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Comments on Bulger: The responsible conduct of research, including responsible authorship and publication practices

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For a sociologically informed research ethics

The starting point of *research ethics* can be found in Robert K. Merton's classic essay from 1942, "The Normative Structure of Science" (Merton 1973). This essay is usually taken as a key contribution to functionalist sociology of science, but I would argue that it also holds relevance for the new field of research ethics. In the essay the American sociologist described and codified the so-called '*scientific ethos'* – the normative framework for the conduct of science – as consisting of a series of norms and values that could be summed up in the acronym CUDOS: communism, universalism, disinterestedness and organized scepticism. To avoid misunderstanding: the norm of 'communism' implies that the results of scientific research are published within a reasonable time and assigned to the community: "Secrecy is the antithesis of this norm; full and open communication its enactment" (Merton 1973, p. 274). Merton wrote his essay long before universities in the USA and elsewhere would be thoroughly commercialized: filing patents was still frowned upon. But Merton was keenly aware that the scientific enterprise was not just co-operation but also competition (he used the phrase 'competitive co-operation'). The competition was not so much, or not primarily, about the acquisition of monetary rewards, but turned on recognition and esteem, credit and reputation. Nevertheless, as frequent priority disputes testified, the struggle could be very intense.

In her paper Professor Bulger writes: "Authorship of original articles describing scientific research is still the coin of the realm of science". Merton said something similar, but with a slightly different accent: "Honorific recognition by fellow-scientists is the coin of the scientific realm". So it is not authorship *per se*, but the credit that it may earn among one's colleagues, that in Merton's view is the currency of science. He sees science as a social system in which original contributions, of course after having been published and thus made available to the community, are exchanged for recognition and esteem and all the possible rewards that may go with these. Such rewards range from *eponymy* (e.g. Boyle's law, Brownian movement, Mendelian genetics), medals, fellowships and membership of prestigious organizations to ennoblement (in the UK) and, of course, the Nobel Prize. A modern quantitative measure of 'recognition and esteem' is provided by the Science Citation Index.

The reason why I recall Merton's work on the sociology of science here is that I want to argue that *research ethics* needs an injection of sociological realism (or better sociological scepticism) to become less naive and also much less anodyne in its

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prescriptions. Merton made one assumption that seems immensely reasonable to me, to wit, that scientists are just like normal mortals and do not possess special moral qualities; they are *not* "recruited from the ranks of those who exhibit an unusual degree of moral integrity" (Merton 1973, p. 276). At the very least, this seems a good 'null hypothesis' to start with. Merton also remarked – remember: back in 1942! – that fraud and deceit are extremely rare, virtually absent, in the annals of science. For Merton, the only way to explain this fact (if it is a fact) was to point out that "scientific research is under the exacting scrutiny of fellow experts" and that "the activities of scientists are subject to rigorous policing", thus strengthening the effect of a successful internalization of the scientific ethos (Merton 1973, p. 276). Since Merton wrote his path-breaking essay, the practice of science – or at least our views of science – must have changed dramatically. From the late 1970s on, the science journals and the mass media have been busily reporting an unending series of affairs involving fraud, deceit, plagiarism and other forms of 'misconduct', especially in the biomedical sciences. In fact this trend has also been the main factor behind the rise of *research ethics*, at first in the USA and later elsewhere. The question is whether research ethics can offer useful solutions to do something about the problem. I am somewhat doubtful.

Will the mandatory instruction in research ethics for those who receive grants, as demanded by the National Institutes of Health (NIH) and the Office of Research Integrity (ORI), really help to reduce future incidents of misconduct, especially in hot areas of science where competition is intense, commercial pressure heavy and the stakes are high? Will the pedagogical effort to impart the values of 'honesty', 'integrity', 'truthfulness' and 'objectivity' on PhD students and young researchers be effective in promoting decent behaviour on their part? Or will this simplistic pedagogy to instill ethical 'correctness' perhaps more likely evoke rebellion? (To me 'environmental correctness' sounds almost as bad as 'political correctness'; if I have to show respect for the environment by undertaking only 'important' research so resources are not wasted, I am strongly inclined to say: "To hell with the environment!".)

In this connection I want to draw attention to a problem which Professor Bulger also obliquely broaches. Referring to Sheila Jasanoff, she writes: "Although agreed-upon norms have been defined in some areas of scientific endeavour, the majority of situations that the scientist must address lie in grey areas that remain undefined, murky, with many pros and cons on how to proceed and little agreement among scientists as to a uniform solution". This could give the impression that we have, on the one hand, domains governed by agreed-upon norms where it is clear for everyone how to proceed, and, on the other hand, grey areas where everything is murky. This would be the wrong impression, I think. One major point of criticism of Merton's sociology of science by the newer, 'post-Mertonian' and 'social-constructivist' school in science studies (to which Sheila Jasanoff also belongs) is precisely that general norms (even if agreed upon) are *always* to some extent negotiable and open to multiple interpretation when they have to be applied to concrete situations.

