"Innovation Studies":
The Invention of a Specialty (Part I)

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“Innovation Studies”: the Invention of a Specialty

The study of technological innovation is over one hundred year old (anthropology, history, sociology, management, policy and economics). However, over the last twenty years or so, “innovation studies” has become a label used by many to name research concerned specifically with the economics, policy and management of technological innovation. What are the origins of this specialty? There is no linear history, but two completely different traditions. Part I (this paper) is concerned with the American tradition on ‘technological change’ while Part II concentrates on the European tradition (‘innovation studies’).

Abstract

In 1960, the US National Bureau of Economic Research (NBER) held a conference on *The Rate and Direction of Inventive Activity*. The conference was one of the first to be devoted entirely to the study of science, technology and innovation, and it was an influential one. To varying degrees, the conference helped to crystallize many ideas that would come to define future economic thinking about technological innovation.

This paper examines why the conference and the book it produced turned out to be influential, and looks at the ways in which that occurred. Through a survey of the early history of economic thought on technological innovation, the paper documents the concepts used, the framework developed, and the measurements constructed by economists beginning in the 1930s. It discusses how economists got into the field of technological innovation studies, adding a new dimension to the study of an old category (invention), then inventing a new category (technological change), and finally looking at innovation.

"Innovation Studies":
The Invention of a Specialty (Part I) [1]

Introduction

The systematic study of the economics of technological innovation in the United States owes its emergence to two think tanks. [2] One is RAND, created in 1946 as a research project of the United States Air Force and established as an independent non-profit organization in 1948 (Smith, 1966). D. Hounshell has written interesting pieces of work on the history of RAND and its early contribution to technological innovation studies (Hounshell, 1997; 2000; see also Amadae, 2000). The other think tank is the National Bureau of Economic Research (NBER), set up in 1920. One excellent study on some aspects of the early history of the NBER is Alchon (1985); another is Fabricant (1984). But neither focuses on NBER's contribution to technological innovation studies. This paper is a small contribution to a history of "innovation studies" – which as yet remains to be written – through the lens of the work conducted at the NBER.

My perspective is entirely historical. I seek to answer two questions. First, why did the NBER conference held in 1960 and the book that followed it turn out to be influential? Second, how did that come to pass? My analysis is based on historical evidence: archival material and the published literature. I take a look back through history to document the contributions economists have made to the study of technological innovation, and then forward again to the present, assessing the influence they have had in that field. My thesis is that economists' contribution to technological innovation studies went from the study

[1] A first draft of this paper was prepared for and presented at the pre-conference to the NBER Anniversary Conference "The Rate and Direction of Inventive Activity: a New Agenda", Dana Point, California, 13-14 July 2009. The author thanks the Rockefeller Archive Center for access to the material from the preparation of the SSRC conference (1951) as well as that of the NBER (1960). Unfortunately, no similar material exists at the NBER. I also sincerely thank R. R. Nelson who agreed to share his memories of the NBER conference with me, F.M. Scherer who read carefully and commented on a preliminary version of this paper, and I. Feller for most relevant suggestions.

[2] By think tank I mean a group of academics active in both research and consulting. In the present case, the think tanks are not "ideological", and the term is not used in a pejorative sense.

of invention toward the study of technological change and, finally, the study of technological innovation *per se*.

The Three Stages of Economic Thinking
On Technological Innovation

1. Invention: economists borrow and adapt a category.
2. Technological change: economists invent a disciplinary matrix.
3. Innovation: economists catch up.

I examine the NBER conference from the point of view of how and to what extent it contributed to this development. Let me be clear from the start: I do not suggest that the conference was the cause or source of technological innovation studies among economists, but it did contribute, among other things, to this emerging field. My story of the NBER is thus intimately linked to the story of the field of technological innovation in general: the conference as a moment that was witness to the ideas economists brought to the study of technological innovation.

I trace this history through the study of 1) the concepts used, 2) the framework developed, and 3) the measurements constructed. The first part of this paper discusses how economists added a new dimension, the economic dimension, to the study of an old category, that of invention. Here, economists simply borrowed and adapted an existing category for their own purposes. This part of the paper also documents the specific framework that was developed for the study of invention among economists, namely that on efficiency. The second part of the paper discusses how, in a second step, or direction, economists invented a disciplinary category for the study of invention, that of technological change, and adapted an existing tool, the production function, for its measurement. The final section examines technological innovation as commercialized invention, and the recent efforts of mainstream economists to catch up with a "neglected" aspect of the study of technological innovation. To different degrees, the NBER conference helped crystallize many ideas at these three levels, ideas that would define the future development of economic thinking about technological innovation.

Invention

When researchers gathered in 1960 at the NBER conference on the *Rate and Direction of Inventive Activity*, they elected invention as the category to study. Invention as a category first emerged two thousand years ago in rhetoric (Cicero), then appeared in legal and civil affairs and early literary criticism. At about the same time, the few "mechanical" writings that existed made use of the category – as would do patent privileges and laws from the late Middle Ages. From about the Renaissance onward, invention came to mean both discovery and fabricating, particularly in natural philosophy. Then in the 19th century, the category of invention appeared in anthropology as a factor contributing to the evolution of societies, in history to make sense of modernity, in sociology as a "cause" of social change, and in management as an output of research laboratories.

What the NBER conference did was add a new dimension to the study of an old category: the economic analysis of invention. The NBER conference helped crystallize a new sub-field that was almost non-existent before economists got together at the University of Minnesota. The conference was only the second ever organized on the subject. [3] In April 1951, the US Social Science Research Council (SSRC) had organized a conference on "Quantitative Description of Technological Change" at Princeton. The idea for the conference came from discussions at two committees of the SSRC: the Committee on Economic Growth, chaired by the economist S. Kuznets, and the Committee on the Social Implication of Technological Change. Following a meeting held in October 1949, Kuznets had circulated a memorandum of suggested topics for the conference. He proposed looking at measurements such as: patents, lags in technology use, censuses of machines (or mechanization surveys of industries), counting of new (consumer) products,

[3] Mention should be made of some previous conferences of a more limited character, for example those of the NBER on capital formation and economic growth (1953), of George Washington University on the patent system (**19??**), and of the US National Science Foundation on R&D and economic growth (1958).

and input/output ratios. Comments were received from several researchers. All shared their enthusiasm for a conference, and proposed to present their own methodology.

Thirteen papers were prepared (see Appendix 1), and about sixty people attended the conference, among them G. Debreu, S. Fabricant, J. L. Fisher, S. C. Gilfillan, S. Kuznets, W. E. Leontief, W. R. Maclaurin, J. Schmookler, S. H. Shryock and A. P. Usher. There had been a project to publish the proceedings as a book, but this was abandoned because "the papers [were] in most cases of a very exploratory character, with quite different points of view and without a sufficient thread of unity to be published in a single volume". [4] In fact, the closing session concluded that "thus far research efforts on many of the most significant aspects of technological change have failed to produce conclusive results". But "there was agreement that persistent efforts must be made to develop and test new research approaches". [5] Instead of attempting to publish the very diverse set of papers, it was decided to "distill" them into a shorter publication that would include discussions. Kuznets committed to such a paper his thoughts on "technological change", making use of the conference, but he never completed his preliminary draft (Kuznets, 1951). Kuznets's draft dealt with measuring the contribution of technology to production, mainly through input-output analyses. The paper was of a methodological nature, discussing what knowledge is and how to measure it, the problem of subtracting technology as a residual from other factors or changes, [6] and the problem of attribution. Kuznets concluded that "we may be doomed to a position in which we can measure only economic growth, but not its causes".

In retrospect, the reason the conference's organizers did not publish the proceedings appears as a rather severe judgment. The papers offered analyses, methodologies and data that would define the field in the decades to come, particularly the concept of the production function, and many authors published their papers independently in academic

[4] Letter from A. J. Coale to J. L. Fisher, 6 July 1951. The Rockefeller Archive Center: Social Science Research Council Archives, Accession Two, Box 148, Folder 1690.
[5] Attachment to letter from P. Webbink to R. R. Nelson, 5 August 1960. The Rockefeller Archive Center: Social Science Research Council Archives, Accession Two, Box 148, Folder 1690.
[6] To the best of my knowledge, this was the first occurrence of the term "residual" in economic studies of technological change.

journals. The only numbers that were not discussed were expenditures on research and development (R&D). Systematic data would only become available a few years later by way of the US National Science Foundation's surveys, and would be discussed by some speakers at a second conference, that of the NBER in 1960.

