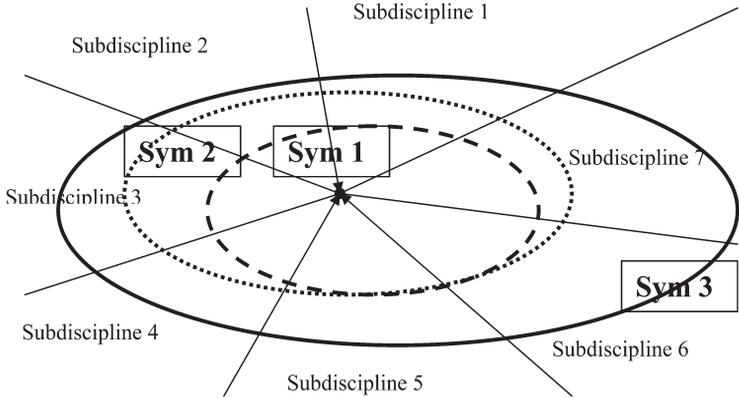


Figure 2. An ideally consolidated discipline of the federated type

We come here to the possibility of drawing two completely different diagrams that will depict two different forms of organisation of a disciplinary community's status system. The first can be compared to a political confederation, and the second to anarchy.

The confederated diagram assumes — impossibly, at first glance — that the various sectors of the discipline continue to use the same symbols of academic status. Each of these zones carries out selection among those who are vying for places that come free in the 'centre' of the discipline, in accordance with the zone's own principles (editors pass articles on gender studies to reviewers known for their work in that field; and in Western universities dissertations in gender studies are discussed by dissertation committees made up of specialists in gender studies). The absence of general criteria for perfection does not destabilise the authority of the discipline, because the symbols of academic status awarded to successful candidates themselves remain the same: a sociology degree from an Ivy League university or a publication in the *ASR* is taken seriously whatever the sub-disciplinary field within which it was received.¹ In this system, academic revolutions remain palace coups. They are staged by the brilliant pupils and younger colleagues of those against whose scholarly authority they are directed. In fact, the development of a successful 'theory group' ([Mullins 1977]; [Wiley 1979]) requires control over the institutional infrastructure, which is associated with

¹ In fact observation of this system sometimes gives rise to the suspicion that the authority of the 'centre' is not necessarily destabilised by the absence of any criteria for perfection at all. The usefulness of any symbol as a test of academic achievement can be determined solely from its 'construct validity' — the convergence of the results it gives with the results of other similar tests. As the example of modern psychology demonstrates, a testing industry can exist and develop for a very long time in the absence of any clear idea at all as to what the tests are actually measuring. It is hard to set anything against these pessimistic doubts except the experience of unexpected mutual understanding with members of rival gangs, as recounted in the previous note.

the highest academic class — access to degrees from the best universities, publication in the best journals, etc. Only those who have all this as their start-up capital can enter the corresponding curator groups and ensure that their supporters are promoted through the academic system, thereby ensuring the success of their movement — and they therefore have no interest at all in the complete annihilation of their own symbolic resources.

The diagram of disciplinary anarchy is much closer to the picture observed by the Russian official we left behind at the beginning of the article (Figure 3). Various sectors possess different status symbols, each of which is seen as having any value only by people who are closely connected with its own curator group. Others refuse to recognise that someone's possessing the symbol obliges them to show the possessor any respect at all. It might even mean complete disqualification in their eyes.¹ The first diagram looks Utopian, but it gives a more or less accurate characterisation of the state of affairs in American sociology. The second is much closer to Russian reality. But the question that needs answering is the question of what determines the approximation of the discipline in a concrete country to one or the other ideal type.

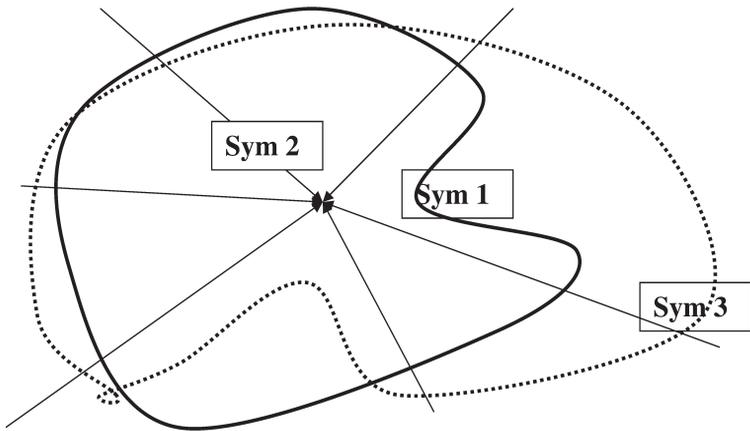


Figure 3. An 'anarchic' discipline with an unconsolidated status system

¹ 'Someone who defended their dissertation in front of N... well, maybe he's OK... but I'm scared he'll be a nutter as well — it's mostly N's students who defend in front of him, and, to put it mildly [smile] that says a lot.' 'Western degrees are all well and good, but you need to know Russian reality as well, you need to spend many years studying it, not just turn up back and straightaway start lecturing everyone. Knowing Western theory's important, but you've got to be able to adapt it, that's the thing. On its own Western theory doesn't make you a good sociologist, not in Russia at any rate.'

We must find a partial answer in the history of the development of the discipline within the boundaries of the national academic system. American sociology developed from one centre, which for decades was the University of Chicago ([Wiley 1979]; [Abbot 1995]). Chicago saw the emergence not only of the first sociology faculty in the new world,¹ but also the first journal (the *American Journal of Sociology*) and the first professional association (the American Sociological Society, now the American Sociological Association). The leading positions were held for a long time by the same people, who worked at the University of Chicago, with other universities as junior partners. The first intellectual oppositions (supporters of ethnographic case studies versus researchers who were orientated more towards statistics) arose within this small group, whose members curated the same symbols. Chicago's comparative loss of influence in the 1930s, and the emergence in the front rank of Columbia University and Harvard, were accompanied more by a gradual transfer of control over these symbols to the latter than by the creation of new symbols: the oppositionists had already invested years of hard work in acquiring the old ones.

It is tempting to explain the development of a confederated organisation of the discipline in terms of the relative smoothness of this path. Some other cases, however, force us to reject an approach that would see the historical trajectory as the only significant factor. The institutionalisation of American sociology took place at much the same time as the great transformation of American medicine described by Paul Starr [Starr 1982]. The condition of the latter in the middle of the nineteenth century might bring tears of glad recognition to the eyes of any Russian sociologist. In Starr's book he will find 'diploma mills' competing to award Doctor of Medicine degrees in the shortest possible time (with the record at six months, extra-mural), and a multitude of competing schools, each with its own theory, explaining every illness on the basis of a single cause and prescribing a single universal medicine for them all (one of these panaceas was mercury), and national associations incapable of persuading even a minority of colleagues to pay membership dues — let alone to accept the need of following any kind of professional code of ethics. In the four decades from 1880 to 1920, however, this chaos gave way to a stable professional corporation with a single, universally accepted status system, one that turned out to be capable of defending its autonomy against the government and the largest corporations. Starr identifies several reasons behind this change: (1) the high-profile success enjoyed by vaccination experiments and other achievements in that field that gradually became medical

¹ Unless one counts the faculty in Kansas, which disappeared almost immediately and whose existence was only remembered several decades later by historians of sociology [Becker 2001].

science; (2) powerful grant support for the leading research universities, connected both with these achievements and with changes in legislation, from magnates like Rockefeller; (3) economic growth and the increase in effective demand for the services of professional medics—demand that was orientated towards degrees from the research medical schools as a standard of quality. Together these factors allowed a group of medics working at the research universities to establish their dominance in the profession and to force others to accept their standards.¹

An example of the opposite is represented by Soviet sociology, which, from the standpoint of status organisation, stood at the end of the twentieth century roughly where American medicine had been in the second quarter of the nineteenth. Paradoxically, its development until the end of the 1980s resembles the above-described experience of sociology more than that of medicine. Soon after its creation, sociology in the USSR acquired an uncontested organisational centre (the Institute of Concrete Social Research of the Academy of Sciences, 1968 to 1972) where the large majority of its leaders worked—leaders who also occupied the key posts in the Soviet Sociological Association and wrote a significant proportion of articles published in the only sociological journal, *SOCIS* (from 1974). Within the discipline there were generally recognised authorities, whose leading position no-one challenged.² The only fault that showed up in the working of the status system concerned how degrees were conferred: many undoubtedly recognised researchers still did not acquire them, while many of the sociologists' Party curators did. In general, however, there was nothing to foreshadow what was to happen later. The starting point does not itself determine the whole trajectory.

Another answer to the question of why one or other type of status organisation emerges refers us to the second fundamental simplification made in the ideal model. It implicitly assumes that the award of a symbol is motivated only by the desire of the candidate and the curator group respectively to add to their crystallised status. It is obvious, however, that in any real act of this kind the transfer of the symbol is only one dimension of the exchange. A university does not simply confer a degree, it also sells it to people who have paid for their education, and supervisors generally bother with students not

¹ For instance, they managed firstly to make membership of the American Medical Association, which they controlled, into the main certificate of quality for medical services in the eyes of the mass of consumers — and then to set six years' medical education at an Association-approved university programme as a condition of membership.

² In fact these people still constitute the majority of those who enjoy nationwide fame. It is interesting that their vision of the past of Soviet sociology and the part each of them played in the development of scholarship has remained the same, despite the ideological conflicts that have subsequently divided them (cf, e.g., the interviews with Osipov, Yadov, and others in [Batygin 1999]).

because they want to assist the latter's professional growth, but because they'll simply be dismissed if they refuse to do it. Exchange can be, and usually is, asymmetrical: the different parties exchange different resources. Take the example of co-authorship. One person grants another the right to use his/her own name in the other's list of publications. When these names are of unequal value, we presume that the *mésalliance* has been compensated for by investments of another kind: the junior author has done a lot of work collecting data to support the senior author's hypothesis, or a graduate student has put forward an excellent idea and the supervisor has lent his name to ensure publication in a much-cited journal, or the boss has found the money for the project within which the results have been achieved.¹

4. Back to the Russian case

The chief Russian difficulty to which this article is devoted has its sources precisely in the excessive dominance of asymmetrical exchanges, in which academic status symbols are either simply sold by members of the curator group or are handed out to as many candidates as possible, because the act of handing them out is either accompanied by payments from some third party² or else becomes an object of barter without any attention being paid to the quality of the work ('our department has always written positive reports on X's students, so now he kind of owes me... a positive report on my dissertation.') The question of how status symbols are organised thus leads us to an investigation of the checks and balances that are intended to make such things irrational. What, in fact, are the conditions that force curator groups to refrain from the tactic of turning the symbols of which they dispose into a source of quick enrichment, albeit at the cost of rapid inflation?

¹ [Merton 1968] showed that the glory also accrues disproportionately to the possessor of the more familiar name ('the book by Latour and... what's his name?'). The junior partner, however, also gains substantially precisely from the background knowledge of how such exchanges are effected. If we can show a work written jointly with Robert Merton in our list of publications, we force people to suspect that we have been in a position to put in sufficient additional investments to make Merton, who had many potential co-authors to choose from, to prefer us. Whatever these investments might have been — we might be hardworking, or original, or erudite, or (if the observer sinks to the level of cynical academic folklore) sexually attractive, — we've clearly got something going for us.

² The budgetary funding of a faculty in a state university is predominantly determined by the number of its students. The main argument during negotiations with the ministry about expanding admissions concerns competition to secure a place. For reasons that lie beyond the framework of the present article (see [Sokolov 2007]), in Russian conditions lowering what is required of students is the most reliable way of increasing competition in social science faculties. This position makes the 'diploma mill' strategy the only realistic one for them. Finally it should be added that the expulsion of students who have once been accepted leads inevitably to a reduction in income — firstly because it reduces the number of students and therefore the funding, and secondly because the ministry regards a significant dropout rate as a sufficient reason to cut the number of places budgeted for.

Several such criteria can be identified on the basis of the model proposed above.

A. For crystallised symbols to have a substantial meaning, there must be a reasonably large and, preferably, geographically extended labour market within which the labour force can migrate freely. In a network where everyone knows everyone, there is little need for formal certificates of achievement: everyone has some idea of what everyone else is worth even without them. Our credentials become important when we encounter people who will rely on them to decide how they should behave towards us.

B. There must be a high level of competition between curator groups offering their own symbols. The status of a symbol must not be fixed — hence, an unsuccessful use of the symbol can lead to a rapid and irreversible loss of its value.

C. The system of returns yielded by possession of a symbol must be reasonably stable, and the gains from it must be sufficient even at the lowest levels to permit investment in attainments that can pay off only in the indefinite future. Agents' temporal horizon must not be limited by the need to get by here and now [Sokolov, forthcoming].

D. Non-symbolic selective benefits accruing to members of curator groups must be minimised by the institutional arrangements. This means, in the most obvious form, that the transfer of symbols to other people must not become the object of paid exchange.

We see that none of these conditions is fully met in Russia. Neither a national labour market nor a national market for symbols exists in more than an extremely weakened form. The feature that has marked the whole development of Russian scholarship is the exceptionally low geographical mobility of our scholars, by Western standards. The acute housing deficit of the Soviet period and the housing prices in post-Soviet Russia, extraordinarily high in comparison to scholars' average earnings, have combined with the inefficient institutions of the property market substantially to narrow the choice both of possible employers and of attainable symbols. Roughly half the university sociology courses in the country are the only ones on offer in the city concerned, and it is only in Moscow and Petersburg that there are several dissertation councils, research organisations, and periodicals.¹ In Russian conditions moving to another city is a very complex and expensive undertaking, and employers and curator groups located in the same place as the candidate for a status symbol therefore have enormous advantages. Their behaviour is significantly

¹ In 2006 there were 98 universities offering undergraduate or bachelor's courses in sociology. Of those 98 fourteen were based in Moscow, seven in Petersburg, four in Novosibirsk, three each in Kazan and Perm, and two each in another ten cities.

less exposed to the influence of competition than the model assumes. Instead of a single national market we have several local, monopolistic markets.¹

The consequence for individuals is a comparatively low rate of return on investments in crystallised symbols. Since they are locked into a local labour market, they can do much better by cultivating relationships with concrete deans than by accumulating symbols that might impress some other deans in other cities. The consequence for curator groups is that they can boost the market for their symbols by depressing the requisite level of achievement without fearing an outflow of the best-qualified candidates, because that would require enormous additional efforts on their part. Returning to our diagram, the bisecting line can be pushed as low as one likes without losing the upper segment of potential consumers. Symbols thus cease unequivocally to denote any level of academic achievement. The very best scholars, and the extremely mediocre, receive the same degrees and work at the same faculties.

Another reason why symbols have not been able to lose their value altogether is that a certain minimal level of return on them is guaranteed by the institutional arrangements, which guarantee bonuses and benefits for possession of higher degrees and make possession of one a necessary condition for occupying certain positions. This arrangement has created and continues to create a minimal effective demand, and readiness to meet it is further strengthened by the numerous rewards for each concrete operation of conferring a symbol. Thus, supervising a graduate student is counted as a teaching stint of 50 hours per academic year; a successful defence brings the supervisor closer to the title of professor or senior fellow; the institution gains points on the ministry's 'results indicator' ('number of graduate students who defend their dissertations within a year after finishing their studentships'); and the likelihood that the dissertation council will be dissolved is reduced. Nothing but

¹ An example of institutional arrangements tending to conserve such a situation is offered by the policy of many universities in not charging their own alumni for dissertation defence. Given the significance of the sums involved (up to \$3,000 at 2008 prices) this guarantees dissertation councils a certain quantity of defences which might otherwise take place at any other university. This practice is usually seen as a charitable measure; in fact it guarantees the faculty that a certain number of above average dissertations will pass through it, so the value of the relevant qualification is not completely eroded by inflation.

An interesting consequence of all this is the territorial segmentation of the discipline, whereby the major lines of internal division pass not between substantive fields or theoretical approaches but between local groups. This pattern was obvious in the case of Soviet sociology, with its 'Leningrad', 'Novosibirsk', 'Perm', etc., schools between which there were no explicit intellectual disagreements but which nonetheless manifested strong loyalty towards local leaders (the best available description of the dynamic of such schools is in an interview with Tatyana Zaslavskaya — [Batygin 1999: 142–155]). The sectors of the diagram, which in Figure 2 correspond to sub-disciplines, in Figure 3 largely indicate groups with a territorial and institutional attachment (Russian Academy of Sciences, various universities under the jurisdiction of the Ministry of Education, etc.).

vague moral doubts prevents members of curator groups from expanding the number of recipients of their symbol, and although these doubts do sometimes win out (more often than purely utilitarian theory would have us assume), they are insufficient to reverse the general tendency of the system.¹

5. Instead of a conclusion

If our official got this far in his consideration, he would have to ask himself some fresh questions.

Firstly, how far can the case he has studied — sociology — be regarded as typical of post-Soviet scholarship? Perhaps there are other disciplines, in the natural and even in the social sciences, where the situation is better, whether because of peculiarities of the historical trajectory or because they innately possess more definite criteria of academic achievement?

Secondly, how far is this situation responsible for the way Russian scholarship lags behind internationally, and, even more importantly, what might happen if the state (in his own person) were to refrain absolutely from conducting any institutional reforms? If the evolution of the status system continues in the same direction as it has followed in recent years, where will Russian social science be in ten years' time?

Thirdly, if he does reach the conclusion that he needs to take decisive steps, are there any symbols of academic achievement he could take as reliable and inflation-proof — even if only as a provisional measure?

Fourthly, if the only solution is to create new academic status symbols, is it possible to create a system that would (a) prevent purchase or exchange, (b) favour intellectual achievement as well as possible, and (c) be possible to introduce without being blocked by an opposition raised by those who controlled the symbols in circulation hitherto?

The author of the present article confesses that he is unable to answer any of these questions. It is up to readers to try.

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Among the many people who have generously shared with the author the ideas and knowledge that provide the basis for the article, he

¹ It should be added to the picture that dissertation councils award the same degree, whose equivalence is determined by legislation — which creates a charming example of the 'tragedy of the commons'. An individual council cannot change the general situation by raising its own standards; it cannot even increase its members' reputations. It can only cut the number of successful defences, bringing its own dissolution closer.

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Translated by Edmund Griffiths

Responses to Mikhail Sokolov

ALEKSEI ELFIMOV

On Discipline, Authority and So Forth

Mikhail Sokolov's article contains much that is interesting and thought-provoking. For the Russian ethnographical (or equally ethnological, anthropological etc.) profession, articles such as that are especially useful, because the ethnographical profession (owing to its specific professional viewpoint, traditionally aimed at 'others'), is happily unaware of itself even during those rare moments when it is fully conscious. Indeed, ignorance is bliss. However, as the author hints, in fact, in a community, everyone knows everything. Or to be more precise, not in *a* community but in communities. For there is no single overall corporation as such — production is scattered and, as with many other things in Russia, we are living through a kind of transitional period to a post-Fordian landscape, a disintegration into parochialism, where each producer is as trying to scabble through as best they can.

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Indeed, one must agree with the author that ‘Instead of a single national market we have several local, monopolistic markets’, a situation that leaves characteristic traces on the problem of academic authority. As I see it, in the current Russian context we should be discussing not so much local ‘markets’, as local ‘parochial localities’ (here I am in a way following on from the author’s own reflections). We can hardly say we are dealing with ‘markets’ given that the selection of scholarly staff is almost always, just like the selection of mayors, governors, and other official creatures great and small, made behind closed doors. Local scholarly structures (and today they are all local) are no more than pseudo-corporations, mainly serving the interests of their high-ranking backers, and free selection within them is seen as dangerous (to the survival of these high-ranking backers). I am not making an insinuation that something ‘seditious’ is at work: I am not intending to speak judgementally, but to make a neutral assessment of the problems — and effects — of the structural conditions in which the scholarly community in Russia lives.

To begin with, given a situation where horizontal (as well as geographical and social) mobility, as the author correctly notes, is highly constricted, security, rather than mobility, becomes the ideal (and, of course, people have been familiar with this ideal from Soviet times). In fact, in conditions where the prospect of movement is blocked by all sorts of obstacles, including the requirement to register with the authorities in any city one moves to (that insurmountable remnant of serf law), actually getting permission to settle somewhere for good looks to many people like an opportunity to be grabbed with both hands. Really, how could you do better than that? But as scholarly organisations fill up with employees who have such ideals (reasonable though they may be from the point of view of career interests), these organisations turn into ‘endogamous’ one-world-view collectives, in which stability and lack of internal change become absolute priorities, overshadowing the scholarly mission of the organisation.

A second vital factor is that in these conditions, actual academic competence, important though it may be, somehow becomes a secondary factor in selection. The first factor under consideration is loyalty. (In this context, the following observation by Sokolov looks like a statement of the obvious: there is a ‘territorial segmentation of the discipline, whereby the major lines of internal division pass not between substantive fields or theoretical approaches but between local groups’.)

In turn, since the role of the ‘scholarly’ factor is weakened with regard to the selection process, the inevitable result is the relaxation of various other restrictions (for instance, those governing the

activities of scholarly councils, the distribution of degrees and other status symbols, and the emergence of scholarly authority at a local level). In other words, this is a system where there are well-trodden paths to survival (after all, academic life has the same existential characteristics as life of any other kind, though it may have its own idiosyncratic variations in terms of detail). This does not in the least mean that the intellectual component as such in scholarly communities is restricted to vanishing point; it means only that there is a huge tension between intellectual and career concerns. Precisely the same thing is true of scholarly communities in the USA and other countries. With 'us' it is expressed in a particular way. In other countries it appears in different guises, many of which we are lucky not to know about (or have to experience).

