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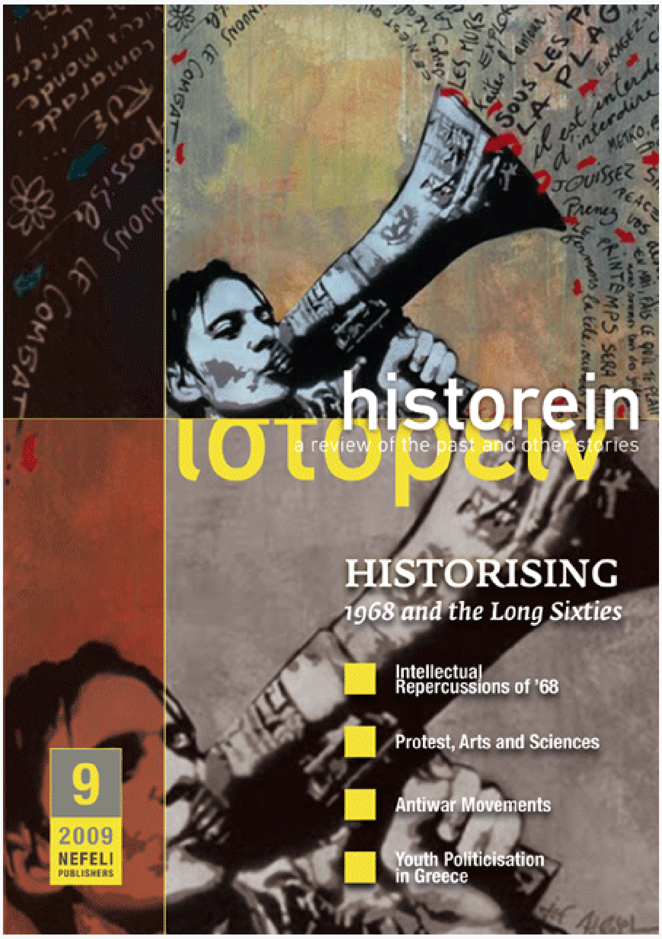

Questioning the Neutrality of Science

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One of the intriguing changes that were brought about as a result of various social, political and intellectual developments during the 1960s were the deep transformations in the way we view the sciences and, more specifically, the physical sciences and technology. It is still not the case that these changes have been fully incorporated either into the public discourse of scientists or the various attempts to popularise scientific developments. Yet, in many ways, they have marked most of the discussions among the historians and sociologists of science during the last 25 years.

Since the end of the nineteenth century and until about the mid-1960s the great majority of historians and philosophers of science – along with the scientists themselves – believed strongly that what the scientists did was to unfold a pre-existing objective structure of nature, that the truth of the scientific 'facts' was universally valid, and that the origins of scientific developments played no role in forming their character. Most scientists still believe that the content of the sciences is independent of human activities, and that the establishment of scientific knowledge is exclusively the result of its truth.[1] But did people during the different historical periods have the same criterion of truth by which they assessed the scientific facts? Were such criteria dictated by strict philosophical considerations or were they the result of certain social activities aiming at guaranteeing some kind of a consensus around what looked 'physiological', 'normal' or 'logical'?

Many supporters of positivism – which includes most scientists – seem to neglect that for many centuries people (and not only scientists) had different but specific reasons for holding as true whatever they believed about the structure and functioning of nature. And in order to change their views, it was not sufficient to produce a formally correct reasoning. Nor was it always the case that producing empirical evidence was *by itself* sufficient to change peoples' minds. For many centuries empirical evidence was not as convincing as theoretical argumentation based on metaphysical views about nature. To believe in the geocentric structure of the world or in Aristotle's theory of motion did not disturb the dominant metaphysics and was *also* empirically satisfying. And when experimental practice first started to become an integral part of the scientific enterprise in the seventeenth century, it took a long time to legitimise such a view on science. To convince all those who considered themselves as having some kind of jurisdiction over the study of nature that, in the practice of the new natural philosophy (what we came to call science) experimentation had to be given a dominant role was not a straightforward process. In some milieus, as for example in England, new institutions like the Royal Society of London had to devise all kinds of novel rhetorical strategies in order to establish *such* a natural philosophy. It may, thus, be possible to consider the notion of truth associated with the criterion for accepting the validity and, of course, objectivity of theories about nature as a historical category, itself being continuously recast, depending on all kinds of historical conditions.