The NBER conference was organized in collaboration with the SSRC. The latter was a natural partner, having organized the 1951 conference. The idea of the NBER conference came from C. Hitch, Director of the Economics Department at RAND, who discussed it with members from the Universities-National Bureau Committee for Economic Research. The big impetus to the conference was R.M. Solow's article published in 1957 showing that most of productivity growth was not due to capital but an unexplained residual called technological change (see below). Also important was the recognized importance of technology to national defense, a topic of intensive study at RAND for over a decade.

Discussions with the SSRC began in 1958 for a conference on what were then called the "factors affecting the rate, direction, and success of inventive activity". On the NBER side, a committee was set up composed of C. Hitch (RAND) as chairman, Y. Brozen (University of Chicago), Maclaurin (MIT), Usher (Cambridge University) and R. R. Nelson (RAND) as committee secretary. According to Nelson's recollection, this committee may never have met. [7] On the SSRC side, Kuznets, as chairman of the Committee on Economic Growth, acted as representative. He was seconded by J. Schmookler (University of Minnesota), from whom Kuznets requested a review of the state of the art. Each side produced a report transmitted to the other, and each side had its own view of the conference. The NBER report, written by Nelson, insisted that in order to answer the questions about the study of factors affecting invention "we must know much more than we presently know about the processes by which invention actually takes place", including research policy and the management of research and development (R&D). [8] To the SSRC, it was clear that the study of factors affecting invention must

[7] Personal interview, May 10, 2009.

[8] Although the NBER committee for the conference may never have met (and may thus never have really existed), the report is nevertheless witness to the views of the NBER (or of RAND?) on the conference. R. R. Nelson, *A Preliminary Report*, Committee on the Economic Aspects of Research and Development, 2

cover the "spectrum from invention [scientific discovery] to economic production" and economic growth, and the emphasis should be on economic aspects rather than policy. [9] To this end, the SSRC suggested a list of precise papers and a list of potential authors and speakers. [10] I will come back in my conclusion to conference's organization and the tensions it involved.

Most of the papers presented at the conference came from economists and researchers interested in the management of research, like RAND (NBER, 1962). Among the speakers, four were present at both the 1951 and 1960 conferences: Brozen, Gilfillan, Kuznets and Schmookler. Sociologists and historians were absent as speakers, but a psychologist (D. W. Mackinnon) was invited to speak on invention as creativity, and Gilfillan and T. S. Kuhn attended as commentators.

In retrospect, the conference can undoubtedly be qualified as influential. It discussed the then-emerging ideas on invention in economics. Long after other disciplines, economists were finally beginning to look at invention seriously. As B. H. Klein put it at the conference, "while economists have probably had little influence on business practices in research and development, the same cannot be said of cost accountants, management experts, and the growing army of business school graduates in general. And to their influence must be added the influence of the engineers" (Klein, p. 497). [11] Certainly, economic thoughts existed for over a century on "machines" and their effects on employment (Ricardo, Sismondi, Marx). It is in line with this tradition that technological 'invention' began to be studied in the 1920-30s (Pigou, 1924; Hicks, 1932; Robinson,

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June 1958. The Rockefeller Archive Center: Social Science Research Council Archives, Accession Two, Series 1.21, Box 148, Folder 1685.

[9] Letters from S. Kuznets to C. Hitch, 24 September 1958, and to J. Schmookler, 20 October 1958. The Rockefeller Archive Center: Social Science Research Council Archives, Accession Two, Series 1.21, Box 148, Folder 1685.

[10] J. Schmookler, *Preliminary Report to the Committee on Economic Growth on Proposed Conference on Science, Technology, and Economic Growth*. The Rockefeller Archive Center: Social Science Research Council Archives, Accession Two, Series 1.21, Box 148, Folder 1685.

[11] In order to distinguish papers from the conference from others cited in this article, I refer to the conference papers without mentioning the date of their publication (1962).

1938). The studies were few, but they gave rise to one influential tradition of research (to be discussed in the next section): technological change. [12]

Over the years, economists developed very few, if indeed any, relations with other disciplines, although many of the concepts used were taken, most often unconsciously, from these disciplines. Equally, some controversies developed, among them that between Z. Griliches and the sociologists from the innovation diffusion tradition (Griliches, 1957; 1960; Brandner and Straus, 1959; Havens and Rogers, 1961; Rogers and Havens, 1962; Babcock, 1962) and between J. Schmookler and sociologists S.C. Gilfillan (Schmookler, 1960) and W.F. Ogburn (Schmookler (1966: 59-63). However, economists would soon develop their own concepts and give rise to a (small) subfield of economics.

The question "'what is invention?' occupied many speakers and commentators at the conference (Kuznets, [13] Sanders, Machlup, Siegel, Schmookler's comment on Kuznets's paper, Kuhn's comments on Siegel). From the economist's point of view, two characteristics of invention deserved emphasis. First, and without debate, invention was understood as technological invention. As Kuznets put it: "we exclude social invention" (Kuznets, p. 19). This was not really a novelty – although it was an influential decision. Sociologists, among them W. F. Ogburn and S. C. Gilfillan, had already made this move in the previous decades. However, from a long-term perspective it was a novelty. In fact, invention is not a category from economics. Before that time, it had been defined very broadly. As American anthropologist and US National Museum curator O. T. Mason put it in 1895, "invention is finding out originally how to perform any specific action by some new implement, or improvement, or substance, or method (…). Every change in human activity, made designedly and systematically, appears to be an invention" (Mason, 1895: 13-14). Among economists, invention spontaneously came to be restricted to technology: invention as useful invention, namely designed for practical use in production (Kuznets, p. 21). Second, usefulness was understood as "major" invention,

[12] The studies on "induced innovation" (1960s), briefly mentioned in the last section of this paper, also owes to this tradition.

[13] Kuznets's paper contains many of his thoughts from the uncompleted draft to the 1951 conference (Kuznets, 1951). The same thoughts also appeared in Kuznets (1959).

namely those inventions that "have a wide technical potential in the sense that they provide a base for numerous subsequent technical changes" (Kuznets, p. 26), and have a broad economic potential for returns and productivity. Again, this was no novelty. Major (or basic) invention is an old idea, precursors of which can be found among sociologists (Ogburn and Gilfillan, 1933: 130) and anthropologists (Linton, 1936: 316) and their classifications of inventions by type, and is a reminder of the "great men" era and the age of great inventors (Macleod, 2007). However, the economists crystallized the idea. [14]

Two motivations explain this understanding of invention among economists. One is ideological and was shared by everyone at the time: the moral hierarchy or economy between major and minor inventions (improvements) (Rosenberg, 1976; 1978; 1982; 1988). [15] Inventive activity is said to exclude development. As Kuznets put it, development "is a job of adjustment"; "it is not original invention" (Kuznets, p. 34-35). Similarly, to Schmookler, development "does not demand the creative faculty which the term invention implies" (Schmookler, p. 45). To others, like David Novick from RAND: "we should stop talking about research and development as though they were an entity and examine research on its own and development as a separate and distinct activity" (Novick, 1965: 13; see also Novick, 1960). [16] Over time, the economists lost their argument: the "D" got into R&D (Godin, 2006a; 2006b). Already in 1960, these beliefs did not prevent many speakers from focusing on development decisions. Thereafter R&D, a category of multiple origins (industrial organization, business schools, and accounting) (Godin, 2005; 2006a; 2006b), became the main one for discussing invention among economists.

