Johannes Angermüller

Beyond Excellence. An Essay on the Social Organization of the Social Sciences and Humanities

(doi: 10.2383/33636)

Sociologica (ISSN 1971-8853)
Fascicolo 3, novembre-dicembre 2010

Ente di afferenza:
()

Beyond Excellence

An Essay on the Social Organization of the Social Sciences and Humanities

by Johannes Angermüller

doi: 10.2383/33636

As academic research is a competitive business, most people agree that jobs, money and other resources should go to where the most excellent research takes place. Most people will also agree that the individuals best qualified to assess quality of specialized academic knowledge are the academic peers. Yet there is little agreement on what excellence is and how it can or should be evaluated. On the occasion of Michèle Lamont's *How Professors Think. Inside the Curious World of Academic Judgment* [Lamont 2009], this essay points out the limits of excellence as an organizing principle for the terrain of academic research. By reflecting on the more general role of peer review for academic knowledge production, it points out the heterogeneity of research with its intertwined logics of knowledge and power.

The Expansion of Higher Education and the Specialization of Academic Knowledge

Ever since research has been done systematically by specialized producers in academic institutions, academic knowledge has differentiated into ever more communities, fields, and niches. If Leibniz is said to have been the last erudite to oversee the (European) academic output of his time, the last person to have even remotely followed the knowledge production of a whole discipline should have stopped his or her efforts by the middle of the Twentieth century. Only ten years ago, it was

estimated that more than 80% of scientific communication was done in no more than three languages – English above all with German and French following far behind [Swaan 2001]. With higher education booming in many non-Western countries, academic knowledge production now largely surpasses the confines of the West. While currently about 30 to 40 new universities are being founded in China, even in the least industrialized parts of the world such as in Africa universities are growing at a much higher speed than in the West. As a consequence, even the most well-read specialist of the smallest field of research can no longer follow the academic debate in its entirety, which may now include publications in Urdu, Korean or Lingala.

This process is likely to continue even though traditional hierarchies between Western elite institutions and the rest may persist. While some new disciplines emerge, older disciplines have parceled out into dozens and hundreds of subfields. The small field of the sociology of science alone, e.g., has given birth to science and technology studies, the sociology of scientific knowledge, the laboratory studies, the social studies of finance, the sociology of objects and artifacts, etc. Let's also think of the countless interdisciplinary "studies" which emerged in the wake of cultural studies: postcolonial, gay, lesbian, and queer studies are now gigantic fields with an abundance of subfields such as whiteness, performance, and visual studies to name just a few well-known examples [Abbott 2001].

There are reasons for this seemingly unstoppable tendency toward ever new specializations, the first being quantity. Never have there been more trained academic knowledge producers than today. Fifty years ago, de Solla Price [1965] estimated that about 90% of all academic researchers who had ever lived on this planet were still alive. Even though today many industrialized countries no longer see the rapid growth rates of the 1960s, almost all fields have continued to expand, even the humanities despite the well-rehearsed rhetoric of crisis and decline [Hamann 2009]. Thus, in just 200 years small clubs of erudite gentlemen and men of letters have transformed into a multi-billion industry with thousands of full-time teachers, researchers and professionals whose output is published in probably more than 100.000 academic journals worldwide.

If academic knowledge is breaking down into small and smallest fields of specialization, this tendency is not only a consequence of an ever-increasing number of producers and products but also of the values of the academic system which holds repetition and imitation in low esteem and bans plagiarism. Generally, new members entering the system are encouraged to create knowledge that nobody else has produced before. As a consequence, academics usually spend considerable time and

energy on acquiring some unique expertise distinguishing them from everybody else in the field. Yet the problem is that the most significant achievements in research are often, in some way or another, the least commensurable ones which can be hardly understood or evaluated properly by the peers. From the point of view of the academic producers, a strategy which aims at creating incommensurability is anything but irrational – for who would like to be compared and ranked with, say, 100.000 other linguists? Should the producer not do everything to reduce competition by specializing on a subject for which no one else can claim legitimate expertise such as "regional dialects of gestural language in Northern Africa from a cognitive point of view?"