Scholarship (and education) as a sphere is, of course, an institution that exists in many parallel and perpendicular dimensions, which overlap and intersect one another without any particular symmetry or elegance. The desire of intellectuals to develop forms of scholarly discourse that are stimulating and fit for the purpose, and to direct perception of the surrounding world, forms just one of these dimensions (and, probably one not lying in a position of top priority). Another, perhaps more important, dimension is the state management of scholarship as an instrument of support and legitimacy with complex geo-political potential. There is also the dimension of corporate management of scholarly knowledge as a product (particularly keenly felt, for example, in the USA). Finally, there is the 'social lift' dimension, which sets criteria and standards which do not work effectively in other dimensions, but which still have impact on the means of forming academic authority, because academic authority in fact emerges from status symbols received in any dimension.

The last dimension, that of the 'social lift', is equally as influential in Russia as, for example, in the USA, where the rhetoric of the marketing of academic degrees is very strong, i.e. the ideology of positioning a university degree as a ticket to a social sphere of enhanced prospects and salaries. (A characteristic expression of this is the system of formal and informal rating of universities and even faculties from the point of view of 'the best value for your money' — this is, of course, the same thing that Mikhail Sokolov is referring to when he points out that 'a university does not simply confer a degree, it also sells it to people who have paid for their education'.)

In Russia, where none of this has been formulated in official terminology as it has in the USA, but is the subject of assiduously acquired behind-the-scenes knowledge, the sphere of scholarship and education still experiences a powerful 'drive' from below. Just as in the US, this drive is not connected to the internal goals of scholarship as such, but to the fact that academic education continues

to offer, as it always did (or at the very least, continues to seem to offer) access to the ‘social lift’. In fact, you can go for a ride in this lift without any connection to the goals of scholarship. We should see this not as a negative, but instead as a positive occurrence, since as a result masses of educated people enter society, one way or the other. But the problem is that the current system of scholarly education in Russia is constructed not just so as to allow people to ‘ride the lift’ more or less for nothing (i.e. to expend little or no effort on scholarly activity proper), but also so that these ‘passengers’ are allowed to pass freely into scholarly structures and, above all, positions of authority in the academic system.

All this has a strong erosive effect on academic life (and again, in several dimensions of scholarship as a sphere at once), because there comes a point when, to quote Stalin, ‘the cadres decide everything’. But once again the problem is not at all to do with ‘the cadres’ themselves. It is a structural problem, and in many ways it is maintained by the hierarchical system of status, encouragement and progress, which — to put it crudely — was holding our society to ransom back as far as Nicholas I’s reign (indeed, back to goodness knows when). The hierarchy of ‘*diplomant-kandidat-doktor-chlenkor-akademik*’ [diploma-candidate’s degree-doctoral degree-corresponding member-full member of the Academy of Sciences] which perplexes our American colleagues, is as inflexible as a ruler. In turn, the continuing existence of the hierarchy is dictated chiefly by this powerful ‘social lift’ dimension. Gaining favour in the hierarchy (which also requires behind-the-scenes knowledge) is much easier without links to scholarly activity as such. (elections to the Russian Academy of Sciences are so much empty theatre, and they are reproduced on a micro-level in local *kandidat* ‘elections’, or conversely, looked at the other way round, are the highest embodiment of what takes place on a local level.)

In the USA, in the scholarly sphere, as in many others, the dominant focus is on pragmatism, practice, and evaluating practical achievements (in which, in a deeper sense, we can see the role of precedent, not formal statutory law, in legal judgement). Hence, in the career of any academic (whether scientist, social scientist, or specialist in the humanities), there is only one formal status threshold — a PhD, which has taken on the role of an initiation and transition ritual. It is possible to gain a doctorate both by being engaged in scholarly activity and without being so — it does not matter. What matters is that in moving to a new plane you are (from the point of view of your ‘pursuers’) forever on a level with all the rest, and your subsequent progress is connected only to your *practice*.

In the Russian system progress indirectly extends further, right up until you receive your pension, by a chain of formal status transitions

and the addition of ‘gold stars’ (although, as is written in the scholarly rules, ‘gold stars’ are supposedly given for practical services, in fact everyone knows that ‘gold stars’ and practical services are actually things located in different dimensions). The difference between these two systems is characteristically expressed in a mass of slight nuances and specially-placed emphases — for example, in the way an author is presented in journals and books. In America, an author who refers to possessing a PhD generally sinks in the reader’s estimation (in the rare instances where this indication is present, it is apparent that the author is a neophyte, having only just received the degree), and the blurb says a lot about the author’s place of work, his or her research interests, and main published works. In Russia all this is generally not mentioned, since it is immaterial, but we all know how lovingly authors embellish their biographies with ‘candidate of historical sciences’, ‘doctor of sociology’, ‘Corresponding Member of the Russian Academy of Natural Sciences’ and other such badges or supposed badges of rank.

To me (when I come across some publication by an unknown author), these indications say *absolutely nothing* about the competence of that author, and instead I would actually like to know about his scholarly interests and main works. But I understand that in the ‘service’ system, where by and large the priority is obtaining badges of rank; representations of authority must be formed on other principles.

Therefore in ironic response to Mikhail Sokolov’s remark that ‘sadly, a study of what has been termed the ‘market for dissertation services’ [...] or a single visit to a site offering ‘ready-to-go dissertations’ can permanently undermine one’s faith that the possession of a higher degree necessarily stands for any scholarly merit at all,’ I can make only two comments. Firstly, not sadly, but on the contrary, *happily*. Secondly, there is no truth in what he says, nor should there be. Having a degree does *not* signify (and should not signify) the presence of any scholarly merit — a degree is simply a formal licence for professional (not in the sense of ‘high-quality’, but in the sense of ‘relating to a profession’) research work in a particular academic area. For this reason there should be *just one* of it. But, since in Russia a degree is not a licence to practise, but a false result of practice, a badge of rank (also performing the function of differentiation) and since for the time being there are enough such badges of rank to fill a person’s whole life and career with dreams about them, everything remains, in our system, just as it always was.

I would also like to add to the following to the entirely accurate opinions in the article. Undoubtedly, in the real processes of forming academic authority, not only internal factors, but also external ones are mixed up, and the Russian scholarly community (or communities) can hardly be seen as a balanced or in any sense self-sufficient system.

The presence of ‘the West’, as always, provokes a definite internal imbalance, and strong external academic gravitations constantly create ‘disturbance’ in this system, causing ebbs and flows.

On the one hand, along the channels of ‘our’ system it is quite traditional for some latent knowledge to circulate (possibly ‘idea’ is a better word to use — but, perhaps, it is even ‘assimilated knowledge’) relating to the fact that the centre of the concentration of intellectual scholarly capital is actually beyond its bounds. If that is the case, then everything that happens in our system in some way devalues it in our eyes. On the other hand, in the world-view of an intellectual working in our system and for various reasons not satisfied with it, there is always the potential escape to a ‘correct’ system, which also has a very specific effect on vectors of thought and vectors of activity. In the world-view of a scholarly worker in the USA, for example, there is no such *escape*,¹ and dissatisfaction with one’s system (which in the USA is in actual fact hardly at a lower level than in Russia) cannot be settled by external means.

As regards a comparison of disciplinary structures and the principles of generating authority, the schemes put forward by Mikhail Sokolov seem generally logical. However, in many respects a comparison of this kind is not entirely worthwhile, although useful in pedagogical terms, because, for example, the relative harmony and logic in a confederative disciplinary structure in the USA, being observed on the surface, conceals a mass of awful problems. There is the hyper-inflation of degree-holding specialists, the nervous self-censorship of the academic community, the very specific discursive ‘clamp-downs’ and ‘blocks’ (it may seem to be a paradox, but in a community which is historically more psychologically sensitive, there are more psychological blocks; as they say, a big ship needs deep holes). All these factors, in the extremely competitive environment which exists in the USA, bring the strongest misrepresentation of what we might see as more or less reasonable principles of forming academic authority.

Furthermore, as far as I can judge, the problem of academic authority in the USA is no less acute than in Russia (and was revealed in acute form earlier than in Russia). Clifford Geertz — and not him alone — noted more than a quarter of a century ago that agreement on what constitutes scholarly authority has hopelessly diminished. For the basis on which that kind of authority rests, to which we are accustomed, has become a thing of the past, as he succinctly wrote, together with proper bathrooms, comfortable carriages and many other things, including ‘old books’ and ‘old manners’. Indeed, if we begin to think about what we base our scholarly authority on, familiar

¹ In English in the original. [Editor].

to us according to the ideals of high modern ages (read: ‘the ages of modernism’), we will probably have to make many unexpected discoveries.

In a confederative model of disciplinary structure with an agreed academic authority things are just as complicated as in an anarchic one (in an anarchic model all problems are visible; in a confederative one they are ‘ritualised’). There are, as Mikhail Sokolov appropriately calls them, different ‘supervising’ groups with their secret understandings, in whose ‘chess-like’ interaction, so to speak, there is a logic of authority as a constantly shifting and contested point. In this structural model, for the average scholar there can be (and in fact there is) as much that is ‘unfair’ and even ‘outrageous’ as in an anarchic structure; but, in my opinion, it is nevertheless preferable to the anarchic one and seems to be a more advanced level of academic interaction.

Meanwhile this kind of interaction becomes possible, probably, when the defined intellectual ‘critical mass’ is reached (the ethnographic community in Russia, for example, is still very small and far from the point of saturation). In this sense, a surplus of university-educated staff, however painful it might be in other respects, is unfortunately essential for high-quality saturation of the community. But the issue of preparing staff, unfortunately, leads us to the sadly well-known issue of resources — the most diverse resources, including the same scholarly literature (in American university libraries, even those outside of the ‘top ten’, ethnographers have access to a collection of literature the likes of which only three or four central universities — alas, as always in Moscow or St Petersburg — can depend on).

Without commenting on other disciplines I must remark that the situation in Russia with regard to access to recent secondary literature in anthropology and ethnography for students, both undergraduate and graduate, (not to speak of their teachers) of modern literature is best described as hopeless. Mainly our ethnographers (I emphasise, *mainly*) continue to use the literature of the 1960s-1970s (and also 1930s-1950s) and contend with concepts that have long been ‘passé’.

What can be done so that the situation depicted by Mikhail Sokolov may begin to change?

I think that firstly the outdated hierarchy of academic degrees (à la Nicholas I) should be removed. I understand of course that in removing the hierarchy and badges of rank, a voice of indignation would immediately spring up, saying: ‘What else will be left to do in scholarship?’ (indeed a grey-scale, goal-free society, as is well known, has no meaning). But actually, one goal *will* be left — getting down to some work at last.

Secondly, we should move to the necessary evaluation of an employee based on practice, not badges of rank, so that salary, as in the USA, would be flexible, decided according to practical achievements, and the employee's value to the organisation. (Alas, however, if salaries in Russia were indeed paid in proportion to an employee's value to the organisation, then, I imagine, employees would all immediately become highly-paid and 'valuable' — the Russian situation is one we all know more about than we want to.)

Thirdly, as Mikhail Sokolov absolutely correctly remarks, changes are essential that would be conducive to intensifying the horizontal mobility of scholarly staff in the country (which is impossible in today's situation of developing towns and social spheres, which shrouds everyday life). I agree with the other criteria set out by Mikhail Sokolov at the end of his article.

Having cited Clifford Geertz once again (although here I could have cited Marshall Sahlins or Erving Goffman, or many other anthropologists and sociologists who are 'authoritative' in the system of co-ordinates which I most probably find myself), to complete my commentary I would like to recall the idea (*their* idea, with which I merely agree), that the processes in scholarship, as in any other sphere, are mediated by everyday processes to much greater extent than we sometimes allow ourselves to imagine. Scholarship is simultaneously a professional and an existential project which is affected by stimuli and irritants on either side. In this sense 'our' scholarship (as well as American scholarship in its own context) is a crooked reflection of 'our' everyday life in the mirror of the structural conditions that this very mundane life created. We cannot change these structural conditions without altering the set routine of everyday life. Something must be torn up by the roots.

Once again, thank you to Mikhail Sokolov and *Antropologicheskii forum* for a stimulating discussion which is vitally necessary for honing the self-consciousness of our different disciplinary communities.

KATERINA GUBA

**The Russian Citation Index:
Some Obstacles on the Road to Success**

In his article, Mikhail Sokolov showed that not one existing symbol can easily succeed in exceeding institutional contexts and being unconditionally legitimate in any one part of the sociological community.¹ Moreover, one of the dangers lying in wait for various academic status symbols is the probability of finding themselves designated to a supervising group, which often seeks not to do justice to a scholar's academic achievements, but to turn existing symbols into the subject of piece-rate pay.

These reflections have led the author to believe that it is possible to avoid the unpleasant consequences of using crystallised symbols unfairly, by placing a stake on the use of a citation index for evaluating the activity of Russian scholars. The basis for some degree of hope was probably not the well-known attempt to create a Russian analogue citation index, but the simple fact that its supervision is in the hands of three different agents, something which must complicate attempts on the part of interested parties to meddle.

Firstly, it is we who decide to whom we pay attention in the pages of our scholarly works, whether we try to give credit for the intellectual achievements of our colleagues or furnish our articles with powerful support in the form of

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¹ The author is left regretting only that administrative structures cannot legitimately use informal information concerning the academic activity of scholars. Probably the scholars themselves more or less place themselves in the community in which they must work. They voice opinions about what is going on, attending different kinds of academic events (conferences, seminars, summer schools), making notes in blogs online or posting open letters through institutional dispatches. The main shortcoming of this kind of information is that it is impossible to transfer it to a different context: usually the opinion of scholars is trusted within one segment but outside of that context it is often discredited. Therefore, unfortunately, it would hardly suit the official who is also concerned with the fate of Russian scholarship.

a bibliography. Secondly, journal editors¹ make decisions on the publication of our research results. In this case the decision concerns not only the authors of articles, who, with the aid of publication, receive a symbol of scholarly recognition essential for the furthering of their career, but also those people who find themselves in a list of literature that will be fixed as a positive evaluation of their academic activity.

But this string of active agents is not yet complete: so far we have only a multitude of articles with references, scattered over hundreds of pages in dozens of sociological publications. For example, we can open *Sotsiologicheskii zhurnal* and see a certain number of references after every article, but we cannot view them all together, we do not have the ability to simultaneously open every article of the journal throughout the history of its publication to see the trends and patterns of citation. Only institutes specialising in creating bibliographical databases containing information about citations made by scholars in a particular text have the power to do this. It is precisely they who ‘work wonders’ — revealing the everyday practice of writing an academic text.

It would seem that a citation index is one of the most attractive symbols: it includes a great number of different authors, which from the start means it is hard to turn it into the focus of self-interested negotiation. Any persons interested in obtaining this symbol find themselves in a situation of having to organise a long chain, in which they must include three different groups of agents. Unfortunately all the advantages of a citation index are negated if the conditions for its existence are absent: first and foremost one must be certain that both the authors and editors are general motivated by the intellectual significance of publishing and citing their colleagues.

In this example based on sociological publications, it is plain to see that both authors and editors are probably in control for *administrative* reasons. This refers us to the peculiarities of the journal system, to which we will turn our attention in the first part of this article. At the same time some observations on how it is planned for developers to supervise symbols provides hope for its successful functioning. To describe the actions of the creators of the Russian Scientific Citation

¹ For example, one complaint about the quality of articles in the journal *Sotsiologicheskie issledovaniya* [Sociological Research] concerned the excessive tendency of authors to cite foreign literature at the expense of work by Russian researchers: ‘In the material sent only foreign researchers are cited and the relation to Russian scholars’ work is in no way reflected. How, for example, can one write about an information-oriented society without mentioning a single Russian researcher?’ [Toshchenko 1997: 5]. An indirect confirmation of the fact that the editors of SOCIS are making a special effort to change their author list can be found in the distribution figures of references to foreign and Russian-language journals. Over the last five years only 28% of references in SOCIS are to foreign journals; in the *Zhurnal sotsiologii i sotsial'noi antropologii* [Journal of Sociology and Social Anthropology] (ZhSSA) and the *Sotsiologicheskii zhurnal* [Sociological Journal] the figures are 46% and 50% respectively.

Index (RSCI), the author has made use of the late ideas of Bruno Latour: the creation of a citation index will be looked upon as a combination of various efforts in the spirit of the actor-network theory.

Russian academic journals: the principle of local attachment

If a researcher sets himself the aim of ascertaining which authors are being published by editors of the main sociological journals, then he cannot look through just one journal. Leafing through several issues of the first journal, he will almost immediately find the 'leaders' — organisations that continually occupy a large part of the publishing space. It is worth him turning to a no less respected sociological publication, and as he will discover, the picture changes fundamentally. Suddenly some new faculties and institutes appear, but others disappear without a trace, which leads us to doubt the fact that researchers are dealing with journals of the *same* academic community.

But he can be certain of one thing — in first place will always be organisations that directly produce a journal. In the case of the ZhSSA, it is produced by the faculty of sociology at St Petersburg State University (26%); both SOCIS (14%) and the *Sotsiologicheskii zhurnal* (26%) are produced by the Institute of Sociology at the Russian Academy of Sciences (IS RAS)¹. It is probable that the authors — institute employees — have access to the editorial board as well as the supervising group, which is becoming an important intermediary in the distribution of academic status symbols — in the given case being published in the main sociological journals. In this case, then, the possibility of getting published in an issue of a particular journal depends on one's connections to the organisation producing it.²

The localisation of academic journals takes place not only within specific institutes, but also within local communities, and so we see a minimal amount of exchange between towns when researchers from each town prefer to publish in their own publications. Petersburg sociologists working in various sociological organisations very rarely appear in Moscow publications. For example, in SOCIS over the last five years 1335 authors have been published, but only 56 of them

¹ Here the author makes use of the results of research on the journal system in Russian sociology. Three sociological journals were used as subjects — *Sots. issledovaniya*, *Sotsiologicheskii zhurnal*, and ZhSSA; the time frame of the research was 2002-2006. The definitive database included information about the institutional membership of 1335 people belonging to SOCIS, 302 to the ZhSSA and 156 authors belonging to the *Sots. zhurnal*. In all, the references collected totalled 3220 in SOCIS, 1184 in the ZhSSA and 782 in the *Sots. zhurnal*.

² For example, in the ZhSSA one can find a considerable number of authors from the German universities of Bielefeld, Magdeburg and Gerhard Merkator, which is the result of a programme of collaboration between the Faculty of Sociology at St Petersburg State University and German universities.

were representing the Petersburg community. In this case the ZhSSA is only repeating the pattern of local attachment: over the same period Moscow sociologists were published in it no more than 16 times. In the end, more often than not sociologists choose to submit work to ‘their own’ publications, since they are in both geographical and institutional proximity.

The principle of attachment to printed publications within local communities would not in itself be so very important for describing the current state of the social sciences if authors ever ventured beyond the bounds of the journals in which they publish. Unfortunately, the distribution of attention paid to various journals within the discipline shows that researchers’ citation within one publication is exclusive. All three give due credit to a senior journal — *Sotsiologicheskie issledovaniya* (SOCIS), but it is mainly referenced by the very authors whose articles are printed in it. Here too the usual figures are most striking: out of 1050 references to SOCIS in three journals, 885 were made by authors with work printed in that publication. In other sociological journals, it is just as easy to notice the preference of authors for citing publications in which their own work has appeared. Accordingly, authors from the sociological faculty of St Petersburg State University primarily cite the ZhSSA, and authors working in the Institute of Sociology at the RAS turn their attention to both SOCIS and the *Sotsiologicheskii zhurnal* depending on the localisation of their publications¹.

All this indicates that we cannot find a journal that would create a reputation for the whole community and draw an *identical* amount of attention in all the main sociological organisations². Academic exchange is characterised by keen attention to the publications of an author’s own institute³. A journal system organised in this way creates reputation within a single segment, which allows authors to legitimately ignore not only the remaining sociological journals but also other whole segments of the academic ‘market’.

It is precisely this kind of consideration that forces us to doubt whether in this kind of journal system a citation index could success-

¹ The latter is very curious: one can discuss not only the differences in citation patterns between organisations, but also the differences between people belonging to a single organisation. It would be interesting to compare authors’ surnames — those working for the IS RAS from SOCIS and from *Sotsiologicheskii zhurnal* — in order to see whether we would be able to identify a mutual intersection.

² It is interesting that we do not find a Russian publication of this kind, whereas when citing foreign journals, authors in all three journals are united in their preferences: in joint first place are the *American Journal of Sociology* and the *American Sociological Review*.

³ This is particularly evident in SOCIS: if in other cases one could speak of several limitations, since on the whole the quantity of references is not very great, then here, undoubtedly the most cited journal is SOCIS, to which all institutes are denoted in references. At the same time, authors from the IS RAS and the Russian State University for the Humanities (RSUH) cite the journal published by their own institute far more often than employees of other Moscow institutes — 110 and 76 references respectively.

fully be used to evaluate a scholar's academic achievements. Each journal will have its own list of outstanding scholars and important publications, and these lists will barely overlap in different publications.¹ Therefore the citation index will depend on which journals undergo bibliographical analysis. Even assuming that all sociological journals are selected in the first place,² the situation remains unchanged: each journal will create a reputation within its own segment, which will make the existence of a citation index pointless within the *whole* scholarly community.

Let us move away from reality a little and suppose that skilful institutional measures have destroyed the principle of the local attachment of publications within separate institutes and towns. Now sociological journals in Russia are not connected to a single specific organisation. They have independent funding which allows the editorial board to reject administrative reasons for publishing particular articles and concentrate exclusively on their intellectual merit. Editors are primarily interested in attracting the attention of reputable researchers, so they publish only first-rate articles of a high professional standard, the search for which forces the involvement of a wider circle of authors.

