The Value of Science:
Changing Conceptions of Scientific Productivity, 1869-*circa* 1970.

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Abstract

Productivity has become a kind of buzzword in science studies. Whether you look at the literature on research management, the economic literature on technology and innovation, the literature on bibliometrics or the official literature on science policy and its conceptual frameworks, what you find is analyses on productivity, often accompanied by a plea and recipes for increased productivity. This paper documents how the concept of productivity got into the analysis of science, through the statistics on which the concept rested, and its transformation over one hundred years. It argues that, through history, the concept as applied to science carried four meanings: productivity as reproduction, productivity as output, productivity as efficiency, and productivity as outcome.

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Introduction

Productivity has become a kind of buzzword in science studies. Whether you look at the literature on research management, the economic literature on technology and innovation, or the literature on bibliometrics, what you find is analyses on productivity, often accompanied by a plea and recipes for increased productivity. Policy documents and frameworks share the same "bias". Since the early 1990s and the OECD's program on technology and the economy, the organization's literature on science and innovation policy carries productivity as main objective and yardstick. [1] Similarly, the current European Union's innovation strategy, as well as the European Commission literature on the knowledge-based economy, is totally linked to a rhetoric on gaps in productivity between European countries and the United States. [2]

What is more surprising than the mere quantity of the literature on productivity is to find fields that are not necessarily "economics-oriented" and that are wholly devoted to studying "pure" academic activities (publications) to be concerned largely with the issue of productivity. Bibliometric studies are mostly devoted to analyzing researchers' scientific productivity, ranking universities in terms of scientific productivity and analyzing factors such as the size of groups and institutions and the role of size on scientific productivity.

What this whole literature shares, beyond the idea of productivity, is statistics. In fact, today one would not imagine a discussion on productivity that does not rely on statistics

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[1] OECD (1992), *Technology and the Economy: the Key Relationships*, Paris.

[2] Commission of the European Communities (2003), *Investing in Research: an Action Plan for Europe*, COM (2003) 226, Brussels; Commission of the European Communities (2004), *European Innovation Scoreboard 2004*, SEC (2004) 1475, Brussels; European Commission (2005), *Key Figures 2005*: *Towards a European Research Area*, Luxembourg.

to measure the concept. The concept of productivity arose totally from statistics, and is defined by way of statistics. What the concept really means, and how it got into analyses of science itself, is probably unknown to most of us. How did an activity such as science, long reputed to be not analyzable in “economic” terms, come to be extensively studied in terms of productivity?

This paper documents the emergence of the concept of productivity, as applied to science, through the statistics on which it was based, and its transformation over one hundred years. It argues that the concept came from (social) scientists and their efforts at promoting the progress of civilization and the advancement of science. With time, the concept of productivity moved from a conception centered on the science system itself, or the reproduction of men of science and their outputs, to a conception where economic considerations external to the system took preeminence. This occurred in four steps, and the social context was responsible for the use and transformation of the concept.

The first part documents the very first use of the concept as applied to science. It comes from the British scientist Francis Galton (1822-1911) and his eugenics program: improving the race in the name of civilization. To Galton, men of science were part of the group of eminent men on which civilization rested, and every effort should be devoted to increase the fertility of families of men of science. Productivity as reproduction, or perpetuation of the stock, as I name this first use, guided the very first systematic efforts at measuring science. The American James McKeen Cattell (1860-1944), pioneer of scientometrics, devoted over thirty years of his life to the advancement of science by collecting statistics and measuring the productivity of nations in terms of men of science.

In these efforts, Cattell, as a psychologist, was seconded by his peers. In order to contribute to the advancement of psychology as a science, psychologists made the first systematic use of counting scientific papers in history, doing so from 1903 onward. The statistics served the rhetoric on scientific progress or productivity in psychology: measuring what was produced. It gave birth to a new field, bibliometrics. The second part of this paper is concerned with documenting this second use of the term: productivity as production of a “good” (publications), or output.

In the 1940s and 1950s, new kinds of “statisticians” appeared. Scientists now shared their efforts at constructing statistics on science with officials: government departments and national bureaus of statistics. The focus and the measurements were no longer on men of science and their scientific activities. Officials were rather interested in what they got out of money invested in science (value for money). They therefore multiplied the statistics available, and integrated them into a framework linking what was called inputs to outputs. The third part documents the emergence of this accounting framework as the emblem of a conception of scientific productivity centered on productivity as efficiency.

The last part looks at the shift from issues and statistics regarding scientific productivity *per se*, or productivity in science or the science system, to the impact of science on economic productivity. This meaning I call productivity as outcome. Science was now solicited for contributing to economic growth and productivity. This focus on productivity as outcome comes from economics and its earliest efforts at integrating science into the economic equation *via* a model called the production function.