Hence, an integral part of writing the history of science has been the attempt to understand how people shifted from one site of beliefs into another, how they abandoned what they thought as being true, and how they negotiated the 'new' criteria of truth with respect of which they adopted the new theories or the new picture of the world surrounding them. The study of the processes of persuasion, the understanding of the strategies of rhetoric, has become an almost *a priori* necessity for the history of science. The complex processes and dynamics which originated in the 1960s undermined positivism as the constitutive principle of the history of science. At the same time, there was a questioning of another 'undoubtedly true' characteristic of science: its neutrality. If the questioning of positivism brought about unsettling methodological reorientations in dealing with the past of the sciences, the intense suspicion with which the neutrality of science was viewed brought about changes in the value system itself concerning the sciences.[2]

The neutrality of the sciences

The great majority of physical and social scientists believe that the sciences are a neutral instrument, which becomes useful or detrimental depending on the intentions and aims of those who decide how to use and apply them. This view for the social sciences was clearly expressed by Max Weber. The claim about the value-neutrality of the sciences emphasises that decisions about scientific theories should be dictated only by cognitive (in a way, epistemic) values – thus conveniently transposing the problem to that of the neutrality of the cognitive values. It is impressive that in the West until the 1960s this view was, also, held by most Marxists and, at the same time, their adversaries. And such was, also, the case in the Soviet Union, if one considers the publications of the Academy of Sciences. Of course, throughout history there have been good and bad uses of the sciences, and there have been uses where class strategies were rather evident, but all this is far from expressing the neutrality of the sciences. In the discussion about the neutrality of the sciences, the exclusive emphasis on their use, and not on the way they come into being, masks the fact that the scientists themselves do not function in a social and ideological void. The questions they pose are not independent of the problems faced by specific social groups. What they plan to investigate is highly dependent on their own value system – which not only includes ethical values but, also, constitutive values concerning the subject matter they work on, their everyday practices, their views about cooperating with other members of the scientific community, the ways they will acquire funding, the kinds of people they will employ, etc. Scientists decide which problems to work on, how to go about dealing with them and what particular ways they will choose in order to publicise their results. Such decisions are not independent of the value system of scientists, and, of course, not all scientists share the same value systems.

Thus the framework within which a particular (sub)discipline is formed determines, to a certain extent, through multifarious and complex processes, the subsequent use of the sciences and their social function. This is not to claim that the social function of the sciences is determined in any exact and perfectly predictable manner. Of course, the character of the social function of each (sub)discipline at any given period can only be understood through the study of the specific case. And, if we adopt a view that the sciences are relatively autonomous with respect to various ideological and economic factors, then such a view orients us to study the conditions under which a particular theory, subdiscipline or artefact have been formed. It may be possible to discern the ways value systems are *imprinted on* the processes involved in the emergence of a scientific product – be it a theory, a technological artefact, even a concept. The genesis of the scientific products is formed through processes which historically have acquired a multitude of characteristics, which potentially determine the character (and not, of course, the specific kind) of their future uses, and insisting on the "good or bad use of an otherwise neutral science" seriously undermines the articulation of a social history of science.[3]

The reconsideration of such a dominant view as that of the neutrality of the sciences comprises one of the more challenging historiographical reconceptualisations. Let us, however, first note some aspects that formed the context within which the neutrality of science came to be questioned in the 1960s.

Firstly, there was a dramatic increase in the funding for scientific and technological research, especially for research carried out by public institutions and universities. This was particularly pronounced in the USA, where a large part of the research in the universities was funded by the military, which had established specific laboratories in the universities – inaugurating Big Science.[4] Those who had been actively involved in the Manhattan Project, the codename for the construction of the atomic bomb, became the new leaders in their respective scientific communities. As science was progressively 'militarised' and the military industry was strongly 'nuclearised', it was becoming more and more evident that not only an increasing number of scientists were involved in such research, but a continuously increasing percentage of the funding of scientific research was being provided by the army. But Big Science meant new power relations in the production and management of the sciences. It meant new hierarchies. And, above all, it meant new ways of producing scientific knowledge and meant dramatic changes in established practices, since it heralded the eventual disappearance of the lonely scientist seeking truth.

