Brazilian Journal of Political Economy, vol. 15, nº 2 (58), pp. 258-269, April-June/1995

Knowledge in the wonderland of the market of ideas: deconstructing the Economics of Science of Michael Polanyi

Conhecimento no país das maravilhas do mercado de ideias: desconstruindo a Economia da Ciência de Michael Polanyi

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RESUMO: Este ensaio desenvolve uma interpretação desconstrutivista de alguns aspectos da economia da ciência formulada por Michael Polanyi. Em particular, o ensaio analisa em que medida as posições libertárias de Polanyi podem ser vistas como consistentes do ponto de vista lógico-desconstrutivo. Muito embora o ensaio destaque a existência de inconsistências no discurso de Polanyi, sua contribuição ao debate sobre as especificidades inerentes .ao processo de conhecimento é igualmente ressaltada. Colocando em outros termos, muito embora Polanyi tenha exagerado em sua crença panglossiana na eficiência da coordenação espontânea de atividades científicas promovida pelo mercado, é forçoso reconhecer que suas posições acerca das características do conhecimento devem ser encaradas como um importante antídoto contra ingênuas propostas de centralização das atividades científicas sob uma única autoridade.
PALAVRAS-CHAVE: Metodologia da economia; metodologia da ciência; filosofia da ciência; história do pensamento econômico; Michael Polanyi.

ABSTRACT: This essay develops a deconstructive interpretation of some aspects of the economics of science formulated by Michael Polanyi. In particular, the essay analyzes the extent to which Polanyi's libertarian positions can be seen as consistent from a logical-deconstructive point of view. Although the essay highlights the existence of inconsistencies in Polanyi's discourse, his contribution to the debate about the specificities inherent to the knowledge process is equally emphasized. In other words, even though Polanyi has exaggerated his Panglossian belief in the efficiency of the spontaneous coordination of scientific activities promoted by the market, it is necessary to recognize that his positions regarding the characteristics of knowledge must be seen as an important antidote against naive proposals for centralization of scientific activities under a single authority.
KEYWORDS: Economic methodology; scientific methodology; philosophy of science; history of economic thought; Michael Polanyi.
JEL Classification: B20; B31; B41.

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** I am grateful to Philip Mirowski and Luiz Carlos Bresser-Pereira for their useful comments and suggestions. Needless to say, the usual disclaimer applies.

 http://dx.doi.org/10.1590/0101-31571995-0807

"It is demonstrable, said Master Pangloss, that things cannot be otherwise than they are; for as all things have been created for some end, they must necessarily becreated for the best end (...) it is not enough therefore to say that everything is right, we should say everything is in

(Voltaire, 1759).

This essay is intended to set forth a tentative deconstructivist interpretation of some aspects of Michael Polanyi's assumedly libertarian economics of science. To insist on the fact that mine is an inescapably incomplete and undecidable interpretation would be redundant, for any deconstructivist exercise is itself self-deconstructed.[1] On the other hand, though Polanyi's works on philosophy and sociology of science cover a wide range of topics, I should mention that I intend to discuss mainly his position regarding the need for freedom and detachment from ideology for the growth of knowledge. To that end, I focus mainly upon the economic and political interpretation of the Republic of Science set forth in Polanyi (1962), though some occasional references are made to the broader philosophical and sociological context within which such interpretation is carried out.[2]

Indeed, it was the very problem of the organization of science which moved Polanyi, at least in the first instance, to embark on philosophical reflection. Polanyi began his professional career as physical chemist in 1917 and continued to work as an exact scientist until 1948, when he definitely retired from practical scientific endeavors to write about how the scientific community actually thinks and works.[3] To put it precisely, Polanyi retired from actual scientific practices in the late 1940s with a view to intervene in a vigorous debate regarding scientific planning that was taking place in Europe, particularly in Britain. Polanyi was then concerned to justify to himself and others his strong belief that centralization is incompatible with the steady growth of knowledge; in his view, a libertarian form of organization, operating through consensus rather than directly through planning, is absolutely

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[1] The term deconstruction is used throughout this essay in its Derridean sense, thus entailing that the univocal text is undecidable, while the decidable text is not univocal. To put it another way, a deconstructive interpretation seeks the moments in which the text differs from itself, thus transgressing its own system of values and becoming undecidable in terms of its apparent system of meanings. See Derrida (1974).