Yet despite this eternal drive toward uniqueness, vastly different research output is judged and assessed by actors *not* coming from the same field. Obviously, there are many situations in which academic work is subject to evaluation. And there are numerous experts dealing with the difficult task of evaluation. Michelle Lamont's study on peer review in five research organizations in the U.S. systematically investigates how they do just that.

Let me start with my own evaluation of this rich and well-researched empirical study on the evaluative standards, the practical reasoning and the implicit knowledge involved in peer review. To my knowledge, this is the first major study to systematically investigate peer review in different research organizations covering different disciplines the social sciences and humanities – a path-breaking contribution to the sociology of (scientific) knowledge and a must-read for everybody who wants to know how the social sciences and humanities work. As peer review is an object which the academic sociologist cannot observe without being peer reviewed him/herself, field access is particularly difficult to obtain. While Lamont could sit in on a few panel meetings, the bulk of her study is based on interviews she conducted with panelists shortly after their meetings. What makes it unique is its broad empirical base and its reflexive insights into an object of which she herself is a part.

At several points, Lamont insists on the professional attitude she encountered when she was doing her study. Given the pressures and dynamics of panel deliberations, one understands how panel members are obliged to exclude "extra-scientific" considerations. However, surprisingly enough, Lamont also finds that nobody was able to define what excellence is even though apparently everybody claimed to recognize what it was when it was there. While Lamont, too, refrains from venturing any hypotheses about the nature of academic excellence, she seems to share the interviewees' firm and unshakeable belief in the primacy of excellence. Yet can there be such a thing as excellence out there like a language that everybody can speak without knowing how to account for its rules and structures? If excellence is a more or less

universally recognized gold standard for academic research, it is not a standard that has the hard and solid reality of a substance like gold. On the contrary, excellence is a fuzzy object which miraculously seems to evaporate once you try to zoom in on it, to get close to it and to feel its fabric as it were. Hence, the problem is that "evaluation is contextual and relational, and the universe of comparables is constantly shifting. Proposals demand varied standards, because they shine under different lights. In some cases, the significance of the proposed work is determined by the likely generalizability of its findings. In others, how a topic informs our understanding of broader processes is more important. In yet another, significance is assessed by the deeper understanding that results from a particular interpretation. In panel deliberations, the ideal of a consistent or universalist mode of evaluation is continually confronted with the reality that different proposals require a plurality of assessment strategies" [Lamont 2009, 241].

Contrary to traditional "great men – great ideas" approaches that prevail in many studies on the social sciences and humanities, the process of academic knowledge production involves much more than producing interesting ideas and putting them on paper. While it is Lamont's merit to have studied some of the sites where ideas from specialized knowledge producers turn into institutionally recognized knowledge, the purview of her study does not include the wider web of practices into which academic research and its assessment is usually embedded. However, what happens inside panels relates in many ways to the academic world outside, where researchers are published (or not), hired (or not), cited (or not), etc. Therefore, in order to account the significance that peer review holds for academic knowledge production, I want to go further and ask how peer review relates to the terrain of academic knowledge production more generally.

Against the background of Lamont's many productive insights in peer review processes, I will sketch out some preliminary ideas about the social organization of the social sciences and humanities. In the following, I will argue that excellence, defined as the assessment of research quality, is not necessarily what academic research is about, at least not always. Firstly, excellence is not the only organizing principle for academic knowledge production which is characterized by at least two competing types of practices: the stratifying logic of top/bottom distinctions and the tribalizing logic of inside/outside distinctions. Excellence only points to the latter aspect, viz. practices of judging, assessing and ranking which is especially important for the organizational dimension of higher education. Secondly, I will point out the non-conceptual circumstances from which peer review experts draw to decide on a proposal. Hence, excellence is never achieved through the intrinsic force of an idea; the con-

struction of excellence is a practical achievement embedded in a web of knowledge and power. In a word, since there is no such a thing as pure conceptual excellence, peer review is a complex, creative and in many ways "messy" procedure that is firmly grounded in the social practices of the academic world.