The second motivation for concentrating on major inventions was measurement. Measuring economic "magnitude" or usefulness is easier than attempting to measure creativity, said Kuznets (Kuznets, p. 21-22; see also Sanders, p. 65 on novelty as non-

[14] Terms used among the speakers at the conference were many: major/minor (Kuznets, p. 25-26; Peck, p. 280), fundamental/accessory (Machlup, p. 161), important (Schmookler, p. 199; Mueller, p. 324).
[15] On the continuity of such a moral economy today, for example in analyses of high-technology, see Godin, 2008c.
[16] On an early (sociological) analysis of the distinction between research and development, see Jewkes et al. (1958).

measurable). Major invention is tangible and easier to track than the daily minor improvements. This is clearly related to a third motivation, the disciplinary or economic perspective: "we are interested in inventions because and insofar as they contribute to the growth of economic production" (Kuznets, p. 22). Only major inventions have such an effect, so it was thought (contrary to the view of sociologists and their evolutionary framework).

Having defined invention, economists chose to study invention using a framework based on efficiency. As Nelson put it, the conference's papers "reflect the economists' interest in economic growth and in problems of efficiency" (Nelson, p. 7). In order to appreciate the "novelty" of this framework to invention studies, it has to be contrasted to previous frameworks. Until the 1960s, two frameworks existed for the study of invention, one social and the other historical. The first came from sociologists and concerned cultural lags (Ogburn, 1922). Inventions were measured as increasing at an exponential rate, so claimed Ogburn, and as giving rise to social maladjustments or cultural lags. Hence the need to plan and forecast the introduction and the impacts of inventions on society, that is, on family, politics, communication, etc. (Godin, 2010b). The other framework was what came to be called the linear model of innovation. Technological inventions derive from basic research, which gives rise to applied research, and then development (Maclaurin, 1949). It was seen as the task of government and industry to support basic research (Godin, 2006b).

In light of these frameworks, the NBER conference was witness to a shift from sociological and historical to 'classical' economic analysis. Until then, the main analyses of invention among economists came from economic historians. In the early 1940s, W. Rupert Maclaurin from MIT approached the SSRC's Committee on Research in Economic History, itself interested in promoting investigation of the entrepreneur's role in American industry, with a proposal to jointly sponsor an investigation of technological and industrial expansion. Supported by a grant from the Rockefeller Foundation, Maclaurin initiated the first systematic and long-term research program on "The Economics of Technological Change". Under Maclaurin's guidance, MIT's department of economics launched a series of economic studies on technological change that addressed two major problems: determining the principal economic factors responsible for the rate of technological progress in various industries, and determining the conditions in industry that are most conductive to steady technological progress with a minimum of frictional unemployment. By the early 1950s, Maclaurin and colleagues had developed a whole program of research, which led to several publications on various industries such as glass, paper, electricity (lamp) and radio, and which all arrived at similar conclusions (Godin, 2008a). From this program came Maclaurin's sequential model, the first 'linear model of innovation', which he presented at the 1951 SSRC conference (Maclaurin, 1953): pure science, invention, innovation, finance, acceptance (or diffusion).

The NBER conference made a *tabula rasa* of analyses of a 'qualitative' or a historical nature – a lost tradition which would be felt strongly on the field later (see the section on innovation below). [17] As J. Stamp put it long ago, invention "has too mechanical a connotation" (Stamp, 1937: 5); "mechanical and scientific discovery, even in practical form, is not economic wealth until man has learnt to enjoy it in an economic sense" (Stamp, 1929: 120). To the economist, the study of invention is of interest to the historian only: invention is an act of intellectual creativity and "is without importance to economic analysis" (Schumpeter, 1939: 85). Inventions "acquired relevance only as and when the new possibilities were turned into commercial and industrial reality" (Schumpeter, 1939: 9). In fact, no economic historians attended the NBER conference, either as a speaker or commentator (Maclaurin had died the previous year). As a historian, only T. Kuhn, the new 'guru' on the history of science, had an official role in the conference. Certainly, several papers made use of history to document their case (Peck, Enos, Mueller, Nelson). However, this was not history as understood by Usher or Maclaurin, but simply a historical background to prove a hypothesis, or case-study.

"Classical" economic analysis got into the conference through three features. First, economists studied invention in terms of the factors conductive to it. A first series of

[17] In 1953, the NBER had organized a conference on Capital Formation and Economic Growth (NBER, 1955) with a wider diversity of views than the conference in 1960. Among the participants were M. Abramovitz, S. Kuznets, A. P. Usher, W. E. Maclaurin, and W. W. Rostow.

factors concerns basic research (Mueller, Nelson). The interest in basic research was a direct consequence of the then-popular sequential model of innovation. Basic research would become widely discussed as a supply factor in the following decades. A second series of factors dealt with market factors in a classical framework: invention and the substitution of resources (labor and capital) (Fellner; Minasian), invention and the structure of the industry (Peck), and invention and the role of demand (Schmookler). The study of these two series of (supply and demand) factors would proliferate in the following years, and soon give rise to the "push/pull" controversy (Mowery and Rosenberg, 1979; Scherer, 1982a).

A second feature of the classical analysis was the introduction of the concept of information into the study of invention. Certainly, information was already a central category of neoclassical economic theory: it was taken as a given that people have perfect information on the markets, and firms have perfect information on the technology of the time, or on production opportunities. This is the familiar assumption of economic theory concerned with rational order, coordination and equilibrium, and its modern formulation owes its existence to J. R. Hicks, P. Samuelson and G. Debreu. However, studying the economics of information was relatively recent, and was based on the combined influence of philosophy (epistemology), mathematics (cybernetics) and economics (F. Hayek) (Godin, 2010c). Many speakers to the conference, particularly those from RAND, introduced a model of rationality and decision theory under uncertainty to study invention as a process (R&D). [18]

To most economists, invention thereafter came to be discussed in terms of R&D, and the latter became the main indicator for invention. To Nelson, the discussions were early steps toward opening up the "black box" of invention, a task to which economic historian N. Rosenberg would devote himself later on (Rosenberg, 1982; 1994). Nelson suggested that "one would suspect that, in the future, research on the economics of invention will draw intensively on the concepts of information being developed by economists, decision theorists, and mathematicians" (Nelson, p. 14). As a matter of fact, one impact of this

[18]Analysis of risk was not without precedent, but from a different perspective (innovation rather than invention). See Lange (1943) and Strassman (1959).

analysis in the following decades, particularly in K. Arrow's paper [19] as well as a then-recent paper from Nelson (Nelson, 1959b), was a series of analyses of science (or basic research) as information with the characteristics of a public good: uncertainty, non-divisibility and non-exclusiveness (for surveys, see Stephan, 1996; for revisions of the theory, see Nelson, 1989; Dasgupta and David, 1994; Callon, 1994). This gave rise to a whole literature and a series of arguments for policy-makers. This idea of science as information also gave rise to concepts like the knowledge-based economy and the information society, the origins of which go back to F. Machlup and M. Porat.

A third feature of the economic analysis was the introduction of three concepts of political economy into the study of invention and decisions about invention: allocation, balance and efficiency. Allocation of resources refers to the optimal (but utopian, I would add) level of resources a firm or a nation should devote to R&D. Balance concerns the equilibrium between types of research and what level of resources should be assigned to basic research as opposed to applied research (an arbitrary decision). Efficiency is couched in terms of input and output: how to get the most output from the inputs invested, or "achieving an appropriate level of output and a minimum economic cost" as Nelson once put it. In the following decades, efficiency gave rise to studies on invention and rates of return (value-added) – the first such studies came from the speakers at the NBER conference (Griliches, 1958; Mansfield, 1965; Minasian, 1969) – and studies on invention and productivity by the dozen.

Efficiency as input-output was widely discussed at the conference. Most of the papers were concerned with an input-output semantics. [20] As Z. Griliches reported, the conference's focus was "on the knowledge producing industry, its output, the resources available to it, and the efficiency with which they are being used" (Griliches, p. 347). Equally, to Machlup, "the analysis of the supply of inventions divides itself logically into three sections": input, the input-output relationship (the transformation of inventive labour into useful inventions), and output (Machlup, p. 143).

[19] See also Arrow's comments on Mueller's paper.
[20] At the time of the conference, W. Leontief (and his Harvard Economic Research Project) was certainly a forerunner in the use of the semantics in the economics of invention.