[2] Polanyi 's controversial piece was originally a lecture delivered at oosevelt University, Chicago, on January 11, 1962, a slightly modified version being published in the same year in *Minerva*. Throughout this essay, all references to Polanyi 's piece are from its original version.

[3] I intend to avoid *whiggish* moves in this essay, but it is worth recalling that some Mertonian authors (e.g., Ben-David, 1970, p. 418) argue that it was Polanyi who first elaborated the to-become Kuhnian notion of 'scientific community' and used it as a revealing description of the way scientists enforce strict discipline, amid a great deal of individual freedom, through training, referring of publications and purely informal sanctions of approval and disapproval.

necessary to science due to what he considered the unspecificability inherent of the process of discovery, of understanding, and even of verification and refutation. [4]

Before proceeding with the main theme of this essay, it is worth mentioning that even during his career as physical chemist Polanyi had already made several analytical incursions into more socially-oriented issues. In addition to his criticisms of the Marxian sociology of science that I discuss below (e.g., Polanyi, 1941, 1945), his book on Soviet Russia (Polanyi, 1935) had already granted him some academic recognition in intellectual circles. However paradoxical it might seem, Polanyi had even contributed to the diffusion of the Keynesian revolution (Polanyi, 1948), the main purpose of his intervention being to convert the Keynesian theory into a matter of common sense; in his view, Keynesian economics had to be made much simpler and clearer before it could become the common property of all thinking citizens. For Polanyi, Keynes' discovery in 1936 of the mechanism by which the level of output and employment is determined revealed that conditions of overproduction are actually quite common in the course of economic development. But it showed, at the same time, in Polanyi's view, that such an evil does not represent a necessity inherent in capitalism, but it is due rather to an incidental defect or imperfection attached to the system. Hence, Polanyi did not hesitate in embracing an interpretation of Keynes' economics of employment along neoclassical-synthesis lines; indeed, J. Hicks is explicitly referred to in the preface as one of Polanyi's friends among economists whose help Polanyi had enjoyed in the course of writing his contribution to the Keynesian literature.[5] By explicitly considering the Keynesian theory a veritable egg of Columbus, Polanyi argued that all that needed to be done to eliminate mass unemployment was to fill the ever-threatening deflationary gap by means of a budget deficit. Moreover, Polanyi intended to show that this could be done in a neutral form, that is, in a way requiring no materially significant economic or social action to accompany it; in his view, the process of maintaining aggregate effective demand could be carried out without leading to serious distortions in relative prices, to the wastage of production resources through improvised and ill-conceived public works and to the gradual extension of the government sphere of influence. A convinced Keynesian and at the same time a staunch liberal who believed in the superiority of the market system, Polanyi firmly argued that

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[4] Polanyi's unspecificability thesis is referred to in the transaction costs branch of the so-called New Institutional Economics as clearly corroborating the view that assets can be idiosyncratic (e.g., Williamson, 1985, p. 53). Refining Coase's (1937) classic argument, Williamson's conclusion is that the ownership structure of the firm or its governance system can face the so-called asset specificity problem better than the market, this being a relative advantage which explains the very existence of the firm. As it goes beyond the scope of this essay a detailed analysis of such an incorporation of Polanyi's thesis, I would only venture that Williamson's treatment of knowledge as an asset like any other is a natural move for an approach that treat institutions as nothing but a specific kind of technical relation! The best general reader on the New Institutional Economics is Putterman (1986), interesting contributions being also contained in Langlois (1986). A critical outline of this literature is provided in Lima (1994).