Legitimacy vs. Excellence: Membership Claims and the Practice of Research

While producers and products are judged, compared and ranked on the noble grounds of excellence, excellence may not be the be-all-and-end-all of academic research. Excellence certainly matters to academics but it may not be the problem they are worried about most. Their key concern is legitimacy, i.e. the legitimacy to speak, write, and act as a member of a scientific community. Indeed, before anybody can waste a single thought about how to distinguish between top and bottom, one is confronted with the question of who is inside and outside. Therefore, the more existential pressure for academic producers is to conform to cultural norms and ideals than to successfully compete on a market of excellence. From this angle, academics are always busy making membership claims in order to be seen as legitimate representatives of this little niche on Alfred Musset's poetry or that little specialty on nation-building in Western Africa, to be recognized as a member of French studies or of cultural anthropology. The first question, therefore, is who one is, for nothing is less certain and more existential. Indeed, even the most competitive journal has to decide at some point whether a contribution "fits in." And doesn't even the most purist funding agency have to define who may apply and who not? Therefore, academic producers constantly draw lines between us and them, between the peers whose work one can legitimately claim to understand, talk about, and maybe even judge and the others who live on a planet too far away to even think of.

The game of identifying each other's membership(s) may take place unconsciously but not less consequentially as it seems to be a fundamental requirement for scientific communication to succeed. Conference gossip, e.g., usually does not start until the conversation partners have signaled to each other "where one comes from." More generally speaking, it is difficult to get across the simplest idea if you don't sketch, in some way or another, the (disciplinary) public one likes to address. Therefore, even though there may be many different motivations for somebody to cite somebody, the most central one is to delimit the academic community with which the authors wants to engage in scholarly exchange. If academic researchers want to

be understood, they need to situate their thoughts in an epistemic field, which they can do implicitly by using this concept or that name or explicitly by means of mappings, citations and bibliographies. References, therefore, may not identify the most excellent producers in the field. Their more important function should be to shore up one's own membership claims. Hence, citing somebody serves to show who one knows, who said what and who takes what position vis-à-vis others. Oftentimes, excellence and legitimacy are inextricably bound up with each other as the most legitimate representative of a field is also often regarded as the most excellent one in one way or another. Yet, while research without excellence may be conceivable, it cannot do without researchers claiming to be members of certain communities. Without membership no excellence.

Yet if the first objective in scientific communication is to explore the many manifest and hidden boundaries distinguishing between people inside and outside, what is the meaning of peer review then? Is peer review just a futile exercise which conceals a struggle between insiders and outsiders, an ideology that masks the uneven distribution of economic and political power among producers? As this is obviously not the case, I want to distinguish between two competing, but interlocked logics that organize the production of academic knowledge. The first one is the "tribalizing" logic of inclusion and exclusion which organizes the terrain of knowledge, defines membership and allows to delimit academic communities. The tribalizing logic is the dominant mode of most instances of scientific communication, when specialized academic knowledge producers produce and receive knowledge and refer to each other within a given niche, field or community. This is the logic that dominates in academic everyday life when knowledge producers talk to this or that person or cite this or that source.

The second logic is the logic of stratification which organizes the terrain of institutionalized power, which institutes relations of legitimate inequality between the producers through organizational hierarchies or arrangements of governmental technologies. This "stratifying" logic is transversal to the academic communities in that it tends to characterize the classifying and ranking activity of actors with administrative, economic or political power who are involved in decision-making processes [Musselin 2005; Münch 2009]. The actors engaging in stratifying practices may be confirmed scientists (i.e. "peers" broadly understood) but they are rarely specialized in the same niche as the producers and products they assess. Rather than veritable peers who know each other personally and share the same disciplinary or subdisciplinary expertise, they are experts versed in the evaluative culture of their discipline and competent decision-makers. Their task is to translate the ambivalent, multi-faceted, and protean knowledge of specialized academic producers into the binary code of

bureaucratic organizations, governmental structures and mass-media discourse: excellent – not excellent, passed – failed, one – zero. Therefore, if there is no excellence without membership, excellence and membership are in most cases inextricably intertwined.

