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Evaluating Creative Minds: The Assessment of Originality in Peer Review

By Michèle Lamont, Marcel Fournier, Joshua GuetzkowGrégoire Mallard and Roxane Bernier

What a good thing Adam had — when he said a good thing, he knew nobody had said it before.

Mark Twain

Introduction

Originality plays a key role in academic research: the main objective of scholarship is to discover something new, to say something that nobody has said before. In psychology, one finds many studies concerning the creative, imaginative, or inventive personality and its early identification (Brockman, 1993; Czikszentmihalyi, 1996; Dervin, 1990; Simonton, 1988). Historians of science often use the term 'genius', or more modestly, the notion of 'divergent thinking' (Kuhn, 1970), to point to innovators and their contributions. Sociologists, for their part, have tended to focus their attention on the social factors that lead to innovation (e.g. Collins, 1998). Researchers have largely ignored the question of how scholars assess originality (but see Dirk, 1999). However, as clearly revealed by the most elementary involvement in the scholarly reviewing process, how to recognize originality is a question that looms large in academic evaluation.

The peer review system – a central institution of the scientific world – plays an important role in institutionalizing definitions of 'originality'.[1] Borrowing implicitly or explicitly on the Mertonian (1942/1973) dichotomy between universalism and particularism, previous studies of the peer-review system have been concerned with issues of fairness. They focus their attention on whether judgements about 'irrelevant', particularistic characteristics, such as the author's age and reputation, affect the evaluation of his or her work (Bell, 1992; Cole, 1992; Cole and Cole, 1973; Cole *et al*., 1978; Dirk, 1999; General Accounting Office, 1994; Liebert, 1976; Merton, 1996; Roy, 1985). The questions posed by these researchers often imply that a pure evaluation would be possible, once particularistic considerations are eliminated. Moreover, these questions imply that judgements about the qualities of the researcher, as opposed to the research itself, are inappropriate and corrupt the peer-review process. In our research, we

break with these assumptions and show that qualities imputed to researchers are often central to judgements made about the creativity of their work. To do so, we draw on systematic interviews with individuals serving on American and Canadian funding panels in the social sciences and the humanities to explore how they think and talk about originality.

In taking a cultural approach to the study of peer review, we join a growing line of work in the sociology of knowledge that focuses on the cultural dimensions of knowledge creation (Abbott, 2001; Becher, 1987; Bender and Schorske, 1997; Bourdieu, 1984; Clark, 1983; Wagner *et al*., 1990). This literature often posits important differences across disciplines. For example, Bourdieu (1984), Knorr-Cetina (1981; 1999) and Somers (1996) examine how the production of knowledge is guided and bounded by the beliefs and practices (in a word, the culture) of disciplines and disciplinary organizations. These authors would lead us to hypothesize that specific beliefs about originality also differ between disciplines. In fact, our interviews suggest weak disciplinary differences in the significance attached to originality.[2]

Bourdieu (1975; 1984) and colleagues (Bourdieu and de Saint-Martin, 1975) are among the few scholars who have examined qualitatively academic evaluation systems. Drawing on the structuralist tradition, they 'read' or identified matrices of hierarchically ordered oppositional categories at work in evaluation processes. Rather than presuming that originality is defined through such binary matrices, we focus on the meaning actually given to originality by panelists, paying particular attention to the types of arguments they deploy when judging originality. Our findings suggest that binary matrices cannot adequately capture the types of arguments scholars make when talking about originality.

Our study also reveals that panelists often conflate the originality of the proposal with qualities of character attributed to researchers themselves – qualities such as integrity, courage, independence and audacity. At the same time, originality is also often identified through the substance of the project itself: the questions it asks, the topic it focuses on, the theory it offers, its research design and the data itself are all elements around which arguments about originality get to be articulated.

Because we find character is key to how originality is conceptualized by panelists, our research resonates with the work of Steven Shapin (1994: xxvii), who, in his study of the evaluation of scientific credibility in seventeenth-century England, shows that judgements about scientific value are often social judgements: a scientist's character (defined in terms of honor, modesty, civility and courtesy)[3] largely determined the extent to which his results were trusted and credibility established. However, our study goes beyond Shapin's. While in his conclusion, he suggests that in the contemporary world, credentials, training and expertise have eclipsed character in establishing trust and credibility – 'Modernity guarantees knowledge not by reference to virtue but to expertise' (1994: 413) –,[4] we show the continuing salience of character in evaluations. We find that when panelists interrogate the qualifications of the researcher, which is akin to evaluating trustworthiness (is he/she likely to deliver what he/she promises?), they do look to credentials, training and track record. But when they

interrogate the originality of the potential contribution, they often turn to arguments about the applicant's character and particularly their personal moral and mental qualities. Finally, we find that panelists often express their reaction to proposals they perceive as original in emotional terms, in terms of what is exciting to them and that some panelists offer their emotional reaction as proof of the project's originality and of its potential contribution: if the project 'excites' the panelists, it will surely excites others.