Secondly, there was the Vietnam War and the role played in it by scientists. The public image of scientists, especially the 'paradigmatic' category of the physicists, came to be marred for the first time in the 1960s: those who in the minds of many citizens had contributed in such a dramatic manner to ending[5] the Second World War, became, during the Vietnam War, the protagonists of the most extreme, degenerate scenarios, inventors of systems which could annihilate thousands of non-combatants. Of course, throughout history, scientists had an active role in war. The construction of the atomic bomb had been hailed as the great success of the collaboration of scientists with the military in what appeared to be an ethically unquestionable aim: the defeat of Hitler's fascism. But the Vietnam War did not have the social legitimation of the Second World War. It was realised that in many universities, unknown to many, a number of scientists were conducting research for the army, and, in many cases, with the consensus of the university administration. The varieties of their activities shocked the scientific community. At the same time, the Pentagon could not use its ultimate weapon and the US Army was defeated in Vietnam. The 'denuclearisation' of the war in Vietnam was followed by the invention of many lethal gadgets and all kinds of new bombs. And, at the same time, there was a strong reactions from all kinds of social groups (including many scientists) that the universities should not be involved in war-related research.

Thirdly, during the 1960s there were discussions and breaks among the political parties of the left, with long-range consequences. This period was preceded by the rather serious blow to the prestige of a number of influential Marxists who, starting in the 1930s, had been developing a critical stand towards the social function of science, but who also happened to be among those who had defended Lysenko.[6] These people had written many works on the history, and, especially, the sociology of science in England and France and had been rather influential in the founding of Unesco (United Nations Educational Scientific and Cultural Organisation) and, particularly, in achieving to include the term 'scientific' in the title of the postwar organisation.[7] Ideological reorientations, breakaway groups, and new radical collectives became the order of the day for the left. Ideological discussions among the different expressions of the political left intensified. Perhaps the only thing they all shared was a critical attitude towards the Soviet Union. Any new ideas aiming to convince society at large, it was claimed, would have to involve a critique of the Soviet Union. Such a massive consensus – and strong opposition as well – around this particular issue was something new for the left. It is within such a framework that there started a discussion about the social function of science, and what was being investigated was not only the role of science in capitalist societies but the neutrality of science itself. The emergence of the New Left, and the criticism of the Soviet Union, undermined another one of the cornerstones of left thinking: the scientific-technological revolution as one of the decisive factors for the foundation and further implementation of socialism. But despite the emphasis the Soviet Union gave to science, there did not appear to be any dramatic improvements in socialism. Might it have been the case that the development of the sciences may not be one of the necessary conditions for socialism? At the same time, the black uprisings and the radicalisation of many scientists subjected science to a serious critique. There were just too many cases of 'science gone wrong': the many instances of black women being labotomised, talk of eugenics, the burst of technologies related to eavesdropping, extreme environmental damage, etc. Inevitably the discussions started revolving around the issue of technological progress, the problems it created and whether these problems could be solved with more technology.[8] Science and its processes came under scrutiny as well. How it is funded, who decides on who investigates what, what is the character of the hierarchies which had formed in the scientific communities, what is the role of the citizens in a democratic society concerning the directions of scientific research etc. Something had gone seriously wrong, and even though many proposed a more democratic control of what one can do with science, some individuals started questioning the foundational issues of the sciences themselves, such as their neutrality. This climate reflected in a number of emblematic books: Barry Commoner's *Science and Survival*, Steven and Hilary Rose's *Science and Society*, Levy Leblond's *Autocritique de sciences*. And, Stanley Kubrick's *Dr. Strangelove* and, his later film, *Clockwork Orange* brought to the fore the (forgotten) public image of the scientist *qua* Frankenstein.

Fourthly, there was the question of the 'autonomous' political and social culture of a number of social groups. The 1960s inaugurated new and thoroughly novel ways of viewing youngsters, blacks, ethnic minorities, and, of course, women. It started becoming apparent that the dominant narrative (be it historical, ideological or anthropological) could not accommodate issues related to the newly emerging articulations of self-identities – with their ensuing needs and their intense social assertiveness. And, as the discussions concerning the social construction of gender were later intensified, the feminist critiques of the sciences contributed greatly to a political as well as epistemological critique of the neutrality of sciences.[9]

Contingency

It is within such a framework that a number of historians and sociologists of science started posing some new questions. If the scientists were not those intermediaries who brought about the unfolding of 'objective' nature, might it be the case that scientific developments were contingent and not unavoidable? Might it be the case that the sciences could have developed differently than the way they did? Might it be the case that the development of the sciences may have been also due to the scientists' decisions which were dictated by external conditions? If, as we mentioned, empirical data and formally correct syllogisms were not by themselves sufficient to persuade the scientists and the public about the new truths concerning nature, should we not orient ourselves towards considering the categories of truth, objectivity and causality as categories where their historicity becomes their dominant characteristic? And, finally, if the multifarious functions of the scientists with respect to the theoretical schemata they choose, the calculational techniques they develop, the experimental setups they devise, and the legitimising strategies they plan, play a decisive role in the development of the sciences, might it be the case that we should rethink the character of scientific practice? Contingency in the development of sciences, the historicity of the categories of truth, objectivity and causality, the processes of certifying valid knowledge and the character of scientific practice, all these together, led us to the need to study the social and class imprints on the structure and content of the sciences. It became obvious that the neutrality of the sciences had to be viewed not as expressing an epistemological characteristic of the sciences, but, rather, as part of their ideologies.

