

## Comment

# Real science is excellent science – how to interpret post-academic science, Mode 2 and the ERC

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When thinking about this contribution, an *homage* to John Ziman, one question occurred to me repeatedly: what would John have made of the European Research Council? Here is a newly established institution with the sole objective to fund ‘frontier research’ at EU level, based exclusively on scientific excellence and subject to pan-European competition of the best researchers. Would he have interpreted it as a vindication of academic science as a culture, a deliberate turning away from ‘post-academic science’ or even of overcoming it? Or, would he have seen it as the establishment of a small niche, to be recognized (and praised) for its respect for the norms of academic science, whose wider impact still has to be seen? A third possibility would have been to suspect that the ERC is merely another clever move to compensate for, or to ‘beautify’ a research landscape that increasingly comes to resemble what he called the ‘scientific coal face’ – a vast and growing area of research, propelled by the search for utility and guided by the ubiquitous pragmatism that has taken over, asking specific questions instead of following the curiosity of the individual researcher.

Not that John was overtly combative in opposing what he called “post-academic science”. He was a realist in accepting it, albeit with much regret for what seemed to him irrevocably lost. He was interested as many others were, including my collaborators and myself, to probe the ongoing deep transformation that the science system was undergoing over the past fifteen years or so. “Real science” for him also meant to embed science in realistic circumstances. He therefore accepted developments that evidently were happening, seeking to analyze and interpret them. But he deliberately went one step further than most of us, in subjecting the changes in institutional and scientific practices not only to academic scrutiny, but in probing their likely impact on what he saw as the inalienable bedrock of ‘real science’. He wanted to offer an overall naturalistic account of “what science is and does”. In effect, he introduced a sociological dimension, not to replace the traditional philosophical dimension, but to enlarge it. For him, ideas were cultural elements as well as *cognitive* entities. *Individual* acts of observation and explanation gain their scientific meaning from *collective* processes of communication and public criticism. The notion of scientific method is thus extended outside the laboratory to a whole range of social practices.<sup>1</sup>

This is not the place to revisit John’s impressive oeuvre of 22 different books or to assess the lasting legacy of the ‘knowledge’ series that began with *Reliable Knowledge* and led to *Real Science* (see the commemorative volume of the Journal of Consciousness Studies, 2006).<sup>2</sup> Rather, I want to focus on commonalities and differences that unite and separate when John’s notion of post-academic science is compared with Mode 2 knowledge production. The advent of post-academic science, as he saw it, was part of the larger transformation in which we were also interested. He did not equate the two. Rather, he thought that the transformation did not only alter the production of knowledge, but rather introduce a whole new way of life. Post-academic science is the resultant of innumerable improvised solutions to immediate practical problems. It is the product of expediency, not design. It does not suggest a total repudiation of traditional goals, nor does it only stand for discontinuity. Historically, it came out of academic science and many of the old institutions – universities, research institutes and other knowledge producing institutions – are still there, but their function and way of doing science has changed. Essentially, post-academic science grew out of what John had diagnosed early on as the ‘steady state’ of state funding. From the 70s onwards, especially in the UK, the budget cuts from the government, initially triggered by a situation of economic stringency, were never to return to normal, but became a new normality themselves. In their wake, the practices of doing science changed in what now seems an irreversible way. The new way was characterized by the relentless pressure of grant-writing to obtain the necessary funding from other sources. It became deeply enmeshed in a culture of searching utilitarian

objectives, driven by norms of efficiency and accountability. It fitted neatly into what Michael Power later acutely dissected as the 'audit society'.<sup>3</sup>

Our observations and their interpretation diverged in a somewhat paradoxical way. John, perhaps triggered by the autobiographical fact of having lived through the Thatcher years and having witnessed how they profoundly changed British universities and research institutions within a very short period of time, took the personal decision to leave Bristol University, opting for an official pre-retirement offer and hence for life as an independent scholar. He must have felt that academic science had given way to a way of life in which the pressure to give more obvious value for money became the dominant criteria. While my collaborators and I spoke of institutional boundaries becoming blurred, of researchers forming new and changing collaborative configurations that were formed around a common problem definition as their epistemological and pragmatic guide, working on their solution in specific 'contexts of application', John saw predominantly the external pressure threatening the creative insights of the individual. He saw science as becoming pressed into the service of the nation as the driving force in a national R&D system, a wealth-creating techno-scientific motor for the whole economy.