Despite this centrality of 'character' in the evaluation of originality, panelists tend to view personal characteristics as irrelevant to scholarly evaluation when questioned on their self-understanding of their own standards. As if they were abiding by a Mertonian description of the ideal-universalist norm in science, many of them insist on keeping the sphere of knowledge and the sphere of the social separate. And they do describe what they do as attempting to be neutral, objective and 'fair'.

We briefly position our argument in relation to the available literature on originality in science. We then move on to discuss our results, showing how respondents judge originality on the basis of substantive aspects of the proposal, the character traits that they ascribe to authors, and their emotional reaction to proposals. But first we will discuss the data used in this study.

Data

The study is based primarily on interviews with individuals serving on five different funding panels that distribute research fellowships in the social sciences and the humanities in the United States. A total of ten funding panels were studied over a period of two years, with interviews being conducted with panelists at the following institutions: the Social Science Research Council, the American Council for Learned Societies, the Woodrow Wilson National Fellowship Foundation, an anonymous Society of Fellows at a top research university, and an anonymous foundation in the social sciences.[5] All of these panels are interdisciplinary, meaning that each panel was composed of people from different disciplines, though some panels were more oriented towards the humanities while others were more oriented towards the social sciences. In two cases, we were able to observe the panel deliberation process. Otherwise, the Principal Investigator, Michèle Lamont, conducted an hour-long in-depth interview with panel members about what happened during the panel deliberation, about the criteria of evaluation they used to assess proposals, and in some cases, about how they understood the criteria used by other panelists. Other questions concerned how panelists interpret the process of selection and its outcome, and how they recognize excellence in their graduate students, among their colleagues and in their own work. We also interviewed programme officers about the selection of panelists. Panel members originated from a wide range of disciplines: anthropology, art history, economics, English, geography, history, musicology, philosophy, political science, sociology and women's studies. A total of 78 interviews were conducted, but this essay is based on an analysis of the first wave of interviews only, which included 42 individuals.

A Canadian study, conducted by Marcel Fournier, is adding a comparative dimension. This study analyses over the course of three years (2000–2003) seven disciplinary and interdisciplinary funding panels of the Standard Research Grants Program of the Social Sciences and Humanities Research Council of Canada (SSHRC), the federal agency responsible for promoting and supporting advanced scholarly research and research training in the social sciences and humanities in Canada. In the Standard Research Programme distributes research grants in the various social sciences and humanities disciplines. There are 24 adjudication committees, including one interdisciplinary/multidisciplinary committee. The committees that have been selected for this study are Panel 2 'History' (e.g. history of science, technology and medicine), panel 7 'Economics' (e.g. macro/micro-economics), panel 15 'Interdisciplinary/multidisciplinary' (e.g. sociology, linguistics and literature), panel 16 'Anthropology and archeology' (e.g. Latin America and China), panel 19 'Literature' (e.g. Canadian and contemporary literature), panel 20 'Health, women and social work' (e.g. nursing and feminist studies) and panel 24 'Political science' (e.g. public administration and law). This essay is based on an analysis of 18 interviews conducted in the first year of the project. Although we have not begun to systematically compare the cases, these interviews will provide a number of points of cross-national comparison.[6]

Prior Research on Originality

Originality as an Institutional Norm of Science

In the canonical literature in the sociology of science, originality is described as a primary goal of research and an institutional norm of science: 'It is through originality, in greater or smaller increments, that knowledge advances' writes Merton (1957/l973: 293). This institutional emphasis on originality can be counteracted by other institutional norms, mainly modesty and humility. But the first virtue is given priority: 'Great modesty may elicit respect, but great originality promises everlasting fame' (Merton, l957/l973: 308). In his text on 'The Normative Structure of Science', Merton does not identify originality as an institutional imperative, as a part of the ethos of science (Merton, l942/l973). But in his article, 'Priorities in Scientific Discovery', he states that

> Recognition for originality becomes socially validated testimony that one has successfully lived up to the most exacting requirements of one's role as scientist. The self-image of the individual scientist will also depend greatly on the appraisals by his scientific peers of the extent he has lived up to this exacting and critically important aspect of his role
>
> (Merton, l957/l973: 273)

The cultural emphasis on originality is so great that it can produce 'deviant' behaviour when scientists try to obtain credit for an original discovery by all means available. Merton suggests that fraud and plagiarism are two types of response to this emphasis, but there are also alternative responses, such as retreatism.