Let us look at a characteristic case: the development of elementary particle physics after the Second World War. The 'success' of the atomic bomb was repeatedly used in order to project the 'peaceful uses' of atomic energy as a panacea for all energy needs. The rhetoric of the supporters of nuclear energy expressed a enviable utopia: those societies which suffered so much as a result of the war could be quickly restored to their former glory, and underdeveloped countries would find what they needed most for their development. The reality concerning nuclear energy was, of course, somewhat different: One could have nuclear plants only if there is an industry for uranium enrichment, and, thus, new materials and much of knowledge for the construction of the plants and their functioning became highly classified knowledge. This industry was at the beginning developed in the USA, and, then, Britain, France and the Soviet Union. Among the arguments in support of nuclear energy, one noted the absence of any mention of the impossibility of solving the problem of nuclear waste, there were no studies to deal with emergencies in the event of an accident, there was no mention of what would happen when the plants aged and what it would mean for many societies to be so dependent on such a concentrated production of energy, and, of course, there was complete silence on the relations concerning the military perspectives of the 'peaceful uses' of nuclear energy.[10] The propaganda for nuclear energy did not only depend on ignoring the problematic aspects of producing energy in such a way, but also on the systematic identification of nuclear energy with progress and the creation of an imaginary future where almost all of the problems related to cities and people would be soluble.[11]

The end result of all this was the very generous funding of research in nuclear physics and elementary particle physics. New hierarchies were created in the academic community, its protagonists being most of those who were involved in the Manhattan Project (and not only those who were involved in Los Alamos, since many more worked for the Manhattan Project than those who worked at Los Alamos). The dominance of those who worked on elementary particles would continue until the beginning of the 1990s, when the plans for the large supercollider in Texas were canceled. The complicated management of large experimental facilities, and the extensive network of theoretical physicists who thought of new theoretical schemata in order to interpret experimental results, and new models which predicted new phenomena whose observation needed even more expensive experimental setups, had its repercussions on the structure of the community of physicists and the academic, economic and political interests groups which were being formed within such a framework. No one, then, can claim that the specific developments in physics were dictated by 'nature of nature': specific political and social choices determined the specific developments and hindered alternatives. And the problem we are discussing here is not, of course, related to how much the truths unveiled about nature through this particular development of physics are socially determined. We are discussing the specific direction of scientific developments, the institutional setup of such developments with the new power relations that came into being, with the development of new experimental techniques and mathematical methods and the way these influenced other branches of physics, with the development of a particular technology and the economic activities around such technologies, and, of course, with the gigantic enterprise for popularisation – the necessary condition for any social legitimation. "We shall look for nothing less than the Truth," said Nobel laureate Leon Lederman in 1988 during congressional hearings concerning the building of the supercollider. The physics of the second half of the twentieth century is not only about the truths we have learned about the objective structure of nature and which we derived through a scientific manner. It is all the above, which also make up the framework within which we study nature, and which determine the content of the views we form about nature as a result of our social and political choices.[12] And it is interesting that the utopia of an everydayness where there will be endless energy to meet all social needs started to fade at a time when another utopia started becoming dominant: molecular biology and bio-engineering promised an endless supply of food, the curing of all diseases, and the genetic encoding of all the mysteries of human characteristics from jealousy to obesity.

If, then, the form and content of the sciences are not dictated by their 'nature' nor by the 'nature of nature', then the sciences could have been different, without, however, being able today to know what their alternative developments would have been. This is not hypothetical history but defines the fields of possibilities in the past. The contingent character of the development of the sciences gives rise to a different social character to the sciences than the one adopted solely on the basis of their social uses. Contingency forces us to concentrate our attention on the understanding of the conditions during the initial stages of scientific production, and make us particularly receptive to the study of the way the imprinting of the values in the initial stage of production determines to a certain extent the future use of the sciences. The acceptance of contingency as an analytical historiographical category could redetermine the whole *problématique* concerning the neutrality of science. Contingency does not mean that at every juncture all possible avenues of development are permissible; it does not mean that 'anything goes'. Contingency can only be understood as something that results from the relative autonomy of the sciences, and it creates each time a framework, a complex of constraints, while at the same time a spectrum of possibilities for the multitude of choices that will determine the development of the sciences.