[5] A critical survey of the major competing interpretations of Keynes' economics of employment can be found in Lima (1992).

correct Keynesian policies could regenerate free competition and re-establish capitalism on renewed foundations.

It was in the 1930s that a massive critique was first put forward of the value of pure science and its associated ideology, a value and ideology that were widely and strongly held among working scientists at that time. The critique came primarily from a small group of eminent British practical scientists who came to be known as the scientific humanists, which included J. Bernal, F. Soddy, L. Hogben, H. Levy, J. Needham, among others (Barber, 1990, p. 4). They were interested in both sides of the social aspects of science, namely, the effect of society on science and the social function of science. Moreover, they were horrified by the social disaster of the 1930s depression and by what they thought was also an accompanying general retreat from reason and from its chief embodiment, namely, science. ln their view, science was a guide to and a means for social reform. They were anxious to have science adequately supported in the universities and properly used in industry, public health, and education as well as in the defense of Britain against the Nazi onslaught (Bunge, 1991, p. 529).

The scientific humanists were influenced not only by Marxian theory but also by what they thought was the large and beneficial use of science in Marxian Russia at that time. More precisely, they admired how the Russians were planning science itself and also using it in planning social development in general, as well as in selected programs in industry and agriculture. Marxism had its most direct impact on the scientific humanists through the papers read by the Russian delegation to the Second International Congress on the History of Science that was held in London in 1931. From the day of their arrival to the day of their departure, the Russian delegation created a great frisson, not least of all for their call for a new theory of science based on Marxian materialism and their promises for science under the new Soviet nation. The reading of the papers had a shock effect on their auditors for their explicit assumptions about the interactions of science and society. In truth, their papers were to shape the thought of an entire generation of British leftist intellectuals, prominent among them those who would come to constitute the scientific humanists. The paper presented by B. Hessen was that which caused the greatest sensation among the auditors. In "The Social and Economic Roots of Newton's Principia", Hessen put forward what at that time was a radical thesis, namely, that the formation of ideas has to be explained by reference to material practice. Since for the scientists of the day, and for some thereafter, Newton' s Principia was the divine elixir of pure science, to argue its social origins, as Hessen did so well, was a most striking indication of the need for a socially involved and socially useful science. The thesis that aroused controversy was not that science is useful, but that the usefulness of science is important in its origins. He argued that Newton's Principia was not merely an abstract treatise on mathematical physics, but rather a response to concrete technical and economic problems facing seventeenth century British capitalism. ln a word, Hessen's piece held that Newton's work was a child of his class and time, so that his scientific work was a deliberate attempt to solve technological problems posed by the rise of capitalism.

Inspired by Hessen's sociological analysis of Newton's Principia, the subsequent writings of the scientific humanists, especially those of Bernal, came to have a large influence both on other natural scientists and on social scientists. To put it precisely, they challenged the absolutization of pure science by arguing that political values and interests usually play an important role on the process of generation and dissemination of knowledge. It should be mentioned that their influence was so large that by the late of 1930s a strong scientific optimism, combined with a sense of the possibility of planning science – the so-called Bernalism – had come to represent the orthodox line not only in the British Communist Party but also among a wide range of liberals (Proctor, 1991, p. 218). But such a euphoria was not without dissenting voices. More precisely, the Bernalist challenge to the ideal of a pure science did not itself go unchallenged by those offended by some aspects of the former. A conservative counterattack occurred in the late 1930s and early 1940s to face what was considered a unified threat of Soviet communism, Nazi tyranny, and the British planned science movement. To put it directly, in 1941 a group of conservative scientists and philosophers, led by Polanyi, and which eventually included Popper, Hayek, von Mises, among others, formed the Committee for Freedom in Science in explicit opposition to the supposed extreme views on planning of Beal and his fellow scientific humanists (Barber, 1990, p. 5). Having gone through such historical intermission, the stage is set for the reappearance of the main protagonist of this deconstructive narrative.