Let us suppose also that the articles in these journals give authors considerable academic bonuses (provided by special administrative measures), the possession of which becomes essential to construct a successful academic career. If several such journals are circulated in the community, this prevents the editorial board from lowering the level of its demands on publications, since otherwise a competitor would take first place. All this indicates that publications in these kinds of journals are like an academic status symbol for the whole community.

How would authors function in this case? Most probably they would begin to endeavour to have their articles published in the journals mentioned, which would be very much concerned with academic achievements. Now there would be a perceptible choice standing before them: publish in 'their own' journal which loses out on the scale of prestige but saves time and effort, or try to get an article published in a journal that is reputable in their discipline. Although the latter would require an increase in costs, it could contribute tangibly to the academic reputation of the authors.

It is probable that first of all they would have to raise the professional standard of their works to get them published. If we introduce into

¹ This is to be expected in the case of specialised journals that publish material in diverse theoretical areas.

² As Sokolov quite rightly notes, this project brings with it additional difficulties of defining the remit of sociological publications.

our imaginary journal system an actively effective citation index to which both the academic community and bureaucratic structures respond, then researchers would have to try to draw the attention of as many scholars as possible. It is true that in this case it would be necessary to introduce additional conditions for it to function successfully, firstly by including references to scholars made by colleagues from other sociological organisations.

Ideally, a potential strategy for authors lies in the careful study of the publications of other researchers, citation of which could draw reciprocal attention. Then we would witness active references to extremely diverse Russian sources within publications. It would become disadvantageous for authors to pay attention to their immediate institute colleagues, although previously this cut costs considerably, when it was not necessary to expend additional effort studying publications in other journals. Now, the best strategy would be to broaden the scope of one's attention, which would prevent not only a significant number of sociological journals¹, but also whole segments of the scholarly community, being ignored with impunity.

But meanwhile, all we can observe at present is an attempt to create a Russian citation index. Whether it will lead to the desired transformation of the academic community very much depends on how it is created. Here the people taking the role of supervising the index are not authors and journal editors, but those who specialise in collating different references, so that in the end we have an internet site with a convenient user-friendly interface allowing us to browse a multitude of references simultaneously. Let us look more closely at their work.²

Creating a Russian citation index: an attempt at deconstructing the network

In order to describe the creation of a Russian citation index it is tempting to make use of the ideas of Latour, who sought to see how networks consisting of different allies would produce an implicit everyday knowledge [Latour 1997]. The essence of the network lies in the firm linking of different groups or potential allies, who together comprise a coalition-network. A network as an association of diverse

¹ The pattern of citation in Russian sociological periodicals is most often as follows: scholars actively cite 2-3 journals, at times referencing a huge number of other publications. At the same time the discrepancy between the quantity of references to different publications can reach a colossal size. For example, employees of the RSUH publishing in SOCIS cite first of all SOCIS (76 references) and only then Political Studies (4 references).

² The subsequent discussion is in many respects based on analysis of a report on research work on the theme of 'Developing a system of statistical analysis of Russian scholarship on the basis of data on the Russian citation index' over the course of 2005. It is currently unknown to what extent the plan has been implemented in reality. The RSCI is now an open bibliographical database, accessible at: <<http://www.elibrary.ru>>.

elements is strong because of its bonds; its success depends on each of its links. It is quite difficult to find these main points, particularly in an established network: they lose their visibility. If a network is only passing through the stage of configuration — the collating of various elements — then it is possible to try to fix them.

Developers of the Russian national citation index have only recently created a network, having carried out a search for possible allies, in order to turn the citation index into an ordinary element of the normal academic world, and so that social actors would not see any other way of reaching their goals, with the exception of those decreed by its creators. A closer look at the allies who are trying to bring together the creators of the RSCI will reveal how successful the actions of the network are likely to be.

The attempts at creating a national citation index are a perfect example of the limitations of the extent and durability of networks, when ideas are free, but the national budget must pay for it all [Kharkhordin 2006: 39]. The distribution of ‘black boxes’ in time and space should be paid for by the incredible increase in the number of elements connected together in a single network. The case of the RSCI developers shows that sometimes it is more worthwhile creating a new network than trying to extend an old coalition of social actors. So the ISI¹ incorporates a rather small percentage of Russian journals: in all the database contains around 70 Russian journals, whereas it has around 1500 American journals, which is almost 40% of the overall number of indexed journals [Report 2005: 9]².

Accordingly, the ISI as a network is insufficiently extensive for other countries to be incorporated in a meaningful way. Even when demand drives it to try to encompass a larger space, the system of measures for achieving this is often impracticable. So the ISI has quite firm requirements of journals included in the index: strict regularity of publishing issues, the availability of English versions for bibliographies and, particularly desirably, for article annotations. Journals which deviate from these conditions can be excluded from the database. Those which formerly crossed the complicated qualification threshold essential for the functioning of the network could, because of

¹ On the world market of scientific information there are two main citation indexes. The well-known product of the Institute for Scientific Information in Philadelphia (ISI) includes more than 8700 publications in English and partly in German. Recently its competitor has been the index of the company Elsevier, called ‘Scopus’. It deals with more than 15,000 active journals. Publications in English predominate by far.

² The same thing can be seen in the case of the creation of the Chinese Scientific Citation Index. An article about creating the Chinese national database began with the bitter remark: ‘Despite the fact that China occupies fifth place in the world in terms of its population of scholars, and the number of researchers is no less than 925,000 people each year (which puts it third place behind the USA and Russia), the number of Chinese publications in the SCI has never been higher than 5%’ [Wu 2004: 335]

unpredictable funding, cease being able to cope with these requirements and forfeit their place in the ISI [Terekhov et al.].

The RSCI project was designed in response to a ruling by the **state** within the areas of science and technology requiring the creation of an objective means of evaluating the activity of scholars. In 2005 the Federal Agency for Science and Innovation announced a competition ‘Developing a System for the Statistical Analysis of Russian Science and Scholarship on the Basis of Data from the Russian Citation Index’, whose adjudicator was the Scientific Electronic Library. Finding themselves in a situation where it was necessary to develop their own citation index, and in essence create a new network, the RSCI developers set off to get as many allies on board as possible, gaining the interest of the necessary social actors as much as putting administrative pressure on them.

The type of interest shows that social actors choose, from a multitude of potential opportunities, whatever helps them to reach their goals; in this instance the victor is the person whose path turns out to correspond most closely to the interest of the state, which is becoming one of the most important links in the network. Generally the state participates through the existence of set standards that regulate scholarly activity on various levels. For example, journals in the database should be selected from the list prepared by VAK. The State Rubricator of Scientific and Technical Information is used in the creation of rubrics. Bibliographies not only provide an opportunity to create indexes, but also regulate standards. In this way, we see the presence of the state in practically all of the main points.

The creators of a citation database necessarily do not only co-ordinate the interests of allies with their own, but also exercise control over them, to make their actions predictable [Latour 1997: 121]. For a citation index to function it is essential that a series of requirements is fulfilled. First of all, every article should be furnished with a bibliography on the basis of which the citation index is created. If authors do not compile a bibliography, and editors print their work, then it is impossible to create any kind of citation index. In the first instance, the correct collation of references is followed not so much by editorial politics as by the community itself through the norms of scholarly citation. In the second instance the citation index creators have already had to expend effort to interest **editors** in the fulfilment of their demands.

For example, in the past, with the aim of reducing the cost of each journal issue, many Chinese journals limited the space provided to authors for bibliographical references. Editorial boards placed a limit on the number of footnotes, and several editors even removed references made by the author altogether. The functioning of

CSTPC¹ led to a reasonable amount of prestige being associated to inclusion in a list of journals referenced by the database. And if the editorial politics did not provide an entirely fitting bibliography, then the journal would have no chance of reaching this list. If CSTPC workers got to hear of such a case, they would even exclude the journal from an existing list [Wu 2004: 392].

In this way, a plan operates where for the editors of journals it becomes beneficial to be in the database, which gives evident bonuses. Therefore it is important to keep the interests of all allies united, otherwise they could disperse, each in their own direction. The strength of the network lies in the fact that after the allies fulfil the requirement of the database creators they no longer have the possibility of backing out. As a result their actions become controllable and predictable.

In the project, RSCI developers' work with journal publishers also had an important role: special actions were taken to gain the interest of publishers. The supervisors of journals entering the VAK list were sent letters explaining the advantages of their inclusion in the RSCI: the wider distribution of information about their journal; the increase in the citation of articles published in it; the growth of authority and attractiveness for authors, and as a consequence the increase in subscribers [Report 2005].

The inclusion of publishing houses in the network is one of the main points on which the whole chain of events is built. Now the RSCI creators have achieved the only way of receiving electronic versions of journals — through direct contact with publishers requesting that they send an electronic version of the issue in book layout. Several other considerations on the part of the authors in the project relate to the fact that they cannot always hope to persuade essential allies — sometimes they seek to gather their support with the help of administrative measures. For example, there are distant plans for the impact factor of journals to influence decisions about their inclusion in or exclusion from the VAK list [Report 2005: 15].

The range of social actors at work within the network for the creation of a citation index also includes **scholars** who might themselves join the database. Authors and authorised representatives of scholarly organisations can add to UCSP information (Unified Classification of (Russian) Scientific Publications) of their own volition, and can also introduce amendments — this latter power is based on the American system. Here also the developers propose not so much to gain the interest of scholars as to compel their involvement through tougher means of stimulation.

¹ CSTPC — China Scientific and Technical Papers and Citations

In the first option it is supposed that authors will be aware of the fact that the level of state funding, the likelihood of receiving grants, success in competitions and receiving prizes and titles will also depend on RSCI evaluations. Even if this does not affect scholars, an obligation could be introduced entailing their inclusion in a list of publications in the UCSP as a necessary condition to applying for various competitions or reports on state contracts and programmes [Report 2005: 28-30].

A reconstruction of the network creating the Russian citation index would be rather incomplete if I did not finish my narrative by turning my attention to **inanimate actors** — IT technology, powerful processors, computer technology and Internet sites. Without them functioning successfully it would be impossible to collate references and create a database that forms patterns of citation. In this respect there is no difference between national databases: they are all constructed on the basis of powerful computer technology¹.

Developers of the RSCI attempted to take into account the experience of existing interaction between technology and humans that is characteristic of our Western counterparts. The traditional approach, used in the SCI, is based on interaction between machines and people (operators). It begins with the scanning and recognition of all issues of the chosen journals. Then the operator manually highlights the article title, authors, address, volume number, pages etc. and copies this data to the fields of the database, at the same time processing any bibliographical references to the article. The bibliographical description for each reference is automatically verified in the database, and if it is available, it is added to its citation counter.

The creators of the RSCI decided that this way of processing publications was too labour-intensive, and that too much was delegated to humans, which increases the chance of errors creeping into the database [Report 2005: 17]. Overall their efforts at modifying the system were intended to minimise human/operator involvement in the task of processing publications, because it cannot always relied upon (in contrast to machines)². Ideally the system should be 'intelligent': requiring an operator only when it cannot make an independent decision.

¹ This kind of situation seemed improbable even several decades ago. In Liansheng's article [Liansheng 2000] there is a wonderful description of the conditions for creating the Chinese bibliographical database (including a citation index). Up until the 1990s only theoretical developers existed; the situation changed fundamentally in the early 1990s when new technologies appeared and computer systems began to constantly improve (for example, a computer telecommunications network was installed which was connected to the Internet). Only then were many libraries and information centres able to install computer systems, and they started to create their own databases.

² Arguments of this kind advanced by the project leaders are a remarkable illustration of the advantages that machines have over humans. See further: [Latour 2003].

Undoubtedly, this brief attempt to examine the allies working together within a network to create a Russian citation index is not exhaustive. But it is useful in that we can see not only the intention of the project developers to try to gain the interest of necessary allies, but in particular their endeavour to guarantee the allies' participation in the network, making use of the vast opportunity afforded by administrative leverage¹. The author supposes that the use of administrative leverage, eliding self-serving initiatives or vested interests from scholars, might turn a citation index into an integral part of the academic world, and, with a small number of modifications, into a reliable academic status symbol.

For this to happen it is essential that institutional regulations concern not only the participation of authors and editors in creating a citation index, but also several special features of the publication of scholarly material. First of all, the measures should be intended to destroy the chain whereby scholars publish in the journal of 'their own' institute and cite only their own colleagues, a state of affairs which hinders extensive intellectual exchange.

In another respect it is not difficult to predict the trajectory of the journal system, and indeed both authors and editors are interested in the institutional concentration on authorship in journals, which reduces costs significantly. Authors can be certain that there will always be space for their works to be published, and editors that their journal will not be left with empty pages².

Evidently then it is necessary to create constraints like these, breach of which would cause considerable harm to the advancement of one's academic career. The plans of the Russian citation index's creators bode well for the direction in which they are moving, regardless of how mildly unpleasant it might be for us to encounter institutional pressure.

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¹ This strategy probably arose also because of material considerations (for example, the participation of both scholars and publishers can noticeably lower the costs of creating a bibliographical database).

² Otherwise, when sending his article to another institution's journal, an author is not entirely certain that it will actually be published, all the more so because the consideration of articles often takes a substantial amount of time. Equally it is also fairly unprofitable for editors to seek out authors from other towns and organisations, which is becoming particularly important in situations of low funding for journals, something which is often complained about in editorial remarks.

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VLADIMIR GUELMAN

Resources and Reputations in Provincial Markets: Post-Soviet Social Science

Mikhail Sokolov's article 'The Failed Consolidation of Academic Authority in Post-Soviet Scholarship: The Case of Sociology' presents issues that go beyond concrete instances and which are important for post-Soviet (and not only post-Soviet) social science as a whole. It is not so much that the picture painted by Sokolov could, with some variations, refer to a number of other disciplines and states, but that besides ascertaining the calamitous state of affairs within scholarship, this article establishes the reasons for these occurrences and possible ways of overcoming them.

One cannot say that the situation analysed by Sokolov, where different status symbols compete within the same academic community, is in itself unique to the social sciences. We need only remember Pierre Bourdieu's 'Homo Academicus', where these processes are analysed in a case study of the French academic environment of the 1970s [Bourdieu 1988]. However, the focus of Sokolov's attention was entirely different: he examines the lack of co-ordination between status symbols and the crisis of academic authority as one of the reasons for the institutional decline of the post-Soviet social sciences. It is difficult to disagree with the fundamental theses of his article, but the model suggested by Sokolov needs clarification in some important respects. It is essential to understand how the given situation was caused structurally, and whether things are doomed to remain so, at least in the foreseeable future, and the extent to which reforms are capable, if not of changing things for the better, then at least not making them worse.

In this context it is vital to re-examine those functions which represent academic authority within scholarship. Continuing Sokolov's market analogy, the closest economic equivalent to academic authority is the reputation of a firm that is active in the market. Reputation allows market participants to lower information costs and the risks of obtaining poor quality goods; accordingly, in conditions where they must compete for consumers, firms take care of their reputation, investing resources in creating and supporting it, not to mention advertising expenses. In a similar way the names of generally accepted classics in the social sciences are akin to brands; conventionally speaking, the names of Aristotle or Weber can be likened to 'Mercedes' or 'Coca Cola'. However, the institutional organisation of the academic market in the post-Soviet social sciences is such that it considerably lowers stimuli to invest in scholarly reputation, for individual market participants, just as for academic institutions generally.

The indisputable fact that manufacturers of scholarly production are its own principal consumers (in contrast to the case of medicine mentioned by Sokolov) is a peculiarity of the academic market of the social sciences. The exceptions found in various disciplines, connected to several applied uses of social sciences knowledge (economics, psychology and so on), rather prove this rule. On the one hand, this circumstance effectively lowers information costs for market participants, but on the other hand it lowers stimuli for investment in reputation, insofar as a low return is given in those markets where the pool of consumers is knowingly limited. It is no coincidence that the process of authority crystallisation in American sociology described by Sokolov lasted a decade and was connected to (1) broadening the pool of academic market participants and (2) the

growth of competition within it between universities on a background of (3) the more or less stable institutional organisations of the scholarly community.

This combination of conditions (noted by Sokolov as essential) is, however, rare, particularly for those markets which are by definition peripheral. A partial example of such a market is the post-Soviet social sciences. They are in a situation of a kind of ‘dual periphery’: in relation to the natural and technical sciences which draw the larger share of resources, as well as in relation to the disciplines themselves, in whose framework today have formed international (if not global) academic markets, where post-Soviet scholarship is present if only marginally.

The peripheral nature of academic markets in the post-Soviet social sciences came about as a result of a long historical evolution, and in the short term this will most probably not change. In Eastern Europe the social sciences survive the process by which their local academic markets have absorbed others across Europe, a process that has impact on the institutional organisation of the social sciences (from its general orientation, to the inclusion of individual scholars and whole teams in pan-European scholarly networks, to acknowledging the ISI citation index as the fundamental indicator of academic authority).

For Russia, this kind of development of events is unlikely: it is hindered not only by the low sense of competitiveness from most of the Russian scholarly community with regard to external academic markets, but also by the significant aggregate size of local academic markets. Therefore, however Russian sociology develops, it is difficult to expect Russian sociologists of today and tomorrow to strive in droves to claim grants from large American and European scholarly funds, to write works in English and publish them in the *American Journal of Sociology* — and not only on the strength of their qualifications. In a large country the stimuli to seek internal research funding, write in Russian and publish in *SOCIS*, could hardly be destroyed without serious repercussions.

Peripheral markets, including academic ones, differ in lag effect not only from the point of view of manufactured goods, but also from the point of view of stimuli for the behaviour of participants. They can be likened to a fair in a village, where the local peasants in charge of farming gather, sometimes exchanging lard for bread, or potatoes for moonshine (Sokolov’s article is full of examples of this kind of exchange in Russian sociology). The villagers that now and then visit these markets from neighbouring villages, or passing merchants, do not alter the general picture. The peasants, for their part, trade only in their village (or at most in a neighbouring one), since they do not dispose resources for moving into external markets, where they will

probably not be able to compete with large manufacturers. Understandably, the peasants are hardly interested in investing resources in their reputation. To be more precise, their reputation is principally local, and is conditional not upon the quality of their agricultural produce but on the range of resources accessible to them and the strength of their social networks.

The localisation of scholarly communities and the forming of stable academic networks around local university faculties and departments equipped with resources, which Sokolov notes, is becoming the natural state of the peripheral academic market. Moreover, the time-horizon of peripheral market participants is quite severely limited — they simply cannot shoulder the burden of long-term investments. Just as it is sometimes more important for peasants, even when not officially trading, to sell perishable foodstuffs to their neighbours, so post-Soviet social researchers must look after their official status in the local scholarly community ‘here and now’ instead of striving long and hard for acknowledgement beyond. So there is no point being surprised by the fact that the value of specialists’ academic authority in the social sciences turns out to be much more subject to inflation than official status (a senior doctorate, a post at a leading faculty etc.), which can at least bring short-term return in the form of an influx of resources.

Let us add to this that if in this village (read: in the local scholarly community) the norms of ‘amoral nepotism’ prevail, of the kind noted in the south of Italy by Edward Banfield [Banfield 1958], then among peasants there will hardly be the stimuli to widen production and elevate the quality of their produce. It is precisely such (at first glance economically sound) behaviour that threatens to undermine their local reputation and even to bring with it the condemnation of neighbours. The spectrum of an effective peasant’s behaviour in situations of this kind — from the acceptance of established norms as givens (‘follow the crowd’), to taking one’s earnings into town or hiring out farm-labourers to the owner of a nearby farm — it is well known to all those familiar with the practice of post-Soviet social sciences.

The majority of specialists in the domain of the social sciences are faced with a move to the capital or (far more rarely) abroad, and/or departure into non-academic activity, or loyalty to existing norms. Paradoxically, the approaching academic globalisation is reinforcing these tendencies — peripheral markets react to these processes in their own way by involution, cutting down ‘inside’, or fencing themselves off from the markets in which there is simply not enough space for them.

How can alternatives to certain institutional changes influence the behaviour of academic market participants? Sokolov quite rightly

notes that the influx of larger-scale funding into the social sciences is an essential condition of overcoming the peripheral nature of this market. However, this single condition looks by no means to be sufficient — indeed we never hear, for example, about the successes of the social sciences in the oil-rich countries of the Persian gulf, although the sum assigned to local universities has been enough to equip libraries and laboratories and also to pay the honoraria of American professors invited from time to time.

Obviously, an increase in the amount of resources in peripheral markets does not in itself draw long-term investments in the reputation of their participants. An influx of resources can create new stimuli for academic market participants only if the institutional environment of the scholarly community changes.

It would seem the higher education reforms initiated by the Russian authorities were intended specifically to achieve this. Officials of various levels speak quite truthfully about the necessity of competitiveness in Russian scholarship; the government is forming a pool of federal universities called upon to be the driving force behind scholarship's break into the world market, at times even declaring the intention to involve Western specialists in Russian scholarship, with the aim of creating Russian scholarly schools.

The aims of these transformations are entirely justified, but the means of their implementation can lead to a totally opposite result. Let us take as examples the plans of Russian President Dmitry Medvedev in July 2008 to cut the number of higher education institutions in the country to around 150, and the schemes for creating federal universities by dint of a mechanised merging of several existing regional universities of different types.

Even in the best case scenario, these solutions are clearly not capable of bringing about the desired effect. So, the creation of federal universities is like collecting the parts of several broken-down Minis to make one car, subsequently painting it red and expecting it to drive just like a brand new Ferrari. In the worst case scenario, well-known consequences, along the lines of 'chopping up' the funds that have arrived on the academic market, lead only to a slight rise in living standards for a section of those representing local scholarly communities, not leaving any important or notable results (which cannot knowingly be achieved in a short space of time).

