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The old economics of science, the nonlinear model of innovation, and the economics of patents

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Résumé : Le modèle linéaire de l'innovation, qui stipule l'existence d'une relation unidirectionnelle et non simultanée entre les deux sphères distinctes de la science et de la technologie, est encore aujourd'hui débattu. Les objectifs de cet article sont de montrer que les économistes des sciences des années cinquante et soixante ne développèrent pas le modèle linéaire, d'examiner les fondements épistémologiques de leur représentation des liens entre science et technologie, et d'en analyser les conséquences pour l'économie des droits de propriété intellectuelle. Nous étudions tout d'abord les contributions fondatrices en économie des sciences de Richard Nelson et montrons que celui développe une représentation interactionniste des liens entre science et technologie. Nous expliquons ensuite le développement par Nelson d'un modèle interactionniste par son adoption d'une épistémologie évolutionniste (se traduisant par une représentation circulaire des relations causales et par un accent mis sur la continuité historique) provenant de l'influence de la théorie évolutionniste sur ses travaux réalisés à la RAND Corporation et au Carnegie Institute of Technology. Nous étudions ensuite la généalogie du concept de « paradigme technologique » développé par Giovanni Dosi dans les années quatre-vingt et montrons qu'elle conduit également aux contributions fondatrices en économie des sciences, à une critique du modèle linéaire, ainsi qu'à une épistémologie évolutionniste. Nous analysons enfin la relation entre le modèle interactionniste des liens entre science et technologie, l'épistémologie évolutionniste, et la critique des droits de propriété intellectuelle sur la science et la technologie.

Abstract: The linear model of innovation, which assumes a unidirectional and non simultaneous relationship between the two realms of science and technology, is still a debated framework. The aims of this article are to show, in contrast with the received view, that economists of science in the fifties and in the sixties did not develop or support the linear model; to examine the epistemological foundations of their criticism; and to examine its consequences for the economics of patents. We first focus on Richard Nelson's seminal articles on the economics of science. We show that Nelson actually developed an interactionist representation of the links between science and technology. We then explain Nelson's development of an interactionist model by his adoption of an evolutionary epistemology (represented by a defense of circular causal relationships and an emphasis on historical continuity) which stems from the influence of evolutionary theory on his research carried out at that time at the RAND Corporation and at the Carnegie Institute of Technology. We then examine the genealogy of Giovanni Dosi's notion of "technological paradigm" and show its links with the contributions in the old economics of science, a criticism of the linear model of innovation, and an evolutionary epistemology. We finally study the links between an evolutionary epistemology, an interactionist representation of the relationships between science and technology, and the criticism of patents on science and technology.

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Abstract

The linear model of innovation, which assumes a unidirectional and non simultaneous relationship between the two realms of science and technology, is still a debated framework. The aims of this article are to show, in contrast with the received view, that economists of science in the fifties and in the sixties did not develop or support the linear model; to examine the epistemological foundations of their criticism; and to examine its consequences for the economics of patents. We first focus on Richard Nelson's seminal articles on the economics of science. We show that Nelson actually developed an interactionist representation of the links between science and technology. We then explain Nelson's development of an interactionist model by his adoption of an evolutionary epistemology (represented by a defense of circular causal relationships and an emphasis on historical continuity) which stems from the influence of evolutionary theory on his research carried out at that time at the RAND Corporation and at the Carnegie Institute of Technology. We then examine the genealogy of Giovanni Dosi's notion of "technological paradigm" and show its links with the contributions in the old economics of science, a criticism of the linear model of innovation, and an evolutionary epistemology. We finally study the links between an evolutionary epistemology, an interactionist representation of the relationships between science and technology, and the criticism of patents on science and technology.

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Résumé

Le modèle linéaire de l'innovation, qui stipule l'existence d'une relation unidirectionnelle et non simultanée entre les deux sphères distinctes de la science et de la technologie, est encore aujourd'hui débattu. Les objectifs de cet article sont de montrer que les économistes des sciences des années cinquante et soixante ne développèrent pas le modèle linéaire, d'examiner les fondements épistémologiques de leur représentation des liens entre science et technologie, et d'en analyser les conséquences pour l'économie des droits de propriété intellectuelle. Nous étudions tout d'abord les contributions fondatrices en économie des sciences de Richard Nelson et montrons que celui développe une représentation interactionniste des liens entre science et technologie. Nous expliquons ensuite le développement par Nelson d'un modèle interactionniste par son adoption d'une épistémologie évolutionniste (se traduisant par une représentation circulaire des relations causales et par un accent mis sur la continuité historique) provenant de l'influence de la théorie évolutionniste sur ses travaux réalisés à la RAND Corporation et au Carnegie Institute of Technology. Nous étudions ensuite la généalogie du concept de « paradigme technologique » développé par Giovanni Dosi dans les années quatre-vingt et montrons qu'elle conduit également aux contributions fondatrices en économie des sciences, à une critique du modèle linéaire, ainsi qu'à une épistémologie évolutionniste. Nous analysons enfin la relation entre le modèle interactionniste des liens entre science et technologie, l'épistémologie évolutionniste, et la critique des droits de propriété intellectuelle sur la science et la technologie.

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1. Introduction

> Physical science and industrialism may be conceived as a pair of dancers, both of whom know their steps and have an ear for the rhythm of the music. If the partner who has been leading chooses to change parts and to follow instead, there is perhaps no reason to expect that he will dance less correctly than before. (Arnold J. Toynbee cited in Price 1965, 553).

Recent evolutions in the relationships between states, industries and universities, especially those resulting from recent legal reforms on intellectual property rights such as the Patent and Trademark Law Amendments Act (Bayh-Dole Act) passed in 1980, have led to numerous debates in economics, sociology and philosophy on the vices and virtues of a supposed commercialization of science (Popp Berman 2012).[1] In economics, these debates have revived the interest in the economics of science and invention which emerged in the post-war era with the seminal articles of Kenneth Arrow (1959, 1962a) and Richard Nelson (1959b).[2] As David Hounshell (1997, 2000) shows, Nelson's and Arrow's articles on the economics of invention in the late fifties and in the early sixties were a product of RAND's research project on the

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[1] The Bayh-Dole Act enables non-profit organizations such as universities to retain ownership of patents issued on federally funded research.

