

THE (INFRA)STRUCTURAL BASIS OF ACADEMIC VICE AND VIRTUE
(ON SOME CONVERGENCIES BETWEEN SOCIOLOGY OF SCIENCE AND SCIENCE
AND TECHNOLOGY STUDIES IN MICHEL LAMONT'S RECENT WORK)

The aim of this paper is to explore some of the ways in which sociology of scientific knowledge, social studies of technology, and sociology of academic organization could benefit each other. It is argued that Michel Lamont's work on peer evaluation in social sciences becomes pivotal to the "Third wave" of STS if Harry Collins's description of it as a prescriptive discipline offering meta-expertise on abilities and limits of scientific expertise is taken seriously. The nature and reliability of meta-expert knowledge by social scientists themselves then becomes an issue which leads us back to comparative analysis of academic organization and community life long ago abandoned by STS authors. Lamont describes practice of academic virtue among Anglo-American academics; the present paper complements this description with analysis of academic vice dominant in less lucky scholarly worlds. It is argued that moral assumptions implicit in academic evaluative judgment, as well as infrastructure silently present in functioning of scholarly communities, are responsible for the differences.

"The Third wave" of STS and turn to neglected sociology of the social sciences

In their seminal paper, Harry Collins and Robert Evans (2002) counterpoised what they called the "First wave" of Mertonian sociology of science to the consequent "Second wave" of sociology of scientific knowledge (SSK). Mertonian sociology was essentially an extension of the then-dominant approaches to community structures and social stratification to academic organization. Thus, for example, the "Merton thesis" was an application of Weber's explanation of Puritan economic success to the English success in establishing experimental science (Merton, 1938, see Goldstone, 2000 for upgrade on this line of thought), and treatment of the origins of the "Matthew effect" directly refers to Parsonian explanation of functions of social stratification (compare Merton, 1968 with Parsons, 1963; Parsons and Platt, 1970). The "Second wave", claims Collins, shifted focus of interest from academic organization as a species of social

institution to the nature of scientific knowledge, which the 'First wave' accepted uncritically. It was discovered that the characters of academic institution, as well as that of the wider social context, were influencing the contents of scientific knowledge, not only the speed with which it was produced. While providing undeniably important insights, that shift of focus came at its own price: subjects such as academic power or patterns of recognition distribution fall into relative neglect. Their existence was acknowledged, and they were used in ad hoc explanations, but they seemed to lose much of their appeal as a subject of exploration in their own right.¹

Another side-effect of this shift was lack of exploratory attention which social sciences and humanities experienced. When academic organization was the major subject of analysis, scientific and non-scientific disciplines were studied on equal basis, and a few authors tried to benefit from comparing organization of disciplines occupying different places in epistemological spectrum between high- and low-consensus fields (e.g. Lodahl and Gordon, 1972; Hagstrom and Lowell, 1982; Whitley, 1985). Perhaps, the social sciences received even more attention as studying them required much less time investments into getting a grasp of their subject matter, than was the case with the natural sciences. The things changed when the subject of study was re-defined. Understanding natural-scientific knowledge seemed now much more important, both intellectually and politically. The best work in which social sciences and humanities figure as subject of analysis after 1980 were largely written by the authors outside of the 'Second wave' mainstream – either by those, who largely continued to follow the 'First wave' patterns of work (Whitley, 1985; Becher 2001(1989), or those from completely different fields, such as organization studies, stratification, or sociology of higher education.

The recent developments in the STS, which Collins describes as the advent of the 'Third wave' are probably being changing the focus once again. Collins himself identifies the 'Third wave' with positioning the STS specialists as 'meta-experts' able to give counsel on the amount of credit to be given to scientists in particular circumstances (e.g. Collins and Evans, 2002). Allocation of these responsibilities to the students of science would inevitably raise anew the question of when and how far the experts – these time, the meta-experts - should be trusted. As STS remains a social-scientific field, that brings social sciences into the focus of the scholarly attention once again. Secondly, during the last decade STS undoubtedly were one of the most innovative fields in the social sciences; the understanding of how humans interpret reality and

¹ Admittedly, this is a typical establishing-the-ground first-paragraph overgeneralization. Some important models were developed in this field; suffice is to mention Latour and Woolgar's cycles of credit (1979). Nevertheless, I dare to say that many classical STS studies tended to rely on rather schematic visions of the academic world. Thus, Bloor's (1978) application of Mary Douglas's typology of cultures to explanation of how institutional structures influenced the ways mathematicians approached disagreements used minimalistic (and not particularly clear) classification of academic institutions, which was incomparable in depth with earlier university ethnographies (e.g. Baldrige, 1971; Clark et al, 1978)

impose their interpretations on others (especially as far as material infrastructure of persuasion is concerned) reaches far beyond explanatory models prevailing in other specialties. It might be the proper time to extend this vision to other fields of sociology, including the long-neglected studies of academic organization. Lamont's recent work moves in both of these directions at once: it analyses the character of expertise in social sciences in the light of what social studies of science taught us about human knowledge.