Conclusion

The 1960s witnessed the systematic questioning of one of the basic tenets of scientism: the neutrality of the sciences. Though Weber made extensive use of the term in the social sciences, there is no study about the genealogy of this 'characteristic' in the physical sciences. It is, however, the case that the neutrality of the sciences has been part and parcel of the legitimation processes of the sciences, and, hence, the origins of such an attribute should be sought in the seventeenth century. It provides the scientists with a kind of freedom from moral constraints and, at the same time, makes all those who are already in positions of power even more powerful, since it provides the justification to decide on the use and application of the physical sciences – this new means of power – without being part of those who create it. Undermining the neutrality of the sciences was realised in the same context as that for the reconceptualisation of a number of the fundamental tenets of many other disciplines, among them history, psychoanalysis, linguistics and, of course, anthropology. In the case of the sciences, four important developments became the constitutive elements of the framework that favoured the questioning of the neutrality of the sciences: The establishment of Big Science, the close collaboration of the scientists with the army during the Vietnam War, the breaks in the left which precipitated the search for an alternative view on the sciences, free from the positivist norms, shared by the most advanced institutions of capitalism and those which were the bulwarks of orthodox Soviet dogma and the origins of feminist critiques. But despite the advancement of a number of convincing arguments against the neutrality of the sciences, there is still a serious lack of specific studies to investigate the ways values and ideologies are imprinted on the sciences during the various stages of their development.

NOTES

1 Kostas Gavroglu, *Το Παρελθόν των Επιστημών ως Ιστορία (The Past of the Sciences as History)*, Irakleio: Crete UP, 2005.

2 Barry Barnes, *Scientific Knowledge and Sociological Theory*, London: Routledge and Kegan Paul, 1974; Barry Barnes, David Bloor, John Henry (eds), *Scientific Knowledge: A Sociological Analysis*, Chicago: Chicago UP, 1996.

3 Kostas Gavroglu, John Stachel, Marx Wartofsky (eds), *Science, Politics and Social Practice*, Dordrecht: Kluwer, 1995.

4 Angelo Baracca, "'Big Science' vs. 'Little Science' in Post War Physics", in Michelangelo De Maria, Mario Grilli and Fabio Sebastiani (eds), *The Restructuring of Physical Sciences in Europe and the United States, 1945–1960*, Teaneck, NJ: World Scientific Publishers, 1989.

5 Relatively recent research has dramatically altered this common view and has brought to surface the role played in the decision to drop the two bombs in August 1945 to stop the Soviet Union from entering the Pacific front and to put it in a defensive position in the negotiations of the post-war 'arrangements'. See, for example, Gar Alperovitz, Robert Messer, and Barton Bernstein, "Marshall, Truman and the Decision to Drop the Bomb", *International Security* 16 (1992): 204–221.

6 Gary Werskey, *The Visible College: The Collective Biography of British Scientific Socialists of the 1930s*, New York: Holt, Rinehart & Winston, 1978. Robert Young, "Getting Started on Lynsekoism", *Radical Science Journal* 6–7 (1978): 81–105.

7 S. Lilley, *Report to the Commission for the History of the Social Relations of Science* (26 June 1947). Bernal Papers, Cambridge University library.

8 William McGucken, *Scientists, Society, and State: The Social Relations of Science Movement in Great Britain, 1931–1947*, Columbus: Ohio State UP, 1984; Jerome Ravetz, *Scientific Knowledge and its Social Problems*, Oxford: Clarendon, 1971; Nils Roll-Hansen, *The Lysenko Effect: the Politics of Science*, New York: Humanity, 2005; Hilary Rose and Steven Rose, *The Radicalization of Science: Ideology of/in the Natural Sciences*, London: Macmillan, 1976.

9 See, for example, Evelyn Fox Keller, *Reflections on Gender and Science*, New Haven, Conn: Yale UP, 1985. Helen Longino, *Science as Social Knowledge: Values and Objectivity in Scientific Inquiry*, Princeton, NJ: Princeton UP, 1990.

10 David Edgerton, "British Scientific Intellectuals and the Relations of Science, Technology, and War", in Paul Forman and José M. Sánchez-Ron (eds), *National Military Establishments and the Advancement of Science and Technology: Studies in 20th-Century History*, Dordrecht: Kluwer, 1986.

11 For the different social and economic factors for the establishment of 'peaceful nuclear energy', see Baracca, "'Big Science'".

12 Ian Hacking, "Weapons Research and the Form of Scientific Knowledge", *Canadian Journal of Philosophy* Suppl 12 (1986): 237–60.