[2] For a study of recent developments on the economics of science, see Ballandonne (2012), Dasgupta and David (1994), Mirowski (2004, 2011), Mirowski and Sent (2002), Sent (1999), Stephan (1996, 2012), Tyfield (2011), Wible (1998). Charles S. Peirce is considered as an ancestor on the economics of science with his article titled "Note on the Theory of the Economy of Research" published in 1879 (see Rescher 1976; Wible 1994, 2008).

economics of research and development headed by Burton Klein, and of the broader Cold War context of scientific and technological competition with the East.[3]

In almost every contribution on (the history of) the economics of science is the expression "linear model of innovation." The linear model is a conceptual framework used to examine the relationship between science and technology. In order to understand its characteristics, we should distinguish between its static and dynamic components (Stokes 1997). The static part of the linear model implies an ontological difference between science and technology: while science would develop knowledge about Nature, technology would develop knowledge aiming at practical objectives. The dynamic part of the linear model implies that technology needs prior science in order to develop. Hence, the linear model considers that there is a non simultaneous and linear (unidirectional) relationship between the two realms of science and technology. The relevance of the linear model for contemporary science and technology policies is thus a crucial matter that led to numerous studies on its origins, developments and consistency.

Historians of science and historians of economics have examined the emergence and diffusion of the linear model.[4] David Edgerton (2004, 31) argues that the linear model is an "invention of academic commentators" and contends (38) that economists did not elaborate or

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[3] Klein earned his PhD in economics from Harvard in 1948. He joined RAND in 1952 and became head of the Corporation's economics department in 1961.

[4] It should be noted that the expression "linear model of innovation" is of a recent vintage and that contributions on the links between science and technology in the fifties and in the sixties did not use that expression. A search of the expression "linear model of innovation" in the JSTOR database (February, 2015) shows that the oldest contribution using it is Richard Hydell's 1979 review of Abernathy's 1978 book *The Productivity Dilemma: Roadblock to Innovation in the Automobile Industry*. In his review, Hydell (who earned his PhD on the economics of technological diffusion from MIT in 1977 where he was supervised by Peter Temin) notes that Abernathy criticizes the linear model.

diffuse the linear model. Nevertheless, Edgerton does not examine the seminal articles on the economics of science to support his argument. Benoît Godin (2006) argues that the linear model (which he defines as the sequence Basic research; Applied research; Development; (Production and) Diffusion) is not found in Vannevar Bush's 1945 report *Science, the Endless Frontier* and that economists did participate as early as the fifties in the stabilization and diffusion of the model. Indeed, for Godin, while Bush's report would be responsible for the static and dynamic links between basic research and applied research (a part of the linear model according to Godin), researchers in business schools added development, and economists studied the last stage of the model, that is production and diffusion. In addition, Godin (640) argues that Nelson did participate in the diffusion of the linear model at the end of the fifties. Nevertheless, Godin (659) concludes that "despite its widespread use, the linear model of innovation was not without its opponents. As early as the 1960s, numerous criticisms were leveled concerning, among other things, the linearity of the model. [...] However, the model continued to feed public discourses and academic analyses despite the widespread mention, in the same documents that used the model, that linearity was a fiction." It is thus difficult to understand how academics participated in the diffusion of an analytical framework while considering it at the same time as a fiction. For Godin, the reasons of the wide diffusion and use of the linear model lie in its simplicity (Godin unfortunately does not provide a comparison criterion) and in its use in science policies statistics. The latter argument is nevertheless not directly related to Godin's argument on the role played by economists in the stabilization of the linear model.[5]

[5] Godin (2008) examines the work of the MIT economist of innovation Rupert MacLaurin (1950, 1953, 1954) and argues that the latter was an early contributor to the development of the linear model (Godin 2008, 343). If Godin uses textual evidence to support his claim, other ones can be used to oppose his argument. Indeed, we can find in MacLaurin's 1953 article evidence that he criticized both the static and dynamic components of the linear model. Concerning the static component, MacLaurin writes (98) that "it cannot be assumed that pure science is

Philip Mirowski is another supporter of the argument that economists of science elaborated and diffused the linear model in the Cold War era. For instance, in his book *Science-Mart, Privatizing American Science*, Mirowski (2011, 53) argues that the linear model "was forged as a necessary accessory to the postwar development of neoclassical economic theory, and it was concocted almost without any empirical inquiry into the relevant histories of science and technology." He also adds about Nelson (1959b) and Arrow (1962a) that they "sought to combine the linear model with the theory of public goods to account for science as an input to economic growth" (see also Mirowski 2011, 49; Mirowski and Sent 2002, 39; Sent 1999, 101). Moreover, citing an interview of Paul A. Samuelson by Barnett (2004, 531), Mirowski (2004, 52) argues that Samuelson wrote the passages of Bush's report supposedly containing the linear model. However, although Samuelson acknowledges that he did participate in the writing of Bush's report, he never examines the issue of the linear model in the interview. It should eventually be noted that Nelson's contributions are used in a somewhat instrumental way in the recent literature on the history of the linear model. In Balconi, Brusoni and Orsenigo (2010) as in Mirowski (2011), Nelson's recent articles are used as examples of criticisms of the linear model (see Cohen, Nelson and Walsh (2002); and Colyvas et al. (2002)) while his early contributions are cited but not directly linked with that issue.

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undertaken without any thought of material ends. Since modern science began to emerge in the seventeenth century in Western Europe, it seems highly unlikely that the principal contributors have not been influenced by practical objectives." Concerning the dynamic component of the linear model, MacLaurin cites the following passage from Schumpeter's 1939 *Business Cycles* (Schumpeter in MacLaurin 1953, 105): "It is entirely immaterial whether an innovation implies scientific novelty or not. Although most innovations can be traced to some conquest in the realm of either theoretical or practical knowledge that has occurred in the immediate or the remote past, there are many which cannot. Innovation is possible without anything we should identify as invention" (see also MacLaurin 1953, 106).