What can change the stimuli for the behaviour of academic market participants and create scholarly reputations in the social sciences if not now, then in 15–20 years? To begin with, by all appearances one must admit that today's state of affairs hinges on a serious and long-standing problem — the vast majority of Russian scholarly institutions in the social sciences are incapable of being sources not just of

academic authority, but also of new scholarly knowledge as such. Post-Soviet social sciences cannot be reformed, they can only be allowed to die out.

Instead of fruitless attempts to instil new life into existing faculties and departments, it would be sensible to invest resources in new institutions which must be created 'from scratch', made up of Russian graduates of American and European post-graduate courses, or by attracting colleagues from Russian educational institutions who are focused on obtaining foreign academic status symbols.

The institutional strengthening of the inequality between 'new' and 'old' scholarship (at the level of payment of labour, principles of apportioning funds for carrying out research etc.) will compel Russian scholars sooner or later either to re-orientate themselves to new status principles, or to abandon the academic market.

The peasants at the agricultural fair are unlikely to learn to behave differently: even if they begin to cultivate big harvests or if the prices for their produce rises sharply, that fair will not turn into a supermarket on its own. But if we once and for all refuse to place stakes on the peasants, the reform of the fair must be implemented at full scale: taking workers to the village who will construct a new building from scratch, a path leading up to it and a means of communication. With a suitable level of investment, and skill from the builders and managers, after some time, out-of-town shopping centres will replace the peripheral market, and there will simply be no market space for peasants.

However severe (or cruel) this way for the Russian academic market to escape its peripheral condition may be, any other alternative for successfully reforming it that I have seen seems barely possible. The fulfilment of the given recommendations is unlikely, but all the same, I hazard a guess that the attempts undertaken to implement cosmetic changes in the current state of affairs threaten the Russian social sciences with further stagnation, if not complete disintegration.

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PETER RUTLAND

Sokolov's article presents a plausible account of a discipline that is trapped between some bad habits of the Soviet past and some new behaviours that are the result of market-like budgetary pressures. Post-Soviet sociology has inherited more hierarchies and symbolic generators than its Western equivalents (Academy of Sciences, large dissertation committees, doctor of science degrees, etc.) At the same time, pressure to boost student enrolments and publications records has led, Sokolov argues, to a deterioration in standards. Rather than developing as a unified field, Russian sociology has continued as a series of self-referential and self-validating cliques, based around certain institutions and their respective journals, with a strong regional component.

This is a convincing picture. But is it that different from what one finds in Western countries? With the proliferation of publication outlets, and the sheer volume of publications, it becomes harder to track and calibrate hierarchies within the disciplines. In the United States there are some visible superstars, then there are hundreds, thousands of social scientists, clustered into networks based on sub-fields or regional centres, without clearly identifiable hierarchies of expertise. But is this necessarily a bad thing? All the more so as disciplinary boundaries are becoming more porous, while the rise of the Internet and online publications increase the velocity at which knowledge and ideas are circulating.

Indeed, it seems that there is more of a national academic market in the US than in Russia, and that presumably gives a qualitative edge to American researchers. It also appears that American journals are more rigorous when it comes to blind peer review than their Russian equivalents. But that is something which it should be relatively easy to fix: surely there would be rewards for a journal to become more exclusive and move itself up the food chain. I would be interested to see data on editorial practices at

the various journals, the number of submissions and rejections. Also, I am curious to what extent Sokolov believes that good work can still be done, despite rather than because of the overall dynamics of the discipline?

SERGEI SOKOLOVSKY

Nothing Disturbs the Peace: On the Situation of Russian Anthropology

I agree with Mikhail Sokolov's opinion that 'Russian scholarship today is not only failing to gain in stature, it is in the process of losing the stature it once had', although here of course, it would be interesting to have a more differentiated evaluation in separate disciplines and research areas. As a reader of different academic and would-be academic output, primarily in social and humanitarian disciplines, I am extremely impressed by the successes of Russian sociologists and philosophers over the last 20–25 years. From my point of view, they have managed to profit by political liberalisation and create conditions for the continued development of their research fields, partly thanks to the realisation of ambitious translation and publishing schemes, publications 'written for the desk drawer', and also by developing new methods and their uses for new research subjects.

However, nothing even close on the scale of developing an international disciplinary heritage has been achieved in ethnography or anthropology. The translation schemes of the series 'Anthropology/Folklore' and 'Nation and Culture', sponsored by the Open Society Institute, were limited to the publication of a few excerpts of classic anthropological literature of the 1940s–60s.¹ The publication of the

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¹ The books of Murdock, Evans-Pritchard and Caillou. The 'Ethnographic Library' series of publications, supported by the same fund, also failed to get beyond the middle of the last century (Evans-Pritchard, Mauss, Radcliffe-Brown, van Gennep and Mead — the last three books were published with money obtained by the publishers from other funds). Only certain works by Edmund Leach and very recently published books by Maurice Godelier date from the 1970s. In the 'Culturology of the 20th Century' and the 'Book of Light' series (translations of which were again funded by Soros), Clifford Geertz's thematic collection was published, which was based on his articles of the 1950s–60s, as well as the works of

international and Russian legacy in anthropology was not made up of hundreds or even thousands of volumes, but a mere 15–20 books illustrating the development of knowledge over a period of five decades.

As a result, whole regions and continents of modern anthropology have become practically unknown to the Russian student — such as the anthropology of organisations, medical anthropology, the anthropology of social work, the anthropology of professional communities etc. The lack of translations in these very important and rapidly developing areas also signifies a lack of corresponding Russian systems of concepts and terms (or their very poor cultivation), which entirely explains the current situation, where independent research into the aforementioned areas is written in ‘American’ rather than in Russian, and is linguistically and stylistically indigestible.

New objectives and methods always demand new linguistic means, and if visual anthropology can adopt its own language from the vocabulary of film directors and critics, then the anthropology of organisations, because of a total lack of serious linguistic work (of the kind that would be carried out by a good translator, if works were regularly translated and published) is compelled to rely on *merchandizing*, *mobbing-protsess*, *freiming-experience* and other terms that replace the functions of analysis and description by incantatory mumbo-jumbo.

As regards the loss of ‘the stature it once had’, everything depends on the time ‘once’ refers to. If it refers to Russian ethnography-cum-anthropology a century ago, then evidently we have something to deplore, although it is long since time to stop deploring, and move onto a sober analysis of the circumstances which caused the chronic need for ‘catch-up’ during the entire subsequent development of Soviet/post-Soviet anthropology. The external (mainly political and economic) circumstances that caused a halt in the development of anthropological knowledge in Russia and in the West are completely understandable. What is not understandable is how under the same conditions, some research fields have still maintained a global standard and have even been an example for imitation and borrowing. As Loren Graham, the well-known specialist in the history of Russian science, writes, the fact that Soviet scholarship survived at all is what needs to be explained [1998: 225]. If nuclear physics, atomic energy, and space exploration can be considered products of the military-

Bronislaw Malinowski and A.L. Kroeber. In the ‘*Conditio humana*’ series, with the support of the same fund, a translation was published of another work by Radcliffe-Brown. That, in fact, is all, not counting the translations of almost all of Lévi-Strauss’s main works, the majority of which were translated and published with the support of the French Ministry of Foreign Affairs, and the popular work ‘*Mirror for Man*’ by C. Kluckhohn, providing a depiction of the mid-1940s (St Petersburg: Evraziya, 1998).

industrial complex, then how can the exceptional successes of soil science, phyto-sociology, and ornithology be explained?

In this context we can note that even in areas that are traditional in our scholarship, where, it would seem, there were no special reasons for any intellectual backwardness, in this ethnographic *Summa Technologiae* — research into the time-honoured triad of food, clothing and dwellings¹ — we are, if not fatally then at least very considerably, lagging behind. Here, as far as ethnographers are concerned, for a long time there have been no new theories, new methods or new objectives, excluding the isolated publications dedicated to analysing everyday Soviet life in its, so to speak, material dimension.

The initiative for improving the methodology and theoretical understanding in these areas has passed to by Russian culturologists and specialists in the philosophy and sociology of culture. It is they who analyse fashion from a semiotic point of view. It is they who document the changes in food preferences in different strata and groups of our society, noting the advent of *gourmandise* and gastronomical extravagance; they interview oenologists and wine-tasters, they know the relevance of biodynamics to drinks, and the differences between vintages, they are aware that the ‘nutritious meals’ in the Young Pioneers camp and the Soviet dining room were as semiotically rich as the funeral meal of the Uigurs and so on and so forth. They are also documenting the ‘ethno-’ consolidation in fashion, music and haute cuisine.

Ethnographers, as in days of old, write about what once was, but not about what is. In their books you will not find a description of what their pupils use to ‘keep the wolf from the door’ on a daily basis, which in their gastronomical universe used to mean either tinned stewed meat (*tushenka*) or a clump of melting boiled sweets (*podushechki*); in their analysis, ‘the aromas and smells of culture’, lovingly being collected and classified by culturologists, are lacking. Their standard descriptions of ‘traditional cuisine’ are paradoxically speculative and abstract; there is no love of everyday detail (what and why, who specifically thinks something is tasty, how one cooks x or y, what can be substituted if something you need is missing, how you eat it and what you feel about it, etc. etc.).

This kind of information is sometimes included in books on so-called ‘national cuisines’,² but only when the books are not written by

¹ The persistence of the ‘relay race of generations’ here is reliably secured by the conservatism of teaching in the oldest faculties in the country (and those that joined them in the mid-1980s to early 1990s) : the history faculties of ethnography/ethnology of St Petersburg State University, Moscow State University, Novosibirsk, Omsk, Tomsk, Kazan and Irkutsk, together with the uninterrupted flow of Russian publications devoted to ethnography’s traditional triad.

² I can’t recite citing one such recipe (a showpiece of ‘Karaité cuisine’): ‘CHOCOLATE TRUFFLES. Ingredients: 1 cup sugar, 2.5 cups ‘Malysh’ milk formula with rice, 75g butter, 2-3 tbspc cocoa powder, 25ml

ethnographers (professionals reject this ‘unnecessary’ information, scouring ‘tradition’ down to fantastic authenticity, and at the same time not noticing that fantastic authenticity is in itself an oxymoron). The ethnographic vocabulary of smells is so poor that it can be counted on the fingers of one hand.

Almost the same claims (of course, with some amendments and reservations) can be levelled at researchers of domestic space. If the symbolism and semiotics of habitation has more or less become a fixed part of the curriculum (as both the symbolism and semiotics of funeral meals has done), then the types of activity connected with these and conditional on them are regularly overlooked by ethnographers. Architects have talked about specialised spaces of vital activity for centuries; for ethnographers, a dwelling-place is first and foremost a *building*, a construction made of materials. The simple idea that a dwelling may be not a building at all, but just a specifically-denominated natural space has so far not taken root.

And this is despite the fact that beyond the bounds of Russian ethnography (in both disciplinary and spatial respects) the revolution in researching places of habitation of human groups has long since happened. An abode is understood first of all to mean the sum of specialised and rhythmically transforming places for specialised types of activity (preparation of food, eating, sleep and rest, leisure and so on), the spatial marking processes and rhythm of which delineate a house particularly as an *abode*; the construction of a house as a *building* should ideally correspond (insofar as it is formed under their influence) to these types of activity¹.

In the works of Russian researchers of domestic space you would hardly find a description of the aforementioned transformations, rhythms and configurations of ‘habitation’ depending on the time of day, season, family composition and so on. The study of types of abode in Russia takes place along different lines (in essence, much like those in builders’ manuals), with specific construction details, types of joints, materials etc., laboriously inventoried. However, we must be fair: in place of details drawn from studies of the strength of materials, and building regulations and norms, we do find a little information on historic folklore and semiotics.²

vodka, cognac or dry wine, 25ml water, 1 cup shelled nuts, 1 packet wafers. Remove the cream filling from the wafers [!], dry them out a little and finely grate. [...] Pour the milk formula into an enamelled bowl, add the butter, water and cognac and mix. Sprinkle with cocoa powder and sugar [...] Put in the fridge.’ [Lebedeva 1992: 232].

¹ To make things more convincing I quote the encyclopaedic article of well-known theorist practitioner of habitation research, British architect and anthropologist Amos Rapoport: ‘Any consideration of built environments must take into account not only the ‘hardware’ but also people, their activities, wants, needs, values, life-styles and other aspects of cultures’ [Rapoport 1994: 461].

² Semioticians in this context, particularly of the Moscow-Tartu sort, as George Pocheptsov said, the ‘geneticists and cyberneticists’ of the human sciences [2001: 665], should, however, be assessed ac-

Reading the works of Russian ethnographers that have the reputation in Russia of being theorists convinced me that in our discipline, theory as such is either completely lacking or appears in homeopathic doses, but even then it is suspected of being unnecessary philosophising. All that Russian theoretical thought has achieved consists of attempts to cope with the diversity of observations, that is classifying and typologising. The next step would be the interpretation of separate types or classes in terms of a causal explanation — however, this is not usually undertaken or even planned, since classification is considered the pinnacle of ‘theoretical’ effort. Here one can also look for external factors, and they can be successfully found in the ideological atmosphere of Soviet times, but since then a fair amount of time has passed, and things haven’t moved forward an inch.

So the matter evidently lies less in external circumstances and more in our own. Internalistic interpretations of the development of scholarship, however, have always competed with its externalistic versions, and to fail to take into account the political, ideological, economic and other contexts relevant to the development of both scholarship as a whole, and specific disciplines, schools and centres separately, is quite pointless. Indeed, if someone managed to create a system of support for scholarship in which ‘talent and achievement (and only talent and achievement)’ always received an appropriate reward (it is true that there are already method problems here — what to do with newly-emerged talents whose achievements are so far

cording to a different measure — one on a much larger scale. Scholarship, like the economy, should constantly grow and develop, but if this model is sometimes doubted in relation to the economy (nil increase in ideas, stable development with full reproduction of resources and so on), then in relation to scholarly knowledge, hypotheses of development would hardly be contested. The inquisitive reader longs to ask the question: what changed in the approaches of Russian semioticians after they became familiar with, whether long before or straight after, their publication in Russian, the works of Peirce, Lewis, Morris, Barthes, Todorov, and Eco? Or were the ideas of all these eminent authors in fact anticipated or surpassed by Trubetskoi, Jakobson, Bakhtin, Propp, Lotman, Uspensky, Gasparov, and Stepanov? Can we today talk about the new development of the ideas which were around in the time that created the semiotics of culture in the early 1960s, as described in the works of Lotman, and then his colleagues and pupils? If not, what prevented this once leading area from being followed up by a top academic school researching culture, including anthropological research?

I make these remarks in no spirit of faultfinding or an attempt at provocation, I have no grounds for that, least of all moral. I am simply expressing the interest of readers who do not know about the internal and external obstacles that hindered the development of so many promising areas. The same Graham finds an explanation of one of the reasons for the success of Soviet physics, mathematics, geology and soil science in the years of Stalinism and mass repression in the fact that by this time Soviet scholars had already long made their entry to world scholarship [1998: 227]. In this instance of the semioticians, on the other hand, the price of this inclusion was evidently too high — for various reasons Russia was, either forever or for long periods, abandoned by a whole pleiad of the leading figures: Boris Uspensky and Vyacheslav Ivanov, Boris Gasparov and Sergei Averintsev, Alexander Zholkovsky and Igor Melchuk. In Pocheptsov’s opinion, it is precisely this circumstance that played a critical role in curbing the subsequent development of the school [2001: 657]. It would follow that for any school to succeed, it needs personal and immediate contact between pupils and *ma tres*, a kind of magic or energy which cannot be expressed in the abstract terms ‘knowledge’, ‘information’, ‘concept’ or even ‘method’.

modest, and how to deal with achievement on the basis of other experience, discipline, ability to work in a team or ability to administer, that, is not entirely scholarly or not related especially to intellectual etc. gifts), this would be a solution if not to all, then at least to the majority of the problems in the development of scholarship.

Further, however, we can only advance with the help of the subjunctive mood. If, for example, our professional association were not simply a mechanism for organising congresses (the alternative — the Ministry for Education and Science of the RF — is only a mechanism for distributing funds), then it would be possible to evaluate the state of the social sciences in the country (and anthropology in particular) in the spirit of notable British reviews — Clapham's report [Clapham 1947], Heyworth [1965], Rothschild [1982] and Mills [2006], by attracting leading representatives from foreign schools as members of a commission for the definition of the qualities, failings, and the most effective potential areas of development of anthropology in the country.

Or, for example, if the selection of staff and their periodical assessment were led and controlled not by the management, but by scholarly councils or the leaders of certain academic schools and areas (within the bounds of these areas and schools), then perhaps we could see (albeit from an outsider's viewpoint) a very different composition of institutes and faculties.

As regards status symbols and the criteria for defining achievements and gifted people, Sokolov lets the cat out of the bag by revealing 'as a matter of fact' that scholars themselves know who's who and what they're worth. The problem, of course, lies only in co-ordinating the criteria for this kind of confidential integral evaluations, and in evaluating the assessments of the self-proclaimed 'experts'. The problem of completeness is partly solved by calculating the topography of existing areas of research, since status hierarchies, in my opinion, can only form more or less correctly within each of their boundaries.

Around seven years ago, I was interviewing my colleagues in several towns in Siberia about the developmental paths of the centres there and the presence of independent schools and approaches. One of them admitted to me that he was writing his (extremely interesting, in my opinion) works for no more than two or three people — one in Moscow and a couple in St Petersburg. I think that if I had tormented him a little more, he would have been able to name another dozen qualified readers of his works both in Russia and elsewhere. As a matter of fact, those are roughly the boundaries of the circle of experts, twenty (or taking into account likely variation in the lists, perhaps fifty), individuals, who would, if a survey was carried out,

have no difficulty at all in carrying out a ranking, making it very clear who is a leader, who generates new ideas and conceptualisations, who collects and processes primary with great talent, and who may have published on a given topic, yet can only lay claim to a place at the back, etc.

The community of Russian anthropologists, despite a growth in the number of faculties and courses, remains comparatively small; it barely exceeds a thousand people. Within it, there will be 20-30 more or less actively developed areas and specialisations. Finding the solution to this kind of task may be time-consuming, but it doesn't equate to solving the Binomial theorem, as Korovyov memorably said to Andrei Fokich Sokov.

There are still a few 'what ifs' which, in my opinion, if not immediately then at least in due course, would influence the situation in the necessary way, were they to be realised. For example, if the huge amount of money that the RAS spends on global umbrella-projects, covering all and sundry and therefore in the end becoming absolutely incoherent ('ethno-cultural interaction in Eurasia', and so on), were instead to be spent on providing academic libraries with modern literature and online access to international databases of online journals, on prizes for publishing decent textbooks, on support for translations of classic and modern anthropological literature, on adopting genuinely anonymous and independent reviewing of all scholarly outputs (with obligatory and regular rotation of the members of expert councils), on a distinct and well-grounded identification of the high-priority academic areas, it might well become possible to determine at first hearing which precise period a particular article related to — if it was written in the late 1960s or just this year.

In the meantime, we prefer to be proud of our achievements and traditions, so continual and unwavering that the reader's eyes skim over any 'new' text's usual surface of polished clichés in the certainty that nothing new will disturb the peace, nothing will stir things up. Many people, however, no longer read these texts — there's no point, you really need to write. The quality of what is written hardly worries anyone. Firstly, there are editors, and secondly — there will always be space to get published, even if it is 'at the author's expense'. Amen.

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ANDREI STARODUBTSEV

Between Efficiency and Stability

In the introduction to his article, Mikhail Sokolov writes: 'If our official's thoughts were to run along the same lines as those of most of his counterparts around the world, he would come to the conclusion that the essential problem lay in the inefficiency of the system by which resources are allocated among scholars. He would probably tell himself that the greatest result could be achieved if maximum opportunities were concentrated in the hands of the most gifted, and conversely, if talent and achievement (and only talent and achievement) were always to receive a timely reward.'

All subsequent discussion in the article with which, generally, one can only agree, is based on the assumed existence of this 'ideal' official. However, the development of a research area such as policy analysis shows that 1) the officials responsible for teaching a particular political policy are not the only social actors making decisions about the means of its implementation; and 2) 'efficiency', as understood by the mind of an official (or officials) is not necessarily connected to 'securing a leading role for national scholarship across the world'.

I will begin with efficiency. 'Securing a leading role for national scholarship across the world' is a strategic goal that looks marvellous on paper

and in reports. However, officials think in short-term tasks, among which the main task is to maintain stability in society ‘here and now’: a given political policy is ‘efficient’ because it maintains the status-quo and at the same time brings dividends in the short-term.

What does the strategy described as ‘maximum opportunities concentrated in the hands of the most gifted’ actually mean, when it is implemented by someone like this official? In the language of officials and politicians, this formulation means that the larger part of financial resources will be in the hands of ‘the scholarly minority’ that even now actively enjoys grants from Russian and foreign funding bodies. Moreover, this minority will be concentrated in the big cities and a couple of regional centres. As a result we can expect a situation to form in which the majority of Russian scholars will be compelled to exist on the minimal money remaining after the main government contractual work and grants have been distributed among the outstanding representatives of Russian scholarship. In the conditions of the existing ‘Soviet legacy’, in the form of the presence in every region of a scholarly centre with a large quantity of scholars, this way of distributing financial and other means would not work to maintain the existing situation, but rather see it deteriorating for thousands of provincial representatives of scholarship, which could provoke protest from them.

Thus, one might suppose that for officials, the ‘leadership of Russian science and scholarship’ is expressed not in results of a scientific or scholarly kind, but in the extent to which scholars and scientists themselves (all of them!) are satisfied with their socio-economic position. It follows that the result of any choice between social stability and ‘efficiency’ in an academic sense is far from easily predictable.