I now tum to a textual deconstructivist analysis of Polanyi's controversial paper on the political and economic theory of what he referred to as the Republic of Science (Polanyi, 1962). Given the purpose of this essay, I should mention that I focus primarily upon the economic dimension of Polanyi 's argument; but to the extent that the narrative that follows is carried out in a deconstructivist fashion, the political dimension of Polanyi's discourse cannot be put completely aside, so that at least brief references to the latter are in order. The central thesis of Polanyi's paper is that the community of scientists is organized in a way that resembles certain features of a body politic and works according to economic principles similar to those by which the production of material goods is regulated (p. 5). In this context, it is worth noting that Polanyi believed to be recasting the subject from a novel point of view which could both profit from and have a lesson for political and economic theory. By conceiving the Republic of Science as being organized according to economic principles, Polanyi considered himself to be making a relevant contribution not only to sociology of science, but also to political and economic theory themselves. For it is in the free cooperation of independent scientists that one can find, Polanyi argued, a highly simplified model of a free society, which presents in isolation certain basic features of it that are more difficulty to identify within the comprehensive functions of a national body. For Polanyi, an aspect that should be clearly emphasized is that scientists, by freely making their own choice of problems and pursuing them in the light of their own personal judgement, are in fact cooperating as members of a closely knit organization. The principle of their natural coordination consists, in turn, in the adjustment of the efforts of each to the hitherto results of the others (p. 6). Polanyi's argument seems to run as follows. To the extent that individual scientific efforts usually build on the previous contributions of other scientists, they are closely linked to the broader scientific network comprising the existing aggregate stock of scientific knowledge. Somewhat idealistically, Polanyi seemed to believe that scientific knowledge evolves in a cumulative way: an individual scientist simply picks some results achieved by other scientists and makes his marginal contribution to the stock of knowledge; science is therefore a naturally progressive and cumulative enterprise.

Not surprisingly, Polanyi made use of a Smithian-type logical argument to support his principle of spontaneous coordination of independent initiatives. He illustrated its operation with the following naturalist analogy. Imagine that we are given the pieces of a very large jigsaw puzzle and suppose that for some reason it is important that our giant puzzle be put together in the shortest possible time. We would naturally try to speed this up by engaging a number of helpers; the question is in what manner these could be best employed. It is easy to see that the method of sharing the pieces out equally among the helpers and let each of them work on his lot separately is totally ineffectual, for few of the pieces allocated to one particular assistant would be found to fit together. It is also easy to see that providing duplicates of all the pieces to each helper separately, and somehow bringing together their several results, would do only a little better. The only way the assistants can effectively cooperate is to let them work on putting the puzzle together in sight of the others, so that every time a piece of it is fitted in by one helper, all the others will immediately watch out for the next step that becomes possible in consequence. Under this system, each helper will act on his own initiative, by responding to the latest achievements of the others, and the completion of their joint task will be greatly accelerated. For Polanyi, "[s]uch self-coordination of independent initiatives leads to a joint result which is unpremeditated by any of those who bring it about. Their coordination is guided as by 'an invisible hand' towards the joint discovery of a hidden system of things. Since its end-result is unknown, this kind of cooperation can only advance in stages, and the total performance will be the best possible if each consecutive step is decided upon by someone most competent to do so" (p. 7).