Scientists are rewarded for making original contributions; they therefore wish to be the first to announce 'original' discoveries and for this reason, they frequently engage in 'priority disputes' over who was the first to make a discovery. Presumably, scientists can always agree that a particular discovery is original, but often it is not clear who should get credit for it. Other researchers have also defined originality in similar terms (e.g. Gaston, 1973; Hagstrom, 1974). Gaston compares art and science: 'In art, the chances that two creative artists will produce exactly the same sculpture or painting are extremely low, but in a competitive field of science the chances are very high that two or more scientists will make simultaneous discoveries.' In other words, 'we would not have the Fifth Symphony without Beethoven, but we would have had relativity without Einstein.' In science, 'original research consists', according to Gaston, 'of not just doing something on a topic that no one has ever worked on before, but rather in doing something that no one ever worked on before and that will add to knowledge appreciated and acknowledged by the scientific community'(Gaston, 1973: 3–4).

This understanding of originality is doubly restricted: first, it equates originality with making a new discovery or developing a novel method, ignoring other forms of innovation, such as proposing a middle-range conceptual shift; second, 'making new discoveries' in the name of scientific progress is a conception of originality that more aptly applies to the natural sciences than to the humanities or even the social sciences. Indeed, even in the natural sciences there is reason to believe that innovation is not the overarching imperative Merton believed it to be – at least in the specific context of peer-review panels. Indeed, Chubin and Hackett (1990: 13) argue that reviewers' propensity to recognize innovativeness is limited. For instance, established scientists who reach beyond the 'conventional wisdom' or pursue topics outside their areas of acknowledged competence are frequently rebuffed. Far from being an objective matter, defining and establishing originality is itself the object of conflicts and negotiations.[7]

Studies of Peer Review

Defining originality is an arduous enterprise. Indeed, 'the difficulty in being original is made more difficult by the problem of evaluating originality' (Dirk, 1999: 765–66). It seems that it is difficult for some panelists to define what originality means in the abstract. When we ask the panelists what is the difference between an innovative and an original project, they don't find an easy answer:

> An innovative project? Hum, that's a good question (coughs). There's a bit of tension because a think, by definition, an innovative project is one that addresses a topic in some sense no one else has addressed. But that of course conflicts with the idea that the topic has to be grounded in the existing literature. So I guess, the innovation comes from the answers that a person wants to, the potential answers to the question the person wants to explore [...] I look for people to be suggesting innovative answers to those questions and innovative methods for exploring or testing those hypothetical types of answers (Political Scientist).

By definition, 'originators have no peers' (Horrobin, 1990). Therefore, who can judge them? And how? Peer review is a critical gate-keeping mechanism in

academia, and it has been the object of numerous studies. Social scientists working on the topic have been particularly concerned with the fairness of the outcome and the biases of panelists, and they often equate fairness with consensus.[8] The question then is whether consensus about quality is typical of funding panels. The answer is not clear. On the one hand, studies of evaluation processes point to a relatively high level of consensus (Cole, 1992). One study finds a common 'language' used in the evaluation of the quality of research, with a focus on scientific excellence (Aguilar *et al*., 1998). Referees use the same descriptors to discuss proposals such as 'exceptional quality' and use a few common categories of classification, such as the distinction between original and unsurprising research (Fournier *et al*., 1988). On the other hand, other studies point to low consensus. For instance, based on a review of the literature, Langfeldt (2001) concludes that there is a low degree of agreement between referees who tend to have various kinds of contradictory biases. Along similar lines, in an attempt to improve reviewer consensus, Dirk (1999) conducted an experiment where she developed a typology of eight different types of originality (in relation to hypotheses, methods and results) and asked respondents to rank their publications according to each type. In response to her questions, 40 per cent of respondents declared themselves uncertain of whether the typology she proposed would help make journal peer review fairer, and an additional 16 per cent believed it would not.[9] This outcome also suggests low consensus among evaluators. Paying attention to the meaning that panelists give to various criteria, such as originality, may shed light on this ambiguous question of consensus by stressing the construction of quality as defined by the panelists, as opposed to objective qualities embodied in scientific projects that panelists come to recognize.