The speakers discussed at length how to define and measure inputs and outputs, and what the relationship is between the two. Important doubts were expressed about the possibilities of input and output measurement (problems of causality, problems of lags, and the difficulty of measuring returns) and about the relationship between input and output. "Our economy operates on the belief that there is a direct causal relationship between input and the frequency and extent of inventions", recalled B. S. Sanders from the Patent, Trademark, and Copyright Foundation of George Washington University. "No doubt there is a direct relationship of some kind, but we have no evidence that this relationship does not change" (Sanders, p. 55). [21] Griliches asked the participants "whether an increase in inputs in the knowledge producing industry would lead to more output" (Griliches, p. 349). Machlup's answer was: "a most extravagant increase in input might yield no invention whatsoever, and a reduction in inventive effort might be a fluke result in the output that had in vain been sought with great expense" (Machlup, p. 153).

In every discussion, analyzing efficiency was intimately linked to its measurement. As a matter of fact, mathematics (and formalization) was a fourth feature of the classical economic analysis of invention dominant at the NBER conference and to which we now turn. From the 1960s onward, the measurement of the role of invention in economic growth and productivity becomes a *leitmotif*.

Technological Change

A few years after the conference (1963), a group that called itself Inter-University Committee on the Microeconomics of Technological Change, members of which were A. Conrad, Z. Griliches, E. Mansfield, J. Markham, R. Nelson, M.J. Peck, F.M. Scherer and J. Schmookler, got a $150 000 grant from the Ford Foundation to pursue studies on technological change. It enabled the group of young economists (most of them present at the NBER conference) to meet together from time to time. The meetings helped build a sense of community. The work culminated in a 1966 conference held in Philadelphia, attended by most of the Americans who would work on technological change problems in coming years (Scherer, 2005: 5). The Foundation funding was good investment. As a

[21] For a highly lucid analysis on the same topic at about the same time, see Shapley (1959).

matter of fact, in 1967 the Ford Foundation justified the non-extension of the grant as follows: "the economics of technological change is now a thriving subject", "there are now substantial amounts of research funds", and members of the committee "seem to be well financed". [22]

The study of technological change is based on either one of two approaches. One deals with micro-economics and is concerned, among others, with validating (or invalidating) the late Schumpeterian hypothesis concerning the role of (small and) large firms in technological innovation (or rather R&D). This work has led to a large number of studies (for reviews, see Kamien and Schwartz, 1975; 1982; Cohen, 1995). The other approach focuses on macro-economics: the contribution of technology to productivity and economic growth. This section is concerned with this later tradition, which has been the cherished one at the NBER.

What characterized the study of invention until the 1960s was the many and non-standardized statistics used to measure invention, as was the case during the 1951 conference. At the NBER conference, there were as many statistics used – expenditures on R&D (Minasian, Brozen), labour (Machlup, Worley), patents (Schmookler, Thompson) – and many methodologies – production function (Minasian, Fellner), case-studies (Peck, Mueller, Nelson), and pure theory (Arrow). Very few statistics were said to be without limitations, which led Schmookler to conclude that "no one will dispute that accurate measures of a thing are always better than an uncertain index of it (…). In the meantime, much as we might prefer caviar, we had better settle for plain bread when that is all we get" (Schmookler, p. 78). [23] However, over time one methodology and statistics became dominant as follows – econometrics and the production function.

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[22] Letters from P.E. de Janosi to J.W. Markham, 31 March 1967. I owe copy of this letter to F.M. Scherer.

[23] In every of his thoughts concerned with invention, technological change and innovation (Kuznets, 1951; 1959; 1972; 1974), as well as his contribution to the NBER conference, Kuznets was definitively a pessimist as regards measurement. At the NBER conference, he has been criticized on that count. The NSF representative, H. I. Liebling, accused Kuznets of applying "somewhat more rigorous standards to the R&D series than he does to the national income category we have learned from him" (Liebling, p. 89). To Liebling, "in the construction of any complex set of statistics, attention must be given to its operational requirements in obtaining a successful measure, often requiring the adoption of certain conventions" (Liebling, p. 88).

Increased interest in technological change among economists can be traced back to the years following the Great Depression, when the bicentennial debate on the role of mechanization on employment re-emerged (Fano, 1991; Bix, 2000). [24] There were optimists, like sociologist Ogburn and the economic advisers, and pessimists. In the 1930s, efforts began to be invested into measuring the "dark prophecies" on technological unemployment, as economist D. Weintraub called it (Weintraub, 1937).

The issue of technological unemployment is really at the origins of studies on technological change. Automation and technological unemployment – an issue which would occupy economists for decades – gave rise to a series of theoretical classifications of technologies as to whether they are capital-saving, labour-saving or neutral (Pigou, 1924; Hicks, 1932; Robinson, 1938) and to quantitative analyses of productivity. [25] Changes in labour productivity (defined as output) due to changes in factors of production (input) came to be equated with invention and called technological change: productivity is witness to technology's use in production.

The US Department of Agriculture – concerned about the declining share of agriculture (as opposed to manufacturing) in the economy (US Department of Agriculture, 1940; Hopkins, 1941) –, the US Bureau of Labor Statistics (1931; 1932), the US Work Projects Administration (Weintraub, 1937; Weintraub and Kaplan, 1938; Magdoff et al., 1938; Gill, 1940), and the NBER (Weintraub, 1932; Mills, 1932; 1936; 1938; Jerome, 1934; Altschul and Strauss, 1937) were forerunners in such measurements. The Work Projects Administration has been quite influential. We owe to the Administration the widespread use of the term "technological change". [26] With over sixty studies conducted between 1938 and 1940, among them one on the impact of the depression on industrial laboratories (Perazich and Field, 1940), Weintraub, as director of the project on

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[24] For bits of history on classical and neoclassical economic theories on technological unemployment, see Gourvitch (1940) and US Temporary National Economic Committee (1941).

[25] For an early economic writer framing the problem of technological unemployment in terms of productivity, or ratio input-output, see Douglas (1930a; 1930b) and Douglas and Director (1931: 119-164).

[26] To the best of my knowledge, the term appeared somewhere in the late 1920s, jointly with technological (or technical) progress (or advance or improvement). The first occurrences I know of are: Kuznets, 1929; Douglas, 1930a; US Bureau of Labor Statistics, 1932; Jerome, 1934. At the very beginning, the term had a loose meaning: "when the main technical conditions have been introduced, a fundamental change has taken place" (Kuznets, 1929: 541). Such a loose meaning remains widespread in the literature today.

Reemployment Opportunities and Recent Change in Industrial Techniques, thought, in line with a study he conducted for the NBER in 1932, that measuring labour productivity as a ratio of "quantity output per employee man-year" would answer the question on technology and unemployment: "since the net effects of the underlying economic factors find their quantitative expression in the net changes of the volume of production and employment, a statistical analysis of the relationship between the total volume of goods and services produced in the country and the number of hired workers engaged in the production offers an approach toward a better understanding of the nature of a problem which has come to be referred to popularly as that of technological unemployment" (Weintraub, 1937: 67). To Weintraub, "the unit-labor-requirement ratio indicates changes in man-years employed per unit of total output" (Weintraub, 1937: 72). If the same number of workers or less is required to produce the same level of output or more, it means that technology causes increased productivity (and therefore unemployment).

From the 1960s, technological change, defined as substitution of labour for capital as factors in industrial production, became the economists' category rather than invention. We owe this shift to the use of the production function, an equation invented in the late 1920s (Cobb and Douglas, 1928), and its input-output semantics. The production function links the quantity produced of a good (output) to quantities of input. There are, at any given time, or so economists argue, factors or inputs (labour, capital) available to the firm, and a large variety of techniques by which these inputs can be combined to yield the desired (maximum) output. As E. Mansfield once put it: "The production function shows, for a given level of technology, the maximum output rate which can be obtained from a given amount of inputs" (Mansfield, 1968b: 13).

Using the production function, economists began interpreting movements in the curve as technological change (Schumpeter, 1939; Valavanis-Vail, 1955), while others equated labour productivity with science (technological change is likely to result, all other things being equal, in labour productivity). Early on, the NBER has been quite active in productivity studies (Stigler, 1947; Fabricant, 1954; Abramovitz, 1956; Kendrick, 1961; Brown, 1967) and, from 1979 onward, a NBER Productivity (and Technical Change) Studies Program directed by Griliches would continue this kind of analysis and publish a large volume of working papers. [27] Without doubt, this is one of the most influential contributions of the NBER to the study of technological change.