Hence, studies arguing that the linear model did exist in the sixties emphasize the role played by economists in its emergence and diffusion, while those claiming that it never existed do not specifically deal with the influence of economists. In other words, contributions on the economics of science in the fifties and in the sixties are used to support the argument that economists played a leading role in the development and diffusion of the linear model during that period.

In contrast with that received view, the aims of this article are to show that economists of science in the fifties and in the sixties did not develop the linear model and adopted instead an interactionist representation of the links between science and technology, to examine the epistemological foundations of their representation, and its consequences for the economics of patents.

To do so, we first focus on Nelson's seminal articles on the economics of invention. Nelson earned his PhD in economics from Yale University in 1956. He became assistant professor at Oberlin College in 1957, economist at the RAND Corporation from 1957 to 1960 and from 1964 to 1968, an associate professor at the Carnegie Institute of Technology from 1960 to 1961, and a staff senior member of Kennedy's Council of Economic Advisors from 1961 to 1963.[6] Moreover, as we recalled, Nelson is one of the founding fathers of the old economics of science in the fifties and in the sixties and was thus a key contributor in the field at that time. We shall see that Nelson has actually never developed the linear model and that his interactionist representation of the relationship between science and technology is based on an evolutionary epistemology which emphasizes the role of historical continuity and of circular causality.

An important point is that Nelson's interactionist representation of the links between science and technology is not developed *against* the linear model, simply because for Nelson

[6] Kennedy's Council of Economic Advisors was chaired by Walter W. Heller from 1961 to 1964.

the latter did not exist at that time, and because, as we recalled in the introduction, that expression spread only in the eighties. As Nelson notes (personal communication, 2015) about his seminal articles in the economics of invention: "I never was much oriented to arguing for or against the 'linear model'…I did not take a point of view that I was arguing against another well articulated point of view." When we shall talk about Nelson's (or another author's) criticism of, or opposition to, the linear model or its components, it should thus be considered as a convenient way to examine Nelson's representation of the links between science and technology and to contrast it with what we *now* call the "linear model." That is also a convenient way because, as we recalled, the received view in the history of the economics of science is that Nelson did adopt and develop the linear model in the fifties and in the sixties.

We then examine the genealogy of the notion of "technological paradigm" developed by Giovanni Dosi in the eighties, and show its links with the old economics of science, a criticism of the linear model, and an evolutionary epistemology. We finally examine the links between an evolutionary epistemology, an interactionist model of the relationships between science and technology, and the criticism of property rights on science and technology.

2. Nelson's Interactionist Model in the Fifties and in the Sixties

We examine below Nelson's approach to the links between science and technology in his seminal contributions on the economics of science and invention. Before we proceed, two caveats are in order. First, in the late fifties and in the early sixties, Nelson did not use the term technology but invention. We shall consider the two terms as being equivalent since, as we shall see, Nelson's definition of invention is different from his definition of science and emphasizes the concrete application of scientific research.

We focus on three of Nelson's articles. The first is "The Economics of Invention: A Survey of the Literature" published in 1959 in the *Journal of Business* (Nelson 1959a). As its title suggests, the aim of that article was to describe the theoretical and methodological states of the art in the economics of invention at the end of the fifties. The second article we study is "The Simple Economics of Basic Scientific Research" published in 1959 in the *Journal of Political Economy* (Neson 1959b) and considered as Nelson's seminal article on the economics of science. The last article we examine is "The Link Between Science and Invention: The Case of the Transistor" published in 1962 (Nelson 1962b). That article is important for two reasons. First, it was published in the NBER volume edited by Nelson himself titled *The Rate and Direction of Inventive Activity: Economic and Social Factors*. The latter contains the contributions and comments presented at the conference on the "Economic and Social factors Determining the Rate and Direction of Inventive Activity" held at the University of Minnesota on the 12th May 1960 and is considered as the contribution institutionalizing the economics of science as research field (Godin 2010; Lerner and Stern 2012). Nelson's 1962b article, as well as the other articles included in that volume, thus had an important influence on the subsequent theoretical and methodological developments on the economics of science. Second, Nelson's research on the history of the transistor is a cornerstone article towards his complete rejection of the linear model.

2.1. The Interaction Between Science and Technology

In his literature review on the economics invention, Nelson (1959a, 105) argues that science and invention should be conceptually distinguished even though they increasingly tend to be linked in practice. This suggests that Nelson adopts the static component of the linear model in that article. Nevertheless, in contrast with the dynamic component of the linear model, Nelson remarks in the section “invention and scientific knowledge” of his article that in the research laboratory of the twentieth century (105-6) “science stimulated invention and invention stimulated science.” Moreover, for Nelson science and technology increasingly tend to interact in the twentieth century (106). If Nelson argues that the dynamics between science and technology is interactionist, he also suggests (105) that technological applications do not necessarily need prior science to develop but can emerge through trial and error procedures.

Moreover, in that article Nelson examines Seabury Gilfillan’s book *Sociology of Invention* (1935a).[7] In contrast with the theories of technological progress which attach each invention to the breakthrough efforts of a heroic inventor, Gilfillan adopted an evolutionary approach to inventions and emphasized the continuous nature of their developments (5): “An important invention is a perpetual *accretion* of little details. [...] An invention is an *evolution*, rather than a series of creations, and much resembles a biologic *process*, because it has a basic kinship with this, thru innate human mentality.” Among the thirty eight principles of invention Gilfillan examined, principle four states (6) that “Invention need not be based on *prior science*. It often precedes and evokes the apposite science.” Gilfillan’s approach to invention is thus a criticism of the dynamic representation of the links between science and invention embedded in the linear model. Gilfillan also emphasized the role played by social