For him, Mode 2 was essentially a post-industrial hybrid of the academic and industrial modes of research – a view with which we can identify. But he went one step further, with which we publicly took issue in *Re-Thinking Science*. In a characteristic passage he wrote: "Although 'Mode 2' may also incorporate traditional scientific values – including, of course, the sheer obduracy of physical reality – it is clearly an activity where socio-economic power is the final authority".<sup>1</sup> This is clearly not our view. Where we saw a distribution of knowledge production across many, heterogeneous sites throughout society, thanks to higher education, mass universities and the ubiquitous spread of information- and communication technologies, John saw – or feared – the extinction of the scientific ethos and its commitment to search for knowledge without regard to utility or practical ends which only the individual could carry out. For him, 'real science' was at stake, where we – perhaps naively – saw merely the practice and image of science changing. We saw its core as being much more resistant and actually quite adaptable. We believed, perhaps again naively, that the reliability of science was the indisputable *sine qua non*, since science simply was judged by its ability to work. We later argued in a direct exchange with John, that science having become entangled in many societal contexts, had to enlarge its scope. Its reliability (and the practices and processes safeguarding it) would remain indisputable, but in order to meet the public and its occasional contestation of what science and technology had produced or how it impacted on people's lives, science in addition had to become 'socially robust'.<sup>4</sup>

For us, the transformation was brought about not by the decisions of one or many governments to level their funding and the ensuing pressure to become 'useful' as well as 'accountable', sometimes in the most obnoxious and silly ways, but by a societal process which we called the 'contextualization' of science. While still being a very special kind of institution, science was nevertheless subjected to a wider set of processes that had become the hallmark of modern societies, like democratization and accountability. There were, we argued, co-evolutionary processes at work in science and in society, although seldom in harmony with each other. They differed in how certain key dimensions were expressed, what results and impacts they produced and to which kind of novel problems they would give rise. We were thus casting our observational and analytic net wider than John, trying to capture also some of the turbulence where society and science meet. This is perhaps why we never doubted the ability of science and of its ethos to survive (although it too was subject to change) – we took it simply for granted. Science, *real science*, in our view was resilient and adaptable enough to way beyond the trials and tribulations of a any national science system.

This brings me back to the ERC and to its founding principles of funding basic research as a bottom-up, curiosity-driven activity with individual scientific creativity as its driving force. In *Real Science* John is very skeptical about basic research as a policy category. Basic research is often defined by exclusion, he argues, a residual category of activities that do not fall under instrumental, or strategic purposes. In the end, the notion of pure science cannot be defined in policy terms, since policy is all about future action. Besides, the trouble with formal research objectives is that nobody expects them to be met to the letter. Policy talk is so steeped in practical intentionality that it cannot attach any precise meaning to a non-instrumental activity. He is also reluctant to attach too much weight to curiosity-driven research, if curiosity is solely defined as a psychological, individualistic trait. The only firm institutional basis that he finds acceptable for 'real science' is academic science as a culture.

The ERC will fund individual, curiosity-driven research which is truly bottom-up and which does not know any constraints of pre-defined themes or objectives of practical utility. Research funded by the ERC which meets the criteria of excellence, to be established through a pan-European competition, will predominantly be carried out inside universities across Europe (although, as is to be expected, in a highly skewed distribution). It will therefore embody and strengthen the kind of academic culture of science that John had in mind. And yet – the universities will be very different from what John envisaged. At present, especially in continental Europe, they are caught in the middle of a very painful transition. They have to become competitive – for funds, for good students, for shaping their own profile which sets them apart from other competitors and, preferably, bestows some comparative advantage on them. And – and this where the ERC and a policy category like ‘frontier research’ enters: they will have to compete for scientific excellence. Where John took it for granted that academic science as culture would result in scientific excellence, this now needs to be proven. I will show in a highly selective way.

There is another way of looking at it. Post-academic science arose from academic science and built on it. In doing so, it had to respond to the various pressures brought upon it from State, Industry, Society – and, increasingly, from a globalizing world. Yet, in the end, *real science*, meaning *excellent science*, turns out not only to be socially robust, but scientifically robust as well. The pursuit of excellence needs an autonomous space, where curiosity is the driving force, pursued by individual creative minds. But this autonomous space is not there as a free-for-all. It needs to be built and nourished. It needs to be cultivated, and cultivation depends, among other, on competition and selection.

Major advances in scientific understanding depend on selecting research priorities and the right kind of research question. They depend on the individual who pursues them –but also on choosing the right kind of person who will be funded in order to do so. Let us hope that the ERC will succeed in setting new standards for excellent science as a culture – just as John had intended in his work, quite without putting it this way.

## Notes and references

<sup>1</sup> J. Ziman, *Real Science. What it is, and what it means*. Cambridge: Cambridge University Press (2000).

<sup>2</sup> J. Ziman, *No man is an island*, *Journal of Consciousness Studies* **13** n.5 (2006).

<sup>3</sup> M. Power, *The Audit Society. Rituals of Verification*. Oxford: Oxford University Press (2006).

<sup>4</sup> H. Nowotny, P. Scott and M. Gibbons, *Re-Thinking Science. Knowledge and the Public in an Age of Uncertainty*. Cambridge, Polity Press (2001).

<sup>5</sup> M. Gibbons, C. Limoges, H. Nowotny, S. Schwartzman, P. Scott and M. Trow, *The New Production of Knowledge: the dynamics of science and research in contemporary societies*, London, Sage (1994).

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