Against this background, academic knowledge production follows (at least) two contradictory logics: tribalization and stratification. While tribalization usually dominates in the production and reception of specialized knowledge within a given community, stratification tends to predominate when it comes to organizing the distribution of legitimate power, notably through the organizational structures of higher education – cfr. Burton Clark's remark that higher education "must be centered in disciplines, but it must simultaneously be pulled together in enterprises" [Clark 1983, 32]. Following Michel Foucault's ideas on power/knowledge – especially his theory of governmentality [Foucault 2007] – I want to conceive of the academic terrain as an uneven and heterogeneous field of practices following contradictory logics: the tribalizing logic of disciplinary knowledge and the stratifying logic of organizational power. It is one thing, therefore, to spend time and energy on browsing the stocks of the library, to read and write texts, to present papers and to talk to people at conferences; it is another thing to participate in decision-making procedures where some proposals have to be accepted, others rejected. The challenge for most academics is precisely to bridge both logics by articulating ever new, unique and sometimes extremely complex articulations, which call for their unique problem-solving competence, practical capacities and creative imagination. Thus, by constituting an uneven and shifting terrain where power and knowledge is necessarily bound up with each other [Maeße 2008], research activity revolves around the – ultimately irresolvable – contradiction between tribalization and stratification, which pushes academic producers toward ever new distinctions and innovations.

The Non-Conceptual Background Knowledge of Peer Review

With these remarks from a poststructuralist background, I want to account in a preliminary way for the social organization of the parceled and uneven terrain of academic knowledge with its inbuilt drive for uniqueness. While the sociology of science has traditionally been characterized by a cleavage between macro and micro theorists, such as, on the one hand, institutionalists [Merton 1962; Weingart 2003], neo-institutionalists [Meyer 1980] and class theorists [Bourdieu 1988] and, on the other hand, post-Goffmanian ethnographers [Knorr Cetina 1981] and actor-network-theorists [Latour 1987], the poststructuralist approach to the sociology of

science aims to integrate micro and macro approaches by stressing the heterogeneity of academic knowledge crossing different fields with different logics and rationalities.

Accordingly, the panelists of Lamont's study not only reunite different sorts of expertise but they also mobilize their non-conceptual background knowledge about the fields the applicants come from. Therefore, it is instructive that Lamont makes a point of the role of "non-scientific" criteria such as the perceived "moral" quality of the applicant [Lamont 2009, 194 ff.] or the overall balance of the selected projects along regional, institutional, disciplinary, sexual or ethnic lines [*ibidem*, 217 ff.]. As Lamont emphasizes time and again, the panelists do not lack a professional attitude if they do not abstract from these considerations as even and especially in the most professional panel deliberations there is a lot more going on than just identifying excellence as such, understood as the conceptual, argumentative, and intellectual soundness of a project. Indeed, the construction of excellence involves much more than qualifying "proper and logical thinking;" it is a practical achievement of the reviewers who draw from all kinds of ideas, assumptions, and beliefs in order to come to a decision. Excellence, in other words, is not some inherent quality that can be simply read off from a text or proposal; it needs to be constructed by experts who translate complex, specialized and incommensurable input into "binary" output, i.e. into decisions and rankings according to which producers can be recruited and resources distributed. As the construction of excellence requires passing from a tribalizing into a stratifying circuit, peer review is bound to be a messy and in many ways contingent process – see journal peer review procedures [Hirschauer 2005]. How does it come that many participants display an unshakable belief in its fairness and objectivity, not least Lamont herself? Now if that's not a miracle…