Michel Lamont's study of social-scientific meta-experts deliberations

One of the greatest achievements of STS can be put in the following way: while scientists prefer to explain success of certain theories in terms of their 'correctness', 'legitimacy' is, in fact, a much more accurate term. Theories and theory groups prevail in controversies, because they follow what are currently considered established procedures, because they fit better with the rest of the scientific knowledge than the approaches they challenge, because they offer a solution (or promise of a solution) for a practical problem facing important constituencies, because they are more plausible according to certain wider world-views, because they facilitate political claim-making by a certain group, finally, because those who put them forward are credible by certain standards. The Nature enters the debates in but the most indirect way.

Much recent research in history of science was aimed at exploring changes in such legitimization systems. Steven Shapin's 'The Social History of Truth' (1995), exploring the role played by aristocratic code of conduct in the rise of modern impersonal experimental rhetoric, is an exemplary and perhaps the most brilliant of such attempts. Lamont's study of grant committees' deliberation opens another alley for studying the same topic. Her approach is informed by microsociology of Goffmanesque and ethnomethodological varieties. She treats legitimacy of the final decision and of the whole procedure as an on-going achievement to be sustained by following certain 'customary rules of fairness'. The whole operation of the committee thus turns into a 'protective ritual' in Goffman's sense (Goffman, 1956, and also other pieces in Goffman, 1967). What requires protection here, however, is not only identities of all those involved as virtuous academics. The ritual maintains the definition of the event as a fair and unbiased judgment. Moreover, it maintains the vision of the whole academic stratification, in which the experts are likely to occupy prominent places, as merit-based and just. Finally, by voluntarily 'cognitive contextualization' and respect to 'disciplinary sovereignty' (Lamont, 2009: 107-158; Lamont and Huutoniemi, *manuscript*), the reviewers celebrate status of their disciplines as equal and worthy intellectual enterprises. There are thus important symbolic benefits to be gained from virtuous behavior. However, there are also probable losses, especially

if one has some kind of personal stakes in the outcomes of the deliberation, and there are limited opportunities for policing the behavior of the deviants.

The vulnerability of the academic rituals involved directly follows from the fact that they rely on what Lamont describes as *“customary rules of fairness”*. STS was one of the first sociological specialties which wholly embraced Wittgensteinian dictum holding that no rule contains conditions of its application. Thus, the rule *“reward scholarly excellence”* depends on definition of what does *“excellence”* stand for, but definition of excellence could be given only in terms of some even more vague qualities (Oxford dictionary helpfully offers *“the quality of being outstanding or extremely good”*). As the process of struggling with the dictionary is evidently infinite, any theory describing behavior as mechanically following a final set of rules cannot be accurate. Wittgenstein is usually held to believe that the arising paradoxes could be resolved by supposing that actions involved in following most rules belong to the Weberian traditional kind *“roughly, people use words, as they do, because they are accustomed to it.”*

However, this might be more accurate for some rules, than for other. To add further complication, the *“rules”* are present in most treatments of the subject in at least three qualities *“(a) as hypothetical mental structures governing perception and behavior; (b) as reconstructions of these structures an observer could make for him/herself; (c) an account the actor would give when asked by the observer.”* The former and the latter, practice and account of it, are connected, but not identical *“the subject familiar for most sociologists by the ethnomethodological treatment of it (see Lynch 2002, for application to laboratory studies).”* Pointing to the same direction, distinction between *“the context of discovery”* and *“the context of proof”* were in wide use among philosophers of science and scientists themselves long before that (Popper used it in 1934, Reichenbach in 1935). It seems that invoking rules *“in the latter sense, as a legitimization of one’s actions - tend to have a much stronger instrumental (Weber’s zweckrational) flavor around it.”*

Imagine now a social scientist reporting her findings before an audience of critics. The critics do not see the data themselves and cannot follow the initial line of her reasoning; they can only evaluate the account from the point of view of legitimacy of the moves involved in it, perhaps checking it against general background of assumptions on what is possible.² As Lamont