NBER Conferences
Directly Related to Technological "Innovation"
(and publications, in parentheses)

(1920-2007)

1953, Capital Formation and Economic Growth (1955).
1960, The Rate and Direction of Inventive Activity (1962).
1966, The Technology Factor in International Trade (1970).
1981, R&D, Patents, and Productivity (1984).
1999, The Patent System and Innovation (2001).
2000, Innovation Policy and the Economy, a series of annual meetings (2000-2007).
2003, Hard-to-Measure Goods and Services (2007).
2005, Academic Science and Entrepreneurship (2007).

By the 1960s, productivity, which was considered a measure (proxy) of technological change in the 1930s, came to be seen as an effect of invention: research and development (R&D) as input gives rise to productivity and profitability. Economists began to correlate R&D with productivity measures. What became influential was measuring multifactor productivity (MFP). Invention as a factor in production came to be added to the production function by R. M. Solow (Solow, 1957). Solow formalized earlier works on growth accounting and technology (decomposing GDP into capital and labour), and equated the residual in his equation with technical change – although it included everything that was neither capital nor labour – as "a shorthand expression for any kind of shift in the production function". Integrating invention into the economic equation was thus not a deliberate initiative, but it soon became a fruitful one. In the following years,

[27] See www.nber.org/papersbyprogram. Mention should also be made of four books: H. Jerome (1934), *Mechanization of Industry*; Z. Griliches (1998), *R&D and Productivity: the Econometric Evidence*; A. Jaffe, J. Lanjouw and J. Lerner (2001), *The Patent System and Innovation*; and B. Z. Kahn (2005), *The Democratization of Invention*. Depending on how one defines "innovation", publications on human capital could be included in this list.

researchers began adding variables (factors) into the equation in order to reduce the residual (and better isolate technological change) (Denison, 1962; 1967), and adjusting the input and capital factors to capture quality changes in output (Jorgenson and Griliches, 1967). Since these first calculations, the literature on measuring invention and productivity has grown exponentially, becoming an "industry". [28]

At the NBER conference, J. R. Minasian analyzed invention as such a factor or input to economic growth and productivity and concluded "beyond a reasonable doubt, causality runs from research and development to productivity, and finally to profitability" (Minasian, p. 95). According to Nelson, Minasian's paper presented "encouraging evidence that classical economic theory can be applied fruitfully to inventive activity" (Nelson, p. 8). However, according to many speakers, the production function was "a very unsatisfactory tool of analysis" (Griliches, p. 348). To Griliches, "none of these studies [Minasian, Nelson, Peck, Enos] comes anywhere near supplying us with a production function for inventions": the relationships between input and output "are not very strong or clear" (Griliches, p. 350). In the following decades, the criticisms continued. W. Leontief argued that "elaborate aggregative growth models can contribute very little to the understanding of processes of economic growth, and they cannot provide a useful theoretical basis for systematic empirical analysis" (Leontief, 1970: 132). Mansfield qualified the mathematics behind economists' models as "not strong enough to permit very accurate estimates (…). At best, the available estimates are rough guidelines" (Mansfield, 1972: 478). Similarly, Rosenberg called the production function a "fiction" (Rosenberg: 1976: 63). Twenty-five years later, Griliches concluded again that "the quantitative basis for these convictions [links between investments in science and economic growth] is rather thin", and pleaded for realism (Griliches, 1998: 41).

Econometrics based on the production function was only one aspect of measurement emerging from economists' studies of invention and technological change. The other concerned the development of indicators. The very first lists of indicators on invention and technological change were produced by economists R. C. Epstein (1926), followed

[28] For early surveys of the field, see Brown (1966), Lave (1966) and Nadiri (1970).

by Maclaurin (1953). For example, for each of the steps of his sequential model of innovation, Maclaurin identified a series of measurements. [29] From the 1960s onward indicators came to be organized into an input-output framework, and Machlup was influential here.

At the NBER conference, Machlup argued that the production function was "only an abstract construction designed to characterize some quantitative relationships which are regarded as empirically relevant" (Machlup, p. 155). In its place, he soon suggested collecting indicators. Two years after the conference, Machlup published what was the first collection of multiple statistics on invention, or knowledge as he called it: education, R&D, communication, and information. In his chapter on R&D, Machlup constructed a much-quoted table where a list of indicators on input and output were organized according to stages of research (basic research, applied research, development, innovation) and according to whether they were tangible or intangible, and measurable (Machlup, 1962: 178-79). Machlup's table marked a transition here. From a theoretical and 'abstract construct', the production function became a 'practical' tool: official statisticians would follow Machlup and adapt the semantic on input and output to their efforts at measuring science, the first among them being the US National Science Foundation and the OECD (Godin, 2005). The input-output framework and semantics now had a life of its own, being part of the cult of efficiency. [30]

This kind of analysis was negatively received by economists. In reviewing Machlup's book in *Science*, Nelson expressed his disappointment that, "Machlup is concerned principally with identifying and quantifying the inputs and outputs of the knowledge-

[29] Epstein's indicators are: important mechanical inventions, inventors, pure scientific discoveries, length of time required to bring to commercially successful application, earnings received, patents and trade activity. Maclaurin's indicators are, for Pure science: major contributions, classified by field, country, and over time; prizes, awards and medals; budget; forecasts on commercial applications; for Invention: patents (major/minor); research workers (because they are correlated with the volume of invention); records of inventions by firms; for Innovation: inquiry over time industry by industry on annual sales volume, productivity figures, investments for new/minor products and new firms/established (great) corporations; for Finance (capital supply): number of new firms launched each year, their capital investments; new plant constructed; for Acceptance (or diffusion): growth curves for a wide variety of products and services under different types of conditions, by region, between cultural groups. Length of time required for mass acceptance.

[30] For a reflection on the cult of efficiency at this time, see Bell (1965).

producing parts of the economy and only secondarily with analyzing the function of knowledge and information in the economic system" (Nelson, 1963). Despite the criticism, this kind of analysis and the descriptive statistics became very influential among European researchers.

With indicators, invention came to be discussed as both input and output. In the literature on technological change, invention is an input to output or production. Then, as in studies on the management and evaluation of research, invention is considered as an output itself, an output coming out of input or resources invested in research activities. The assumption here, criticized at length by Sanders and Machlup at the conference, is that of efficiency. Within three decades, a whole array of indicators came to be constructed to measure different dimensions of invention, and these became the kind of (descriptive) measurement used in many studies of technological innovation: indicators of inputs such as money devoted to R&D and human resources involved in research activities; indicators of output like papers, patents and high technology trade; and indicators of impacts like the technological balance of payment (Godin, 2005).

Among this collection of indicators, the money spent on R&D became the most cherished indicator. At the NBER conference, there were few uses of the indicator. However, as Nelson put it at the conference: "the establishment of the National Science Foundation has been very important in focusing the attention of economists on R and D (organized inventive activity), and the statistical series the NSF has collected and published have given social scientists something to work with" (Nelson, p. 4). R&D statistics came to be standardized at the international level in 1962, and remained the only standardized one for three decades (OECD, 1962). Economists would make wide use of R&D statistics in their productivity studies (econometrics), and descriptive statistics on R&D would become the preferred measurement for many researchers in the field of technological innovation studies. The irony is that the statistics includes precisely what some economists considered in 1960 as non inventive activity: two-third of R&D is concerned with development.

Innovation

The most recent series of indicators concerns innovation. The numbers come from national surveys based on the OECD/Eurostat manual, the first edition of which was published in 1992 (OECD, 1991a). As with R&D indicators, innovation indicators emerged from policy considerations – namely governments' concerns with economic growth. There were few discussions on public policy at the NBER conference. Nevertheless, economists' views on invention and technological change from the 1960s onward have had a huge impact on policy: input-output became a policy framework beginning with the very first national accounting exercises on science and technology in the 1960s (Godin, 2007). Conferences were organized by officials on technology and productivity, where economists, among them those from the NBER, participated (US National Science Foundation, 1972; OECD, 1991b). [31] The results of productivity studies by economists were widely cited in policy documents and got into recent OECD analyses on the new economy (Godin, 2004). Productivity became a doctrine in science and technology policy, as can be seen in national and international policy documents (Godin, 2009).