The Meaning of Criteria: From Cognitive Categories to Types of Arguments

The assumption that objectivity in the evaluation of knowledge often goes hand in hand with the assumption that judgements about scholarship and judgements about scholars are clearly independent from one another. In this context, assessments based on personal characteristics (such as reputation) are often viewed as contaminating the peer-review process. Hence Merton argued that there are two meanings of excellence: one which refers to the personal qualities of scholars and one which refers to the work itself.[10]

In recent years, sociologists of science have repeatedly questioned the Mertonian firewall dividing the sphere of the social and the sphere of knowledge. Pierre Bourdieu (1984; 1989) and colleagues (Bourdieu and de Saint-Martin, 1975) are among the few who have examined criteria of evaluation of scholarly knowledge. Drawing on the structuralist tradition, they uncovered hierarchically ordered, bipolar oppositions, such as those contrasting work and scholars qualified as original/banal, brilliant/dull, gifted/motivated, distinguished/vulgar, cultivated/academic, eloquent/awkward and refined/crude. This system of oppositions is what Bourdieu and de Saint-Martin (1975) called the 'catégories de l'entendement professoral' ('the categories of professorial understanding').

In Bourdieu's sociology, these oppositions correspond to two underlying ideal types of academic success: the most highly valued type evokes an image of charismatic qualities based on *individual talent*, while the other suggests that success comes through *hard work* and determination. Bourdieu and de Saint-Martin (1975) argued that the most highly valued criterion, 'talent', tends to be used to describe the work of students from higher social backgrounds. Hence, they demonstrate that the categories of perception through which work is assessed are inherently social.

Their research on scholarly evaluation stands in stark contrast to most research on the peer-review process that sharply and normatively divides the spheres of the social and of knowledge. While this research inspires our study, we also go beyond it. Contra Bourdieu, we do not presume that originality is defined primarily through a pre-existing hierarchically ordered matrix of oppositional categories. Rather, we pay attention to the various meanings given to originality by panelists, and we focus in particular on the types of arguments they deploy to establish the originality of specific researchers and proposals. Classifications are always flexible, and categories and boundaries between categories are often uncertain (Knorr-Cetina, 1999: 137). Hence, they are the result of ongoing argumentations, interpretations and negotiations: they need to be justified, that is to be accompanied by sets of arguments.

Moreover, we take issue with Bourdieu's focus on disciplinary differences. He argued (1984; 1989) that constructions of worth vary between disciplines. Focusing on struggles surrounding the legitimization of various dimensions of academic activities (e.g. the opposition between research and teaching), he showed that some disciplines value and promote the pole of the opposition that they are most closely associated with. For instance, literary scholars put more emphasis on teaching than scientists in evaluating their peers.[11] Other studies have also focused on the cultural dimensions of disciplinary boundaries (Abbott, 2001; Becher, 1987; Bender and Schorske, 1997; Clark, 1983; Heilbron, 1995; Wagner *et al*., 1990). Rather than positing that disciplines are strongly differentiated, we show that the types of arguments used by panelists – how they talk about originality – span disciplinary boundaries. For example, people from any of the disciplines we studied might have referred to a proposal as 'daring' or said that it 'brings new evidence to bear on an old question'. However, the specific content academics give to 'daring' or 'an old question' appears to be determined by their disciplinary (or sub-disciplinary) background and their expertise. Finally, we contribute to the literature by showing, contra Shapin (1994), that judgements about character remain intrinsic to academic recognition, and to judgements about originality in particular.

Findings

Our analysis of interviews suggests that panelists point to three types of evidence when describing how they go about identifying and evaluating scholarly originality: evidence having to do with the substance of the proposals themselves; with the applicant's character, defined as personal virtues (such as courage) and other

qualities (such as brilliance); and with the panelists' own emotional response to the proposals and the researchers. While the distinction between these types is generally clear, panelists may use them in combination with each other when discussing any particular proposal.

Substantive Qualities of the Proposal

Unsurprisingly, in assessing the originality of a proposal, panelists often point to evidence having to do with the substance of the proposal itself. They make reference to the topic, question, theory, method, or data of the proposed research – and often the relationship between two or more of these components. In discussing these various dimensions, they typically point to small-scale innovations that are identified as innovative based on the panelists' own expert knowledge of the field – knowledge that allows them to *recognize* what constitutes new data or a new perspective. This is expressed by a historian who describes in general terms the extent to which the proposals reviewed by the panel on which he served exhibited originality. He explains that the panelists recognized as original 'those things that drew on especially new sources of information, that added new perspectives to relatively complacent fields of research – that was what we found interesting.' Echoing these remarks, but adding more specificity, an English literature specialist states that originality is about bringing new perspectives to established research fields. He praises a proposal that approaches the study of Aramaic through a socio-historical perspective, which he contrasts with the more traditional philological and 'new criticism' perspectives. Another interviewee, an anthropologist, praises a proposal because it deftly recycles old theories to apply them to new problems 'in a generative way'. Finally, an economist illustrates innovative research in the following terms:

> An innovative project would be something that, for example, would use lots of techniques that have not been used before [. . .]. A project that brings methodologies that have not been applied to economics, in econometrics, yet, and has a lot of potential for the analysis of time-series in the future.