[7] Gilfillan graduated from the University of Pennsylvania in 1910 and earned his MA (1920) and PhD (1935, supervised by the sociologist A. A. Tenney) from Columbia University. Gilfillan was acting assistant professor at the University of the South from 1922 to 1925, and instructor in sociology and economics at Grinnell College from 1925 to 1927. From 1928 to 1929 he was curator of transportation, communication and the social aspects of invention at the Chicago Museum of Science and Industry. He taught sociology at Purdue University from 1937 to 1938. Gilfillan’s books *Inventing the Ship* and *The Sociology of Invention*, both published in 1935, are the outcomes of his PhD dissertation (see Gilfillan (1970) for his autobiography). Gilfillan’s recent influence in economics can be seen through Arthur’s mentions of Gilfillan in his book on the evolution of technology (Arthur 2009).

needs in the development of invention (6-7) and underlined, as another instance of his evolutionary approach, the learning by using aspect of the improvement of inventions (8). As Nelson (1959a) also noted, the learning by using aspect of Gilfillan's evolutionary approach echoed the research on learning processes in the airframe industry and their modeling through learning curves carried out at that time at RAND (Asher 1956).[8]

Despite some criticisms, Nelson (1959a, 103) considered Gilfillan's contribution as "one of the most interesting studies of invention," which shows that he was aware of, and influenced by, the evolutionary approach to the development of inventions as early as the late fifties.[9] It should eventually be noted that Gilfillan participated at the 1960 Minnesota Conference where he commented on Barkev Sanders's article on the issue of the measurement of inventions (Sanders 1962).[10]

Nelson's argument on the close relationships between science and technology is also found in his most famous article on the economics of science, "The Simple Economics of Basic Scientific Research" (Nelson 1959b). In that article, Nelson defines (299) scientific research as "the human activity directed toward the advancement of knowledge" and invention as "the human activity directed toward the creation of new and improved practical products and processes." These quotations suggest that Nelson here again adopts the static component of the linear model. As in his 1959a article, Nelson (1959b, 299) also emphasizes that invention can develop with no prior scientific knowledge, calling into question the

[8] RAND studies on learning processes carried out in the airframe industry influenced Arrow in his 1962b article "The Economic Implications of Learning-by-Doing" (Ballandonne 2015). Even though they both emphasize the role played by learning processes, the notions of learning-by-using and of learning-by-doing should be distinguished.

[9] For Nelson, Gilfillan's approach is too mechanical (the evolutionary approach would not permit to explain the emergence of radical novelties (see North 2013, ch. 3)), goes too far in denying the role played by individual scientists, and over-emphasizes the demand side of the innovation process.

[10] Sanders earned his PhD in sociology and statistics from Columbia University in 1929.

dynamic component of the model: “Though many inventions are made possible by closely preceding advances in scientific knowledge, many others require little knowledge of science or occur long after the relevant scientific knowledge is available.” In addition, in that article Nelson adds a distinction between “basic scientific research” and “applied scientific research” and argues (300) that it would be difficult to distinguish them and that “applied science and invention are closely linked.”

2.2. Science With Practical Problems: Nelson’s History of the Origins of the Transistor

Nelson pursued his study of inventive activities by a history of the invention of the transistor developed in 1951 at Bell Telephone Laboratories (Gertner 2012) especially by the physicist William Shockley (1950). In that article, Nelson’s aim was to examine through an historical case study the role played by scientific knowledge in stimulating changes in inventive activity. Nelson first remarks (554) that scientific knowledge determines itself through an evolutionary process, current scientific developments building on past ones: “The current state of knowledge is the result of an evolutionary process operating on ideas. Therefore, in order to understand the post-World War II research at Bell, it is important to sketch the history of prior semiconductor research.” Nelson also relies on an evolutionary analogy when he examines the issue of the allocation of scientists (572): “Given the nature of scientific research, and an organization where individual scientists have a wide degree of freedom, the allocation of the scientific staff among competing alternatives is likely to be accomplished by an evolutionary or a natural selection process.” Nelson’s history of the origins of the transistor is a cornerstone contribution in the evolution of his representation of the links between science and technology. Indeed, that article corresponds to his rejection of the static component of the linear model:

> When I wrote my paper on 'The Simple Economics of Basic Scientific Research,' [1959b] I still was partly captive to the proposition, which unfortunately remains the conventional wisdom now as well as then, that 'basic research' in a 'scientific discipline' proceeds with little or no awareness of, much less interest in, potential practical applications. My research on the origins of the transistor shook those blinders from my eyes, and I came to recognize that in many scientific disciplines a considerable portion of the work that is called basic research proceeds, as Rosenberg suggested, with puzzles about how technology works, or more general practical problems that have defied solution, very much in mind. (Nelson 2006, 9)

Indeed, in contrast with the static component of the linear model, Nelson shows (1962b, 560 and 581) that several scientists at Bell Laboratory, including Shockley, had practical objectives in mind when they carried out their research on the transistor.

Finally, Nelson argues (581) that technology influences the developments of science, in contrast with the dynamic component of the linear model: "Advances in technology itself certainly affect the direction of scientific research."

Nelson's contributions on the economics of science and invention in the late fifties and in the early sixties testify that he developed an interactionist representation of the links between science and technology, opposing both the static and dynamic components of the linear model. It is true that we can find in Nelson's early contributions textual evidence that could be used to support the claim that he adopted the linear model, since he did remark that science influences technology. As Nelson (1997) has recently noted, "there are many striking examples where the simple linear model looks right." However, we have shown that, according to Nelson, the linear model is a *particular case* of a more complex and interactionist representation of the links between science and technology. We now examine the epistemological foundations of Nelson's interactionist representation of the relationship between science and technology.