As to policy, there had been no discussion on globalization at the NBER conference – Posner's influential paper was published the year after (Posner, 1961). However, a few years later, the NBER organized a conference on technology and international trade, which again was influential (Vernon, 1970). It contributed, among other things, to launching studies on gaps and convergence between countries, studies which developed particularly following Abramovitz's paper (Abramovitz, 1986).

[31] As example, the OECD organized an international conference on technology and productivity in 1989 where Abramovitz, Griliches, Kendrick and Nelson, among others, participated (OECD, 1991b). The purpose of the conference was to "identify various factors that influence the development, adoption and diffusion of technology and, ultimately, the rate of productivity growth", and particularly to shed light on the productivity (or Solow) paradox: although there was evidence of acceleration of industry's technological efforts in most member countries, this had not yet been reflected in an upturn in productivity. The conference was part of the OECD Technology and Economy Program (TEP) which led to a clear and definitive economic orientation of work at the Directorate for Science, Technology and Industry (DSTI) and to later work on productivity and growth (the New Economy) at the organization in the late 1990s-early 2000.

What about innovation? In retrospect, one could argue that all papers from the 1960 conference were concerned with innovation. But this is a far too simple answer. To a large extent, the answer to this question depends on how one defines innovation. Innovation as a category has a complex story, and I can only offer some bits of this story here (Godin, 2008b). First, and unlike the 1951 conference, the NBER conference was "narrowly focused" on invention (or R&D) as Nelson put it (Nelson, p. 4). It did not study either innovation or diffusion. [32] In fact, the term innovation does not even appear in the index. Furthermore, there are very few references in the papers to Schumpeter, the now-symbolic father of technological innovation studies, or to Maclaurin. Similarly, the many NBER working papers on patents since the 1980s are really concerned with invention, not innovation. As Griliches and others suggested, a patent is invention not innovation (Pavitt, 1985; Griliches, 1990).

Second, at the time of the conference innovation was a contested term among economists. Certainly there was an emerging literature on "induced innovation" (Fellner, 1961; Kennedy, 1964; Samuelson, 1965), [33] but to many economists innovation was too subjective a category. As a matter of fact, those who had studied innovation extensively until then, namely the sociologists, defined it as any idea *perceived as new* by its adopter (Rogers, 1962). This view, among others, led to economists' criticisms and reluctance to use the term. To Machlup, "we shall do better without the word innovation" (Machlup, 1962: 179). To others, the term is in need of a more rigorous definition, because it "has come to mean all things to all men" (Ames, 1961: 371). In the 1970s, the skepticism continued: the "use of the term innovation is counterproductive", claimed the authors of a survey conducted for the US National Science Foundation, because each individual has his or her own interpretation (Roberts and Romine, 1974: 4). [34] At about the same time, a

[32] In 1962, a conference on the economics of research and development held at the Ohio State University had the same focus. Many of the speakers to this conference were present at the NBER conference too: Griliches, Klein, Mansfield, Markham, Nelson, Scherer, Schmookler (Tybout, 1965).

[33] Induced innovation is defined as changes in input prices which induce changes in capital-labor ratios. Many of the issues studied are not really different from those of technological change (innovation is understood as invention used and the method used is econometrics and the production function) and invention (the study of economic factors, in the present case prices). See Binswanger and Ruttan (1978) for a survey.

[34] For early essays at definitions on and distinctions between invention, innovation, and technological change, see Redlich (1951), Ruttan (1959). See also Fores (1970).

debate arose on the costs of innovation. Two researchers (Mansfield, 1971: 118-19; Stead, 1976) refused to accept the numbers coming out of the first US government measurement of technological innovation (US Department of Commerce, 1967) – later surveys would confirm the results. Based on a definition of technological innovation as composed of five categories of activities (R&D, design engineering, tooling and engineering, manufacturing, and marketing), the Department had measured that R&D amounted to only a small fraction (5-10%) of technological innovation costs in industries. The debate is witness to the then economists' downgrading of innovation – as contrasted to R&D.

Third, and despite Stamp's and Schumpeter's distinction between invention and innovation, when the category innovation was used in the economic literature, it was often interchangeably (even in the same paper) with that of invention (as example, see Kamien and Schwartz, 1975). [35] To still others, technological innovation came to be considered synonymous with technological change. As Schumpeter once put it, the production function "describes the way in which quantity of product varies if quantities of factors vary. If, instead of quantities of factors, we vary the form of the function, we have an innovation" (Schumpeter, 1939: 87; see also Lange, 1943 and Brozen, 1951). "Whenever at any time a given quantity of output costs less to produce than the same or a larger quantity did cost or would have cost before, we may be sure, if prices of factors have not fallen, that there has been innovation somewhere" (Schumpeter, 1939: 89). So, is there a difference between technological change and technological innovation other than a semantic one, and what is that difference, if any?

The 1960 speakers who dealt most with innovation in the sense that the category would acquire later in non-mainstream economics were W. F. Mueller and J. L. Enos. Certainly, some others used the term innovation (Minasian, p. 95; Feller, p. 171; Siegel, p. 451). However, the use had no consequence for their analysis. Mueller looked at innovation defined as **commercialized** invention. Mueller was here concentrating on a meaning of

[35] Later, non-discrimination continued between innovation as a thing (new product) and innovation as an action (process). For example, in Kamien and Schwartz (1982), the authors define innovation as a process from basic research to commercialization (p. 2), but use the category for new products on every page.

innovation as products of inventive activities introduced to the market. Innovation as commercialized invention is in fact the second construction that came from economists. After having restricted invention to technological invention and to business practice (which led to the study of technological change), economists turned to the study of innovation defined as (the first) commercialization of a technological invention. [36] This definition would become influential among policy-makers, and turned out to have a spontaneous understanding among the public at large.

Enos for his part followed Maclaurin, preferring to define innovation as a whole **process** from invention to commercialization. [37] Enos in fact produced one of the first economic studies on time intervals, or lags, between invention and commercialized invention (innovation). [38] The study gave rise to the much-quoted sequence invention → innovation → diffusion, as a variant of the sequential model of innovation, [39] and was accompanied by a growing number of economic studies on diffusion (like Manfield's), often called imitation in the economic literature, and studies on gaps between countries in terms of innovating (defined as both inventing and adopting). This was definitely fruitful. Before such sequences, invention and diffusion were seen as opposed (see the diffusion controversy among anthropologists (Smith, 1927).

These authors have had few influences on mainstream economics. The study of innovation (as commercialization of new products) developed elsewhere than among mainstream economists. As a matter of fact, the study of technological change was concerned with inventions as **processes** and the impact of R&D on costs and productivity. However, as early as the 1940s, Maclaurin had offered economists another definition of technological change. Before Maclaurin, the term technological change

[36] To Schumpeter, one man (the entrepreneur) is "the *first* [my italics] to decide on the production of a new consumer good" (Schumpeter, 1939: 131). To Maclaurin, innovation is "the *first* [my italics] commercialization of a new or improved product or process" (Maclaurin, 1953: 105).
[37] For a similar distinction with regard to invention, see Siegel, p. 441-42.
[38] The very first such studies came from sociologist Gilfillan (Ogburn and Gilfillan, 1933; Gilfillan, 1935; 1952; the latter paper was produced for the 1951 conference). Later studies from economists are Posner (1961), Mansfield (1961), Lynn (1966), Mansfield (1968: chapter 4), and Gold et al. (1970).
[39] The use and discussion of similar sequences are to be found in many papers from the conference. Only Siegel makes a case for interaction rather than sequence.

appeared sporadically in the economic literature. As mentioned above, in the late 1930s, the US Works Projects Administration started using the term more regularly to discuss changes in employment due to technology (job displacement). Then, in the early 1940s, Maclaurin gave the term a new meaning concerned with the development of new **products** (commercialized inventions) rather than the use of technical processes in production (the substitution of labour for capital). By the early 1950s, Maclaurin was using both terms, technological change and innovation, as was the case for many speakers at the SSRC conference of 1951, as was also the case at the NBER conference of 1960, and as would be the case in the literature for the following decades, right up to today.