Essentially the same is true, Polanyi argued, for the advancement of science by independent initiatives adjusting themselves consecutively to the results achieved by all the others. So long as each scientist keeps making the best contribution of which he is capable, it is reasonable to argue that the pursuit of science by independent self-coordinated initiatives is likely to assure the most efficient possible organization of scientific progress. Moreover, he sustained that any authority which would undertake to direct the work of the scientist centrally would bring the progress of science virtually to a standstill. For Polanyi, such argument regarding the highest possible co-ordination of individual scientific efforts by a process of self-co-ordination may well recall the self-coordination achieved by producers and consumers operating in a free market. Indeed, he admitted that it was with this in mind that he referred to the 'invisible hand' guiding the coordination of independent initiatives to a maximum advancement of science, just as Adam Smith invoked the invisible hand to describe the achievement of greatest joint material satisfaction when independent producers and consumers are guided by the prices of goods in a market.[6]

It is worth of mention that Polanyi not only built on Smith's argument, but also explicitly subsumed it as a special case of his own. By making explicit use of the rhetorical device of subsuming someone's argument as a special case (Arida, 1991), Polanyi argued that the coordinating functions of the market are but a special case of his notion of coordination by mutual adjustment. In the case of science, adjustment takes place by taking note of the published results of other scientists, while in the case of the market mutual adjustment is mediated by a system of prices broadcasting current exchange relations, which make supply meet demand (p. 9). Moreover, Polanyi argued that the complex decisions of a scientist choosing a problem and pursuing it to the exclusion of others may be said to have a clearly economic character. Now making use of a Robbins-type argument, he argued that those decisions are designed to produce the highest possible results using a limited stock of intellectual and material resources. As Polanyi had an essentially cumulativist notion of the progress of science, I would argue that a contradiction seems to emerge here, namely, how one can reconcile his notion that science evolves in a cumulative way with the notion that scientific decisions are designed to maximize results subject to the constraints imposed by the very scarcity of intellectual resources. ln other words, as long as to conceive science as evolving in a cumulative way implies the relaxing of those constraints through time, in the sense that they would progressively bind less and less the maximizing behaviour of the scientific community, one should expect science to start stagnating at a given future point intime. Even though for a physicist of his reputation the formal intricacies of the Hamiltonian were certainly well-known, Polanyi did not deal with this unpleasant dynamic implication of his adjustment principle. Borrowing his own analogy, one might well suggest that it is a jigsaw puzzle whether Polanyi did not do it precisely because he somehow anticipated such unpleasant implication of using the laissez-faire metaphor.

Moreover, Polanyi's economics of science is in fact an aspect of a larger sociological conception of the scientific activity. While the system of prices ruling the market not only transmits information in the light of which economic agents can mutually adjust their actions, the scientist responding to the intellectual situation created by the published results of other scientists is argued to be motivated by current professional standards. For Polanyi, a scientist assesses the depth of a problem and the importance of its prospective solution primarily by the standards of scientific merit accepted by the scientific community, even though his own work

[6] Though in the paper in question Polanyi does not mention any liberal philosopher other than Adam Smith to support his libertarian view, Polanyi (1960) relies on Hayek (1952), his fellow in the Committee for Freedom in Science, to support his conclusion that in a scientific society submitted to scientific rulers the liberty of conscience would disappear and the progress of science would be virtually brought to a standstill.

may demand these standards to be somewhat modified. ln my words, scientists maximize the production of scientific knowledge subject to the prevailing standards of scientific merit, or as Thomas Kuhn (1962) was to contemporaneously put forth, scientists simply practice normal science. For Polanyi, the criteria on which the merit of a proposed contribution to science depends are the following: its degree of plausibility; its scientific value as measured by accuracy, systematic importance, and intrinsic interest of its subject-matter; and its originality.

I would suggest that another contradiction in Polanyi's discourse seems to arise here, namely, how can one reconcile his notion that any authority which would undertake to direct the work of the scientist centrally would bring the progress of science virtually to a standstill with the notion that scientific decisions are ultimately governed by the standards of scientific merit established by the scientific community. I would argue that these scientific standards do not emanate from Nature at all, but rather emerge from a complex and permanent process of social negotiation: a process among actual scientists and those who are physically outside the scientific arena but whose interests play a decisive role in the shaping of what is accepted as scientifically meritory, as Proctor's (1991) political analysis of knowledge consistently demonstrated (Lima, 1993b). In fact, one might well suggest that since any prevailing standard of scientific merit is something socially manufactured, Polanyi 's belief on the existence of an autonomous and invisible scientific authority in charge of safeguarding the pursuit of science is itself a social projection. Moreover, one cannot but wonder what would be the problem, from Polanyi's perspective, with a committee comprised of representatives of the consensual standards of scientific merit and virtue being in charge of centrally directing the work of the scientific community; in other words, what is so appealing about laissez-faire that renders it the most adequate device to exercise the authority of scientific standards? Even though Polanyi did not raise these questions in his apologetic narrative, I will try to construct a possible answer to it through a deconstruction of the sequence of his argument.