Mikhail Sokolov’s article addresses the situation of sociology, but it is hard to imagine a government official who would actually be concerned with the development of just one discipline. He or she would be much more likely to be faced with the task of developing ‘Russian science and scholarship’ in the broadest sense. And such an official would be likely to find that Russian science and scholarship was world-leading in some areas, while being highly marginal in others (which latter list would, by all accounts, include sociology).

On a practical level the ‘stake on leaders’ would thus signify the channelling of funds in favour of physics and mathematics. But this would mean that the state essentially abandoned the development of all remaining areas, which is intolerable for a ‘world leading’ culture. A leader — from an official’s point of view — should be first in everything. Consequently, funds from the state would be spread out over various scholarly disciplines.

Of course, we cannot forget about another factor at work in the distribution of means — the strategic importance of particular scholarly areas. If the state placed a stake on the development of nano-branches, then a large part of funding would go towards creating and supporting nano-centres, whereas geography, say, would be ruled by the distribution-equalising principle of funding. But what should be done with history, which is strategically important for a different reason — from an ideological perspective? One could suggest that it is precisely these thoughts that would overwhelm our ‘enlightened bureaucrat’, the person responsible for scholarly politics in the country. One could, of course, hope that sociology and, closer to my interests, political science, would become equally important disciplines, but there is an apprehension that even if this happened, the golden rain would pour down not on the ‘gifted minority’, but on the loyal majority.

Another problem is that decisions about the nature of carrying out political policies like these would be taken not by just one official, or even just one department. As is well known, any political direction derives from a conflict of interests between officials, politicians, and interested groups. Politicians, who are oriented towards visible, positive, and large-scale actions which allow them to get re-elected, only aggravate these tendencies to differential funding described above. As regards the interested parties, a sense is forming that the dominant player here is the Academy of Sciences, the only one that has lobbying potential at a governmental level. The RAS is a huge, inefficient economic complex that requires constant financial input whatever happens, and so one can suggest that this organisation has a negligible interest in finding ‘points of growth’.¹

Therefore it seems that the project of re-orienting financial funding to more cutting-edge academic groups and products is doomed to failure, even if someone can work out a method of finding these ‘points of growth’. I am convinced that Russian scholarship cannot be reformed from within, i.e. while taking into account the opinion of all interested parties. In essence, our ‘ideal’ official should choose tough and unpopular reforms, in which no one has an interest apart from the ‘gifted minority’. But it is doubtful whether they will be strong enough to see the reforms through to the end.

¹ The equivalent cliché in recent British funding-speak would probably be, ‘pockets of excellence’. [Editor].

KIRILL TITAEV**Officials, Deans and Contracts Invisible
to the World: A Response
to Mikhail Sokolov**

My commentary to Sokolov's article will take the form of a proposal for additions to the argument offered. The text is undoubtedly the most in-depth analysis of the situation in Russian scholarship that I have come across. However, I am inclined to contest Sokolov's implicit theory that Russian scholarship is a space of interaction between scholars and the state. Furthermore, I wish to show existing examples in Russian practice of the successful resolution of the problems Sokolov describes.

The picture that Sokolov's article presents is typical for many disciplines in the post-Soviet era. The situation is described in the political sciences [Blyakher 2001]; as far as I am aware, works are being prepared which pinpoint an analogous situation in ethnography/anthropology (B. Wiener). There is a widely-accepted description of the situation in the humanities where many of those phenomenon described in the article are mentioned [Forrat et al. 2008]. However, all these texts, like Mikhail Sokolov's article, stop at the level of a commentary on a sad situation. I would like to describe an experience which can be considered successful. This experience is perhaps not known to my colleagues, since, as far as I am aware, it is not described in any academic literature.

In the course of my research on the technology of producing applied expert knowledge (in marketing, management consulting etc) I found, much to my surprise, that several years ago the situation in these areas was no different to the one we can see in Russian sociology. All the signs listed by Sokolov were present, as a result of which identical problems with regard to the non-existence of reliable professional status

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symbols existed. However, this problem did not prevent the more or less successful development of the profession.

In order not to weary the reader, I will provide just one example in which, it seems to me, a situation can be seen that is analogous to the current state of affairs in Russian scholarship.

Last year I had a vacancy come up that I could not fill with my own staff. It was announced; the resumé's arrived. Two of them were just brilliant — one had five years' experience in a similar position in X (an important competing company in which the informant works), and the other had had a series of articles published in Y journal (in the informant's opinion, the most authoritative Russian journal in marketing). I summoned them — brought them across the whole country — and I thought to myself I'll choose one of them. The first turned out to be an official for whom this position was simply like a large and conventional bribe, and who understood nothing of this business, and the second was a teacher who, on commission from a journal, had regurgitated Swedish books on management, and had never looked a living salesman in the eye (the Russian manager of a major tobacco company).

In this way we see a picture in which (just as in scholarship) existing symbols turn out not to be transparent. Possessing them in no way guarantees the presence of any merits in the symbol holder.

Of course, I am not about to assert that the situation was completely identical. In business there is no or almost no economy of attention. Correspondingly there is no necessity to make decisions about whether or not to read particular authors. The main criterion is the issue of hiring an employee or entering a contract with a consultant. This greatly simplifies the situation.

Today in this area there are stable reputations — everyone more or less agrees which journals are unconditionally authoritative, and which companies always hire professionals of a high standard. However, according to informant responses, five (or even ten) years ago the situation was analogous to the one we see in scholarship today. Everyone possessed some symbols, but no one could interpret them.

According to my observations, outside of the higher echelons this has remained the case to this day. There is a consensus that experience of working in RBC¹ is extremely good. However, one need only go one step down in the hierarchy of consulting companies, and it is suddenly incomprehensible to anyone whether it is good that a person has, for example, experience of working in the consulting company 'Pro-paganda'.

¹ RosBusinessConsulting [Rozbizneskonsalting] — the main consulting and analytical company in Russia.

I will describe the mechanisms which allowed Russian marketers, consultants and so on to embark upon a path leading to a single scale for evaluating professional authority, that is to move away from Sokolov's model depicted in fig. 3 and towards the model in fig. 2¹.

The fundamental peculiarity, the main, if you will, resource, was the fact that instead of supervising groups (symbol 'issuers'), supervising persons emerged (or small groups of persons), which completely privatised these symbols. In this way the situation gave rise to a system of symbol supervisors, personally responsible for the actions of symbol holders.

This entirely fits the model described in Sokolov's article. Furthermore, as the model suggests, the attention paid to the quality of symbol holders has begun to grow on the part of supervisors, since both directly and indirectly (through reputation) their income depends on it.

It is true that the situation in marketing and consultancy today is far from ideal. It is also true that to introduce 'universally recognised symbols' onto the market one needs very significant initial resources (financial, intellectual and in terms of reputation). Those who do not have them continue on in a world of opaque and closed (indecipherable) symbols. But with each year that passes, the space for open and transparent reputations grows.

This example allows us to see the key fault in Sokolov's article. In applying a model to the Russian scholarly community attention is allotted to the 'average sociologist' and 'official'. As is visible from the very first page, the heroes of the article are the 'degree holder' and the minister. In actual fact this is not quite right.

The academic market is not a space in which ministers and scholars play. It is a space in which officials (acting like a group of firms² providing investment), and firms with personalised management, interact to produce 'scholarly' produce (faculties, institutes, laboratories and things of that sort).

The problem is not that there is no interest in raising the quality of scholarly produce from the point of view of the ministry management (Sokolov's article also talks about this). For instance, on 15th September 2008 an ordinary pack of methodologies was published on the subject of evaluating the effectiveness of scholarly activity³. The

¹ I hope that it is obvious to the reader that the necessity of evaluating professional competence in this area is no less essential than in scholarship as such and that the segment of space for non-intersecting disciplines is no less (even if no more) than, for example, in sociology.

² The term 'firm' is here also used in the sense in which supporters of neo-institutional economic theory use it. A firm is a group of agents, who on a permanent basis strive to reach common goals through mutual action.

³ <http://www.strf.ru/themes.aspx?d_no=15502>.

problem is that the ministry (or the state system for managing scholarship and for investment in it) does not comprise a single overall and coherent management; it is made up of specific officials' 'firms'. These firms complete the concrete transaction (evaluating the efficiency of work, distributing funds and so on). The stable practice of implementing the rules that they arrive at is a hundred times more important than the rules themselves.

The problem is equally not to do with the fact that the 'average scholar' is not interested in an increase in the quality of Russian scholarly production.¹ The intricacy lies in the fact that the 'scholar' is forced to play according to the rules relayed to him by his immediate superiors (or officials — in cases where a direct interaction begins, for example, in competing for scholarship support from state funding). He has too few resources to participate in the formation of conventions or to create stable coalitions and defend their interests. He is excluded from this space, he is a simple player who is forced to obey the rules relayed from above as a given.

In this way, we see that the zone of fundamental interest is the space of contact between the managers of scholarly firms and managers of bureaucratic firms. Contracts arise between them which in time become constant and gradually transform into institutions² that regulate relations in that market. It is in this 'swamp' that good ministry initiatives and the no less good intentions of individual scholars drown.

Does scholarship need crystallised symbols? Undoubtedly. But not the symbols which Sokolov writes about. Not publications and projects. Symbols like membership of the Study-Methodical Association (and also other innumerable would-be-state structures) or a position in management. This is what allows one to become a fully-fledged market participant. For simple scholars, as Sokolov quite rightly notes, it is essential to build relations with a specific dean, not to get published in the *American Sociological Journal*.

So now I propose to return to the issue of what happened to Russian marketers in the twenty-first century and to American doctors in the twentieth century. Why did they succeed and why can sociologists now not? Because every case of successful discipline consolidation (perhaps in any profession, cf. [Freidson 1986]) is characterised by the *absence* of a stratum undertaking the hazardous, back-breaking work of creating regulated institutions.

¹ Although it is worth paying attention to the described situation, in which a scholar in fact does not know about the existence of the scholarly activity standards, apart from those which have been adopted in his faculty (see [Titaev 2008]).

² This term is also meant in its neo-institutional sense — as a set of rules regulating transactions.

In this way, the model described by Sokolov is not entirely applicable, because the players, or ‘participants’ of this market, are not the right ones. Sokolov’s article analyses the state-scholar relationship, not the dean-official one. At the same time, that academic space which, like the Russian human sciences, began its development in the 1990s, essentially from scratch, is still evolving very successfully thanks to a lack of ‘dean-official’ dominated territory separating scholars, users, and investors.

Accordingly, one of the most important steps whose completion will bring us closer to a normalised situation in Russian science and scholarship would be the destruction of all the cosy agreements set up between all those innumerable deans and directors and officials from government ministries appointed to regulate science and scholarship, and the creation of a field of relatively impersonal and formal communication.

However, each of us represents only too well that chaos which actions of this sort give rise to in their early stages, and, taking fright, as a rule we prefer the current situation.

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MARINA VOLOKHONSKAYA

**Why Psychologists Are Not Academics:
Remarks by an Insider**

The discipline of psychology, like any other, opens up only gradually to the view of its adherent. Possibly, if the author of these brief remarks were actually in possession of any status symbols, even if rather ambiguous ones, then she would experience great delight on the grounds of the academic career standing before her, and this affective reaction would help her in solving the complicated task of the stratification of Russian scholars in terms of the honours awarded to them. However, it's quite likely that all that will be audible in these remarks will be the author's own usual voice, with a flavour of not-quite-suppressible optimism.

Scholarship develops within the framework of its corresponding community, and, as Mikhail Sokolov notes quite fairly in his article, the academic community in Russia is notable for a series of specific traits, which by no means lessen its effective functioning and development. There is a desire to supplement this series with one's own reflections on the discipline of psychology. My observations concern not so much the system of defining the status of psychologists, as psychological factors, which at the time of writing hinder the use of *any* status systems by scholars.

In order to avoid possible incomprehension, we must anticipate that these observations are of an explicitly phenomenological kind, including a) that they are exclusively subjective. and b) that analysis of their results adopts the logic of monographic research of a single object — a Certain Faculty that Shall be Nameless in a Certain University that Shall be Nameless, with which the author has had a close relationship over the course of ten years.

So, the first peculiarity of psychology that becomes evident is that scholars working in state universities (with the exception, probably, of

employees of universities of federal significance), experience acute pressure from their own university management. The author has heard first-hand a number of complaints from the research management department (RD) of the above-mentioned Faculty that Shall be Nameless in a Certain University that Shall be Nameless to members of staff in a Certain Department that Shall be Nameless, which, among others, included:

- the complaint that only a relatively small number of post-graduate students had reached the point of defending their doctoral thesis, accompanied by the threat to reduce the number of places for post-graduate study;
- the demand to increase the number of applications for grants being put forward through the university RD, not individually;
- the accusation of there not being a sufficient number of significant academic publications.

These complaints, whose tone forces one to suppose that the staff of the abovementioned Faculty that Shall Be Nameless consists wholly or mainly of out-and-out parasites and saboteurs, would look entirely well-founded if it were not for the presence of several factors which unambiguously are ignored by the governing body of the university.

a) Post-graduate students who enter the Faculty that Shall Be Nameless, regardless of declared aims, in more than in half of all cases say their real goal is not to defend their doctoral thesis, but to defer national service, search for an advantageous marriage, develop their career in an entirely different place, and prolong their childhood for as long as they can. As a result only those post-graduate students whose real, precise goal actually is to defend their doctoral thesis ever do so, or those whose academic instructors were able to find the time, strength, and desire to carry out the work of the post-graduates for them. For its part, a university cannot influence the life goals of its post-graduates, but in principle it could motivate academic instructors to expend the aforementioned time, strength and desire. However, no actions on this account are actually being undertaken by the university. A reduction in the number of places available for post-graduate study does nothing to help solve the issue that the research management department of the university finds so distressing.

b) The university takes so much of the money brought in by the aforementioned grants that for the academics working in the Faculty that Shall Be Nameless, it makes no economic sense to apply for those grants.

c) A university in no way supports the publication of its employees' academic works. The sum required to publish an average sized article in a university edition of a periodical corresponds approximately to the monthly salary of a *dotsent* [senior lecturer or associate professor] working in that university as a full member of staff. A university's publishers also remove the right to the free publication of collections of academic works written by faculty employees.

In this way we arrive at our first observation: a university with psychologists on its staff carries out sabotage of their academic activity, ignores the real situation with post-graduate students, and simultaneously accuses the academics themselves of working badly. The author is filled with pleasant surprise that after this, her colleagues in the Faculty that Shall Be Nameless still remain her colleagues.

The second peculiarity of the case of psychology, as realised in the Faculty that Shall Be Nameless, is that academic psychologists simultaneously have a role as teachers in the university. When the author of this text was still studying for a master's degree, she was presented with exhaustive explanations relating to a teacher's obligations in high school. Included in these obligations, as is well known, is teaching, research work, and also the study of teaching methodology. In the University that Shall Be Nameless, which at the moment is the subject of our research, the combination of these types of activity possesses the following peculiarities:

a) The official workload of a university teacher includes *only* lecture hours. This means that the workload is calculated according to generally accepted norms. In the Faculty that Shall Be Nameless, where the workload situation looks not at all bad in comparison to other faculties, the average workload for a lecturer is 520 lecture hours, 500 lectures hours for a reader, while for professors, this indicator differs too much to make calculating a meaningful mathematical average possible.

b) All teachers are supposed to carry out teaching method work. For example, in the last academic year faculty teaching method work mainly involved constructing teaching method schemes in the approximately 120 disciplines being read by employees of the faculty. A teaching method scheme consists on average of 50 pp.. The faculty has around 30 employees (on average 4 disciplines @ 50 pp. per person).

The author supposes that if teachers were robots, specially programmed to carry out academic research and work on teaching methods, then undoubtedly they would be left with enough time to visit the set of the sequel to the film *Transformers*. But unfortunately teachers are not robots nor statistical individuals, and academic work suffers first of all from obstacles created by fulfilling other obligations.

It is possible from year to year to read exactly the same lectures, one can become a dab hand at throwing together a teaching methods scheme in no more than two hours, but one cannot do academic research automatically. It's no wonder that one of the most important pieces of advice the author received (but, alas, could not apply) during her time in post-graduate study was that a doctoral thesis should be written during the summer, in the vacation. At any other time of the year this task is doomed to rapid failure.

On this point it should be noted that in order to receive the relevant research degree it is not sufficient to write and defend a dissertation, one must also go through a series of bureaucratic motions which take up a great deal of time and energy. Judging by the amount of time spent, these bureaucratic motions catch up and overtake the time spent on the strictly academic work essential for writing a dissertation. The same can be said for receiving the title of professor, or publishing an article in a number of different journals, and other actions intended to increase one's academic status.

In this way, our second observation is that scholars teaching in university have no spare time or effort to do any (even absolutely legitimate) academic work.

And finally, let us ask a question: how does this situation influence employees of the Faculty that Shall be Nameless? Any obstacles, including those mentioned in this text, can be overcome when the individual overcoming them has a powerful desire to be a scholar. But reality shows that observing all the rules imposed on those wishing to be a scholar (working in a faculty, successfully defending one's candidate's thesis, doctoral thesis and so on) does not strengthen or support, but very much weakens and diminishes this desire. The author is not sufficiently qualified to fully analyse all the factors leading to such an unexpected effect, but without a shadow of a doubt everything mentioned above is relevant.

Up until now the author has intentionally not broached the issue of remuneration for university teachers, insofar as the amount in no way depends on their academic activity, but increases primarily based on the fulfilment of additional administrative duties. The only exception, of course, is the bonuses awarded for higher degrees and for the title of professor. However, in these two cases the material recompense received is so eccentrically related to the enormity of effort spent, that for someone even marginally talented and oriented towards achievement, it is more beneficial to realise one's talents in any area one likes rather than in psychology, assuming one doesn't want to emigrate.

All the aforementioned observations lead us to a distressing deduction about the fact that in existing conditions the only people who can

work in the Faculty that Shall be Nameless are a) those so dedicated to research that it is entirely unimportant where they do it; b) those for whom university is a place where they receive a formal status (*kandidat, dotsent* [reader], *doktor, professor*), in possession of which it is possible to become more practically successful; c) those for whom it is unimportant what they do so long as the team is pleasant. Out of these three categories of people only the first can claim success in academic activity. The author hesitates to specify the size of this first category.

The author would like, despite everything, to remain optimistic. After all, she works in this faculty too. Therefore this text will conclude with a very optimistic observation: external pressure from a university leads employees of the Faculty that Shall be Nameless to feel a sense of solidarity and become a unified team, ready to successfully parry blows from an external enemy. It is only a pity that this parrying of blows saps this wonderful team of time and effort that could otherwise be spent on the development of academic potential.

VLADIMIR VOLOKHONSKY

Psychology in Its Own Right

Mikhail Sokolov's article was extremely interesting to read, since it raises issues that are becoming ever more topical, and the image of a civil servant in the ministry of education who wants to understand how best to equip Russian academia is not so fantastical. Let us take a look from the suggested point of view at Russian psychology and in passing answer the first question — *can this situation in sociology be considered typical for post-Soviet academia?*

First of all we note both a striking difference between psychology and sociology, namely, the almost complete lack of notable groups of scholars focused on gaining status in foreign scholarship¹. Of course, such status is welcome in every way possible, no one will say anything

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¹ Not counting people who have bought diplomas from the so-called New York Academy of Science, which cost just 115 dollars per year.

bad about it, but at the same time striving for it enjoys no popularity at all.

There are rather a lot of reasons for this situation. However, the main one is that the renunciation of Marxism-Leninism did not at all signify the renunciation of Soviet psychology, for, in contrast to sociologists, psychologists had the possibility of using Marxist constructions in their work as nothing but a special protective amulet allowing them to get published. There was no academic revolution, revision of values or paradigms, nothing of the sort happened with psychology. What is more, it became completely eclectic. And, incidentally, no one was forced to renounce Marxism.

Psychologists almost all ceased to see themselves as belonging to any old psychology school — the phrase ‘our explanation of childhood fears is based on the ideas of Sigmund Freud and John Watson’ will scarcely surprise anyone, and no-one is likely to demand that the author at last make a choice between these two contradictory authors. Yet in the event that such a question was asked, the answer would most probably be along the lines of the idea that the best elements of these theories are united in the author’s conception. It is interesting to wonder whether in sociology there are analytical models in use that successfully incorporate the virtues of Engels, Luhmann, and Luckmann, and also a series of ancient Greek authors, all rolled into one?¹

It was becoming clear by the late 1980s to early 1990s that the syllabus of the founder of the Leningrad State University’s faculty of psychology, B. G. Ananyev, which placed psychology in the centre of human knowledge, was failing. A Russian psychologist, facing a huge demand for the practical application of the discipline, not only abandoned the bounds of philosophy, but set off on the path leading to a self-sufficient academic discipline that has virtually severed all links with other disciplines, including the human and social sciences².

Contact between Russian psychology and its Western counterpart also has a feeling of fortuitous connections.³ It is true that the stars of American psychology sometimes come on tour to Russia, and our

¹ On the admissibility of such situations, see Yurevich, A. V., ‘Methodological Liberalism in Psychology’ // *Questions of Psychology*. 2005. No. 1.

² A clear example of this is the fate of the Institute of Complex Social Research, St Petersburg State University, founded by the same B.G. Ananyev, which called for unity of specialists with different profiles — among those working with Ananyev were hardly any psychologists who received their diploma in post-Soviet times, and the growth of the sociological component in the Institute’s complex research led eventually to an amalgamation with the corresponding faculty, and the creation of a centre for the research and teaching of sociology.

³ However, in the area of psychophysiology contacts seem little more systematic, but in this area what is entirely indicative is that the people who have such contacts more often institutionally belong to the various sub-disciplines of biology, and not psychology.

psychologists visit international symposia. But in order to find just one instance of a Russian psychologist (other than an émigré) getting published in a Western journal, one must search very hard indeed.¹ Therefore it is senseless to use international acknowledgement as a rating, because there will be units of scholars for whom this kind of indicator will be equal to zero.