There are really two conceptions or representations of technological "innovation" then, and two traditions in this specialty: that of mainstream economists concentrating on (the adoption or **introduction** of new) processes in industry (technological change), and that looking at (the invention and **commercialization** of new) products (technological innovation). [40] The first representation introduced the study of 'innovation' among economists. It was concerned with innovation as use of invention, like the sociologists and political scientists did – with their own framework. The origins of the invention did not matter: this is why innovation was exogenous (to the economists as well as the sociologists); innovation is adopting or inteoducing (something which exists) not inventing (something new). Technological change is in fact the direct consequence of innovation defined as (introducing) change from the 16th century onward (Godin, 2010a). Technological change is change of techniques in production (new uses).

The second tradition, of which the 1951 conference is an early witness, opened the black-box, considered the creation of these new techniques, and defined innovation as (the first) commercialization of new products. As a matter of fact, an early critic of the first tradition, in criticizing the production function, in a sense identified the source of the difference between the two traditions: the bulk of the research and development effort is

[40] Since the 1930s, products and processes are often discussed in term of a dichotomy. However, one industry's new product often becomes another industry's process (Scherer, 1982b).

not devoted to cost improvement (processes and productivity), claimed W. E.Gustafson, but to new product development (Gustafson, 1962). In the 1930s the US National Research Council had already documented the trend in a survey of industrial R&D (Holland and Spraragen, 1933). Similarly, to Rosenberg, it is the perennial emphasis on the substitution of factors in the production processes that is problematic in the first tradition. The decision to choose labour-saving inventions in industry is not based on price, but on 'history' (Rosenberg, 1969). British historian Maxine Berg, following the footsteps of historians on the "consumer revolution" like McKendrick, Brewer and Plumb (1982), is certainly an original author deserving mention here. His reinterpretation of the industrial revolution suggested to him that "a broader concept of technological change" is required than capital-to-output ratios and productivity change in the industrial sector (Berg, 1991b). Berg thereafter conducted many interesting historical studies on technological innovation as commodity and its diffusion among different social classes (Berg, 1991a; 1998a; 1998b; Berg and Clifford, 1999).

The development of technological innovation studies in the second tradition is due to social researchers (for early reviews, see Kelly et al., 1972; Radnor et al., 1978), of whom non-mainstream economists (Griliches, 1957; Mansfield, 1963; 1968; 1971; Nelson, Peck and Kalachek, 1967) [41] and 'evolutionary' economists (Nelson and Winter and their papers from the 1970s onward; [42] Freeman and his colleagues from SPRU in the 1970s: Freeman, 1971; 1974; SPRU, 1972) form a part. [43] Mention should also be made of early American contributions from the 1940s and 1950s, many of a historical type (the 'MIT

[41] Griliches and Mansfield were concerned with innovation as products, unlike the technological change tradition. However, and until the late 1960s in the case of Mansfield, they were still concerned with use (diffusion) of innovations, the invention of which remains exogenous. For an early analysis of innovation as commercialization of new products from the mainstream economic tradition, see Usher, 1964.

[42] The Nelson-Winter evolutionary tradition began with S. Winter's Yale PhD thesis, published in *Yale Economic Essays*, Spring 1964.

[43] Nelson, Peck and Kalachek (1967) from RAND and Mansfield (1968b) are interesting works because they are witnesses to the "transition" from the first to the second tradition, in this case the consideration of both traditions. Strassman (1959) and Hill and Harbison (1959) are also part of this transition. Although they have defined innovation as use of invention, they have studied the process of innovation inside firms.

school'), [44] as well as the 'Manchester school' (Carter and Williams, 1957; 1958; 1959; Langrish et al. 1972).

In a sense, evolutionary economics is an effect of mainstream economics. In point of fact, its promoters generally contrast their field to that of mainstream economists. While in 1960 Nelson predicted with success the impact of the concept of information on innovation studies, he did not predict the coming of evolutionary economics. The field developed subsequently, mainly in Europe – although Nelson has also been a major author in the field with his 1982 book, among others (Nelson and Winter, 1982).

The evolutionary economics of technological innovation is well known for its emphasis on institutions, among others (Nelson, 1993; Nelson and Sampat, 2001; Nelson, 2008). [45] Another specific of the evolutionary economics of technological innovation is, unlike classical economics, a focus on policy. However, and like mainstream economics, evolutionary economists put growth, productivity, competitiveness and the industry or firm at center stage in the innovation system. To date, evolutionary economics has had few impacts on mainstream economists. It has rather had (some) effects on other social researchers' understanding of technological innovation, as well as on public policy (by way of experts acting as consultants to governments). However, more recently, mainstream economists got into the study of the topics studied in this tradition. New growth theories are the perfect example of such a 'catch-up'. [46] A look at the NBER productivity program and recent working papers also shows a new kind of studies on technological innovation at the National Bureau (see Berndt, 2007). Innovation has become so fashionable a category that no serious researcher, group and discipline can (or should) ignore it.

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[44] A.A. Bright, Y. Brozen, J.L. Enos, B. Gold, W.R. Maclaurin, W.F. Mueller, N. Rosenberg, W.C. Scoville, P. Strassman and some others.

[45] In the 2001 and 2008 papers, Nelson has re-labeled "institutions" as "social technologies". Nelson's "rhetorical move" is motivated (to my mind) by his insistence on trying to persuade mainstream economists to better integrate the role of institutions into economic theories. The term "technology" may appeal to economists more than "institution" does. In the 1970s, S. Kuznets already was using "social technology" and "social innovation" in this sense (Kuznets, 1972; 1974).

[46] To some like Nelson, new growth theories' only novelty on technological innovation is formalization of existing knowledge (Nelson, 1994; 1997; 1998).

In the end, the history of innovation studies is that of two contributions: that of mainstream economists and that of social researchers, including evolutionary economists. Part II of this paper will deal with the latter. It is far too easy to contrast and oppose the two perspectives. It is far too simple to forget one while writing the history of the other. The concepts, the frameworks and the measurements attest to a common background.

Conclusion

Two years after the NBER conference, another conference on the economics of invention was held at the Ohio State University. The book that came out of this conference elected *The Rate and Direction of Inventive Activity* as "the most recent definitive compendium" and claimed that "in less than a decade, the subject of economics of technological change has come into its own as a research area" (Tybout, 1965: 4). The NBER conference of 1960 was without doubt a moment of historical significance to technological innovation studies. It was one of the first conferences on the subject. It gathered together many speakers who were or would soon become central to the field. It discussed a diversity of approaches, some of which were quickly forgotten but others regularly cited (Nelson) and discussed (Arrow). Among these approaches, one got major attention among mainstream economists: econometrics and the production function. The NBER has certainly been a major contributor to this approach through its studies on productivity. Finally, the study of technological change has had a major impact on public policy. In sum, the conference contributed to launch the systematic study of innovation among economists.

It took two years to organize the NBER conference (1958-60). There were definitely some 'biases' and tensions during this period. One concerns sociology, sociologists and the social aspects of invention. The SSRC, through the Kuznets/Schmookler report, minimized sociology's contribution to the field and its participation in the conference. [47] Schmookler suggested that there had been little research on the sociology of science (R.

[47] J. Schmookler, Preliminary Report to the Committee on Economic Growth on Proposed Conference on Science, Technology, and Economic Growth. The Rockefeller Archive Center: Social Science Research Council Archives, Accession Two, Series 1.21, Box 148, Folder 1685.

K. Merton, B. Barber). On the other hand, he admitted that "sociological work centered around the problem of 'cultural lag' [W. F. Ogburn's work] is [voluminous,] too voluminous even to attempt to list". He also suggested that "the subject of innovation and diffusion has been a subject of concern to social scientists for many years". Hence he recommended "only one paper on the topic" – and in the end, there has not been even one. J. Jewkes and his colleagues from Great Britain were the sociologists then working closest to the topic of the conference. They were known to Schmookler, but were absent from the conference. In fact, the conference turned out to be an all-American conference. Another source of biases is history. Schmookler completely ignored economic history: in his review of the literature, no mention is made of Maclaurin. To Schmookler, the history of invention is the history of science (Kuhn; C. Gillespie) [48] and the history of technology (Usher; L. White). Consequently, the Kuznets/Schmookler list of suggested speakers did not include Maclaurin.