Clearly, the cornerstone of originality lies in a delicate balance between the old and the new, and assessment of originality concerns as much the predicted substantive impact of research as its generativity for understanding other questions. But this substantial definition brings us only so far. A fuller understanding has to take into consideration the heroism associated with originality, and this requires crossing the sacred firewall that separates knowledge from those who produce it.

Talking about Character

The *Concise Oxford Dictionary* mentions the moral and mental qualities of a person as key dimensions of character. A systematic analysis of the interview transcripts reveals many mentions of such qualities – and particularly moral qualities – by our panelists. Wining applicants were deemed courageous, ambitious, risk-taking, independent, curious and intellectually honest. They were also characterized as 'challenging the status quo' and as 'exhibiting a passion for ideas'. Likewise, the vocabulary used by panelists for describing lack of

originality had a clear moral tone. Those at the losing end of the competition were deemed to lack ambition, energy, or creativity. The terms used by panelists to describe them include: complacent, tired, hackneyed, 'rehashing', 'spinning their wheels', traditional, 'gap-filing', or alternatively, trendy and facile.

While panelists were ostensibly evaluating proposals, they very frequently and easily slipped from discussing the substance of the proposals – the imputed qualities and traits of their authors (as if the latter suffered from guilt by association and as if proposals could be read as templates of character). And this, despite the fact that several panelists professed their desire to maintain a clear distinction between proposals and proposers – following the Mertonian dictat. In doing so, they employ what we call the metonymic mode, using 'the name of one thing for that of another of which it is an attribute or with which it is associated' (*Merriam-Webster's Dictionary*, 2002). Just as 'the oval office' is often used to refer to the president, the panelists will describe a proposal as risk-taking, while they are in fact describing the author as risk-taking (since the proposal does not act by itself). They will also describe a proposal as conformist or trendy, suggesting that the author is a crowd-pleaser, perhaps someone who lacks backbone, independence and individuality. In some cases, the judgements that bear on character are not made in a metonymic mode, but refer explicitly to the author's character (this is what we call the 'personalistic mode'). The metonymic mode, which is most often used, helps panelists legitimize their assessments by framing them as concerned with substance as opposed to people.

One panelist, an art historian, slipped in the metonymic mode to discuss the courage and passion of the researcher as embodied in his/her work, which she takes to be an equivalent of originality. Among the criteria of evaluation she privileges,

> [c]ourage is important [. . .] to go against the received so-called consensus, to be suspicious of that, to ask interesting questions [. . .] none of us can be original, but certainly I think amongst all of us, and for myself as well, [what we are looking for is] a nose for, a real passion for ideas, regardless of whether they get the grant or not, a real love of working with their minds [. . .]. And somehow, it's an aroma.

The 'aroma' let off by the proposal helps the panelist develop an impression of the level of emotional commitment of the researcher ('a real passion for ideas') which she equates with intellectual quality. Similarly, a historian extrapolates a lack of independence and a certain laziness in a proposer from his proposal when he says:

> I don't flop over with joy when somebody in history comes in and says 'I use race, class and gender as my categories'. That could be OK, that could be fine for a different project, but it's what everybody does. It's the line of least resistance now. When people do the line of least resistance and flow that in the rhetoric of subversion, I tend to get very turned off.

Also criticizing the character of the proposer via the proposal, in a perfect metonymic mode, a philosopher says about a particular project: 'I thought it was very trendy, politically correct, [a topic] that we've all tired of'. The author could be nothing but politically correct if his/her work is described as such.

Another way in which judgements of originality are made about people rather than proposals is when the proposed research is seen in light of the author's prior work or that of his advisor. As a musicologist puts it, '[y]ou know, if they're just rehashing what they did as a doctoral dissertation, that's probably where they're going to be stuck for the rest of their academic career'. For doctoral students, this mimetism can be directed towards their advisor's work. A historian explains that 'I react very strongly when I see work that's extremely derivative. When I see dissertation projects which are spin-offs of the advisor, I always say: 'Oh well, I'm not sure about this person'. And why is being derivative a problem? As one sociologist put it:

> I believe that there is a tremendous inertia in academic life, a tremendous inertia to reproduce what's going on, to reproduce advisors, projects, frameworks, theories or whatever. There is a tremendous self-imposed constraint about emulating what's considered hot, which obviously generates its own form of conformity.