3. Epistemological Foundations: Evolutionary Theory and Circular Causality

Nelson's research on the economics of science and invention in the late fifties and in the early sixties are most often characterized as neoclassical. A study of causality theory in economics can explain that characterization. Indeed, the linear model implies a linear representation of causality, akin to the mechanistic approach to causality of neoclassical theory which finds its origins in the physical analogies used by economists in the nineteenth century (Mirowski 1989). The widespread (but, we argue, erroneous) belief that Nelson adopted the linear model in his early contributions on the economics of science is thus used as an instance of his supposed neoclassical approach. Such a characterization is useful for economists of science and for historians of economics since it permits to group Nelson's and Arrow's contributions on the economics of science in the early sixties under the same neoclassical label, before differentiating it with the new economics of science which emerged in the nineties (Dasgupta and David 1994) and which would be more heterodox because of the attention it devotes to institutions. Contributions on the history of the economics of science thus imply that there would be two Nelson: one neoclassical in the sixties, and one evolutionist since the eighties (Nelson and Winter 1982).

We argue instead that Nelson's development of an interactionist representation of the links between science and technology in the late fifties is based on his adoption of an evolutionary epistemology which entails a circular representation of causal relationships.

We should nevertheless emphasize that the meaning of neoclassical economics evolved between the sixties and the eighties. As Nelson remarks (personal communication, 2014): "In the years when I was a young scholar, the American economics community was much more flexible and pragmatic about appropriate modes of theorizing than it became in later years. [...] The hardening and narrowing came around 1980." Put in their historical context, Nelson's research on the economics of science in the fifties and in the sixties can thus still be considered as neoclassical, even though from today's point of view his adoption of an evolutionary epistemology would entail a criticism of such a characterization.

3.1. The Influence of Evolutionary Theory: The RAND Corporation and the Carnegie Institute of Technology

A study of Nelson's institutional context in the fifties and in the sixties is crucial for understanding the influence of evolutionary theory in his contributions at that time. Indeed, as Clément Levallois (2008) showed, the influence of evolutionary theory on Nelson at RAND came about through three key characters: Armen Alchian, Sidney Winter, and Burton Klein.

First, Alchian, who became associated with RAND at the end of his military service in 1946 published in 1950 his classic article "Uncertainty, Evolution and Economic Theory" in the *Journal of Political Economy*. In the latter, Alchian criticized the *a priori* assumption of profit-maximizing behavior and used the biological analogy of "natural selection" in order to explain the "survival" of the more profitable firms. Geoffrey Hodgson (1999, 115-6) showed that Alchian's use of a biological analogy should be understood in the broader context of the emergence of neo-Darwinism (a synthesis of Darwin's theory with population genetics) in the forties. Nelson was thus directly influenced by the father of the renewal of the use of evolutionary analogies in economics in the fifties.

Second, Winter, who received his PhD from Yale University in 1964, also worked at RAND as a research economist from 1959 to 1962 before he became a staff member of

Kennedy's Council of Economic Advisors. Winter's adoption of an evolutionary approach finds its origins in his reading of Alchian's 1950 article during the first year of his commitment with the Corporation (Winter 2000). It should also be noted that Winter was a participant at the 1960 Minnesota Conference and commented on Jacob Schmookler's 1962 article titled "Changes in Industry and in the State of Knowledge as Determinants of Industrial Invention."[11] Schmookler, who received is PhD from the University of Pennsylvania in 1951, was a key contributor on the economics of invention in the sixties. In his 1966 book *Invention and Economic Growth*, he showed through the use of patents statistics that inventions are mainly driven by demand.[12] Schmookler's demand-pull theory is thus a criticism of the linear model since it is not science which mainly leads to new inventions but the size of the market.[13] Schmookler, who notes (9) that "inventive activity is often so intertwined with research and development today that a scientist or engineer might have trouble deciding which function he was performing at a given moment," further criticizes the representation of the linear model in Chapter three of his book, titled "The Role of Intellectual Stimuli." In the latter, he examines the links between science and invention in

[11] Schmookler's dissertation was titled "Invention and Economic Development."

[12] Godin and Lane (2013, 12) argue that "Schmookler was an isolated author whose views, incidentally, were not considered in the debates of the 1960s [...]. He was alone and preaching in the desert." For them, Schmookler's contributions essentially spread in the eighties. We disagree with this interpretation. Indeed, Schmookler was a well-known economist in the sixties: he was a student of Simon Kuznet at the University of Pennsylvania (Schmookler's 1966 book is dedicated to Kuznets) and, as we recalled, published an article in Nelson's 1962a edited volume on the economics of invention. Moreover, the acknowledgements section of Schmookler's 1966 book testify that he was far from being an isolated scholar but was instead in relationship with the most famous economists of technological progress at that time (*ix*): "Earlier versions of the manuscript were read and criticized by Professor Kuznets, Zvi Griliches, Edwin Mansfield, Richard R. Nelson, M. J. Peck, and Frederic M. Scherer."

[13] Schmookler defines invention (208) as the "production of technology generally."

the industries of petroleum refining, paper making, railroading, and farming. Schmookler concludes (67) that "*in no single instance is a scientific discovery specified as the factor initiating an important invention in any if these four industries*" and points out (70) that "if by conventional view is meant the proposition that a given scientific discovery is not only a necessary but also ordinarily a sufficient condition for the occurrence of the latter invention based on it, I cannot agree."

Third, Klein, a former student of Schumpeter at Harvard who recruited Nelson at RAND (Levallois 2008, 182), introduced both Nelson and Winter to Schumpeterian evolutionary economics.[14] Regarding Schumpeter's influence on the contributions published in *The Rate and Direction of Inventive Activity*, Nelson remarks (2012, 36-7) that "explicit reference to Schumpeter is quite limited. Where there was such reference, it mostly was in discussion concerned with whether significant innovation in an industry required that the firms in it be large ones. However, [...] a Schumpeterian view that innovation is the principal means of competition in many industries is implicit in several of the papers." As Winter also recently recollected (2005, 28): "At RAND, Dick Nelson and I became increasingly aware that we were following Schumpeter's path, and increasingly appreciative of how valuable the master's guidance actually was."[15] Moreover, Schumpeter's influence is crucial for the understanding of the subsequent development of evolutionary economics by Nelson and Winter: "When later we began explicitly to try to develop an evolutionary theory of economic change, we recognized that we were trying to develop a rigorous version of Schumpeter" (Nelson 2003).