Productivity as Reproduction

Statistics on science emerged in the nineteenth century in a context where issues about the decline of the race and of civilization were being widely discussed: great men were not reproducing enough. [3] Many authors devoted themselves to the study of genius and its sources (heredity or environment), because of the contribution of genius to “civilization”. Also, many believed that the stock and, above all, the quality of populations was declining because the “unfit” were reproducing at a greater rate than the

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[3] D. J. Kevles (1985), *In the Name of Eugenics*, Cambridge: Harvard University Press, chapters 5 and 6. On demography and degeneration, see: J.C. Waller (2001), Ideas of Heredity, Reproduction and Eugenics in Britain, 1800-1875, *Studies in the History of Biological and Biomedical Sciences*, 32 (3), pp. 457-489; R.A. Soloway (1990), *Demography and Degeneration: Eugenics and the Declining Birthrate in Twentieth-Century Britain*, Chapel-Hill: University of North Carolina Press; D. Pick (1989), *Faces of Degeneration: A European Disorder, c.1848-c.1918*, Cambridge: Cambridge University Press.

professional classes, from which most eminent men came. One solution was that imagined by the British statistician Francis Galton: eugenics, or the improvement of “the conditions under which men of a high type are produced”. [4] To Galton, “civilization is the necessary fruit of high intelligence”. [5] “The qualities needed in civilized society are, speaking generally, such as will enable a race to supply a large contingent to the various groups of eminent men (…)” (p. 393). From 1865 onward, Galton would study heredity and its role in “intelligence” and genius. “Much more care is taken to select appropriate varieties of plants and animals for plantation in foreign settlements, than to select appropriate types of men” (pp. 40-41), claimed Galton. “A man’s natural abilities are derived by inheritance, under exactly the same limitations as are the form and physical features of the whole organic world (…). It would be quite practicable to produce a highly-gifted race of men by judicious marriages during several consecutive generations (…). [But] social agencies of an ordinary character, whose influences are little suspected, are at this moment working towards the degeneration of human nature” (p. 45). “If we could raise the average standard of our race only one grade”, suggested Galton, “what vast changes would be produced! The number of men of natural gifts equal to those of eminent men of the present day, would be necessarily increased more than tenfold” (p. 398).

Galton was the first to quantify the “decline” in civilization. In *Hereditary Genius* (1869), he looked at family histories of judges, statesmen, commanders, literary men, men of science, poets, musicians, painters and divines. From biographical dictionaries, he chose 300 families containing nearly 1,000 eminent men (977), of whom 415 were illustrious. Galton found that eminent men came generally from eminent families, but he estimated that Great Britain did not produce enough of these men. He arrived at this result by developing a scale of ability (or intelligence) based on statistical laws, and estimating the distribution of intelligence in the population. To Galton, there were only 233 British eminent men for every one million population, while “if we could raise the average standard of our race one grade” there would be 2,423 of them (p. 398). Similarly for

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[4] F. Galton (1883), *Inquiries into Human Faculties and its Development*, London: Dent and Dutton, p. 44.

higher degrees of intelligence: “All England contains only six men between the age of thirty and eighty, whose natural gifts exceed class G; but in a country of the same population as ours, whose average was one grade higher, there would be eighty-two of such men; and in another whose average was two grades higher no less than 1,355 of them would be found” (p. 399). Briefly stated, fertility, or what Galton called the “productiveness” (p. 36) of eminent families, was too low.

To Galton, men of science were part of the group of highly intelligent people he valued so much for the progress of civilization. He studied this group on three occasions. In *Hereditary Genius*, Galton had calculated that the chance of kinsmen of illustrious men rising or having risen to eminence is, on average, 1 out of 6. Regarding men of science specifically, he found that one-half have one or more eminent relations: “to every 10 illustrious men, who have any eminent relations at all, we find 3 or 4 eminent fathers, 4 or 5 eminent brothers, and 5 or 6 eminent sons” (p. 378). Men of science were thus exceptionally productive of eminent sons.

A few years later, in *English Men of Science* (1874), Galton deplored the fact that the social conditions did not allow men of science to reproduce. He sent a questionnaire to 180 British men of science and questioned its respondents on four aspects, among them their antecedents. Using one hundred returned questionnaires, Galton measured that men of science had less children than their parents, a “tendency to an extinction of the families of men who work hard with the brain” (p. 37), “a danger to the continuance of the race” (p. 38). To Galton, “science has hitherto been at a disadvantage, compared with other competing pursuits, in enlisting the attention of the best intellects of the nation, for reasons that are partly inherent and partly artificial” (p. 258). There is a “tendency to abandon the colder attractions of science for those of political and social life (…). Those who select some branch of science as a profession, must do so in spite of the fact that it is less remunerative than any other pursuit” (p. 258-9). To Galton, “the possession of a strong special taste [for science] is a precious capital, and that it is a wicked waste of

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[5] F. Galton (1869), *Hereditary Genius: an Inquiry into Its Laws and Consequences*, Honolulu, University Press of the Pacific, 2001, p. 392.

national power to thwart it ruthlessly by a false system of education” (p. 196). Tastes “are as much articles of national wealth as coal and iron” (p. 223).