The belief in the system of peer review may appear less miraculous if we consider the many sites where the excellence of academic output is assessed and judged. As Lamont puts it, panels are seen as a "machinery or technology of evaluation around which evaluative cultures are intertwined" [Lamont 2009, 23]. Therefore, the funding panels are embedded in a dispositif of governmental technologies and practices which constitute a terrain of social inequality where academic producers can be controlled and policed, often by and through themselves. Given the strong role of nation-states in financing higher education, these technologies are deeply integrated into "national" institutional arrangements which differ considerably from one country to another. Toward the end of her book, Lamont alludes to the comparative dimension of her project when she compares the U.S. system of anonymous peer review with academic systems in Europe, notably in France. Indeed, what happens in panel deliberations needs to be situated in "evaluative cultures" [*ibidem*, 4] with their

classifying and stratifying practices. Therefore, if the professional ethos is especially developed in the U.S., this is not only an effect of size (as the participants of evaluation procedures are less likely to know each other in a big system like the U.S.) but also a result of an organizational "culture" which deploys certain techniques of ruling and controlling large numbers of producers through universities, associations, journals, etc. Peer review needs these taken-for-granted ideas of how and what to assess properly which differ significantly between disciplinary and national fields.

Peer review is one practice among many constituting the "governmentality" of a power/knowledge complex like academic geography in Italy or institutional psychoanalysis in France. These governmental complexes consist of mentalities, ideologies and habitus which are deeply engrained into institutionalized practices of distinguishing and classifying producers as well as creating and legitimating inequality between them. Differences between these taken for granted "ways of ruling" are especially clear if we take a comparative look at the excellence-conferring instances and procedures in different disciplines and countries. One of these domains is the publishing world, i.e. journals and presses which usually build systems of specialized gate-keepers from the academic communities. The practices of gate-keeping vary enormously: they can involve the loose cooperation of members of an established community (such as the flagship journals of national disciplines) or they form around groups, schools or individuals (such as many interdisciplinary and international journals). Blind, double-blind or not blind at all, reviews are produced by the journal or book series editor, members of an editorial board or external experts recruited on a more or less ad hoc basis. The same is true for publishing houses which sometimes mobilize advice from inside (as most academic publishers in Continental Europe prefer) or outside expertise (e.g. in many American university presses). The decision-making procedures can be extremely long, complex and costly or they can be simple and fast. At the same time, the status and importance that journals and presses enjoy in the academic field can differ considerably. For many literary critics and historians, it is more important to produce books than journals, while economists and psychologists are more likely to group around a few highly competitive journals. Old disciplinary fields usually have more journals at their disposals than new interdisciplinary fields. And there are huge differences between disciplines in different national settings: while American sociology as a discipline is essentially organized around two major journals with large reservoirs of peer reviewers, anonymous peer-review is rare in French sociology where there is no journal recognized by all members of the discipline.

Therefore, the expertise of peer reviewers largely exceeds their conceptual specialty. If they are invited to take part in a panel, they are recruited for their intellectual expertise just as much as for their "governmental" experience. They do not come from a power-free space but from a world where knowledge is subject to certain governmental procedures and practices. This expertise in the governmental aspect of academic knowledge production does not have to contradict their conceptual expertise. On the contrary, if the reviewer wants to be "fair" with the applicants, he or she does not only have to understand the "idea" of the proposal but also be familiar with the customs and "mores" of the respective field – what Lamont calls "cognitive contextualization" [*ibidem*, 58]. To have a full picture of a proposal, the reviewer needs to know what the most important publishing houses are, whether indicators like the SSCI is commonly used or not, whether there are peer-reviews journals work in the applicant's field of origin and many other things. In most cases, of course, this background knowledge about the governmental organization of the academic field is too complex to become the object of explicit negotiation between the panel members and probably most are not even aware of it.