Thus, the SSRC paved the way for restricting the topics of the conference. It remained to deal with the suggestions from the NBER (phantom) committee itself, particularly the members from RAND. In a 1963 letter on the financial contribution of the SSRC to the publication of the papers from the conference, addressed to W. J. Carson, Executive Director of the NBER, P. Webbink, Vice-president, SSRC, suggested that "the conference had turned out to be much more relevant to the latter committee [NBER]'s interests than to those that our committee had had in mind". [49] A copy of the letter was sent to Kuznets, which shows that the economist most probably agreed with the content and may even have suggested the idea to Webbink. It remains difficult to determine what exactly made the SSRC critical of the conference. It was probably not the lack of focus on the social and sociological aspects of invention, topics naturally of concern to an organization concerned with the social sciences. Kuznets, as the SSRC representative, was consciously responsible for this bias. What about the place of 'pure theory', like Solow's work? Although "what has been said on the subject so far is of dubious value",

[48] Early on, Schmookler asked the SSRC committee on the History of Science, chaired by R. Shyrock, for advice on its activities. Letter from J. Schmookler to P. Webbink, 29 July 1958. The Rockefeller Archive Center: Social Science Research Council Archives, Accession Two, Series 1.21, Box 148, Folder 1685.
[49] Letter from P. Webbink to W. J. Carson, January 11, 1963. The Rockefeller Archive Center: Social Science Research Council Archives, Accession Two, Series 1.21, Box 148, Folder 1685.

as Schmookler wrote, he recommended the topic as one of particular importance to the conference. To Schmookler, Solow's participation in the conference "would be very valuable". Unfortunately, Solow did not attend the conference, and neither did P. Samuelson, whose name was suggested by Solow. As a result, little place was left for pure economic theory at the conference.

The SSRC criticism of the conference might also have to do with the lack of focus on the study of innovation – what Kuznets called "a consideration of covering the stages (he also talks of a "spectrum") of economic innovation and effects on economic growth and production". In Kuznets' thinking, and despite direct talks with Hitch to urge the latter to change his mind on the agenda of the conference, RAND researchers may have succeeded in conceiving a program "too narrowly in terms of programming R&D etc. and policy, whether business or government". [50] This reflected perfectly the RAND's research agenda, which was devoted to R&D issues, costing studies and operations research. [51] A look back at Nelson (1959a) (at RAND at the time), and a comparison of his introductory paper to the proceedings, clearly demonstrates the same scope ("this paper is not concerned with innovation, it is concerned only with how inventions occur", p. 102), the same topics (factors leading to invention, information, uncertainty and parallel invention, organization and management of R&D), and a "concern" which gave its title to the conference ("an economic theory of invention should help to explain and predict *the rate and direction of inventive effort* [my italics]", p. 106). [52]

In the end, the NBER conference was not the affair of a NBER-SSRC collaboration, but rather that of a triumvirate – NBER, SSRC and RAND – and contributed to settling the fate of a fourth group: the economic historians. The latter, of which two researchers were

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[50] Letters from S. Kuznets to J. Schmookler, October 20, 1958. The Rockefeller Archive Center: Social Science Research Council Archives, Accession Two, Series 1.21, Box 148, Folder 1685.

[51] Kutznets's views may explain why Hitch ended up as a commentator at the conference. In view of the number of speakers coming from RAND, Hitch may have chosen not to attend the conference as a speaker. Four out of twenty-two papers delivered at the NBER conference were from RAND, the authors being: K. J. Arrow, B. H. Klein, A. W. Marchall, W. H. Meckling, J. R. Minasian, and R. R. Nelson. To this list, T. Marschak could be added for having worked with RAND before joining the University of California.

[52] The "rate" of invention was also discussed at the 1951 conference and the term as such appeared in Kuznets' letter (November 1949) accompanying the memorandum of suggested topics. For early papers on the rate of invention, see: Merton (1935) and Stafford (1952).

then central to the study of invention and innovation (Usher and Maclaurin) and elected as members of the NBER committee of the conference early on, had lost any chance at a possible role from the very beginning of the discussions. [53] According to Kuznets, (mainstream) economists lost too. They perhaps did, but only temporarily.

[53] And really felt they had lost it. On Maclaurin's psychological state of mind in the late 1950s, see Godin (2008a).

Papers Presented at the Conference on "Quantitative Description of Technological Change"

(1951)

J. Schmookler (Michigan State College), Inventive Activity, Technical Knowledge and Technical Change as Seen through Patent Statistics.

Alfred B. Stafford (University of Wyoming), An Appraisal of Patent Statistics.

W. Rupert Maclaurin (Massachusetts Institute of Technology), The Sequence from Invention to Innovation, With Emphasis on Capital Supply and the Entrepreneur.

S. Colum Gilfillan (University of Chicago), The Lag Between Invention and Application.

Anne P. Grosse (Harvard University), Innovation and Diffusion.

Yale Brozen (Northwestern University), Invention, Innovation and Diffusion.

Ansley J. Coale (Princeton University), The Measurement of Changes in Industrial Processes.

W. Duane Evans (Bureau of Labor Statistics), Index of Labor Productivity as a Partial Measure of Technological Change.

Gerard Debreu (Cowles Commission for Research in Economics), Effects of Technological Change on Production Potential.

Wassily W. Leontief (Harvard University), Structural Change.

Joseph L. Fisher (Council of Economic Advisers), Natural Resources and Technological Change.

Simon Kuznets (University of Pennsylvania), Ratio of Capital to Product and Technological Change.

William M. Capron (University of Illinois), Changes in Household Equipment as a Partial Measure of Technological Change.

Papers Presented at the Conference on "The Rate and Direction of Inventive Activity"

(1960)

S. Kuznets (Harvard University), Inventive Activity: Problems of Definition and Measurement.

B. S. Sanders (George Washington University), Some Difficulties in Measuring Inventive Activity.

J. R. Minasian (RAND), The Economics of Research and Development.

F. Machlup (Princeton University), The Supply of Inventors and Inventions.

W. Feller (Yale University), Does the Market Direct the Relative Factor-Saving Effects of Technological Progress?

J. Schmookler (University of Minnesota), Changes in Industry and the State of Knowledge as Determinants of Industrial Invention.

J. S. Worley (Vanderbilt University), The Changing Direction of Research and Development Employment Among Firms.

W. R. Thompson (Wayne State University), Locational Differences in Inventive Effort and Their Determinants.

Y. Brozen (University of Chicago), The Future of Industrial Research and Development.

M. J. Peck (Harvard University), Inventions in the Postwar American Aluminum Industry.

J. L. Enos (MIT), Invention and Innovation in the Petroleum Refining Industry.

W. F. Mueller (University of Wisconsin), The Origins of Basic Inventions Underlying Du Pont's Major Product and Process Innovations, 1920 to 1950.

D. W. Mackinnon (University of California), Intellect and Motive in Scientific Invention: Implications for Supply.

A. H. Rubenstein (Northwestern University), Organization and Research and Development Decision: Making Within the Decentralized Firm.

P. W. Cherington, M. J. Peck and F. M. Scherer (Harvard University), Organization and Research and Development: Decision Making Within a Government Department.

R. S. Merrill (University of Minnesota), Some Society-Wide Research and Development Institutions.

I. R. Siegel (George Washington University), Scientific Discovery and the Rate of Invention.

A. W. Marshall and W. H. Meckling (RAND), Predictability of the Costs, Time, and Success of Development.

B. H. Klein (RAND), The Decision Making Problem in Development.

T. A. Marschak (University of California), Strategy and Organization in a System Development Project.

R. R. Nelson (RAND), The Link Between Science and Invention: the Case of the Transistor.

J. W. Markham (Princeton University), Inventive Activity: Government Controls and the Legal Environment.

K. J. Arrow (RAND), Economic Welfare and the Allocation of Resources for Invention.

Commentators (with text)

S. C. Gilfillan **(then at the University of Chicago?)**
H. I. Liebling (National Science Foundation).
E. Mansfield (Carnegie Institute of Technology).
C. J. Hitch (RAND).
Z. Griliches (University of Chicago).
T. S. Kuhn (University of California).
J. J. Spengler (Duke University).

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