This quotation implicitly constructs personal dynamism as tantamount to originality and excellence. Note however, that working within one's area of expertise and extending one's past work can be seen by panelists as strengths, since it is read as indicative that the researcher will be able to carry out the proposed study. Thus, what might under some circumstances be considered a weakness can be construed as a strength under different circumstances. This observation is incompatible with Bourdieu's (1989) method which consists in analysing the categories through which scholarly work is assessed as ordered in a relatively rigid and stable hierarchy of oppositional categories. His approach ignores context in true structuralist fashion.

While these examples show how panelists cast judgement on character conceived as moral virtue (independence, personal strength), we also find that they often highlight more cognitive or mental aspects of the proposer's personal characteristics, such as creativity and intellectual ability, when making substantive judgements about a proposal. These judgements often bear simultaneously on the substance of the proposal and the personal qualities of the proposer, and suggest once again the impossibility of establishing a clear distinction between the social and scientific criterion salient in the evaluation process. For example, in describing what originality is, one historian said, it is 'the ability to take two ideas that have nothing to do with one another', thus pointing directly to the researcher's personal ability to make unexpected analytical connections. Or, describing the craft of research, a political scientist points to the importance of creative capacities, saying: 'to be a craftsman without insight or creativity, you know, that wouldn't work to create good objects, you have to be creative as well'.

Similarly, panelists will define originality in terms of the way that the researcher's distinctive skills in approaching the empirical material. In describing one of his best graduate students, one political scientist explained: 'he was able to kind of dig up interesting sources of data and was able to bring those data to bear on some thorny issues that people had been debating, but they were just kind of arguing with each other'. Along similar lines, this is what a historian says of a proposal in English literature:

> I liked that she was doing textual analysis in a very elegant and creative way, and using these chronicle texts, as I recall. She shed a real light, not just on history, but

> historiography and the intellectual history as through writing about [Indian] society and culture. And she seemed to me to be doing a very deft, elegant and non-obvious, non-trivial reading of these texts.

These panelists point to the merits of the research by discussing the qualities of the researcher – the two being deeply intertwined.

Emotional Reaction as Proof of Originality: 'Surprising', 'Disturbing' and 'Exciting'

Earlier we mentioned the role of emotions in assessments of originality, arguing that although reviewers recognize originality at the cognitive level, they often experience that recognition as an emotional reaction, which is read by panelists as evidence that they are encountering originality. The language of 'excitement' and surprise thoroughly permeate discussions of originality, and the two are often conflated. For instance, an anthropologist describes a proposal he views as original by saying 'that's a good example of what I would call an exciting proposal'. A historian explains why another proposal is original by saying 'I just found that one really exciting'. Another panelist in interdisciplinary/multidisciplinary (e.g. sociology, linguistics and literature) talks about originality in terms of what captures imagination. Speaking of winning proposals, she says:

> They were innovative in some way, they captured the imagination of the committee, they captured the imagination of the assessors, they kind of sparked, they had something original, they were doing things that were, you know? Many times, we would have said: 'Oh! I'd like to read this when it's published'. 'Oh, I'd like to follow, you know, to know more'. So it captures the imagination of the committee as a whole.

A historian even more clearly equates excitement and originality when she says:

> And then there is that 'something', there's that edge, there's that spark, there's that freshness. And it can happen in any discipline [...]. You get the sense and several people will agree: 'Oh, this is exciting'. And often people will say: 'Oh, I wish I had time to pursue that myself!'

'Edge', 'spark', 'freshness': these words associated with emotions make research exciting and heroic, as opposed to words – dusty, musty and humdrum. It is these emotions that keep researchers going. It is the hope of producing such emotions in others that keep them hunched at their desk as Collins (2004). It is not surprising that these emotions are central to how panelists talk about originality – using emotional as opposed to cognitive terms.

Emotional evidence of originality is often offered together with more substantive and personalistic evidence in the panelists' descriptions of what makes a winning proposal. One anthropologist, for instance, describes a winning proposal and, in a metonymic mode, its author, in the following terms: '[it's] unusual and unique. It's breaking a paradigm [...]. Something that really steps outside of those boxes in a way that [...] in a sense takes a risk. [...] And it's pretty hard: innovative, exciting, new, different work'.