Indeed, "excellent" panelists are connected with the academic world in both ways: as a representative of a specialized knowledge-producing community and as an agent of governmental practices versed in the rules, procedures and techniques of higher education. This is why peer review expertise is so tightly connected with the academic system in which the expert was trained. Thus, when a group of international experts were recently invited to help select the new "Universities of Excellence" in Germany, doubts were raised as to how a sound judgment could be expected from experts with next to no insider knowledge. Similar questions came to the fore when a colleague of mine at Mainz University was invited to take part in a tenure-track procedure at a University in the U.S. Earthlings cannot evaluate Martians even when they share the same (disciplinary) language and can understand each other on a conceptual level.

Power and knowledge have different geometries. While the disciplinary division of academic knowledge has been established almost on a global scale (though with considerable differences between the underlying intellectual traditions, especially in the social sciences and humanities), the organizational structures of academic power are embedded in national settings and are highly path-dependent. Indeed, to form an opinion of the weight of others in scientific discourse, academics need to know what a *Habilitation* implies for an *Assistent* in Germany, what is an *agrégation* for a *maître de conférences* in France or why there is usually no question that adjuncts start a tenure-track procedure in an American state university. Thus,

academic knowledge production is embedded in a framework of administrative rules and organizational techniques which may be more "statist" as in Continental Europe, where many professors are civil servants, or more "liberalist" as in the U.S. and the UK, where more institutions enjoy budgetary and administrative autonomy. This is why job interviews outside one's system of origin are a special challenge (for both sides!) as the expectations and the taken-for-granted assumptions may differ considerably. German professors are expected to have spent some time abroad, to be active as fund raisers and to run a research team, which is rarely the case in France and the U.S. Without some tacit background knowledge about the institutional organization of the academic system, peer reviewers will not understand how the academic business works and, thus, will be at a loss at evaluating research and researchers.

While there is a complex and differentiated apparatus for the production and reproduction of disciplinary knowledge, there are no classes for junior researchers to train the organizational aspects of higher education. To learn the "tricks of the academic trade," researchers need to observe others and "make their own experiences." Unfortunately, Lamont does not reveal more about her own experience as a Canadian trained in France before starting her career in the U.S. [*ibidem*, 239 ff.]. As is well known, American higher education is a tiered system with some, mostly private elite institutions at the top. It is not surprising, therefore, that her panelists are well aware of the cleavages between different types of institutions and their consequences for career chances. Sometimes they bring to the floor the specific positions the applicants' universities and departments occupy in the institutional pecking order. Yet, to rank producers according to universities and departments is anything but natural or universal. In France or Germany, universities and departments are not necessarily the meaningful units of academic knowledge production since they do not have the same degree of agency and autonomy as in the U.S. In France, the *laboratoires de recherche* with dozens of specialists from various disciplines, departments and even universities are probably more important when it comes to distributing resources or jobs whereas in Germany the real organizational agency is performed by subdepartmental teams forming around a *Professor*, the *Arbeitsbereiche* or *Lehrstühle*. As a consequence, if excellence tends to be attributed to departments in the U.S. ("the Harvard law department"), the more likely excellence-harvesting instances would be the interdisciplinary research group in France (e.g. the *Centre de sociologie européenne* at *Ecoles des Hautes Etudes en Sciences Sociales*, sometimes also known as the "Bourdieu clique") or the individual ("Prof. Dr. Habermas" as the incumbent of a chair and leader of a research group) in Germany.

It is clear that these forms of organizational hierarchy go hand in hand with a specific habitus and strategic choices on the part of academic producers. It may not be a coincidence that Germans are the most "entrepreneurial" academics as they are more inclined to engage in the competition for research funding which they need to build up their team. By contrast, nowhere do nationally held state exams play a more important role than in France, where many research positions, especially in the older disciplines like letters or history, are decided in *concours d'agrégation* with their standardizing effects (denounced as "scholastic" by Bourdieu) and their emphasis on general philosophical culture. Finally, the U.S. academic system may be the system where producers are the most prone to forming elitist oligarchies of academic producers who want to be part of the "best" universities or departments (which happen to bear the name of Harvard, Yale or Princeton).