[14] Schumpeter taught at Harvard from 1932 to 1950.

[15] It should be noted that Schumpeter was influenced by Gilfillan's contributions in his classic 1939 *Business Cycles* when he defined and contrasted the notions of invention and innovation, though Schumpeter did not completely agree with Gilfillan's approach (Schumpeter 1939, 227-8 n. 1).

Finally, the influence of evolutionary theory on Nelson's contributions at that time also came from the Carnegie School in economics represented by Herbert Simon, James March, and Richard Cyert (Cyert and March 1963, March and Simon 1958, Simon 1947) who conducted their research at the Carnegie Institute of Technology, which became the Carnegie Mellon University in 1967.[16] The Carnegie School studied the decisions making processes of organizations with an emphasis on behavioral realism (Gavetti, Levinthal, and Ocasio 2007). Carnegie's studies of organizations had a crucial influence on RAND scholars, and especially on Nelson and Winter. At the institutional level, as we recalled, Nelson was assistant professor at Carnegie from 1960 to 1961 and "there was considerable interest at RAND in understanding how large organizations behaved. The Carnegie school theories [...] provided the intellectual basis for an informal organization theory seminar at RAND that met regularly during the middle 1960s" (Nelson 2003). At the conceptual level, the Carnegie School emphasized the role played by routines in the decision making processes of organizations. Under Winter's influence, the role played by routines in organizations became interpreted as being equivalent to the role played by genes in biology, partly grounding the evolutionary approach of Nelson and Winter's 1982 book. It should also be noted that the Carnegie School emphasis on routines influenced Nelson's development of an evolutionary theory of technology. As Nelson (2003) puts it: "The organizational routines idea meshed well with a

[16] Simon earned his PhD in Political Science from the University of Chicago in 1943 and joined Carnegie's Graduate School of Industrial Administration at its creation in 1949 and stayed there until his death in 2001. March earned his PhD in Political Science from Yale University in 1953 when he became assistant professor and professor of industrial administration and psychology at Carnegie until 1964 when he moved to the University of California. Cyert earned his PhD in economics from Columbia University in 1948 and joined Carnegie as an instructor in economics that same year. He then became professor of economics and industrial administration, and the dean of the Graduate School of Industrial Administration. Cyert was Carnegie Mellon's sixth president from 1972 to 1990.

conception of the nature of technologies I had been developing in my work. Technologies could be characterized as recipes. What Cyert and March gave to me was the notion that those technology recipes were organizational routines."

Nelson was thus, through his commitment with the RAND Corporation and the Carnegie Institute of Technology, hugely influenced by evolutionary theory at the end of the fifties and in the early sixties.

3.2. Evolutionary Theory and Circular Causality

As Kurt Dopfer (1986) shows, one of the main differences between what he calls orthodox (neoclassical) and heterodox (evolutionary) economics relates to the theory of causality the two approaches adopt. Regarding causality in evolutionary economics:

> The agents and the environment are not isolated but are rather conceived in the mutual interactions. Both are hence *ex hypothesi* not invariant, but are themselves subject to change. This change brought about by reciprocal causation which implies that A causes B and B causes A. The causality concept itself is not mechanistic, but morphogenetic. This means that A induces a change in B and B induces a change in A. (517)

The theory of causality adopted by evolutionary economists thus entails an interactionist or circular representation of causal relationships. Dopfer (1986) adds that, by contrast with the neoclassical approach, the theory of causality adopted in evolutionary economics would be empirically better grounded. Nelson's history of the origins of the transistor thus epitomizes the links between an empirical methodology and the adoption of a circular theory of causality in conformity with evolutionary principles. Nelson's adoption of an interactionist representation of the links between science and technology in his seminal articles on the economics of science can thus be considered as an instance of one of his first applications of an evolutionary perspective in economics.

4. Technological Paradigms, the Old Economics of Science, and the Nonlinear Model

Building on the evolutionary approach of Nelson and Winter (1977, 1982), Dosi (e.g., 1982, 1984, 1988) developed the notion of "technological paradigm" which can be defined (Dosi 1982, 148) as "an 'outlook', a set of procedures, a definition of the relevant problems and of the specific knowledge related to their solution." In a recent article, Dosi and Nelson (2013) explain their criticism of the linear model by three main points: scientific developments do not necessarily lead to technological developments; technology also influences science; and technological developments can occur without prior scientific knowledge. We thus disagree with Godin and Lane (2013) who argue that the notion of technological paradigm would entail (15) "a return to a focus on scientific discoveries, as the ultimate causal factor in generating innovation," implicitly making Dosi a supporter of the linear model.

According to Dosi (1982, 148) the notion of technological paradigm is close to the notion of scientific paradigms, and is accordingly a reference to Thomas Kuhn's use of the notion of paradigm in his classic book *The Structure of Scientific Revolutions* (Kuhn 1962a). It should be noted that Kuhn participated at the 1960 Minnesota Conference and wrote a comment on Irving H. Siegel's 1962 article titled "Discovery and the Rate of Invention."[17]

[17] Siegel earned his PhD from Columbia University in 1951. His dissertation was titled "Concepts and Measurement of Production and Productivity." From 1936 to 1939 he worked as a statistician in the Research Project on Reemployment Opportunities and Recent Changes in Industrial Techniques held at the New Deal agency Works Progress Administration. Siegel then became associated with the National Bureau of Economic Research and the Bureau of Labor Statistics from 1939 to 1943. He was recruited as a senior economic staff

The recent notion of technological paradigm is thus, through Kuhn, historically linked to the seminal contributions on the economics of science in the early sixties.

Before his contribution to the 1960 Minnesota Conference, Siegel published two articles in which he elaborates on his representations of causality and of the links between science and technology. We examine below Siegel's contributions before we study Kuhn's comment.