In the world of psychology there are not so very many different associations: the Russian Psychological Society — the successor of the Society of Psychologists that existed in the USSR — dominates this area practically uncontested, as the central organisation, along with its numerous regional departments. There are also some smaller associations united on the basis of geography or the subject of their activity.²

As a result the Russian academic community within the area of psychology more or less corresponds to the federal model of a consolidated discipline put forward by Mikhail Sokolov.

There is one interesting peculiarity, however. In psychology there is such a thing as practice. It is here that a problem arises regarding the significance of particular statuses, professional associations, certificates, diplomas and other symbols. Is a diploma of higher education needed to work as a psychotherapist?³ Is it necessary to obtain a certificate within a community of practitioners or a psychologist's diploma in order to work with people? The issue of status symbols in practical work is still more complicated than in academia.

To give an example of the devaluation of a symbol, one can refer to the 'Masters of Psychology' series published by 'Piter' press. It became quite difficult to view publication in it as a status symbol after Nikolai Kozlov's book was published there.⁴ Some symbols, on the strength of the sum of their holders, are even becoming negative markers, reducing authority in the community. So it is quite a poor means of demonstrating one's own status to indicate membership of

¹ The faculty team in which the author works might well be considered one of the leading academic teams in psychology in the country, but over the last five years one can scarcely manage to find even three instances of publication in refereed English-speaking journals.

² Psychology has not evaded the tendency towards increasing influence on the part of the clergy — the Orthodox Society of Psychologists is gradually increasing its numbers and influence.

³ To be more precise, 'psychocorrection' — the word 'psychotherapy' enters into the circle of medical specialisms, and one cannot simply work as a psychotherapist in Russia. But one can work as a psychologist. And this at a time when, in the USA for example, to practice psychology and position oneself on the market as a psychologist, one requires not only higher education, but a PhD!

⁴ Nikolai Kozlov is known for his litigiousness with regard to any criticism, so let us just limit ourselves to mentioning that in 2004, 2006 and 2007 court judgements were pronounced, confirming that the community of his followers, 'Sinton', had connections to totalitarian sects, and also pseudo-psychological cults, which 'do not correspond to reality' (for more detailed information see the Russian version of Wikipedia).

the Russian Academy of Natural Sciences, the International Informatisation Academy or a number of other less well-known public academies.

To some extent a similar devaluation at the turn of the century was endured by all traditional status symbols — specialist diplomas, master's degrees, the degree of candidate of sciences, doctorates, academic titles and so on. As a result, one cannot talk about some symbols being acknowledged in some groups and not in others, rather, one should talk about the scant value of any of these symbols. However, the value of a symbol is largely defined by demand for it. Thus, in provincial universities and the manifold commercial education institutions, academic degrees turn out to be greatly respected since they are essential for the management's formal procedures to confirm the status of the whole university¹. In general the value of a symbol can still be defined through the opportunities which that symbol affords you, through competitive advantage.

In this way, going back to the first question, one can say that psychology is a federal discipline, but that this is not such a good thing as one might imagine — the situation is a result of Russian psychological study being closed off from external influences.

Turning to the second question — to what extent does this situation define the lag of Russian scholarship and what is the long-term prognosis regarding the state's lag in attempting reform — several viewpoints are possible. Firstly, the term 'lag' is not quite suitable. When speaking about lag, for a start one would need to decide on the path this lag is taking, and indeed whether we should catch up with America, who, as we know, is moving right towards the abyss. In this way, as Sokolov himself quite rightly notes, some scholars hypothesise not lag, but the departure of Russian scholarship to leadership positions, to the great envy of foreign competitors. However, having dispelled this delusion, one can boldly say that the lack of vital external motivation to gain particular status symbols is a crucial factor preventing the development of scholarship. Predicting where Russian scholarship might be in ten years' time is fairly difficult — most probably exactly where it is now.

The third question concerns which of these status symbols an official could rely upon. The paradox is that however bad a particular symbol is, choosing any one of them will be an entirely positive event. However, it would be best to be guided by behaviour that reflects actual academic activity in the present, rather than an attained status. Somehow it does not raise particular doubt that, among scholars who

¹ One can note something similar in army structures, where in military line units colonels are practically deities, whereas in major military academies they are absolutely rank-and-file.

have published more than ten articles in the last three years, the share of reputable support is greater than among those who have not published a single one. Of course, among those there will be pulp-writers, plagiarists, and madmen. But there will not be a huge group of idlers. Let a thousand flowers bloom — even the nasty ones.

The fourth question — regarding the new academic status symbols — is also difficult to answer. Of course it would be all well and good to design a special virus which makes bad scholars sprout wool from their ears, as happened in the Strugatsky brothers' famous novel, but for the time being that is consigned to our imaginations.

JANE ZAVISCA

To assess the state of Russian sociology, Sokolov develops a typology of academic status systems as anarchic and unconsolidated versus federated and consolidated. Russian sociology is anarchic, producing status symbols that are neither universally recognized nor distributed on the basis of merit. Sokolov closes by asking (without answering) the question of how Russian sociology, and academia more generally, could consolidate around status symbols that reward talent and achievement rather than financial gain or other, non-intellectual bases of influence.

American sociology exemplifies such a consolidated system, in Sokolov's view. The implication is that Russian social science would be better off if it emulated at least some aspects of the American system (although transporting an entire academic system would be neither possible nor desirable). My goal in this response to Sokolov is to explicate in more detail the status order in American sociology and to offer my opinions on the advantages and disadvantages of the market logic that governs the distribution of symbolic goods.

A key problem in Russian sociology, according to Sokolov, is the emergence of local monopolies over status symbols that prevent the emergence of a national consensus over how to evaluate scholarly potential and achievement. There are

three main causes: institutional insularity, limited geographic mobility, and the exchange of status symbols for money or other forms of non-academic capital. These problems are relatively rare in the United States, in which sociology and other academic fields are nationally organized and success depends on competitiveness in a national market for jobs, publication opportunities, grants, and prizes. A market logic is entrenched in the discipline's discourse: mentors advise students about whether it is the right time to 'go on the job market', department heads tell disgruntled faculty that salaries are non-negotiable without an 'outside offer', and recruitment committees will debate the 'marketability' of various job candidates based on their publication records.

The market for positions. How is this national market maintained? A variety of institutional arrangements prevent the emergence of local cartels and decentralise authority over assessments of academic merit. The process begins at the level of admission of graduate students, in which both departments and students compete for positions in formal rankings of quality. Students' scores on the GRE, a national standardised test, are the basis for the first cut in departmental admissions. Students in turn rely on national rankings of program quality to assess departmental prestige and decide where to apply.¹ There is some debate about whether these metrics accurately measure intellectual quality of program or candidate, and many other factors come into play in the matching process, with reference letters from established scholars being quite influential. The process yields a high degree of consensus, as the same sets of students tend to apply to and be accepted at similarly ranked institutions, leading to intense competition for students once admitted.²

Similarly, there is a national market for assistant professorships. Positions are typically advertised through the bulletin of the American Sociological Association, and application deadlines and interviews are scheduled such that candidates and departments can maximize information and options. Doctoral candidates understand that they will most likely have to move if they wish to work as professors, as it is highly unusual for universities to hire their own PhD students. This avoids the appearance of nepotism, creates incentives to acquire status symbols with national currency, and encourages intellectual

¹ The most important national ranking is conducted by the National Research Council of the National Academy of Sciences, which ranks departments within disciplines approximately every ten years (new rankings are set to be released in late 2008). Ranking methodology and rationale can be found at <<http://www7.nationalacademies.org/resdoc/index.html>>).

² Note that the discipline of medicine in the US has a formal system for trying to reach an optimal equilibrium through the National Resident Matching Program (nrmp.org). Medical students rank their top choices for residencies (intensive on the job training in speciality areas required after receiving an MD degree), while training departments rank their top choices for residents, and based on these data a computer algorithm annually assigns students to departments to try to maximize the meeting of preferences across the nation. Participation is contingent on agreeing to the results of the match.

exchange because young scholars must launch their careers in new environments. My own trajectory is typical: undergraduate education in New York City, doctoral education 5000 km away in Berkeley, California, returning to the east coast for a postdoctoral position in North Carolina, followed by an assistant professorship in the south-western state of Arizona.

The market for publications. The competition for publication space is also organised at a national level, which prevents the institutional insularity of journals observed in Russia. The editorial leadership at the *American Sociological Review*, the top journal in the field, moves periodically to a new institution selected by a committee of the American Sociological Association (for example, it is currently edited at the Ohio State University, but a few years ago was housed at the University of Pennsylvania). Even the smallest regional journals, which may not rotate their institutional base, seek to improve their status by soliciting a geographically diverse editorial board, reviewer pool, and submissions. Authors try to avoid publishing in journals that are edited at their home institutions, since curators of various awards and positions may discount publications in which there is any suspicion of an ‘inside job’.

National integration of the market for various rewards is further promoted by curators’ reliance on ‘external reviews’ for assessing the quality of manuscripts, grant proposals, and candidates for jobs. A journal or book press is considered scholarly if it is ‘peer reviewed’, meaning that editors solicit reviews from a national (and often international) network of scholars, based on their area of expertise.¹ Similarly, grant-giving institutions such as the National Science Foundation assemble panels of experts from outside the institution to evaluate and rank proposals. ‘External’ reviews are also extremely important for promotion and tenure decisions.² The purpose of external review in all of these settings is twofold — to assist non-specialists in evaluating the quality of work in a subfield with which they may be unfamiliar, and to reward achievement that is broadly recognised outside of one’s home institution. The integrity of the review process is strengthened by making reviews ‘blind’: that is, the identities of reviewers (and of authors in the case of publication) is kept confidential. Blind review provides reviewers with the

¹ Scholars write reviews on a voluntary, unpaid basis as a form of civic duty and opportunity to influence what gets published. Editors combine these reviews (typically 2–4 per submission) with their own judgement to decide whether to accept, reject, or ask for revision. Editors then write a letter explaining the logic of their decision, and also provide authors with copies of the external reviews.

² In the American system, after a trial period of typically 6 years, assistant professors are evaluated for whether they will receive tenure (job security for life) or lose their positions altogether. Recommendations are reached by a vote of the faculty, contingent on approval of deans and university presidents. Prior to the faculty vote, scholars from other institutions are asked to write letters evaluating the quality and impact of a scholar’s work. These letters form the basis for justifying decisions to higher levels of administration.

opportunity to critique manuscripts or proposals without fear of recriminations from disgruntled parties, and also provides opportunities for relatively unknown scholars to gain opportunities in based on their work alone. Blind review also helps to discourage discrimination, keeping the gender, ethnicity, age, and position of candidates unknown to reviewers.

The features of the American academic system delineated so far all function quite well, in my opinion, to create a national consensus on status markers and to ensure that awards are disbursed based on evaluations of academic merit rather than financial gain or personal networks. Of course, there are always opportunities for abuse, but the risks to one's reputation are high and there are many checks and balances. Of course these formal mechanisms in themselves do not provide any objective criteria for evaluating merit or quality — all they do is encourage assessments of merit to be the primary basis for distribution of resources and rewards.

Now let me turn to some dysfunctions of the American system: the extreme weight placed on geographic mobility; the persistence of subconscious bias in evaluating merit; and low consensus on particular indicators of merit.

Mobility and Movability. Sokolov attributes the lack of consolidation of the academic field in Russia in part to limited geographic mobility, which encourages the emergence of local monopolies and discourages scholars from investing in status symbols which carry weight outside of their localities. The situation in the United States has reached the opposite extreme. 'Marketability' is the ultimate currency in gaining resources in US research universities, and the primary symbol of marketability is an invitation to move to another institution. Scholars typically go on the job market at least twice during their careers — when looking for their first assistant professorship and in the year of their tenure votes — both to secure a position and in hopes of having multiple offers that can be used as bargaining chips in negotiating a better 'startup package' (salary, research funds, and teaching load). Scholars may use so-called 'outside offers' throughout their careers to negotiate for various perks. Because this job market is national, scholars orient themselves towards acquisition of nationally recognised symbols of merit that will make them more marketable.

An idealised market would efficiently match scholars and positions based on merit. However, the American job market is both inefficient and unfair due to the premium placed on geographic mobility. Scholars are forced to apply for jobs even when they have no intention of actually moving so that they can prove their marketability. Departments spend considerable time and money on recruitment efforts, flying out candidates for interviews and wining and dining

them, only to realise later that they have been pawns in a local game. This means that recruiters attempt to assess how ‘moveable’, or likely to move, someone is before extending an interview invitation or an offer.

Those who are willing to move, or are perceived as willing or able to move, are thus more ‘marketable’, while those who are tied down geographically are not invited to play the game. Moveability has less to do with merit than with family structure and life course. For example, divorce is a moment of opportunity — if an established scholar gets divorced, job offers may come pouring in. Conversely, people with school-aged children appear less moveable. Spouse’s occupation also matters — people whose spouses do not work, or work in occupations where jobs are plentiful (e.g. school teachers, doctors, administrators), are seen more promising prospects.

The premium placed on moveability especially disadvantages women, whose spouses are typically more difficult to move and who are perceived as less willing to move their children. Thus, outside departments are less likely to encourage applications or extend interview invitations or offers, and home departments are less likely to go out of their way to keep female faculty happy and off the job market. Fewer opportunities to demonstrate market value translates into fewer resources: women in the United States are less likely than men to be promoted to tenured positions, are paid less at the same ranks, and are less likely to receive institutional support for their careers such as research funding or reduced teaching loads.¹

This is not to say that removing impediments to geographic mobility isn’t a good idea — improving access to housing would go a long way toward this goal in Russia. However, limitations on moveability will persist due to family and life cycle issues. It is possible to mitigate the consequences, however. Department heads should rely less heavily on outside offers as an indicator of value on the academic market in favour of other achievements—publication record, citation patterns, prizes and grants—not impeded by geography. And curators of national prizes and grants can facilitate the research of talented individuals who are ‘under-placed’ and unable to move to the optimal research environment.

Unconscious Bias. Limited geographical mobility is only one of many factors that explain women’s lagging behind men in academia. Perhaps women are simply less productive than men due to a greater commitment to their families. However, a large body of research suggests that this is at best only part of the story. Unconscious bias is clearly at work in the distribution of academic rewards, despite the

¹ For evidence, see the series of reports from the ASA’s Research Program on the state of the discipline, at <http://asanet.org/cs/root/leftnav/research_and_stats/research_index_page>.

good intentions of curators. ‘Old boy networks’ of established scholars, who tend to be white men, have more visibility and thus more opportunity to monopolize status symbols. Experiments show that women and minorities’ performance at various tasks are evaluated lower than male or dominant ethnic groups, even when actual performance is held constant. Studies of academia have found pervasive unconscious bias, which appears everywhere from letters of reference to hiring practices to perceptions of commitment to a discipline.

Such issues were not raised in Sokolov’s piece and may appear secondary in a system in which basic consolidation has not occurred. My point is that consolidation does not in itself guarantee efficiency or fairness, as the American case demonstrates. Stability and consensus can be achieved while discriminatory assumptions about merit persist. Markets do not in themselves solve this problem, despite the economist Milton Friedman’s famous prediction that discrimination would wither away under capitalism because it is inefficient. Blind review practices help — for example one study found that women were more likely to be hired by symphony orchestras if they auditioned behind a partition. American universities are trying a variety of additional strategies to come to terms with the problem of subconscious bias — from sensitivity training workshops to affirmative action in hiring. Some of these strategies could potentially be adapted by Russian universities without waiting for broader consolidation of the status system.¹

Measuring Merit. By definition, as a consolidated field American sociology has a high degree of consensus about its top departments, scholars, journals, and honours. But any one indicator of merit can be controversial. As Sokolov describes, American sociology is organized as a loose confederation around which the status symbols themselves are broadly recognized, but the share that goes to different subfields, and the criteria within subfields, varies. This leads to a focus on easily constructed metrics such as citation counts of how many times an author has been cited by others as a measure of influence. Journal ‘impact scores’ are also calculated as an aggregate of citations.² These scales surely measure something — having no citations is evidence of little to no impact, while scoring very high suggests influence. However, the correlation between citation scores and

¹ Unconscious bias has been most extensively researched with respect to gender, but similar results have been found with respect to racial minorities. The US National Science Foundation ADVANCE is funding studies and pilot programs at several universities to try to improve the climates for women and minorities. Reviews of the literature on unconscious bias and advice on avoiding it in faculty recruiting can be found at the website of the University of Arizona’s ADVANCE Program: <<http://www.advance.arizona.edu/resources.cfm>>.

² An example of sociology journal rankings by impact scores can be found at <http://in-cites.com/research/2005/december_5_2005-1.html>.

substantive impact is not linear. Many citations are purely ceremonial — a sign of recognition (especially of a potential reviewer who may be offended to find him- or herself missing from a bibliography) without serious engagement.

Rankings of departments based on publication and citations counts correlate only moderately with rankings attained through surveys of scholars (e.g. the National Research Council rankings or US News and World Report rankings). Mid-tier departments such as University of Iowa and SUNY Albany move to the top of the rankings based on pure quantitative productivity, while leading departments such as Harvard and Berkeley drop precipitously.¹ It is not surprising then, that mid-tier departments with status anxiety tend to take citation counts much more seriously, especially in promotion and tenure decisions, while the most prestigious departments tend to dispense with formal metrics in favour of informal assessments of influence based on internal and external reviewers' evaluations of written work.

In my view, over-reliance on citation indices encourages 'safe' but not particularly innovative work — scholars who produce a lot of articles in a well-defined subfield can attain lots of ceremonial citations without having great substantive impact. Prestigious departments provide more favourable environments for higher-risk and potentially more innovative work by focusing less on quantity than quality. These higher-status faculties appear to have more confidence (or hubris) in their abilities to assess quality without recourse to formal metrics.

It is probably impossible to reach consensus on how to measure quality — especially in sociology, which as Sokolov points out is a diffuse and diverse field. Perhaps it is best to think of merit as a latent variable — subject to both measurement error and validity problems. Using a variety of measures can help balance against the errors and controversies inherent in any one. Sokolov mentions that the Russian government is attempting to establish such indices to rank departments and journals; it would be interesting to learn more about how these indices are being constructed. National rankings could be an important step towards consolidation of Russian sociology and other disciplines, contingent on two criteria. First, the principles for ranking must be transparent and clearly tied to multiple measures of merit (e.g. national surveys of program prestige, citation indices, record of graduate student placement). And second, incentives must be established so that a high ranking carries meaningful rewards.

¹ <<http://www2.asanet.org/footnotes/feb00/fn02.html>>.

ELENA ZDRAVOMYSLOVA

In discussing the structure of the field of Soviet sociology, Mikhail Sokolov puts forward two main theories. The first is the lack of consolidated authority in modern Russian sociology. The second is the low quality of sociological products (and particularly of sociological education) in view of the asymmetrical nature of the exchange between supervising groups and those aspiring to gain sociological status symbols.

The first theory is borne out by research on the fragmentation of sociology as a discipline and the communicative ruptures between its autarkic segments. Adhering to a variety of notions of sociology's mission and purpose and adopting many different structural positions, Russian sociologists, in my opinion, are not in a position to put forward common criteria for evaluating professionalism. This puts state officials in an awkward position. An official does not know whom to give money to or how to raise the international status of Russian sociology. All this happens because within Russian sociology anarchy reigns, which leads to a lack of unity within the discipline.

The position of the perplexed official is very convincingly described by the author. Of course the state eye cannot resign itself to this lack of unity in the sociological ranks. But it seems to me that the fragmentation of sociology is not a purely Russian affair. With a surprising regularity concordant with changing generations, sociologists all over the world discuss the crisis of scholarship, looking into the dilemmas of political commitment vs. autonomy, transnationality vs. locality of sociological knowledge, pure discipline vs. inter-discipline. The issue of theoretical diversity and the lack of a united social narrative has been decided positively and absolutely. Peter Berger no longer invites anyone into sociology, but on the contrary laments the loss of fundamental principles of sociological research in the USA. There is an impression that

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the self-reflection of sociologists constantly casts doubt on the legitimacy of scholarly knowledge about society. Like Eisenstadt, I think that this is the characteristic feature of sociological knowledge.

But, of course, it is difficult not to notice the Russian specifics. The institutionalisation of Russian sociology was not a straightforward process — it was extremely lengthy and eventful — and critically dependent on the political twists and turns of the twentieth century. From the end of the 1980s sociological faculties appeared, and the number of graduates began to grow continually, but people remained teachers, the majority of whom had no professional sociological education. The majority of sociology teachers are self-taught (with the exception of a couple of dozen graduates from Western universities). The rapid institutionalisation of sociological education led to a discrepancy between the numbers of vacancies and qualified staff. That is why sociological education in the main universities does not to this day meet set standards.

It seems to me that a debate about authority can proceed in the following way.

The ideally consolidated discipline community (fig. 1) that the author of the article proposes is an abstract model. No social science discipline follows it. All the more so in the Russian case, burdened with the pressure of the current political and economic climate. The strategies of ordinary participants in the sociological discipline are constantly oriented towards political and economic conditions and preferences. This is known as ‘beggars can’t be choosers’. In the battle of the elites that can be seen in today’s situation, the ‘masses’ remain on the sidelines. They are pragmatic. And they will support the strongest side, those who end up victors in the ‘conservatives’ and ‘reformers’ conflict. But all the same, we know some sociologists whose actions are driven by professional duty, not due motives of material gain or political loyalty. This is linked to the fact that motives of creative activity — exactly the kind of activity that occupies professional sociologists — are not limited by social or state decrees, nor aspirations for high earnings, which anyway never reach the levels in academia that they do in business. There is something essentially academic about sociology — love of knowledge, revelation of inter-subjective truth (I avoid the term ‘absolute truth’), and comparatively low salaries.