This is the institutional context that peer reviewers tend to take for granted when they are invited to participate in evaluation and selection procedures. As the "excellence" of research cannot be identified with this non-conceptual background knowledge, they have to learn how to "read" the markers of excellence conferred by the academic system. Hence, an informed reviewer in Germany may not be much impressed with the fact that somebody is a *Mitarbeiter* (junior researcher) at a well-ranked university as he knows that rankings are more or less arbitrary constructions that change from year to year and that junior researchers are not always recruited on a competitive basis (unlike professors, who sometimes have to present before dozens of committees for several years). As reviewers in North America know that supervisors often employ graduate students to produce journal articles, they will understand that co-authorship does not necessarily imply that both authors collaborated on an equal basis. More than in both Germany and the U.S., a French academic expert asked to evaluate somebody may be inclined to evaluate the research group behind her or him as the applicant would be "lost" without the active support of such a group.

The greater part of the peer reviewer's contextualizing work is done unconsciously. It is their habitus that allows them to spontaneously know how to situate the applicant in the field and to evaluate her or his proposal accordingly. To develop an understanding of what the implicit rules, workings and mechanisms of an academic system are usually takes decades. Senior academics are especially likely to forget about the particular academic culture in which they were trained. The longer academics are socialized in a system, the more likely they are to downplay the hierarchies of their own system. Is it a surprise, then, that the first thing they perceive in other systems is their "irrationally" hierarchical aspects? Thus, Germans tend to see high tuition fees as a scandalous feature of U.S. higher education, while especially Americans are ill at ease with the professorial distance that teachers sometimes display toward their

students in France. And of course nobody can understand that in Germany professors employ confirmed (or non-confirmed) researchers as personal assistants. While hierarchy takes different forms in different academic systems, every system defines hierarchies and inequalities it can accept and tolerate. There is no "pure" research; research always involves the articulation of knowledge and power.

Conclusion: Research as Intellectual Practice on a Postsocietal Terrain

As a conclusion, I will try to weave the various threads together by insisting on the peculiar kind of sociality which I dealt with in this essay. Academic knowledge production points to an uneven, messy and heterogeneous terrain where producers group together to form communities with rather fuzzy and unstable inside/outside boundaries. In academia, everybody wants to be more or less unique with his or her specialized expertise – and to a considerable degree everybody indeed is. If it is difficult to apply the all-encompassing gold standard of excellence to this terrain which is fragmented in myriads of fields and subfields, this also recalls a problem for social theory which is accustomed to privilege order, structure and regularity over chaos and flux, seen as, in a way, "less social." Yet the terrain of the social sciences and humanities cannot be modeled after classical notions of society as a closed and fixed container where every element has its functional place in a constituted hierarchy of top and bottom. Parceled as it is into tribes and hordes, ruled by anonymous powers constituting an almost impassable terrain scattered with niches, holes and many foggy patches, the academic order in no way resembles the nation-state. Post-classical impulses are needed which can account for the post-societal sociality of the social sciences and humanities.

Against this background, given the high value it places on excellence, peer review looks like a rather desperate effort to impose order on an essentially ungovernable terrain. Do we have to regard peer review, then, as a more or less futile exercise that clouds vested interests or external powers? Let me recapitulate what I said and what I did not say. While I did say that academic research in the social sciences and humanities cannot be conceived in terms of excellence alone, I did not say that this makes the task any easier for the academic producers for whom it should not be any less difficult to claim legitimate membership in specialized knowledge communities as it is to produce "excellent" knowledge. Then, I did say that the peers construct academic excellence by extensively, but unconsciously drawing from their non-conceptual background knowledge about the academic system as organized and institutionalized power. But I did not say that excellence reflects a "non-scientific" reality of social

power relations, waiting as it were to be clad in the rhetorics of excellence by the academic producers. If power and knowledge point to fundamentally contradictory logics of organization, the task of the academic researcher is to intervene in the academic terrain and to articulate some incommensurably new position. And if research is about much more than the discovery of ideas out there, isn't it precisely the artful articulation of knowledge and power which makes research such an intellectual adventure?