² Social studies of science demonstrated that *“reproduction of experiments”* is a much more unusual event in scientific practice than dominant philosophy of science claimed for quite a while (e.g. Collins, 1981). In non-quantitative social sciences themselves, however, it is next to nonexistent: rare attempts to reproduce fieldwork result in embarrassing occasions like Redfield-Lewis or Mead-Freeman debates. What reviewers, members of dissertation committees, and panelists in grant juries evaluate are internal legitimacies of accounts, not their correctness in any external sense. As Lamont and her colleagues demonstrated (Guetzkow, Lamont, and Mallard, 2004), this legitimacy includes not only intellectual consistency, but also moral character *“boldness in stepping beyond the work of predecessors, at the same time paying them due respect.”* This legitimacy element is especially salient in the case when research actions are evaluated prospectively, rather than retrospectively *“as it is in the case of assessing”*

and her colleagues' studies of social-scientific originality demonstrated (Guetzkow, Lamont, and Mallard, 2004), this evaluation essentially contains reconstruction of rules the presenter presumably followed – has she surveyed all the relevant literatures and ignored the irrelevant ones, was she innovative enough in establishing her approach, has she appreciated all kinds of legitimate rhetoric counter-moves an opponent might put forward etc.

Imagine now that the critic is responsible for evaluating the author. Analyzing his work would again involve rules in all the three qualities – as hypothetical mental structures, as reconstructions of these structures, made by a critic of a critic, and an account the former could give to the latter (for the sake of clarity, I will title them first- and second-order critics, respectively, and designate the first by male, and the second – by female pronoun; in most real settings, e.g., in panels, these roles are constantly changed). The account, again, could be legitimate and illegitimate. It is universally recognized as illegitimate if motivated by parochialism or cronyism. No second-order critic would be convinced by an argument like 'this is an excellent paper because it belongs to the wife of the dean on whom my promotion depends'. Not surprisingly, no first-order critic would give such an account. This does not mean, however, that considerations of such sort – using any symbolic or economic benefits the first-order critic controls as goods to be exchanged for goods the object of his criticism controls – could not be involved in making his decisions. The second-order critic might suspect that – and, actually, for some academic worlds this is a common knowledge that this is the way the things work (the Russian case would be described later, another examples are classical studies of Italian universities – Clark, 1978). Her problem, however, is analogous to that of the first-order critic – while she could reconstruct for herself the rules he follows and define them as ignoble, this reconstruction should yet be legitimated in the eyes of a third-order critic. Moreover, she could voluntarily wave her penalizing powers in exchange for certain benefits first-order critic could provide

It seems that there is a universal gap between human abilities to reconstruct rules others follow – and, to certain degree, to predict their behavior on the basis of such reconstructions – and the abilities the legitimate their perception. In the rest of this paper, I will call behavior of

proposals. The jury is to decide, if certain researcher *might* produce a legitimate account, if provided with requested funding. The only empirical reality against which these requests are tested is panelists' background knowledge of what is organizationally possible (as one of the panelists told in an interview to the present author 'most proposals, especially by the young, fail because they simply do not know, how much one can do with that money – they sometimes promise a representative survey of the Russian population for 1 million rubles we give out [\$35.000] – which is simply ridiculous'). Another reality is the corpus of available scholarly texts which the author should know – failing to mention them results in degrading the applicant as 'ignorant'. What the proposal itself embodies are thus ideas of what is intellectual legitimacy in their purest form. While natural sciences were often used as a model, ideal type of scientific knowledge against which social sciences and humanities are to be benchmarked, as far as legitimacy is concerned, the opposite direction of analysis might be recommendable. That is in the social sciences that we observe how academic legitimacy is constructed in the most uncompromised forms.

powerful critics, systematically abstaining from their best judgment on true value of others academic works and characters, in order to extract benefits for themselves, vicious. I will call abstaining from such behavior virtuous. Vicious behavior could be divided into two types of covert (in the cases first-order critic successfully conceals his misdoings from the eyes of second-order critic so that her reconstruction of his motives is benign) and shameless (when he overtly exploits the gap between observable and provable). Accordingly, the protective ritual involved in peer-review procedures would be called virtuous if the second-order agents involved in it use all possibilities at their disposal to penalize any opportunism on the side of first-order agents; it would be called vicious if they would use them to conceal others' vicious behavior.³

Lamont's *How Professors Think* describes culture in which peer-review panels are virtuous protective rituals. In the next paragraph I'll briefly describe the Russian academic world, in which they catastrophically fail to meet this standard. In the rest of this text Lamont's analysis will be used as a source of clues on why sometimes they don't work. I'll pay special attention to the role infrastructure plays in fostering academic virtue.