A Note on Disciplinary Differences

In general, the same vocabulary of originality was used by panelists from all disciplinary backgrounds. For instance, panelists from all disciplines appreciate

what is courageous and daring, and reject what is hackneyed and trendy. Of course, what a philosopher finds challenging or trendy may be very different from what someone in English considers challenging or trendy. These judgements are grounded in the distinctive disciplinary history and substantive foci. For example, in literary studies, work that focuses exclusively on analysing texts (which is typically called 'close reading') without taking into consideration the social context in which that text was produced is now seen as old-fashioned. In this case, the criteria in use are specific to a particular discipline at a particular historical period. While the specificity of such criteria varies enormously across disciplines, and presumably, overtime, we find that they are likely to be couched within a broader rhetoric of excellence that stresses substantive, character and emotional evidence. These were overall very salient across all fields, and more so than distinctively disciplinary classification systems. But this remains to be examined further in light of a complete analysis of our interview data. The results indicate that disciplinary differences are much less salient than the literature would suggest, at least as far as the discourse of excellence is concerned.

Conclusion

In this essay, we have discussed some preliminary findings of our research on the evaluation of originality in peer review. In contrast to the predominant Merton-inspired scholarship on the peer-review system, we adopted an inductive approach by examining how panelists assess grant proposals.[12] We found that when they discuss originality, panelists employ different types of arguments, sometimes focusing on the substantive aspects of the proposal itself, the character of the proposal's author, or their emotional reaction to the proposal. The finding that panelists often make judgements about the author's character resonates with a rich line of culturally oriented research on knowledge-making that breaks down the divide between the sphere of knowledge and the sphere of the social. We go beyond the disciplinary focus of much of this research (at least for the time being) by showing that the types of arguments made about originality span disciplines.[13] We also show that Merton's contention about the norm of universalism in science holds to the extent that it is important for researchers to legitimize their evaluations as based solely on 'objective' scholarly criteria.

Following Shapin (1994), we showed that assessments of character (having to do with trustworthiness for Shapin and with courage and ambition for us) are central in academic evaluations. Although Shapin contends that character judgements in modern science have been largely surpassed by the importance of credentials and expertise, we showed that the discourse of character is alive and well, albeit used in a different context. Interestingly, the general evaluation of quality concerns not only originality, but also trust. These two terms correspond to a tension in grant proposal-funding decisions between admiration and bureaucratization. On the one hand, judgements about originality are based on an ideology of the talented genius, which is central in literature, science and art (Dirk, 1999; Fuchs, 2001). But the process of evaluation of the quality of research is also a part of a bureaucratic enterprise of grant-giving, and panelists also give weight to more

practical considerations based on establishing trust (in the qualities of researchers, their competence, accountability, scientific dispositions and so on). Different panelists, panels, organizations and disciplines may tend to weigh originality more heavily than trust. We have yet to generate grounded hypotheses to predict these variations.

There are a number of open questions and further aspects of this topic that we plan to explore in future essays. Our discussion here, for example, has completely ignored the fact that the decision-making takes place in the context of a dynamic panel process. First, the proposals are reviewed by one or more external reviewers whose area of expertise is much closer to the proposed research than that of the panel members. What influence, if any, do these reviews have on the opinions of panel members? How are arguments shaped by the disciplinary composition of the panel? Are arguments framed differently if they are meant to convince a philosopher, a political scientist, or an English literature expert? How about the order in which proposals are considered? Does it have a stronger impact at the beginning of a meeting than it does at the end? And what impact do the exigencies of the panel decision-making process, such as limited time, have? Another line of questions would concern whether there is any correspondence between statements in a proposal attesting to its originality and the assessments of external reviewers or panel members.

At a time of important discipline-spanning debates about the construction of the canon and the meaning of academic excellence, studying such issues in a concrete context can be an important complement to more abstract debates. Scholars do tend to debate epistemological issues as if theoretical discussion existed in a vacuum. In the context of peer-review panels, methodological and theoretical issues are discussed and hashed out in a practical context, with all kinds of factors affecting judgement, including the number of proposals needing to be reviewed and the pace of the discussion. Indeed, one would be hard pressed to find a single exchange in the context of our panels in which epistemology is employed without reference to such factors. The constitution of a disciplinary canon and the definition of academic excellence are activities that happen in real time, with real people and real organizational constraints. Our contribution is to examine these au 'ras-du-sol', with the hope of shedding new light over important debates in the sociology of knowledge and science.

Endnotes

1 Peer-review panels are a part of the general system of academic recognition. Members of these committees act as 'gatekeepers' who are also part of a bureaucratic process of evaluation, involving formal rules, explicit criteria and bureaucratic organization. The purpose of these panels or committees is to assess the quality of a proposal and the excellence of applicants in order to give them resources they need to conduct research and thereby gain more recognition. In a more constructivist line of reasoning, we would argue that rather than 'recognizing' intrinsic qualities, peer-review panels designate and elevate proposals and their authors, marking them with a stamp of excellence. See also Fuchs (2001) in this regard.