References

Abbott, A.
2001 *Chaos of disciplines*. Chicago: University of Chicago Press.

Bourdieu, P.
1988 *Homo Academicus*. Cambridge: Polity Press.

Clark, B.R.
1983 *The Higher Education System: Academic Organization in Cross-National Perspective*. Berkeley: University of California Press.

Foucault, M.
2007 *Security, Territory, Population. Lectures at the College De France*. Basingstoke: Palgrave, Macmillan.

Hamann, J.
2009 *Der Preis des Erfolges. Die "Krise der Geisteswissenschaften" in feldtheoretischer Perspektive*. Bamberg: Bamberg University Press.

Hirschauer, S.
2005 "Publizierte Fachurteile. Lektüre und Bewertungspraxis im Peer Review." *Soziale Systeme* 11: 52-82.

Knorr Cetina, K.
1981 *The Manufacture of Knowledge. An Essay on the Constructivist and Contextual Nature of Science*. Oxford, New York: Pergamon Press.

Lamont, M.
2009 *How Professors Think: Inside the Curious World of Academic Judgement*. Cambridge, MA: Harvard University Press.

Latour, B.
1987 *Science in Action*. Milton Keynes: Open University Press.

Maeße, J.

2008 "Wie Bologna erfunden wurde. Eine Diskursanalyse über die Etablierung eines Reformprogramms in Deutschland." *Zeitschrift für Soziologie der Sozialisation und Erziehung* 28: 363-376.

Merton, R. K.

1962 "Science and the Social Order." Pp. 16-28 in *The Sociology of Science*, edited by B. Barber and W. Hirsch. Westport, CN: Greenwood.

Meyer, J.W.

1980 "The Effects of Education as an Institution." *American Journal of Sociology* 83: 55-77.

Münch, R.

2009 *Globale Eliten, lokale Autoritäten. Bildung und Wissenschaft unter dem Regime von PISA, McKinsey & Co*. Frankfurt am Main: Suhrkamp.

Musselin, C.

2005 *Le marché des universitaires*. France, Allemagne, Etats-Unis. Paris: Sciences Po.

Price, D. J. De Solla

1965 *Little Science, Big Science*. New York: Columbia University Press.

Swaan, A. de

2001 *Words of the World*. Cambridge: Polity.

Weingart, P.

2003 *Wissenschaftssoziologie*. Bielefeld: Transcript.

Beyond Excellence

An Essay on the Social Organization of the Social Sciences and Humanities

Abstract: Even though peer review is a widely used practice to assess research quality in the social sciences and humanities, little is known about how it is done and what it does. Against the background of Lamont's *How Professors Think*, it is argued that evaluation experts typically have recourse to a great deal of non-conceptual knowledge when they construct academic excellence. Therefore, the construction of excellence turns out to be deeply grounded in the social practices of the academic world. By placing peer review in the broader context of academic knowledge production, this essays points out the heterogeneity of research as a set of intertwined practices at the nexus of knowledge and power, following different logics, such as tribalization and stratification.

Keywords: Peer review, scientific communities, higher education, power/knowledge.

Johannes Angermüller, Prof. Dr. (JP), is Assistant Professor at the Department Sociology of the University of Mainz, Germany. In 2003, he obtained his PhD from the Universities of Paris 12 and Magdeburg. He has published on intellectual and political discourses, amongst others *After Structuralism. The Discourse of Theory and the Intellectual Field in France* (2007). More information on http://www.johannes-angermueller.de.