A Russian vignette⁴

In October, 2007 First Deputy PM, Dmitry Medvedev, whom then-President Vladimir Putin has already introduced as his successor, announced during a meeting with Minister of Higher Education and rectors of the largest Russian universities: *“Ghostwritten dissertations are disgusting! [They are] a threat of complete devaluation of academic degrees... Currently the*

³ The schema seems so much dependent on un-observable variables such as the rules critics *really* follow in making decisions on the rules *really* followed by others, that from a general point of view, its empirical applicability cannot but be questionable. After all, there are real differences in understanding of what excellence is, and in this respect the analyst is just one more first-order critic who cannot project his/her understanding to the rest of academic community. Surprisingly, however, for an actual fieldwork it turns out to be less of a problem, than might be expected. It seems that vicious rituals inevitably generate much gossip around them, exactly as Gluckman (1955) would suggest. Even those personally involved in vicious rituals feel strong desire to draw a *role distance* between themselves and the part they played in what occurred. After dean's close relative defended a dissertation at one of Russian sociology departments the present author observed, nearly all the outsider visitors were informed by one or more of its affiliates about all the unpleasant details of the event, including that fact that the dissertation itself was written by the dean's graduate students.

⁴ The following account, as well as the whole of the reasoning the present text contains, are the outcomes of two collective research projects financed by the Higher School of Economics (Moscow). The first of them, titled *“St. Petersburg Sociology after 1985: Economic Adaptation, Institutional Transformation, and Pockets of Intellectual Growth in a Local Academic Community”*, was a Chicago-style community study of a population of some 600 sociologists inhabiting a Russian metropolis during the last 25 years whose institutional and intellectual trajectories we followed using statistical analysis of biographic data, survey, some 50 interviews, survey, discourse analysis, and years of participant observation (Web-site of the project www.socdata.spb.ru, in Russian). I am indebted to Timur Bocharov, Katerina Guba, Maria Safonova Lev Shilov, and Kirill Titaev with whom I collaborated in its frames. The second project *“Academic Status Systems: A Historical-Comparative Approach”* was a comparative study of status organization of the five largest Western sociological labor markets: the British, German, French, Russian, and the US ones. In this project I was lucky to participate together with Katerina Guba, Natalia Forrat, Maria Safonova, Sofia Tchuikina, and Tatiana Zimenkova. All of those listed contributed enormously to the present analysis; none of them is responsible for its shortcomings.

humanities are mostly affected, but eventually the wave would reach other parts of the [academic] system, all of the system, like metastases. That's lamentably! Medvedev returned to the subject of diploma and degree devaluation more than once during this presidency; a few of other highest officials did that regularly as well – even those of them, who, like his predecessor and probable successor, Putin, were caught in dissertation scandals themselves.⁵

They were by no way the first Russian political leaders who busied themselves with fighting devaluation of signs of academic status. The first doctoral degree in the Russian Empire was conferred in 1794, when there was still only one university in the country (Moscow); by 1816, when the first dissertation scandal occurred, there were already five of them (Derpt, Kazan, Kharkiv, Moscow, and Vilno). The Ministry of People's Enlightenment was informed that two Doctor of Law degrees were sold at Derpt Imperial University (modern Estonia). The Ministry's reaction to this misconduct created the path which Russian academic bureaucracy has been following ever since: it established universal requirements to degree candidates, and vested the central authority (originally, the Minister himself) with powers and responsibilities to confirm all degrees conferred in the country. The degrees were temporarily abolished after the 1917 October revolution just to be restored in 1934. The two-level system exists in Russia since that moment: after receiving 5-year diploma, students can enter 3-year graduate school after which they are expected to submit Candidate of sciences dissertation. After that some of them present second, Doctor of sciences dissertation (depending on specialty, from 10 to 25% of all Candidates eventually become Doctors)

The public academic institutions in Russia are organized as hierarchies of status markets: thus, assistant professorships could be held with university graduates without degrees, associate professorships (*docent*) are reserved for those holding at least candidate degrees, full professors are expected to be doctors. Similarly, in the research institutes of the Russian academy of sciences, junior researchers are graduates without degrees, senior researchers – candidates, –leading and –head researchers (*veduschij ili glavnyj nauchanyj sotrudnik*) are doctors. Finally, higher academic administrators, beginning with faculty deans, are likely to hold doctorates as well. While certain deviations from this pattern are tolerated, generally these guidelines are followed. Degrees are conferred by standing dissertation committees of some 25 members representing major subfields of a given discipline (e.g. sociology presently consists of –theory, history, and methodology, –social institutions and structures, –political processes, –economic processes, –culture and spiritual life, –sociology and management). Creation of Dissertation