Polanyi argued that while the criteria of plausibility and of scientific value tend to enforce conformity, the value attached to originality encourages dissent. Making use of an essentially functionalist-type reasoning, he argued that this internal tension is essential in guiding and motivating scientific work: the professional standards of science must impose a framework of discipline and at the same time encourage rebellion against it. To put it another way, they must demand that an investigation should largely conform to the currently predominant beliefs about the nature of things, while allowing that in order to be original it may to some extent go against these. Thus, some sort of competition for originality, I would argue, is supposed to guarantee that the scientific innovations required to capture the truth out there take place. Not surprisingly, Polanyi put forth an essentially idealist and naturalist notion of science by arguing that this dual function of professional standards in science is nothing but the logical outcome of the belief that scientific truth is an aspect of reality, and that the orthodoxy of science is taught as a guide that should enable the community eventually to make his own natural contacts with this reality. In my words, science is naively conceived as being nothing but the resonance box of the sacred Book of Nature. The authority of scientific standards is therefore exercised for the very purpose of providing those guided by it with independent grounds for opposing it. The capacity to renew itself by evoking and assimilating opposition to itself appears to be logically inherent in the sources of the authority wielded by scientific orthodoxy (p. 13). I would argue that Polanyi did not attribute anything intrinsically distinctive to this capacity to renew itself that renders it specific and exclusive to a scientific system guided by laissez-faire. To phrase it another way, one could well argue that this capacity to renew itself is logically inherent in the sources of the authority wielded by scientific orthodoxy in general, be it some natural result of Polanyi's mutual adjustment principle or something directly emanated from a Bernalist scientific committee in charge of planning science. In any case, I keep following Polanyi's naturalist and libertarian reasoning, particularly because at this point, he raised a quite interesting question, namely, who is it, exactly, who exercises the authority of this orthodoxy?

For Polanyi, the so-called scientific opinion is the agent of scientific orthodoxy. But to the extent that no single scientist has a sound understanding of more than a tiny fraction of the total domain of science, a question that arises here is how can an aggregate of such specialists possibly form a joint opinion; in other words, one might well wonder how can they possibly exercise jointly the delicate function of imposing a current scientific view about the nature of things, and the current scientific valuation of proposed contributions, while encouraging an originality which would modify this orthodoxy. In searching for a plausible answer to this question, Polanyi suggested another organizational principle, now one based upon the fact that, while scientists can admittedly exercise competent judgement only over a small part of science, they can usually judge an area adjoining their own special studies that is broad enough to include some fields on which other scientists have specialized. In my view, Polanyi seemed to have the analogy with the jigsaw puzzle in mind again. Polanyi argued that we have a considerable degree of overlapping between the areas over which a scientist can exercise a sound critical judgement. Since each scientist who is a member of a group of overlapping competences will also be a member of other groups of the same kind, the whole of science will be covered by a chains and networks of overlapping neighborhoods. Each link in these chains and networks will establish agreement between the valuations made by scientists overlooking the same overlapping fields, and so, from one overlapping neighborhood to the other, agreement would be naturally established on the valuation of scientific merit throughout all domains of science. For Polanyi, this network is the seat of scientific opinion, in the sense that scientific opinion is an opinion not held by any single human mind, but rather one which, split into thousands of fragments, is held by a multitude of individuals, each of whom endorses the others' opinion at second hand, by relying on the consensual chains which link her to all the others through a sequence of overlapping neighborhoods. At this stage, one cannot but guess that any similarity between such a metaphor and the overlapping generations approach then in process of becoming fashionable in mainstream macroeconomics may not be a mere coincidence!