Take for example Professor Bulger's remark (relating to the third principle of competence): "The results of one's study should be *promptly* published so that others can benefit from the fact that they were done" (italics mine; this could be seen as an echo of Merton's description of the norm of 'communism' – 'full and open communication' – see the quotation above). The critical question, when it comes to implementing this norm, would of course be: How 'promptly' exactly is 'promptly'? The sociologist of science Michael Mulkay provides a telling example. When the

radio-astronomy group at Cambridge, UK, published the first paper on the newly discovered pulsars in 1968, rival groups accused them of having unduly delayed publication of the relevant data. The Cambridge group, however, defended their publication policy. They denied that there had been any 'undue' delay or unjustified secrecy at all. After all, they argued, researchers need time to check their results in order to publish high-quality work! Thus this episode shows that there may be strong disagreement among scientists about what exactly a general norm like the norm of prompt publication implies in a concrete situation (Mulkay 1976).

Towards a culture of accountability?

One way to overcome the problem of the interpretative flexibility of general norms, at least partially, is to install special bureaucratic agencies that are charged with overseeing the rules and norms that are to be followed. As part of their mandated mission, such agencies will undertake to clarify and more strictly define the pertinent norms. However, the forceful attempt to ensure that researchers and their institutions comply with a set of imposed formal rules could easily lead to a 'culture of regulation', or a 'culture of accountability', as the British philosopher Onora O'Neill would call it (O'Neill 2002a). To me, even the name 'Office of Research Integrity' (ORI) already sounds quite intimidating and a little bit Orwellian – something like the 'Ministry of Truth'. In practice, however, the ORI does not seem to have real bite, as scientists already resist its attempt to gather information on such low-key unethical behaviour as authors citing papers they haven't read and condemn this as the agency's meddling in areas beyond its purview (Soft responses to misconduct 2002). A more ominous example is provided by the Danish counterpart of ORI, the Danish Committees on Scientific Dishonesty, which in January 2003 condemned Bjørn Lomborg's controversial book *The Skeptical Environmentalist* as "objectively speaking, deemed to fall within the concept of scientific dishonesty" (Abbott 2003). I find it rather disquieting that a research-ethics panel apparently feels no scruples to pass judgment on a lively controversy involving a lot of scientific and political issues. In my view, this is not the way to settle such a debate.

Professor Bulger alludes to the "calls for increased education and accreditation of investigators and administrators as well as increased audits to ensure that regulations are being met within the institutions in which the research is being done". I fully sympathize with her remark that "Scientists must not be creating a culture of regulation, but a culture of conscience". Still I am concerned that *research ethics* 'as-it-really-exists' may become increasingly institutionalized as part of a growing culture of accountability. Onora O'Neill gives a very disturbing account of the 'audit explosion' and the growth of a 'culture of accountability' in all social domains beyond the original financial context, especially in the public sector of the scientific, health and service professions (O'Neill 2002a; see also O'Neill 2002b). The 'new wave of audit' makes environments 'auditable'; "audits do as much to construct definitions of quality and performance as to monitor them"; they produce a "drift to managing by numbers"; "the construction of auditable environments has necessitated record keeping demands that serve only the audit process" (quotations from Michael Power in O'Neill 2002a, p. 132-133). The new culture of accountability may enforce 'trustworthy' behaviour, but it does not breed trust – it rather breeds suspicion! The proliferation of distrust finally raises the question: Who audits the auditors? "Ultimately there is a regress of mistrust in which the performances of auditors and inspectors are themselves subjected to audit" (Michael Power, quoted in O'Neill

2002a, p. 133). A second route to trustworthiness, according to O'Neill, has aimed to construct a more open public culture, by ensuring that information is available to the public. She refers to the Committee on Standards in Public Life (installed by John Major in 1995 and chaired by Lord Nolan), which promulgated seven ethical principles for the conduct of office holders: selflessness, integrity, objectivity, accountability, openness, honesty and leadership (colloquially known as 'the Nolan Principles'). Since the Nolan reports, new requirements have been widely implemented: "Office holders are required to act only in the public interest, to be open, to avoid conflicts of interest and to declare any interest (the standard for identifying a declarable interest is that others would perceive it as such); declarations of interest are made public [...]" (O'Neill 2002a, p. 135-136). The unexamined and questionable assumption of this quest for openness and transparency, according to O'Neill, is that by enforcing trustworthiness among office holders the new culture will actually receive more public trust. The intended field of application of this openness offensive is much broader than the so-called 'ethics movement' in the biological and health sciences, but is its thrust so much different?

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