It seems to me that it is necessary to highlight the formal criteria of prestige recognised in the field of sociology, and the informal ones which can either contradict the former, or reinforce them. Formal criteria are specifically those that help take stock of bio-bibliometric data. This information includes jobs, degrees, and the number of publications in referenced journals. It is of course important. Our official is guided by it.

But we live in a society where all formal criteria provoke doubt — this is a part of our institutionalised mistrust, aggravated by the professional doubt of researchers.

The ranking of positions in a discipline is based upon complicated processes for the attribution of statuses which do not just come down to bio-bibliometric criteria. Reputations are created not only by academic degrees and published works (although these parameters should be not be disregarded). I would dare suggest that the specifics of the Russian situation lie in the fact that the usual bio-bibliometric parameters are insufficient. Is it important for one's reputation where a sociologist defended their thesis? By no means always. More significant, probably, is the distinction of whether it was attained in Russia or abroad. But where in Russia — Saratov, Petersburg, Moscow University — no-one really asks. (And as for where abroad — Berkeley or an obscure university — of course not; moreover, in Russia in these times of corruption and string-pulling, where you defended your thesis really has no meaning).

Is it important which journal one's works are published in — *SOCIS*, *Obshchestvennye nauki i sovremennost* [Social Sciences and Modernity], *Sotsiologicheskii zhurnal*? Not really. Russian journal profiles don't tell one much. But what about the *American Journal of Sociology*...? Now that's something! Is it important which publishing house prints a sociological book? Not at the moment — currently the reputations of all these institutions are not clear, have not been crystallised, so to speak, or have a doubtful character. It is clear that there is some status inconsistency: formal indicators — job title, number of published works, degree — might not conform to intellectual authority... (In the journal *Soziologie* two lists were printed, consisting of German authors who were the most cited in Germany and in the USA — the two lists had practically no overlap!)

Informal criteria (as everything significant in reality) are difficult to measure, but it is possible to approximate them using a reputation method. How can the deadly charisma or authority of Igor Kon be measured? By references to his work, of course. But it seems that this alone is not enough. It is important who has spoken approvingly of Kon, in which categories his activity has been described — as enlightener or as administrator... It is for this reason that the archive material of professional biographies of sociologists collated by Boris Doktorov and Dmitry Shalin is so important for our understanding of the dynamic of Russian sociology as a discipline.

The reputation method consists of a survey of experts on definite methodologies. In this case sociologists should themselves be experts — their selection is also a controversial procedure. I would suggest that we will again get several discipline fragments, several extremely contrasting hierarchical lists, which in itself is fine.

My second comment is connected to the problem of the asymmetrical exchange between the supervising groups that distribute positions with the discipline of sociology and those seeking to gain status symbols. This asymmetry is built into the rules of the game in sociology. Supervising groups, due to their hierarchical positions, possess a great symbolic force. But what is it that forces them to follow the principles of fairness instead of being geared towards profit and misusing the trust provided by status? It is probably guaranteed by the professional honour of those representing the supervising groups which distribute certificates, diplomas and other symbols. Only a professional ethos consolidating the community can prevent exclusively mundane economic and political benefits dictating the behavioural choices of sociologists. If this professional ethos is not formed, there is no protection from the market and politicking.

The professional ethos has been taking its time to emerge, despite the 'sociologist's moral code' adopted in 1988. Its formation is hindered by pressure from the state and the market.

In my opinion, the state, as usual, claims to run the show with regard to distributing status symbols. Standards of education in sociology are state standards. UMO (the Study-Methodical Association, SMA) is an autonomous association only in name. The national domain of politics dictates its own rules to sociologists. Young sociologists preparing preliminary drafts of articles would like to publish in the *Sotsiologicheskii zhurnal* or in the *Zhurnal sotsialnoi politiki* [Journal of Social Politics] (and they do), but both of these publications to this day are not included in VAK lists. Authors are forced to seek out other opportunities. To satisfy the VAK requirements, graduate students search out provincial and relatively cheap (authors themselves must pay for publication!) journals, practically inaccessible to professional folk, and that way they gain access to status symbols. The supervising groups in our situation are the SMA and VAK... But who are the bosses?! Do we know their names?

So, when selecting a journal, authors act on the economic principle of the 'costs of publication'. Among the factors this takes into account is accessibility and the journal's 'VAK status', the extent of the publication remit and so on. A journal's political loyalty is rarely part of the choice criteria, since its politics will more often than not be unknown to young authors, and indeed the choice of sociological periodicals is not great. The motives for publications also differ. Some authors carry out a plan of academic activity which is a condition for providing career development. Others publish because they cannot be silent, because they have something to say and must speak out as soon as possible. About burning sociological topics... But journals, as a rule, are too sluggish for authors of that sort. For these

aims the Internet, where reputations are formed more independently, is increasingly used.

What can be done to avoid misuse which tempts the holders of power, i.e. supervising groups? How can their symbols be opposed by something different? (By creating a competing system of symbols?) In my opinion, the very idea of supervising groups should be deconstructed. Indeed everything is clear to Sokolov's puzzled official. He has no doubt about who to make an expert in the formation of state policy on the social sciences. He knows very well that the people who should be invited are the 'sociological generals' — academics, administrators of sociological institutions, directors of national associations, and if the worst comes to the worst, directors of research centres, but only those who are politically loyal, whose views on Russian society and its prospects correspond to those of the party in power.

I think that even if structural conditions assignable to authors are appropriate, they still do not in themselves guarantee professional autonomy. Structural circumstances are, of course, important. They are relevant to the division between the centre and periphery, and also the low degree of competitiveness between supervising groups — monopolisation — and the instability of remuneration received by symbol holders, and the disproportionate income of supervising groups. But an important structural condition — the separation of sociology from state ideology — is lacking in the list of these conditions. The official must begin from the fact that sociology is not a service for the authorities. It should seek to critique society. There is one other important circumstance — the formation of a sociologist's professional ethos.

In order to be an honest sociologist, what do you need to do? You need to have a conscience. So what is a sociologist's conscience? Unavoidably it turns out differently in different sociologists. His or her ideas about all this are dictated by the image of sociology and the role it should play in society, and particularly whether it is an ideology that enables the carrying out of national projects, or a form of critical scholarship aiming at deconstructing the mechanisms of social exclusion.

Thankfully, the monopoly of official supervising professors is not total. It is strong within education, but weaker in empirical research. In sociology there is vigilance with regards to academics with an administrative calling, insofar as the collective memory of sociologists contains knowledge about the costs of servility. Research centres that do not belong to the Academy of Sciences or a university have a greater potential for political independence than those supervised by government. Their prestige as analysts is quite high and continues

to grow. Let us cite, just to begin with, the demand in the Russian media for the surveys carried out by the Levada Centre.

A comparison to the discipline of American medicine in the 19th century is nice and reassuring. Perhaps even Russian sociology will be all right after a few decades. But is it useful to liken the social sciences to the natural sciences? Will there be a logical development? Is that the demand placed on sociological knowledge? Why do people think that? Is there any firm knowledge on which a consensus can be formed?

A small remark: the author is exaggerating, in my opinion, the extent of the consolidation of Soviet sociology in the 1970s. Fragmentation had already begun then. Batygin crudely but clearly outlined these differences that divided sociologists like dogs into breeds — hunting dogs, show-dogs, and working dogs. Furthermore Firsov described the different strategies of sociologists with regard to co-operation with the authorities. There is much evidence of theoretical pluralism also. But it seems to me that the most steadfast recurring division in sociology is that between professionals and ideologists (which sometimes coincides with the division between Westernisers and Slavophiles). To this day, while this dichotomy, recurring in every generation of Russian sociologists, takes on a political character, we cannot expect a consensus with regard to professional authority, or manipulation of the symbols that at present divide the community. Because to this day the fundamental question of Russian sociology is this: ‘Where is Russia going?’, and it is particularly the visionary nature of the answer to this question that dictates the position of sociologists in relation to the authorities.

How can the level of Russian sociological scholarship be raised? Firstly, it is necessary to go beyond national boundaries in terms of evaluating the products of sociological activity. Some of the formal steps in this direction would be the standardisation of Western and Russian academic degrees, the equalisation of the esteem attached to getting work published in foreign and Russian journals, and a review of the list of journals recommended by VAK. (One could go further: the dismantling of VAK, of state standards and the state direction of scholarship, the granting of autonomy to universities — each being allowed its own standards. Then a hierarchy of university reputations would arise — according to whose graduates and whose degrees were better...)

Secondly, we would need to develop a different side of the political commitment of sociology — its critical orientation — and deal with its social acceptance. Critical sociology — ‘the conscience of sociological scholarship’ — does not only answer the question about whether certain actions are possible, but also analyses their immediate

and long-term consequences, allowing for a mass of attendant circumstances, the consequences of which should be taken into account: social stability, social polarisation, social integration, and a degree of social exclusion.

TATJANA ZIMENKOVA

The Problem of Consolidation in Sociology Outside the Post-Soviet Context: a Response to Mikhail Sokolov's Article

Writing a book about the professionalisation of German sociology, I was after a while disturbed by a strange feeling that the state of this discipline in the West still somehow differs from its state in Russia. Yet after five years of work and laborious analysis, I realised that I had been mistaken. Possibly the disintegrated nature of the German sociological community is not so obvious as it is in Russia. However, it still has a long way to go before it reaches the state which Sokolov called a 'consolidated academic discipline'; I'm not sure that will ever be reached, in fact. In this commentary I will try to compare some results of my research into the professionalisation of German sociology [Zimenkova 2007] with the ideas set out in Mikhail Sokolov's article.

I should make clear that I am assuming here that sociology, owing to the specifics of its subject, has a different path to professionalisation from classical disciplines such as medicine or law [Abbott 1988; etc.], which have been well described by the sociological theories of professionalisation ([Oevermann 1990; 2003]; [Stichweh 1994; 2000; 2003a; 2003b; 2004; 2005 etc]).

The second reservation is that I in no way wish to say that the professionalisation or institutionalisation of a discipline in Germany and in Russia happen in an identical way. I would only like to indicate several common aspects, more connected to the specifics of sociology as a discipline than with the specifics of the development of Russian scholarship or scholarship in the post-Soviet environment.

The disintegrated nature of German sociology is quite distinctly visible in the term ‘hyphenated sociologies’ (*Bindestrichsoziologien*), which describes areas of specialism such as sport, urban, gender and other such branches of sociology. Within these branches there are defined mechanisms of recognition and quality standards which, however, are often acknowledged only by experts belonging to these groups. The same can be said of educational standards. Outside of these *Bindestrichsoziologien*, in some general sociological field, even the definition of the discipline’s basic subject turns out to be highly problematic, which, of course, has a negative effect on the possibility of a consolidated depiction of sociology to non-sociologists and the development of a uniform system of sociological education.

My analysis has shown that the essential curriculum, a code or basic course of studies, generally accepted in the sociological community, is lacking even in Germany. It is not possible to say precisely what a person studied to receive their sociologist’s diploma in Kiel, Berlin or Bielefeld for example, even knowing the name of the course they took. It can be defined only when you know the name of their tutors, being acquainted with their works and their system of evaluating students’ knowledge. In a word, quality control is absolutely non-uniform¹.

Regarding the mechanisms of professionalisation ([Oevermann 1990; 2003]; [Stichweh 1994; 2000; 2003a; 2003b; 2004; 2005]), described by sociologists, one can boldly say that they do not work in sociology itself as a profession — at least in Germany. Let us mention for a start the lack of a uniform education, criteria of quality control or mechanisms of exclusion from the sociological community. Quality control takes place, but only within informal sociological associations (a consequence of this control can be to achieve a good reputation or exclusion from an informal association, but of course not from the community of sociologists as such). I did not manage to uncover a single meeting about a case of exclusion from the German Sociological Association (Deutsche Gesellschaft für Soziologie, DGS).² The possibility of officially refusing someone affiliation to a professional body and taking that assessment to a meeting of non-sociologists in Germany does not exist (when comparing this with the situation in law, medicine or even psychology, we probably appreciate the stark contrast).

It is almost the same situation with sociological education: formal evaluation criteria of student achievements do not exist, just as there are no standardised programmes of sociological study — even at

¹ Which, however, as they say, will change with the adoption of the Bologna Agreement. Being a teacher myself, I have yet to notice any changes.

² <<http://www.soziologie.de/>>.

a basic level. Evidently this cannot fail to impede consolidation. Turning to the history of the discipline, it must be said that sociology in Germany after the Second World War experienced a keen shortage of personnel, which led to sociological education being entirely aimed at restoring the academic environment and cultivating a new generation of scholars [Klima 1976]. There were few sociologists, and getting a diploma, an assistantship place and later even a professorship was significantly easier than today. At the same time, the education students received was in close contact with a tutor, which naturally aggravated the fragmentation of the discipline. Small groups formed that had expert knowledge and had worked out their criteria for professionalism and the mechanisms of recognition.

At the moment, sociology in Germany is faced with the need to educate sociologists for a non-scholarly career. At the same time, there is virtually no connection between sociologists inside the academic environment and those outside of it, which greatly complicates the positioning of sociological education regarding practical needs. Accordingly, so far sociological education has been such that students, prepare to be scholars and only scholars, knowing, however, that this is impossible. Members of the Association of German Sociologists (Berufsverband Deutscher Soziologen, BDS),¹ the only organisation in Europe collaborating with sociologists working outside of academic institutes, are forced to resolve the dilemma of their dual identity which is created by the necessity of self-definition in sociology and the impossibility of that self-definition outside of the academic context. German sociology, not having the possibility of being consolidated with the aid of the definition of one subject of the discipline, common to all, suggests consolidation around affiliation to the academic environment.

The issue of transparency of any scholarly discipline or profession for those belonging to it, also mentioned in Sokolov's article, is a major issue of professionalisation. Its solution in sociology is particularly complex. I believe, with regard to the proximity of the sociological subject to issues that are relevant and interesting to non-professionals [Davis 1994], that it has a particularly strong demand or even necessity to emphasise one's knowledge as particularly complex, professional and scholarly (a temptation which, for example, a quantum physicist does not experience quite so often).

At least among German sociologists one of the mechanisms is an artificial complication of the language of sociology [Lachenmeyer 1971]. The incomprehensibility of this discipline's language passes for proof of a scientific character and authority of sociological knowledge. Proficiency in 'sociological language' is a criterion for

¹ <<http://www.bds-soz.de/>>.

integrating into the community. This phenomenon, however, co-exists with the desire to be perceived by a non-sociological audience, the desire to be accepted by and in demand from the non-sociological community in the capacity of experts.

Here we are dealing with several very interesting phenomena, closely linked to the professionalisation of the discipline and to the loss and gain of scholarly reputation and of the recognition of colleagues.

The issue of being in demand by society and the necessity of educating students not just for a scholarly career, creates the necessity of creating a sociological profession capable of existing outside of the academic environment, being in demand outside of it and containing expert, exclusive sociological knowledge. In German sociology, both of its communities — academic and non-academic (the DGS, and BDS) — are at the moment undertaking attempts to position the category of ‘sociological consulting’ on the market of expert services [Zimenkova 2007]. This term, encompassing any non-academic activity done by sociologists, could hardly be called a precise description of a profession, but it at least refers to some kind of professional knowledge. Be that as it may, consulting is possible only if communication with clients is not complicated by sociological language. But the lack of a special scholarly language deprives sociological knowledge of academic authority. This leads to discourse of translation: in turn, the very mention of translation becomes a declaration of the essentially scientific character of sociological knowledge.

In the context of this commentary, it is relevant to mention sociological consulting since there are evident attempts to consolidate the general picture of the sociological profession for non-sociologists and consumers of sociological knowledge ([Blättel-Mink, Katz 2004]; [Behrendt 2003; 2004]; [von Alemann 2002]); at the same time the absolutely non-consolidated nature of this academic discipline remains a fact.

I am not certain that sociology as such can reach a state of being an ideally consolidated discipline, due to the possibility of a precise definition of the subject of this discipline and the heterogeneity of the phenomena with which it deals. Let us return to the German situation: the nationwide German sociological organisation (DGS) formally unites all sociological scholars in Germany (specifically scholars; it was only a few years ago that it became possible to join the organisation without a Ph.D). Nevertheless meetings of the community do not consolidate the discipline. Communication at conferences takes place mainly within thematic sections, where, if something happens, it is only the formalisation of small circles of scholars, united by one theme.

The ideal scheme suggested by Mikhail Sokolov is, undoubtedly, interesting, but looking at the two suggested schemes for the consolidation of the discipline, German sociology would be somewhere in between them. Supervising groups, *de facto* much narrower than ‘hyphen sociologies’, continue to be the main producers and consumers of reputations.

Where there is a predominance of sociological education aimed at an academic career, and a simultaneous deficit of jobs in the academic environment, the main mechanism of recognition for a sociologist in Germany is affiliation or non-affiliation to a university or research institute. Many of my respondents, people well-placed in the marketing or consulting sectors and financially successful, considered the development of their career as a failure in comparison with the true calling of a sociologist. Then other mechanisms of recognition begin to work (or not to work), but with no kind of universality (with the exception of publications in a couple of respected journals). There are also quasi-scholarly mechanisms such as the demand for expert sociologist knowledge from the government, the economy or the media. However, here the community is divided into those who believe that everything should be done for reasons of pure scholarship, and that any concession to the demands of the market (for example simplifying scholarly language for the sake of its comprehensibility for the wider public) is populism and the ‘de-scientification’ of scholarship ([Köhl 2003; 2004]; [Köhl, Tacke 2003]), and those who accept meeting the non-academic audience halfway.

Of course a phenomenon such as inflation (of university or journal reputations) is present, but even that is formed only by rumours (such-and-such university has a weak dissertation council letting everyone through, such-and-such journal contains a poor review), and generally does not completely take away the status symbols of publications, universities and scholars connected to them. I am grateful to Mikhail Sokolov for his remark that the possibility and naturalness of geographical mobility in the community makes it more transparent and more consolidated, and stabilises the symbols of recognition etc. Undoubtedly, in conditions of geographical mobility crystallised symbols work better, but mobility itself is not a guarantee of their effectiveness.

While researching the professionalisation of sociology, I realised that indicators, the basic mechanisms of professionalisation, function in this scholarly discipline quite differently to how they function in many others. Partly as a consequence of the fact that the discipline as such is constantly seeking and not finding the opportunity to show the exclusivity of its research subject or its approach to it [Funken 2000], its heterogeneity is such that it is not useful to expect

consolidation according to any kind of ideal scheme... I think that the situation so correctly and distinctly described in Mikhail Sokolov's article is more the rule than the exception for the social sciences, and that it is defined not by specific processes of their transformation, but by the subjects themselves.

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MIKHAIL SOKOLOV

Response

The attention of one's colleagues is the most scarce resource in the academic world. One cannot acquaint oneself with the entire stream of articles and books, and everyone must choose those which seem to him or her more interesting and useful than others. The evidence that our world has attracted the attention of colleagues and become the subject of their discussion is therefore a vital academic status symbol — a visible indicator that we have been given preference over countless others.

However, in the satisfaction which I received from reading the replies in the discussion begun by the *Forum*, satisfaction in the role played by my own article remained in second place. The purely intellectual enjoyment which we all get when discovering how many facets any phenomenon has and under how many corners of it one can look turned out to be considerably greater. In this response I will not be able to do justice to each of the authors' ideas and the abundance of facts which they cited — and I will not try to do so. Instead of that I will try — acting as the person summing up — to indicate several points of contact between all of us. As in the

article itself, my examination here will be limited to the case of sociology — although, as the responses clearly demonstrated, its problems are not untypical for neighbouring disciplines.

A section of the authors in their responses suggested alternative interpretations of the origins of the status ambiguity that reigns in Russian scholarship. I will risk summing up their ideas in the following way. The development of scholarship happens in the course of a fierce fight for survival which takes place between theories and the scholars who created them. The driving force of this fight is the economic interests of individuals who calculate that they will improve their position on the labour market at the expense of a theory's success, into whose development they have invested time and effort. An intellectual reputation, earned on the 'ideas market', becomes a trade brand, increasing the value of its services as teachers, experts or researchers. However, this mechanism only works when a number of conditions are fulfilled: (1) the conflict of competing views is able to pick out a victor; (2) information about the outcome is available to all interested parties; (3) the services of the victor has a greater market value than the services of the losers; and (4) this great market value under current institutional conditions can be turned into individual profit, such as an increase in salary or improved working conditions. Where these conditions are not fulfilled, scholars are deprived of a significant part of the stimuli for clarifying whose ideas are 'best', and at the same time for the development of ideas as such. A status hierarchy does not arise, because no one is prepared to undertake the effort necessary to create it.

Tatjana Zimenkova points out that the first of the four listed conditions is not entirely fulfilled owing to the very nature of the social sciences, whilst Marina Volokhonskaya and Vladimir Guelman point out that the third and fourth are not fulfilled due to the specifics of the Russian academic labour market. Guelman believes that in Russia, intellectual reputation is not an advantage to the individual competitor, owing to many factors, one of which is the low mobility of the work force. Instead of one national labour market we see a multitude of local markets, often enclosed within the walls of a single organisation. Each of them has its own theorists (as a rule, working also as the head of the same organisation), who can evolve unimpeded in their intellectual quests in any direction they want.