In 1906, Galton looked at men of science again. *Noteworthy Families* was “to serve as an index to the achievements of those families which [have] been exceptionally productive of noteworthy persons” (p. ix). [6] Galton sent a questionnaire, again, to 467 men of science and received 207 replies. He kept 100 completed returns for statistics, corresponding to 66 families. Galton found, again, that “a considerable proportion of the noteworthy members in a population spring from comparatively few families” (p. ix). He estimated this proportion of noteworthy persons to the whole population as 1 to 100 (p. xx). The main result of his study, however, was a lessening of the population of noteworthy men. Galton observed 207 noteworthy members in the families, as opposed to a statistical expectation of 337 (pp. xxxix-xl).

The term Galton selected to name his measurements was productiveness and, later, productivity. The term was used for the number of children arising out of marriages (fertility), [7] the families exceptionally productive of noteworthy persons [8] or offspring of high talent, [9] the number of eminent men coming out of different schools (universities), [10] the number of great men in different periods, [11] and the number of men of science a nation produces. [12]

The concept of productivity has three characteristics here. First, it refers to the production, or reproduction of men, in a family or a nation. Second, it is measurable, and takes the form of either a number (N), or a ratio like:

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[6] F. Galton and E. Schuster (1906), *Noteworthy Families (Modern Science): An Index to Kinships in Near Degrees between Persons Whose Achievements Are Honourable, and Have Been Publicly Recorded*, London: John Murray.

[7] F. Galton (1869), Hereditary Genius, *op. cit*. p. 36.

[8] F. Galton and E. Schuster (1906), Noteworthy Families, p. ix.

[9] F. Galton (1901), The Possible Improvement of the Human Breed under the Existing Conditions of Law and Sentiment, *Nature*, 64 (1670), October 31, pp. 664.

[10] F. Galton (1874), English Men of Science, *op. cit*. p. 67.

[11] *Ibid*. p. 227.

[12] F. Galton (1873), On the Causes Which Operate to Create Scientific Men, *Fortnightly Review*, March, 19, p. 347.

N/Kinship
N/Population

In the case of science, for example, productivity meant the number of men of science a nation produces: “the different nations vary at the different epochs in their scientific productiveness”. [13] The third characteristic of the concept is allowing comparisons. The numbers were generally computed in order to compare groups, social classes or nations. The general idea behind the concept is that of the ability of a race, particularly its members of a higher type, to reproduce itself in sufficient numbers to maintain or increase culture or civilization: “the possibility of improving the race of a nation depends on the power of increasing the productivity of the best stock”. [14]

This idea of productivity was present in other measurements on science conducted at the time. While Galton was working on *Hereditary Genius*, the Swiss biologist Alphonse de Candolle published, partly as a critique to Galton’s thesis on heredity, a book on the social factors affecting the development of science. [15] This book considerably influenced Galton because de Candolle argued, contrary to Galton, for nurture, not nature. [16] De Candolle concentrated on foreign members of three Academies (Paris, London and Berlin) over the period 1666-1869, that is, “men from whom publications have influenced scientific progress most” (my translation). De Candolle justified his choice of such a select group of men as follows: “le nombre de titulaires [foreign members] est ordinairement limité, d’où il résulte une succession de comparaisons d’autant plus sérieuses qu’il y a moins de places à pourvoir” (p. 12). De Candolle was mainly interested in the causes of scientific “productivity”. Most of his analysis of these causes

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[13] *Ibidem*.

[14] F. Galton (1901), The Possible Improvement of the Human Breed under the Existing Conditions of Law and Sentiment, *op. cit*., p. 663.

[15] A. de Candolle (1873), *Histoire des sciences et des savants depuis deux siècles, d’après l’opinion des principales académies ou sociétés scientifiques*, Paris: Fayard, 1987.

[16] “I undertook” said Galton, “the inquiry of which this volume is the result, after reading the recent work of de Candolle (…). It so happened to me that I myself had been leisurely engaged on a parallel but more extended investigation – namely, as regards men of ability of all descriptions”. F. Galton (1874), English Men of Science, *op. cit*., p. v.

was qualitative (socio-historical). He discussed eighteen causes, among them heredity, education, religion, family, values, government and institutions, culture, and language. But he also produced several descriptive statistics on foreign members by discipline (including social sciences) and epoch, and statistics on the national and social origins of men of science. Above all, de Candolle calculated ratios of men of science divided by total population to compare nations in terms of "productivity" (pp. 159-187). De Candolle used terms like "répartition" and "proportion" (share) rather than productivity or productiveness, but the idea of a ratio to the total population and quantitative comparisons between countries was fundamental to his results. [17] He found that small countries, above all Switzerland, were first in terms of foreign members in scientific societies over the entire period he studied.