In any case, I would argue that there is nothing intrinsically specific about this network of overlapping neighborhoods that renders it specific and exclusive to a system of science based on laissez-faire, in the sense that such a process could well be used for choosing the members of the Bernalist committee mentioned above. I would venture that Polanyi's plea for a laissez-faire form of organization of the Republic of Science is essentially non sequitur, for there is nothing intrinsically distinctive and specific about his logical reasoning that renders a libertarian organization of scientific activities either exclusive or necessarily Pareto-superior; as in Dr. Pangloss's discourse, things are simply supposed to be in the best state they could possibly be.

Given Polanyi's view that his principle of the self-coordination on independent scientific initiatives suggests a generalization of the natural principles governing the market, an outline of some contributions by his brother, Karl, who also achieved world fame, is in order. Even though Karl Polanyi's influence has been greatest among anthropologists and economic historians, it is fair to argue that he has also made important contributions to economic theory. Indeed, Karl's writings addressed fundamental issues for economists, disputing both the basic concepts and the typical analytical conclusions of neoclassical analysis. To put it directly, Karl challenged both the subjectivist and the utilitarian foundations of economic orthodoxy, asserting a different type of value theory and welfare analysis; in his view, the market is not a natural phenomenon, reflecting some of the supposed fundamentals of human nature. Moreover, it is equally incorrect to see the modem market as the inevitable result of growth from 'small beginnings', thus heralded by the trade and markets of the past. Against this, Karl consistently argued that the modern market is rather the result of the contingent combination of varied and independent cultural and ideological elements, stemming from a diverse and changing institutional background.

Unlike his brother's implicit view, Karl argued that the modem market system did not emerge spontaneously but was in many respects the result of political action and even conscious design. There was nothing natural about laissez-faire; free markets could never have come into being merely by allowing things to take their course. As Karl phrased it, laissez-faire itself was enforced by the state, for "to the typical utilitarian [it] was not a method to achieve a thing, it was the thing to be achieved" (Polanyi, 1944, p. 140). Clearly enough, this conception of the market, as something contingent rather than natural, as something intended rather spontaneous, is fundamentally incompatible with the treatment of the market in orthodox economic theory. In the case of the latter a market is presumed to exist in the interstices of human existence, appearing whenever goods or services are transferred from one agent to another. For Karl Polanyi, on the contrary, the market is an historically specific social institution, created, like other institutions, in part through conscious design. Hence, it is reasonable to argue that Karl Polanyi provided a forceful critique of neo-liberalism, his ideas providing an important counter to those of Hayek, Friedman and others of the so-called New Right. But his position as an insightful and inspiring thinker notwithstanding, Karl Polanyi still remains unjustifiably – but comprehensibly – neglected by economists. In fact, Michael's naive economics of science demonstrates that Karl was neglected even at home!

This essay was devoted to a tentative deconstructive interpretation of some aspects of the economics of science developed by Michael Polanyi, special emphasis being given to his notion that a libertarian form of organization of the structure of production of knowledge is absolutely necessary for scientific progress to take place. My purpose, though, was not to take direct part in the complex debate market vs. planning in science; rather, this essay intended to evaluate to what extent, if any, some aspects of Polanyi's apologetic discourse can be seen as providing a consistent and coherent support for a libertarian form of scientific organization. Even though this essay concluded by the existence of several contradictions and shortcomings in Polanyi's discourse, one should also recognize that his is an analysis that correctly pointed out several problems associated with a centrally planned scientific community. To put it another way, while Polanyi did go too far with his Panglossian view of the efficiency of some kind of spontaneous order, his position regarding the characteristics of knowledge can be viewed as acting as counters to those who naively believe that it is possible to reconstruct the mode of production of scientific knowledge according to some comprehensive blueprint or plan.