Victory over someone in scholarly discussion in this institutionalised configuration does not bring any career benefit, but instead, simply spells more trouble. Having made an idiot of the only dean of the only social sciences faculty in town, a young associate professor guarantees only that in the foreseeable future he will not become a professor — neither in the same higher education institute nor in another one, even if news of his success spreads across the whole

country, because transfer to another town is economically extremely difficult. On the contrary, the self-proclaimed scholar loyal to his superior can gain significant advantages in terms of protection and administrative progress. The very act of renouncing one's great mentor, which was the cause of many of the most brilliant careers in the Western social sciences (let us call to mind the disloyal students Freud or Parsons), would in Russia bring about their downfall. Movement along the shortest possible path to success in intellectual competition in this kind of system is at times more likely to complicate one's academic career than to assure it.

Additional thoughts relating to the influence of the situation in academic markets and the institutional peculiarities of Russian scholarship on the productivity of Russian scholars are given in the auto-ethnographic remark of Marina Volokhonskaya. She describes the costs connected to inclusion in a scholarly discussion for a faculty assistant — a person in the same status position in which usually the first and decisive steps to glory are made. In this description a deep irony is concealed. In the Russian social sciences one of the paradoxes of their institutionalisation that is repeatedly remarked upon by American and West-European researchers is simply absent. This paradox lies in the fact that the market value of scholars, as a rule, is by no means defined by their ability to carry out the work for which they are specifically being paid. The majority of sociologists (and also political scientists, psychologists and representatives of other neighbouring disciplines) receive a large part of their income for working as teachers, but their rate on the labour market is not at all defined by their ability to teach, but instead by the citation index and impact-factor of the journals in which they have been published.

This visible contradiction disappears when we take into account the fact that outstanding scholars transfer to their students not just information contained in their reading books, but also connections, and an intangible 'personality knowledge', which allows them as a result to bring some input into scholarship themselves. 'Personality knowledge', as the specific social asset of scholars, however, has a value only for those who are oriented towards an academic career. For others it is useless. Education in sociology in Russia is mainly received by students not really intending to become sociologists (psychologists, ethnologists etc). Educational institutions are forced to focus on the need of this contingent — for whom a teacher's reputation is not a competitive advantage for a faculty.

Whether faculty administrators want this or not, in the current situation a well-disciplined and docile assistant, capable of reading 14 lectures on various subjects every week and chiselling out three study-methodical schemes in his free time, and who doesn't give a single 'two' [dvoika] in exams, so that, God forbid, no one will fail

and be sent down, is for them a significantly more valuable acquisition than a capricious professor who once a year prints a much-cited article in a prestigious American journal. Intellectual reputation will remain a resource in this system — but only a resource of the second rank.

Tatjana Zimenkova's response indicates yet another circumstance making authority in the social sciences so ambiguous, this time not just in the Russian case. Authority is earned through the compulsion of colleagues to acknowledge their own achievements as a group. However, as the whole history of the social sciences bears witness, the compulsory power of the rhetorical arsenal in the hands of representatives of these disciplines is so small that it is difficult to point out even one theory created by a sociologist that would over time gain the universal support of his or her colleagues. German sociology, from whose past and present Zimenkova draws her examples, is institutionally organised completely differently to its Russian counterpart, and incomparably better-off economically. But it too is comprised of many different groups with their own ideas of scholarship and a low opinion of each other. Does this not provide evidence that a lack of consolidation can be explained not so much by the organisation of a scholarly discipline, as by the peculiarities of its subject?

It is an interesting argument, and there is undoubtedly some truth in it. I cannot now cite any examples to demonstrate the existence of qualitative differences between the means of production of professional authority in Russia and Germany, apart from purely anecdotal ones. Worse still, I cannot even imagine what that kind of data should look like. Anecdotal examples, nevertheless, exist in abundance. Can figures in German sociology be found who are like professor Dobrenkov, dean of the sociological faculty of Moscow State University, who maintains positions in every conceivable state body that defines the development of the discipline, despite the existence of proven plagiarism in his works? What about finding an equivalent of professor Grigoriev, author of 'the sociology of vitality', long time head of the only sociological faculty (and dissertation council) in the Altai region? Or of professor Nemirovsky, who dedicates himself (and his numerous post-graduate students) to the development of conspiracy theories — while continuing to serve the head of the faculty of sociology in the recently-created Siberian Federal University? And so on and so forth.

The fact that no one has been expelled from the German professional associations throughout their entire history is sociologically extremely interesting, but can be interpreted in two ways: either as a consequence of the impossibility of putting sanctions into action against those who transgress the norms of the professional code, owing to the weakness

of the controllers of mechanisms, or — in a completely opposite way — as a consequence of these norms being put into action so effectively that no opportunity for the demonstrative punishment of deviants presented itself.

Over the whole history of American justice, the criminal penalty for cannibalism has only been applied three times — but this hardly need be interpreted as an indication of weak American law enforcement agencies, or evidence of a lenient approach to cannibals.

A more convincing argument however, is perhaps the simple experiment which every person who knows some student sociologists is in a position to stage. Ask a group of fifth-year students to name some leading German social theorists — Habermas, Luhmann, and Beck will probably be mentioned. Ask them to recall some Russian theorists, and they will most likely stand in a pained stupor, eventually naming one of their teachers and the authors of a Russian textbook they happened to have read.

The creation of a significant scholarly reputation, as the above suggests, is an uncommon social achievement — and German sociology has demonstrated time after time its ability to do this, whereas post-Soviet sociology has not done it once. Knowing full well what one social world is like and being closely acquainted with its problems, it is easy to see their resemblance to the problems of another social world with which one is much less familiar, but a resemblance in principle can conceal a difference in degree. I suspect that our areas of divergence with Tatjana Zimenkova are traceable to this effect, but only research where the Russian and German cases are on an equal footing will show which of us succumbs to optical illusions most severely.

The strength of such illusions can be felt having compared the short response of Peter Rutland, who found — despite my suggestions — that American sociology is very like Russian sociology, and the detailed text of Jane Zavisca which affirmed my conjectures. If we revert to the points made in comparison with the German case (does an American Dobrenkov exist? How is it that American students know American theorists?), it is difficult to suggest that a difference is altogether lacking. Every person who has ever glanced over the review sections in American journals knows that American sociology is made up of ‘theoretical groups’ that do not hold the highest opinion of one another. But there is low opinion and low opinion. From the point of view of an orthodox theorist of rational choice, ethno-methodology is a very odious undertaking. But Coleman’s feelings towards Garfinkel — however deep his disapproval was — probably still differed from those which the majority of people reading this article feel towards the professor who uses the ‘Protocols of the Elders

of Zion' as a source for data, or suggests that he is in a position to research general opinion by listening to the vibrations of ultra-rays passing through his astral body.

In her article, Zavisca shows the mechanisms which ensure the consolidation of the system of disciplinary authority in the American case, and raises an important question about the social price of supporting it, and about its inbuilt mechanisms of discrimination. So, geographical mobility guarantees the market value of intellectual reputations, and they are created at the expense of the working out of theories and facts, which make it through the fight for survival with other theories and facts.

The social price which the community pays for maintaining active mechanisms of intellectual evolution is high. One of the components of this price is the unfairness which it continually engenders. Professional status symbols — as Zavisca elegantly demonstrates — are acquired through purposeful effort. The resources necessary for carrying out such effort, however, only partly coincide with the characteristics which symbols (supposedly) indicate.

Frequent transfers between universities require a certain amount of repute and recognition, but repute and recognition for their part come easier to those who are talented and gifted at their craft. But abilities and purposefulness are not the only resources which ensure the possession of a corresponding symbolic capital. The presence of family obligations or some other circumstances that tie an individual to one place complicate the obtaining of symbolic capital of this kind, regardless of the individual's other characteristics. But symbols crystallised in the form of lines in a resumé do not allow for an unambiguous deciphering, and we cannot establish whether or not a lack of mobility was a consequence of a lack of professionally relevant characteristics. As always in societies based on modern psychological idioms, structurally conditioned inequality is easily adopts the guise of personality traits.

Another section of the authors who took part in the discussion devoted especial attention to the fictional character through whose eyes I proposed to look at the Russian social sciences — the official responsible for securing the 'world leadership' of Russia in this area. Andrei Starodubtsev, Kirill Titaev and Elena Zdravomyslova found this departing point unsuccessful from various positions. Andrei Starodubtsev compares this official with the real officials whom researchers of the political process observed, and finds a striking dissimilarity. Real-life officials are submerged in routine, which predisposes them more towards avoiding failures and scandals than reaching the ultimate goal by the shortest possible path. When dealing with contractors such as academic institutes that have enough

symbolic resources to form a noisy public counter-attack against ‘trouble-reformers, ruining Russian scholarship’, the ministry refrains from any sharp movements and makes up for any step forward with a retreat.

Kirill Titaev supplements these reflections with an analysis of how in fact the practical running of state politics takes place in the area of scholarship. The ministry’s acceptance of some resolutions does not, of course, lead to an instantaneous change in the practice of progress and the distribution of resources in all academic institutes. If they have any kind of effect (which certainly does not always happen), then it is only during the negotiation process between the administrative bodies responsible for their implementation, and the management of the establishments to which they relate. In the course of this process, Titaev demonstrates, any political measures undergo transmutations which often turn them into just about anything, except what they should have become.

Starodubtsev and Titaev converge on one point: even if the issues of the methods of calculating academic status were completely clear, the ministry would hardly dare to re-construct their policy in disregard of the existing hierarchy. And even if they decided to do so, the practical implementation of such a policy would be entrusted to a multitude of interested parties — groups of ministry officials and groups of academic officials — who would re-construct a complicated system of compromises, reducing the meaning of the whole undertaking to nil.

Of course I am in agreement. The story of current cuts in the RAS is indicative in this respect. They were initiated by the ministry in the aim of avoiding ‘surplus staff’, and should have affected employees who were publishing very little. The basis for the so-called ‘re-attestation’ of staff was PRND [Pokazatel rezultativnosti nauchnoi deyatelnosti, or ‘academic activity impact indicator’] points, which took into account first and foremost the number and quality of publications, with the impact of journals factored in). But what actually happened was that the RAS management allocated quotas for dismissals in equal proportion between all subdivisions, and between all institutes. Both the indubitable leaders and the indubitably marginal received equivalent directions to effect equal cuts. As we have come to expect, the means of calculating points left a huge space for administrative manoeuvre.

As a result, in several institutes, re-attestation turned into a way of settling scores. In others — the more cohesive — teams, an important consideration when making decisions about attestation was the miserable pension that discharged elderly colleagues would receive. To save them from this, institute management often preferred not to

re-attest those who could look after themselves — for example, those who spent a lot of time abroad. As a result, the victims of cuts were usually comparatively young employees who because of their youth had weak influence on the administrative core, and who had gained enough repute and connections outside of the institute to be able to live by book commissions and grants, i.e. the very people the reform was designed to support.

Kirill Titaev points out a Russian example of a ‘success story’ in the consolidation of authority in the social sciences — the area of marketing and consultancy research, which has made, in the words of Titaev, decisive steps over the last five years. He interprets this to be a result of the fact that instead of numerous supervising groups with a changing composition, the relevant symbolic capital is more often appropriated by individuals who would, in this way, have direct reputation losses were the symbol holder somehow to discredit it.

I would add to this two other circumstances. The first returns us to Tatjana Zimenkova’s article: competence or incompetence in the world of consultancy (in any case in several of its areas) quickly acquires an unambiguous quantitative expression. Recommendations to invest money which ends up being lost, or an advertising campaign for mass consumer goods which brings a much smaller financial return than similar campaigns generally bring, cast doubt on the professional qualifications of their authors.

The neo-institutional theory of organisations tells us that these differences in the measurability of effectiveness result in completely different organisational structures. Undoubtedly, they also produce different status symbol systems. The second circumstance relevant to the case is that an individual supervising a symbol stakes not just their name, but also their money, on the person to whom they entrust this symbol. A teacher in one of the sociological faculties complained to me that his dean ‘fixes up jobs for the girlfriends of his friends’ sons’. I suspect, that the friends must be very close, and the sons and girlfriends big favourites with the dean himself, to secure that kind of path to a position, say, as a senior accountant.

Katerina Guba’s article makes quite a contrast with the two preceding texts, as she presents the results of her research on the journal system of Russian sociology, as well as an analysis of the work of the groups behind the creation of the Russian Scientific Citation Index (RSCI). The current practice of citation makes its calculations a very controversial undertaking: authors prefer to publish in ‘their own’ (i.e. belonging to the same institution) journals, and cite other authors who also publish there. In this way, every periodical publication represents a local group of authors, and the deciding voice in deciding whether to measure citation of

its members is the person who decides to include or exclude a particular journal in/from the database.

Nevertheless, in the efforts of the RSCI creators Guba sees grounds for cautious optimism. Indeed, it is not that difficult to stipulate measures which would make a primitive 'citation cartel' impossible: it is enough to decide the issue of a journal's inclusion or exclusion on the basis of the number of references in it to publications appearing in other journals — the impact factor — with no account taken of institutional self-citation. This would make it extremely unprofitable for the editor of a journal to constantly publish the works of members of particular networks who couldn't be bothered to send their articles any further. On the contrary, a winning strategy would be gradually to attract works from members of as many different local networks as possible. The article of a widely-published and widely-read author, who in the distant future could cite himself in a different journal, and force others to do this too, would become a valuable acquisition for a journal.

Pursuing their interest in attracting articles of this kind, editors would select texts more harshly, on the basis of their citability, not the geographical or social proximity of their authors, and they would involuntarily enable citation indexes to become less dependent on an initial decision about whether or not to include a publication in the database. In these conditions, an individual citation index would become more stable, and the ability to write articles that were interesting for a wide circle of colleagues would begin to transform into a valuable asset.

Finally, Elena Zdravomyslova poses the general question of the desirability of state interference in the governing of scholarship. In contrast to the previous authors, who — like myself — preferred to describe the behaviour of scholars in terms of utilitarian interest, she indicates the importance of a professional ethos and the value of intellectual independence. Zdravomyslova does not agree that scholars must try to help the official who deals with the reform of scholarship. On the contrary, a worthy task for scholars is to achieve independence from the state, in which case they simply would not care about bureaucracy. Professional authority under these conditions will perhaps never be consolidated, but that is not so dreadful: closed communities of scholars will appear (she suggests that these will be sociologists oriented towards supporting state ideology, and intellectuals who wish to critique it), which inside their social circles will reach a definite consensus on each of their statuses.

This is a remarkable ideal. The problem is that, for sociology as a scholarly discipline (and for ethnology, history and all the rest) it is evidently little realised — in Russian circumstances less than any-

where else. Modern scholarly disciplines with their journals, centres for professional preparation and thousands of members, can attain autonomy from the state in one of two ways. Firstly, at the price of professionalisation in the narrow sense of the word — the transition to economic support in return for service on a contractual basis to individual and corporate clients. This way has been taken (partially, in any case) by medicine, law and economics, securing them reliable sources of funding that are independent from the state budget.

Sociologists, however, as far as I am aware, have not attained mass market demand for their services in any country. Sociology everywhere has turned out to be dependent on recognition as a university discipline by officials (particularly where, as in France, the state dominates the academic sector), and on new bureaucratic decrees on the carrying out of research (including American sociology, described by one observer as ‘an appendix to the universally prosperous State’).

It is possible to attempt to calculate how much money would remain in the hands of Russian sociologists, were we to discount sources that are directly controlled by state officials, either Russian or Western. Probably, this would be enough to support several centres carrying out opinion polls, several groups studying social problems on privately funded grants, and a couple of dozen researchers who write actively and well enough to live on honoraria. All the rest of sociology somehow or other lives on state money.

This does not mean (in my opinion in any case) that sociologists should faithfully serve those who keep them. In the end, officials do not pay us with their own money. But it does mean that social scientists should be prepared to defend their interests in continual negotiations with state bureaucracy. And autonomy from it can only be achieved in the second way — through the consolidation of professional authority, in the course of which the disciplinary community will monopolise the production of information about the status of its members. If an official is forced to look at a community of scholars through their own eyes, then like it or not he will have to distribute resources according to his opinion of their merits.

Sergei Sokolovsky, in observations that non-ethnologists are likely to find exceptionally informative, describes the intellectual condition of Russian anthropology, which he finds to be lagging behind world scholarship by decades. Further, Sokolovsky suggests his measures for dealing with the crisis, a departure point for the acceptance of which is reckoned to be provided by a sober evaluation of the current condition of the discipline. This evaluation, in his opinion, should be given by a committee made up of those whom scholars themselves select as the most competent representatives (a similar strategy of reputational selection is also suggested by Zdravomyslova).

Sokolovsky attributes to me the view that scholars do ‘in actual fact’ know who is worth what. I agree with the idea that each of us knows it, but also believe what some know rarely coincides with what others know. The process of selecting members of this kind of committee will be extremely path dependent — the first definite point taken at random defines the entire remaining trajectory. It is easy to imagine who will end up in the committee, if the momentum begins with, say, the above-mentioned professor Dobrenkov.

Paradoxically, even making it compulsory to include Western scholars in this kind of body does not guarantee that it will meet professional standards of the kind that would meet the approval of the majority of probable readers of this article. As all those interested could find out from blogs in autumn 2008, in answer to criticism of rebels from the OD-group who had condemned the dean of the sociological faculty of Moscow State University for breaking off relations with international scholarship, Dobrenkov, on the advice of Aleksandr Dugin, a professor from the same faculty, invited to address a student audience the ‘well-known contemporary philosopher’, Alain de Benoist, leader of the French New Right...¹

Aleksei Elfimov, in his remarks, gives Russian ethnography/anthropology a similar evaluation. Describing its inherent institutional forms, he asks whether the general concept of the ‘market’ is appropriate to describe it. This remark leads to some interesting thoughts. I would answer that it is appropriate — but only as a general theoretical framework, which allows us to study any kind of interaction as market exchange.

At the same time, Elfimov is undoubtedly right when he points out that the labour market in contemporary Russian ethnography (and I myself will add, sociology) in many ways has very little in common with the open capitalist labour market, according to which a mobile work force constantly moves around in search of employment and higher pay. This market is dominated by bureaucratic corporations in which patrimonial clans, quasi-political parties and other such monstrous characters fight for control over key positions. The distribution of academic status symbols turns into a continuation of the battle between similar groups by other means, sometimes even bypassing ritual attempts to give the process the visibility of intellectual competition.

Elfimov suggests abolishing any system of devalued symbols, giving the management of scholarly organisations more freedom to decide the size of salaries for employees, and stimulating the geographical

¹ See the official site of the Tsentr konservativnykh issledovaniï (Centre of Conservative Studies) of the Faculty of Sociology at Moscow State University, <<http://konservatizm.org/news/cki/100109165435.xhtml>>. [Editor].

mobility of academic staff. I absolutely agree with each of these measures separately as being suitable and necessary. But their simultaneous implementation in Russian scholarship, where the above organised forms predominate, could cause consequences for staff which we will bitterly regret (Elfimov himself points this out). What exactly in this case will prevent all academic institutes immediately being filled up with ‘the girlfriends of friends’ sons’, and at the same time the sons and friends themselves — particularly if they will be able to get more highly-paid positions that way than by taking their chances and applying to be an assistant at a faculty ranked 11th?

Elfimov sees grounds for optimism in the gradual increase in numbers in Russian scholarship. Here I am compelled not to agree, at least as far as sociology is concerned. We do not always realise how huge the numbers involved in our social sciences are. From a numerical point of view, the number of degrees given out every year and of faculties and periodicals means that Russian sociology is one of the three biggest in the world. Judging by the majority of indicators it has already exceeded British sociology approximately twice over, and is only narrowly beaten by its German counterpart. There is no hope that further growth will automatically lead to some kind of improvement (as a matter of fact, I think that an abatement could have a considerably more beneficial effect).

Vladimir Volokhonsky proposes a review of the status system of contemporary Russian psychology — a case that is extremely interesting in the context of this discussion. Psychology is much closer to the classical liberal professions than the social sciences that have been discussed so far: a large section of people who consider themselves contemporary Russian psychologists offer their services to non-academic clients on the open labour market.

Whether as a result of this circumstance or not, academic psychology in Volokhonsky’s interpretation is much closer to being a consolidated discipline than sociology or ethnology. Despite this, it is dominated more by syncretic teachings, surprising in their eclectic nature, than by neat theories of the sort which we are used to seeing in textbooks. This observation conforms to what Guelman wrote about: neat theories arise in the fight for survival against other theories, but this fight does not exist without the competition of their adherents on the academic labour market, which for psychologists is constructed in roughly the same way as for all others. Competition in the area of private practice does not resolve the task of intellectual selection.

Despite these observations, Volokhonsky is more optimistic than the majority of participants in the discussion, and he is almost only one who suggests to our imagined official a formula whose adoption by real officials he himself believes in. The formula consists simply of

pouring more money into the existing system and hoping that among the profusion of weeds there will be some good corn shoots. This, perhaps, is not the most effective use of budget funds, but owing to a lack of a more complete system this distribution will do.

Volokhonsky's reflections return us to the response of Peter Rutland, which concludes with a question: Do I think, despite all the defects of their communities already mentioned, that Russian social scientists are capable of producing significant works? Based on all that has been said, I will answer thus: I would like to be proved wrong, but probably not. Here I agree with Guelman and disagree with Volokhonsky. Research on scholarship of the last decade has demonstrated that the image of a scholar as a lone seeker of truth requires considerable amendment. New theories are not created in isolation. They are always the result of a social process and in some respect are always created not just by an individual who has put them down on paper, but by a whole community of scholars, invisibly present in them as potential opponents, whose counter-arguments the author is prepared to face, or potential supporters, whose delight he would like to merit. Following the very old idea of Durkheim, we should acknowledge that forms of thought always imitate social forms and vice versa. The stagnation of social thought in Russia, about which many of these authors have written, is a consequence of the social organisation of the academic world which we see today. Only structural changes will allow us to emerge from the dead end.

I would like to thank everyone who took part in this discussion.

Translated by Rosie Tweddle