2 Some of the findings presented in this essay are presented in Guetzkow *et al*. (2004), in a somewhat altered form. That paper goes beyond this one by developing a typology of definitions of

originality, examining disciplinary differences in the definition of originality, and elaborating the argument about the conflation between original work and individual moral worth.

3 At the time, the English gentry believed they were the only ones who possessed this combination of virtues.

4 In the epilogue, he speculates that the personal knowledge gained in face-to-face interaction still plays an important role in establishing trust, but this only applies to the close-knit communities of scientific core-sets, which is a far cry from the impersonal nature of the peer-review panels we studied.

5 The specific competitions studied were the following: the International Dissertation Field Research Fellowship program at the Social Science Research Council at the American Council for Learned Societies, the Women's Studies Dissertation Fellowship program at the Woodrow Wilson National Fellowship Foundation, and the fellowship program in the humanities at the American Council for Learned Societies.

6 Differences between the Canadian and American panels we studied include the following: The Canadian panels are uni-disciplinary, while the American panels are multi-disciplinary; the Canadian system of grant-making is centralized, while the American system is decentralized; the Canadian panels tend to be more conservatives than the American ones and to be more sceptical about claims concerning originality.

7 This is illustrated by the peer-review system in Canada, which has repeatedly been criticized for its unmistakable bias towards conservatism in the name of quality control (National Commission on Research, 1980). A report written for the Canadian government in the 1990s found that many academics view government's funding agencies as supporting the 'tried and true rather than the risky and innovative' (Social Science Research Council of Canada, 1998: 7). The report concludes that members of the scientific community often believe that 'in view of small budgets and its resistant history of conservative committees', research councils do not tend to receive 'the best or most innovative applications'. While these perceptions are not widely shared, they are of serious concern to the Council. The Council must ensure that its programmes and its committees are willing to push the boundaries of existing paradigms and that the Council is not perceived as the funding body for the most pedestrian researchers' (Social Science Research Council of Canada, 1998: 8). Accordingly, originality is now listed in the official guidelines of the Council as one of several important criteria of evaluation to be considered by reviewer. Moreover, colleagues who write letters of evaluation are now directed to evaluate the originality and impact of the investigator's previous work, and the originality and significance of the expected contribution to knowledge of the proposed research. The other 'official' criteria are: intellectual, cultural and social importance of the research, pertinence of the theoretical approach, pertinence of the research strategies and methodologies, and pertinence and efficacy of the plan of diffusion inside and outside the academic milieu.

8 See the special issue of *Science, Technology and Human Values* (1985) on 'Peer Review and Public Policy'.

9 If you identify hypotheses, methods and results as the three main elements of a scientific paper, and if you define originality as 'a permutation of old and new information' (previously reported in the scientific literature/newly reported), the eight originality types are rated by the majority (84 per cent) of the respondents (indicating that they could recognize her typology), and the most frequent type (42 per cent) of originality seems to be new hypothesis/previous-reported methods/new results. But many (40 per cent) are uncertain whether this typology might help to make the journal of peer review fairer, and some of them are not able to grasp all the types (Dirk, 1999).

10 Merton called these two meanings 'excellence in the sense of quality' and 'excellence in the sense of performance' (Merton, 1960/1973). According to Merton, these two definitions of excellence embody two 'doctrines of justification': one by *faith in the individual*, who has yet to prove himself and the other, by *works*; the first is reminiscent of Luther, and the second reminds us of Calvin (Merton, l960/l973: 424). Examining methodically the connections between excellence and recognition for excellence (and how they often diverge), Merton recognizes at one point that the question of the criteria of excellence is complex and he asks: 'What qualities of a seeming achievement are to be judged?' His response is that 'there is a difficult problem, in many spheres of human activities, of discriminating the authentic innovation that merits recognition from the mere novelty that doesn't' (Merton, 1960/1973: 433–44).

11 Literary studies, social sciences and sciences correspond to different ways of defining legitimate professional orientations: whereas the sciences value research at the expense of professorship, the professional orientation in literature studies is highly correlated with professorship and the acquisition of 'academic power' rather than with research activities. Social sciences stand between the two. Bourdieu explains conflicts of evaluations within disciplines by the fact that disciplinary boundaries can blur. For instance, authors in literature can import categorizations from the social sciences into their disciplines: the figure of Roland Barthes is exemplar in the sense that he emphasized research over professorship by orienting his career towards the social sciences.

12 By inductive, we mean both that we abandoned the normative assumptions typical of research on peer review, and also that we did not predefine the categories of evaluation, nor do we assume that they are ordered into a hierarchy of oppositions.

13 We did not study the natural sciences. It may be that the rhetoric of originality is more oriented towards discussions of discovery, but it may also be similar to what we found.

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