4.1. Siegel's Circular Representation of Causality and Its Application to the Links Between Science and Technology

In his 1954 article titled "Conditions of American Technological Progress" and published in the *American Economic Review*, Siegel distinguishes between economic growth (defined as the "incremental or cumulative measure pertaining to a significant desideratum of a society" (162) such as national income), economic progress (defined (162) as the "ratio of a measure of growth to the size of the population") and technological progress (defined (163) as "the ratio of a comprehensive measure of incremental growth, like real national product, to the corresponding total resource input"). Siegel's study of technological progress is embedded in a circular and cumulative representation of causal relationships (165): "In the consideration of cumulative change and progress, the distinction between cause and effect, between condition and consequence, is not clear cut." In addition, Siegel suggests that there would be five determinants to technological progress (man, private economic institutions, other institutions, the rest of the world and nature) which (164) "interact and are reciprocally related." Siegel's

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member of Eisenhower's Council of Economic Advisers from 1953 to 1960 (the latter was chaired by Arthur F. Burns from 1953 to 1956 and by Raymond J. Saulnier from 1956 to 1961), a position which led him to specialize on the issue of the links between inflation and employment (Siegel 1969).

criticism of unidirectional causal relationships was also represented by a graph depicting his argument in favor of reciprocal causal links between the determinants of technological progress, leading to a circular representation of causality (168):

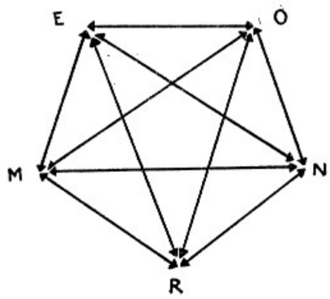

FIGURE 1

RECIPROCAL RELATIONS AFFECTING TECHNOLOGICAL PROGRESS, ONE COUNTRY
M = Man; E = Private economic institutions; O = Other institutions; R = Rest of world; N = Nature

Although the category of technological progress is absent from the graph, Siegel remarks that the five determinants (167) "influence and are influenced by technological development." Siegel's theory of technological progress is thus embedded in a criticism of unidirectional causal relationships.

Siegel's second article titled "The Role of Scientific Research in Stimulating Economic Progress" was published in the *American Economic Review* that same year as the Minnesota Conference (Siegel 1960). In that article, Siegel examines the nature of the links between science and technology and applies to that issue his circular and cumulative theory of causality. Discussing the word "stimulating" used in the title of his article, Siegel points out that another word such as "affecting" would have preferably been used because it would be more neutral (it would allow (341-2) "research to have restraining as well as stimulating influences"). Moreover, Siegel notes that the word "stimulating" would not take into account the reciprocal influences between research and economic progress.

In his article titled "Scientific Discovery and the Rate of Invention" published in the 1962 volume edited by Nelson, Siegel questions the link between discovery (defined (441) as "the act of wresting a secret from nature") and invention (defined (442) as "purposeful and practical contriving based on existing knowledge"). This suggests that Siegel adopts the static component of the linear model. Nevertheless, and in the same vein as his earlier articles, Siegel develops an interactionist representation of causality between the two notions (446). It should also be noted that Siegel's theory of causality also concerns the links between science, technology and society. Science and technology influence society and society, in return, has an influence on science and technology:

> In considering the roles of discovery and invention in the real world, it is desirable not to preclude the possibility of their interdependence and their influence on, as well as their reaction to, economic, social (including political, legal, and familial), psychological, and international factors. [...] As we think further about such interrelationships, the integrity of a modern society becomes obvious. (447-8)

Hence, as soon as the early sixties, and in the volume which institutionalized the economics of science, we find an economic study of the interaction between science, technology and society which echoes the recent developments in innovation studies.

4.2. Kuhn's Comment to Siegel's Article

While Siegel used the notions of discovery and invention, Kuhn (1962b) first remarks in his comment to Siegel's article that the debate is usually framed using the notions of science and technology. Although Kuhn acknowledges his admiration for Siegel's article (450), he aims to inscribe the issue of the links between science and technology in a broader historical perspective. Kuhn argues that interactionist relations between science and technology can essentially be observed since the end of the nineteenth century with year 1860 as cornerstone.

Before this date, science and technology would have been kept separate, which would no longer be the case after (453-4). Kuhn adds that science and technology should be represented by two different lines with “existing knowledge” as a common origin (455-6): “Their possession of a common origin indicates that both the scientist and the inventor-engineer depend for the source and structure of their enterprises upon what is already known.” Kuhn’s emphasis on the role played by prior knowledge in the development of new inventions echoes the evolutionary nature of the invention process underlined by Nelson. Kuhn thus argues for a distinction between science and technology and thus adopts the static component of the linear model (455): “Instances [...] reflecting the rather different goals and drives of the scientist and the technologist, can be multiplied *ad nauseam*.” Nevertheless, Kuhn also argues that science and technology interact. In Kuhn’s representation, the interaction between science and technology would take place (456) “through the pool of existing knowledge.” In addition, Kuhn also argues that the two notions directly interact through (456) “a series of double headed arrows connecting the “science” line with the “technology” line at various distances from their common origin.”

In sum, the genealogy of the notion of technological paradigm leads back to the seminal contributions on the economics of science in the early sixties, to an interactionist representation of the links between science and technology, and to a related evolutionary epistemology.

5. The Interactionist Model, Patents, and Science and Technology as Public Goods

We now examine the theoretical consequences of the development of an interactionist model of the links between science and technology regarding the criticism of patents and the related definition of science and technology as public goods.

5.1. Evolutionary Epistemology and the Criticism of Patents

We have seen that the interactionist model of the links between science and technology is based on an evolutionary epistemology. As the historian of technology George Basalla shows (1988), the evolutionary approach to the developments of science and technology leads to a criticism of intellectual property rights because of the role they assign to inventors. Indeed, according to the evolutionary approach, there is almost no role for the inventor in the developments of science and technology because they are the results of past scientific and technological advancements ("continuous" representation) and not of the ex nihilo product of a heroic inventor ("discontinuous" representation). Because patents personify the scientific and technological developments, they are the symbol of the cultural representation that inventions are the results of geniuses making ground-breaking achievements. Patents are thus a legal apparatus which is in conformity with the discontinuous approach to the developments of science and technology and in opposition with the evolutionary or continuous approach (Basalla 1988, 60). The evolutionary epistemological foundation of the interactionist representation of the links between science and technology thus entails a criticism of patents and intellectual property rights. That criticism of patents is in line with the definition of science and technology of public goods to which we turn now.