⁵ Putin's dissertation contained excerpts from a book on resource management by US professors (<http://www.washingtontimes.com/news/2006/mar/24/20060324-104106-9971r/>); soon after the incident was discovered, dissertations of leading Russian politicians disappeared from public libraries. Nearly all of the members of the present Russian political elite hold academic degrees; Medvedev himself is one of very few who defended his thesis before occupying political office.

committees at given university or institute is a subject to approval from VAK (*Vserossijskaya Attestatsionnaya Komissija*, All-Russian Attestation Committee). The VAK also confirms all candidate degrees and confers doctoral degrees (local dissertation committees where the defense actually occurs only recommend prospective doctors).

In spite of all this extensive architecture of academic monitoring, it was widely perceived by academic public that devaluation of degrees was well under way during all of second half of the XX century. In 1975 Politburo of the Communist Party issued a decree calling to *increasing socialist vigilance and intolerance towards cronyism in scientific and educational institutions*. The VAK was shut down for a year, part of local Dissertation committees closed, and new requirements to degree-holders introduced, including publishing their papers in *scientific editions*. These measures decreased number of dissertation defenses for a couple of years, after which accelerating growth of their numbers (adding approximately 10% each year) continued. Since then, history of the VAK became one prolonged struggle for decreasing the numbers of newly produced Candidates and Doctors. At the meeting from mentioning which this paragraph began, the VAK President, Mikhail Kirpichnikov, proudly reported that 2006 was the first year when there was no increase due to restricting number of *scientific editions* in which prospective degree holders should publish their papers, to so-called *VAK list of peer-review journals*. Since then the numbers grew again in spite of most recent VAK requirement to publish dissertation synopses (*avtoreferaty*) on-line and the fact that the controllers started checking dissertations for plagiarism.

The dramatic history of the Russian dissertation complex is but the most conspicuous of the histories of the Russian peer-review failures. That is widely believed that receiving funds from largest public grant-awarding foundations depends on having representatives of one's institution on their expert panels; it is taken as self-obvious that competitions for research or teaching positions all academic institutions announce are a mere formality, and it is the head of respective division who decides, who would receive the appointment.

The governmental attempts to encourage more virtuous behavior, when they happened, generally followed one and the same vein. Firstly, they were all aimed at increasing the number of first-order critics, preferably those as far institutionally and spatially distant from those they evaluated as possible, in order to prevent collusion from them and those whom they evaluated. Thus, a prospective Candidate and Doctor was to provide reviews of his/hers dissertation by a few individuals outside of his/her organization, publish a paper in a peer-review journal (where, presumably, anonymous reviewers would provide an impartial feedback), and to present the results of his/hers work on-line, where anyone could see them. Secondly, they were aimed at limiting the discretion of those first-order critics, who occupied neighboring position in the

networks and whose participation in evaluation could not be avoided. Ideally, their role was to be limited to that of simply fixing some observable results of others deliberations – such as counting publications in journals from a given list. That was the official policy in introducing *PRND* (*Pokazatelʹ rezulʹtativnosti nauchnoj dejatelʹnosti* – The Academic Productivity Coefficient), a formal estimator which is to be used in calculating increments for research performance. Currently Russian Academy of Science and some leading schools, such as the Higher School of Economics and Saint Petersburg State university, are starting experimenting with various citation counts which remove the first-order critics even further from those whom they evaluate.⁶ Thirdly, the role of most important second-critics was allocated to bodies situated in Moscow (the VAK, boards of the state-financed foundations, etc.), and the evaluation procedures carried out by regional bodies were arranged in the way which made monitoring their actions easy for the second-order critics (by filling standardized forms, presented standardized accounts, etc.).

Paradoxically, these measures led to the results which were somewhat opposite to those intended. Instead of weakening networks of mutual assistance, they strengthened them. Now to be viable, an academic clique has to maintain wide contacts with other groups controlling other institutions, periodicals, and, preferably, having representatives at the VAK and other central bodies. If anything, impartiality of most kinds of first-order critics, such as journal reviewers, decreased. As subscription revenues are miserable and no regular grants for publishing periodicals is available, the majority of disciplinary journals is totally dependent on financial and administrative support from institutions with which they are affiliated. University administrators were thus tempted to regard publications in editions they controlled as goods to be traded or distributed among their clients in exchange for loyalty. A few editors reported to the author, that they found progressively difficult to turn down submissions the administrators had interest in. The reforms stimulated network-building and development of academic infrastructure (such as appearance of Web-sites and periodicals at nearly all degree-granting institutions), but they failed to achieve their major objective. If anything, the dependence of individual careers on patronage from academic bosses increased, and the principles of selection have become less meritocratic. As one disgruntled young academic reported to the author:

This entire dissertation thing is aimed at putting you into moral debt. You have to ask many people to write you positive reviews, or sign the positive reviews you write for them. As a result, they feel you owe them. When I said something against Professor X [a senior academic the speaker criticized for alleged using dissertation procedure to put down a member of a rival academic network], the unanimous reaction was “How could you? He

⁶ Another reason international citation counts seem destined to bright future in the Russian academy is that they embody one of the reasons why Russian government supports the sciences: international visibility and soft power academic leadership presumably brings with it.

supported you by writing you a positive review when you asked him for help a couple of years ago!

Besides, the human cost of maintaining the system of status symbolism increased exponentially: apart from preparing the text of the dissertation according to (very restrictive) genre conventions, the applicant is to prepare several dozens of various reviews, resumes, and covering notes, which extends purely bureaucratic part of the dissertation defense to approximately six months. Not surprisingly, additions of further complications to the procedure are unanimously loathed by academics, and provide a universally accepted rationalization for sabotaging the evaluation procedures.

(Infra)structural causes of academic vice and virtue

What was wrong with the Russian government attempts at fostering academic virtue? Lamont's description of the settings in which virtuous behavior prevails provides us with some clues, especially when seen in the light of STS focus on materiality.

The explanation having most circulation in the country itself points to overall moral corruption of the Russian society. According to the more liberal version of this explanation, this corruption is a part of the "traditional Russian culture" (or even of the "Russian national character"); alternatively, it is considered a legacy of the dark Communist past. According to the more conservative version, it developed as a reaction to unprecedented hardships of the post-communist decades caused by liberal reforms. None of the explanations, however, seems plausible enough in the light of the historical evidence. Let us take the example of degrees. In spite of the "Derpt affair", overall behavior of the Russian 19-th century academics seem admirably virtuous; at certain points the universities had to petition Ministry to allow hiring Masters to professorial posts - the number of doctoral degrees conferred was simply too small to provide personnel for the rather tiny number of existing positions; some universities have produced no doctors in decades, although there were no inbreeding laws preventing them from hiring their own pupils, or even offspring. If anything, uncontrollable re-valuation of degrees was under way then. We cannot say when this trend was replaced by the opposite one, but degree devaluation started long before the collapse of the USSR, in the time now considered the Golden Age of Soviet communism.

If not "culture widely understood", what then? There are two general social-scientific explanations for virtuous behavior on the part of first- and second-order evaluators. The first of them points to schemas cutting down incentives for vicious behavior on the part of first-order

critics and providing them with financial rewards for acting virtuously. The second explanation points to the situation of second-order critics.

The *cutting-down-incentives* logic is best reflected by actions of the Russian ministry which attempted to rely as much, as possible on judgment of distant agents not involved in the same network with those whom they evaluate. Surprisingly at the first glance, the Ministry never considered outlawing inbreeding. Prohibiting institutional incest is a principal institutional device dividing symbolic and distributive power. Under the inbreeding ban, those who can provide an academic with credentials cannot provide him/her with a working place (which makes credentials not supplemented by qualifications much less valuable), and those who provide the working place cannot provide the credentials (which make their possible deviations from meritocratic principles much more exposable). The reason inbreeding has never been outlawed in the Russian academic world is trivial: there was very low, by international standards, academic mobility during the second half of the 20-th century, and in most cities there were only one department in most specialties, so that banning inbreeding would completely deprive it of candidates for higher-level positions.⁷

The low voluntarily population mobility in Russian Empire, the USSR, and post-Soviet Russia seems one of the crucial factors in history of each of these successive states. Several causes contributed to it: one of them was climate and related high housing prices; another ó space and related transportation costs, others were legislative and economic. Ineffective real estates market additionally inhibited mobility, and under serfdom and Stalin's rule, large masses of population were legally bind to particular locations. Above that, there was general bureaucratic-cum-infrastructure paradigm which organized all spheres of life as series of tree-like hierarchical structures centered on Moscow. Infrastructure shaped academic labor market; we'll see later, that it had even more penetrating effect.⁸

As far as second-order critics are concerned, their good conduct seems to depend on three conditions:

⁷ Thus, of some 400 people who occupied positions in St. Petersburg sociological departments and institutions after 1985, no more, than 10% were ever affiliated with any academic employer outside of the city after getting their first (Candidate) degree (visiting scholarships not included). Mostly this 10% were senior academics who departed to Moscow. The typical academic mobility pattern is slightly similar to the French one, but with two significant differences: firstly, mobility of young degree-holders from the capital to provincial towns was relatively rare; secondly, teaching at a provincial university while permanently residing and maintaining social networks at the capital was impossible for logistic reasons.