REFERENCES

ARIDA, P. "A história do pensamento econômico como teoria e retórica". *In* Rego, J. M. (*org.) Revisão da Crise: Metodologia e Retórica na História do Pensamento Econômico*. São Paulo, Bienal, 1991.

BARBER, B. *Social Studies of Science*. New Jersey, Transaction Publishers, 1990.

BEN-DAVID, J. "Theoretical perspectives in the sociology of science, 1920-1970". In Ben-David, J. (ed.). *Scientific Growth,* 1991. Berkeley, University of California Press, 1970.

BUNGE, M. "A critical examination of the new sociology of science", Part I. *Philosophy of the Social Sciences,* vol. 21, 1991.

COASE, R. "The nature of the firm". *Economica* 4, 1937.

DERRIDA, J., 1974. *Of Grammatology.* Baltimore, The Johns Hopkins University Press. First edition in French, 1967.

DIESING, P. *How Does Social Science Work?* Pittsburgh, University of Pittsburgh Press, 1991.

HAYEK, F. *The Counter-Revolution of Science*. Illinois, The Free Press, 1952.

KNORR-CETINA, K. *The Manufacture of Knowledge*. Oxford, Pergamon Press, 1981.

KUHN, T. *The Structure of Scientific Revolutions*. Chicago, University of Chicago Press, 1962.

LANGLOIS, R. (ed). *Economics as a Process: Essays in the New Institutional Economics*. Cambridge, Cambridge University Press, 1986.

LIMA, G. *Em Busca do Tempo Perdido: a Recuperação Pos-keynesiana da Economia do Emprego de Keynes*. Rio de Janeiro, BNDES, 1992.

LIMA, G. "Essays on economic methodology and sociology of knowledge". FGV/SP, Discussion Paper, forthcoming, 1993a.

LIMA, G. "Fragments of a trans epistemic discourse: political economy of scientific knowledge and sociology of economic knowledge". FGV/SP, Discussion Paper, forthcoming, 1993b.

LIMA, G. “Efficiency, authoritarian power and cooperation in a competitive market economy”. University of Notre Dame, mimeo, 1994.
MIROWSKI, P. *Against Mechanism*. Nova Jersey, Rowman & Littlefield, 1988.
MIROWSKI, P. *More Heat than Light*. Cambridge, Cambridge University Press, 1989.
POLANYI, K. *The Great Transformation*. Nova York, Rinehart, 1944.
POLANYI, M. *U.S.S.R. Economics*. Manchester, Manchester University Press, 1935.
POLANYI, M. “The growth of thought in society”. *Economica,* Novembro/ 1941.
POLANYI, M. “The planning of science”. *Political Quarterly* 16, 1945.
POLANYI, M. *Full Employment and Free Trade*. Cambridge, Cambridge University Press, 1948.
POLANYI, M. *The Logic of Liberty.* Londres, Routledge & Kegan Paul, 1951.
POLANYI, M. “Beyond nihilism”. *In* M. Grene, (ed.). *Knowing and Being: Essays by Michael Polanyi.* Chicago, The University of Chicago Press, 1969.
POLANYI, M. *The Republic of Science: its Political and Economic Theory.* Chicago, Roosevelt University, 1962. Reprinted with minor abridgements in M. Grene (ed.). *Knowing and Being: Essays by Michael Polanyi,* The University of Chicago Press, 1969.
PROCTOR, R. *“Value-free Science?”* Cambridge, Harvard University Press, 1991.
PUTTERMAN, L. (ed.) *The economic Nature of the Firm: a Reader.* Cambridge, Cambridge University Press, 1986.
WILLIAMSON, O. *The Economic Institutions of Capitalism*. Nova York, The Free Press, 1985.