5.2. The Interactionist Model and Science and Technology as Public Goods

As is well known, Arrow (1962a) and Nelson (1959b) defined science as a public good in their seminal articles on the economics of invention. For Mirowski (2011, 57), the origin of that definition is to be found in the influence on Arrow and Nelson of Samuelson's articles on public goods in the mid-fifties (Samuelson 1954; 1955). Arrow contradicts that story, claiming that "the idea of knowledge as a public good seemed self-evident to me, and, indeed, I always thought that it was very well known. While I read Samuelson's paper, I did not think the case of knowledge as a public good was a new idea" (2012, correspondence with the author). Mirowski (2011, 59) adds that neither Arrow nor Nelson "paid any attention to property rights" in their seminal contributions. We also disagree. Indeed, Arrow and Nelson paid a careful attention to the issue of property rights, especially of their defects, in order to support their definition of science as a public good. For instance, Nelson (1959a, 302) notes that "it is quite likely that a firm will be unable to capture through patent rights the full economic value created in a basic-research project;" and Arrow (1962a, 615) remarks that "no amount of legal protection can make a thoroughly appropriable commodity of something so intangible as Information" and notes (617) that "the inventor will in any case have considerable difficulty in appropriating the information produced. Patent laws would have to be unimaginably complex and subtle to permit such appropriation on a large scale." In addition, problems of definition and measurement (especially through patents statistics) of inventive activities were discussed at the 1960 Minnesota Conference, with contributions by Kuznets (1962) and Sanders (1962) published in the 1962 volume edited by Nelson.[18]

[18] A PhD in sociology and statistics from Columbia University in 1929 and originally a specialist on health issues, Sanders was a research associate in the "Patent Utilization Study" (1954-1970) which principal investigator was Joseph Rossman, editor of the Journal of the Patent Office Society (Rossman and Sanders 1957). At the end of the fifties, Sanders and Siegel were principal consultants for the research project "Attitudes of American Inventors Toward Defense Invention" (1956-1957) which principal investigator was James Mosel. These two studies were carried out at the Patent, Trademark and Copyright Foundation of George Washington University. From1957 to 1970, Sanders was a member of the editorial committee for the "Patent, Trademark and Copyright Journal of Research and Education" also at George Washington University. It should also be noted that Sanders (1962, 60) was a "good friend" of Gilfillan and that the later considered Rossman's 1931 book *The Psychology of the Inventor, A Study of the Patentee* as "the *best book* that has yet appeared on the sociology of invention" (Gilfillan 1970 [1935], 166).

In contrast with another received view in the history of the economics of science, we argue that the origins of Nelson's definitions of science *and technology* as public goods are to be found in his development of an interactionist model of the links between science and technology.

Criticizing patents on science is consistent with the linear model of innovation. Indeed, science needs to be open to some degree in order to flow to technological developments (Nelson 2004, 460). In addition, as Nelson argues, since science is also developed with practical objectives in mind (in opposition with the static component of the linear model), privatizing science would hamper the development of technology in the first place. Moreover, if science and technology interact, then both science and technology need to be open to some degree, grounding the argument that science and technology are public goods and making stronger the case against patents on science and technology. As Nelson (2004, 456) has recently pointed out, "There is no 'tragedy of the commons' for a pure public good like knowledge. And to deny access, or to ration it, can result in those denied doing far less well than they could if they had access […] if access to certain bodies of *scientific knowledge or technique* can be withheld from certain researchers, they may be effectively barred from doing productive R&D in a field." The case against patents is also stronger because of the evolutionary, cumulative, nature of the developments of science and technology. Not only science and technology interact, but both science and technology build on their past developments (Mazzoleni and Nelson 1998, 281). The economic trade-off is thus to provide the incentives (not limited to patents) permitting the development of science and technology in the first place without, however, compromising the interaction between science and technology and the evolutionary nature of their developments (Nelson 1989). In this regard, Nelson's recent concerns about the rise of patenting and licensing activities by universities are in line with his criticism of the linear model in the sixties (e.g., Nelson 2001).

To sum up, Nelson's development of an interactive model of the links between science and technology as early as the fifties and the sixties, and his criticism of patents on both science and technology are a consistent whole which can ultimately be explained by his evolutionary epistemology.

6. Conclusion

In contrast with the received view in the history of the economics of science, we have shown that seminal contributions on the economics of science in the fifties and in the sixties did not develop or support the linear model of innovation. Indeed, we have seen that key authors in the field, such as Nelson, developed an interactionist representation of the links between science and technology and considered that science could aim at practical objectives, as early as the fifties and the sixties. We then have shown that Nelson's interactionist representation is the result of his adoption of evolutionary theory when he carried out his research at the RAND Corporation and the Carnegie Institute of Technology. Hence, evolutionary foundations in the economics of science already existed in the fifties and in the sixties and are not a special characteristic of the contributions of the eighties or of the nineties. The genealogy of the notion of technological paradigm further testifies the historical and epistemological continuities between the fifties and the eighties in the economics of science and technology on the issue of the links between science and technology. We then examined the consequences of the interactionist representation of the links between science and technology, arguing that it can explain the origins of the definition of science and technology as public goods and the related criticism of patents on science and technology.

Historians of economics have only examined the American developments of an economics of invention in the Cold War era. Nevertheless, an economics of science was also developed in Russia at that time, especially by L. S. Gliazer in the seventies (Gliazer 1971, 1972, 1973, 1980; Gliazer and Kokoshkina 1975). A study of the representation of the links between science and technology and of the role of patents on science and technology in the Russian economics of science remains to be carried out.

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