⁸ The discussion of possible direct economics rewards for virtuous evaluatory conduct is omitted here as such institutional designs are highly untypical for any academic world. They would include insurances from degree-granting institutions to the future employer, guaranteeing that the university would provide another graduate if the present does not meet expectations. The whole economic literature on market signaling, including Nobel-winning works by Spence (2002) and Stiglitz (2000), deals with economic designs ensuring credibility of promises; see also institutional-economic literature on reputations and conditions of good conduct by first-order critics (see Klein, 1997 for review). None of this forms, however, is central to the academic world.

- Firstly, there must be observation channels making monitoring of first- and second-order critics possible for second- and third-order critics;
- Secondly, there must be opportunities for the latter to penalize opportunistic behavior they observed;
- Thirdly, they must have incentives to do so.

Observation channels and enforcement opportunities. Observability, as well as mobility, is a paramount infrastructural achievement. Surprisingly, the type of it which is the most effective in imposing virtuous behavior is at the same time the simplest: physical encounter.⁹ Probably the most salient feature of the jure deliberations Lamont describes is the simple fact that they occur in face-to-face interaction. Goffman (1983), defining the interaction order, pointed that its constitutive feature is richness of information each of the participants exude. Many things easily observable in face-to-face dealings might be concealed in a mediated and asynchronic communication which gives more time for a performer to suppress undesirable information (Klein, 1997). This trait is probably accountable for the nearly sacred status of personal contacts for the academics. It is universally believed that they are a source of unique grasp of others minds. Thus, Lamont's book documents their willingness to provide personal details they know even when that it has questionable appropriateness; various interviews at different countries also discovered role of personal acquaintances.

One problematic feature about face-to-face expert deliberations, however, is that the same quality which makes their participants more transparent to each other, make them much less transparent and accountable to the rest of the world. The logic of the Russian reforms was largely aimed at creating a system of material traces which would document each move made by the first-, second-, and third-order critics, and which could be easily retrieved by any further observer. Generally, the Russian reformers were so mistrustful towards possible experts that attempted to define the range of legitimate arguments they might use accounting for their decisions; and all these legitimate arguments were to point to retrievable traces of some kind (e.g. calculating cumulative impact-factor of an applicant). The dark side of this formality was that it deprived critics of opportunities for sanctions which were easily observable, but not legitimatable to the next-order critics. It seems that, at the end of the day, institutional designs providing evaluators with better observation options, even at the cost of the risk of leaving more discretion with them, work better in terms of eliciting virtuous conduct, than more formal controls – at least as far as other conditions are satisfied.

⁹ It also tends to be one of the most expensive ones.

Apart from observation opportunities, interaction order provides uniquely rich opportunities for immediate penalizing transgressors; even more effective as their occurrence cannot be legitimately proved. Lamont and Huutoniemi (*manuscript*: XX) write of behavioral signs of disapproval which are obvious for all those present, and quite painful for an individual against whom they are directed. A discussion participant, disgusted by some especially conspicuous breaking of appearance of disinterestedness and impartiality, might intimidate opportunist verbally, and the Russian interviews contained even mentions of minor physical violence. Co-presence leaves individuals with much less time to weight possible consequences of the affective behavior. It thus makes them a source of far more effective retaliation threat (Schelling, 1955).

The last condition of those listed above was availability *stimulus to penalize* vicious behavior on the part of the first-order critics. Howard Becker (1963:XX) was one of those who argued convincingly that legal norms have poor chances for being enforced until there is somebody who is interested in their implementation. The critics in a close proximity to each other can easily detect each others' misdoings, but would that prevent them from behaving viciously, or rather encourage entering collusion?

Lamont's paper identifies one of the conditions under which collusion is unlikely: presence of strong disciplinary and sub-disciplinary traditions to one of which each of the pieces of academic work could be ascribed. Having a panel composed of representatives of these traditions provides each piece of work with an advocate to act on its behalf if representatives of other traditions would attempt to downgrade it. Having somebody who would identify with a given style of work at the panel can effectively deter others present from not paying each of the traditions its due respect; disciplinary sovereignty here is buttressed by the requirements of the interaction ritual aimed at the faces of those immediately present (Goffman, 1967).¹⁰ Lamont describes a few examples of intra-panel politics occurring along disciplinary and sub-disciplinary lines which starts if one of the parties feels offended. Collusion is possible in the conditions of such balance of forces, but a collusion of a specific kind: members of different tribes might start giving everybody the same, usually the highest, score. That might provide them with all the emotional pleasures of successful academic ritual, at the same time making the results of their deliberations meaningless. Obviously, the risk of such outcome is the smallest when grant proposals are ranked, and the entire event acquires characters of a zero-sum game. It is the

¹⁰ This solution is not without its own weaknesses: what about those truly original works which go beyond conventions constituting cognitive styles of the established disciplines? And isn't 'respect for disciplinary sovereignty' just another name for vicious protective rituals in which representatives of different academic tribes exchange gifts of civic inattention for what they privately regard as each others' fatal intellectual shortcomings? The contrast with ecology (Lamont and Huutoniemi: XX), in which natural-scientific understanding of all disciplines as participating in search for the common truth might be insightful here.

largest when the symbolic good allocated is not obviously extractable. The latter is the case with academic degrees – a dissertation committee might confer as much of them, as it wishes, without much frustrating anyone's immediate interests. That arguably was the case with the Russian degrees which fall victim to a variety of the tragedy of the commons: usually no one volunteers for a thankless job of failing dissertations of others' pupils. Why such collusion are typical for some national academic systems, but not for others? From what our historical analysis demonstrated it follows that *modus vivendi* of certain scholarly worlds predisposes their inhabitants to regard any competition as a zero-sum game, while that of others make zero-sum connotations weaker. Thus, situation of universities as essentially autonomous bodies competing for students and funding from external sources would arguably foster an understanding of interdependency in the academic multi-organizational field as zero-sum. Their situation as divisions of the same sector of the state apparatus, competing with other sectors for resources, would probably have an opposite effect.

Obviously, this political model sometimes fails to work – as it fails to work in Russia, for example. The reason might be much-lamented (e.g. , 1999) absence of strong theory groups, playing the role of civil society structures making democracy work in this explanation.¹¹ There is no dominant explanation of why theoretically-laden intra-disciplinary groupings are not salient here, and even boundaries between most social-scientific disciplines are much more blurred, than it is the case in most Western countries. A plausible hypothesis, which much of our comparative work supports, is that this is the academic job market structure and, ultimately, physical space and technological infrastructure which may be playing the crucial role here. Theoretical innovations in the social sciences are often described as strategies of creating niches at a wide and competitive market for scholarly labor (e.g. Turner, 1986). Emergence of a new recognized specialty means opening working places; demonstration of loyalty to it serves as an individual advertisement. A necessary condition for making this system work is possibility of changing geographic location; nation-wide demand for a specialty one belongs to is of little help for an academic who is caged at a given department in a provincial town. Personal loyalty to the local academic boss would of much more help here; the professional competition in geographically immobile academic system is likely to occur on network, not categorical, principles. Elaboration and incorporation of the customary rules of fairness simply does not occur in an academic world which has little need for them.

Conclusion

¹¹ In a game-theoretic vein, one more possible consequence of theory-group allegiances is to be mentioned: they turn any kind of evaluative interaction into episode in extended dealings between larger-than-individual entities; Axelrod teaches us that this a key to eliciting cooperative behavior.

In her pioneering research, Lamont describes how successful academic protective rituals work in social sciences and humanities. My major points in this paper were that, firstly, this kind of study is essential for the better understanding of the nature of social-scientific expertise (including that on scientific knowledge which Harry Collins advocates), as well as of meta-expertise in general; secondly, that they could give us an insight into why some scholarly communities prove able to self-policing and promoting best practices, while other do not. The explanation of the latter curved to account for differences between Anglo-American (and Finnish) academic world Lamont and her colleagues describe, and the Russian one points to infrastructural differences, deeply intertwined with organizational differences. Very low rates and centered on the capital city pattern of academic mobility in Russia, cemented by the more general state of infrastructure, seem to ensure lack of division of symbolic and economic power in the Russian Academy, and, at least at the social sciences, absence of strong theory groups, serving as pillars of intra- and inter-disciplinary fairness. The same centralizing and hierarchical logic, together with high logistic costs, forbids using high-discretion modes of meta-expert evaluations, including most kinds of panels. Instead, more "objective" measures of success are promoted, which surprisingly seem to strengthen, rather than weaken, parochial academic networks.

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