Galton criticized de Candolle's work on several grounds, [18] among them on his measurement of the productivity concept. To Galton, de Candolle's "tables of the scientific productiveness per million, of different nations at different times, are affected by a serious statistical error. He should have reckoned per million of men above fifty, instead of the population generally" (p. 347, footnote 2). To Galton, 50 is an "age sufficient to enable [men] to become distinguished" (p. 348). Admittedly, in *Hereditary Genius*, Galton confined his analysis to the proportion of men who were over 50 years of age because a "man must outlive the age of fifty to be sure of being widely appreciated" (p. 51). This definition allowed him to exclude notoriety by a single act, and to focus on a man who maintains his position in time or "has distinguished himself pretty frequently either by purely original work, or as a leader of opinion" (p. 51).

Galton gave rise to a whole literature concerned with measuring men of genius and the role of eminent men in civilization, [19] then intelligence, or IQ. Above all, the concept of

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[17] De Candolle also used metaphors on productivity as reproduction, using expressions like "cities that gave birth to large numbers of foreign members" (p. 166).

[18] De Candolle "has collected next to nothing about the relatives of the people upon whom all his statistics are founded". F. Galton (1873), On the Causes Which Operate to Create Scientific Men, *op. cit.*, p. 346.

[19] C. Lombroso (1891), *The Man of Genius*, London: Walter Scott; A. Odin (1895), *Genèse des grands hommes: gens de lettres français modernes*, Paris: Welter; J. M. Cattell (1903), A Statistical Study of Eminent Men, *Popular Science Monthly*, February, pp. 359-377; H. Ellis (1904), *A Study of British Genius*, London: Hurst and Blackett; P. Jacoby (1904), *La sélection chez l'homme*, Paris: Félix Alcan; F. Adams

productivity and the statistical comparisons it allowed gained widespread acceptance in science. The first user was James McKeen Cattell, student of Galton and pioneer of scientometrics. In 1906, Cattell, an American psychologist (at Columbia University) and editor of *Science* for fifty years (1895-1944), published the first edition of his directory of scientists entitled *American Men of Science.* [20] The directory contained biographical information on thousands of men of science in the United States: name with title and university, department, place and date of birth, education and degrees, positions, honorary degrees and other scientific honors, membership in scientific and learned societies, subjects of research. This first edition contained 4,000 biographical sketches, restricted to those men "who have carried on research work" and "contributed to the advancement of pure science" (natural science). Cattell envisaged two uses for the directory. [21] The first was to study the productivity of men of science in the country (quantity) and their performance (quality). The second motive examines "the old question of the relative contribution of heredity and environment".

As with Galton, Cattell was preoccupied with the state of civilization. To Cattell, "the progress to our present civilization may have depended largely on the comparatively few men who have guided it, and the civilization we hope to have may depend on a few men (…). If we can improve the stock by eliminating the unfit or by favoring the endowed – if we give to those who have and take away from those who have not even that which they have – we can greatly accelerate and direct the course of evolution. If the total population, especially of the well endowed, is larger, we increase the number of great men" (p. 377). [22] To Cattell, the future progress of civilization depended entirely on science: "the entire development of our civilization is due to the applications of science"

Woods (1906), *Mental and Moral Heredity in Royalty: A Statistical Study in History and Psychology*, New York: Holt; S. Nearing (1914), The Geographical Distribution of American Genius, *Popular Science Monthly*, 85, pp. 189-199; S. Nearing (1916), The Younger Generation of American Genius, *Scientific Monthly*, 2 (1), pp. 48-61; E. L. Clarke (1916), *American Men of Letters: Their Nature and Nurture*, New York: Columbia University; E. Huntington and L. F. Whitney (1927), *The Builders of America*, New York: William Morrow.

[20] J. M. Cattell (1906), *American Men of Science: A Biographical Directory*, New York: The Science Press.

[21] J. M. Cattell (1903), Homo Scientificus Americanus, *Science*, 17 (432), April 10, pp. 561-570.

[22] J. M. Cattell (1903), A Statistical Study of Eminent Men, *op. cit.*

(p. 568), [23] he stated. "The rewards of science are queerly out of proportion to what science has accomplished for human welfare" (p. 569).

Cattell's statistics were entirely developed for contributing to the advancement of science. At the beginning of the twentieth century, science in the United States was perceived as lagging Europe in terms of basic research and opportunities. [24] Direct funding of men of science, by way of privately funded philanthropy, was just beginning; industrial laboratories that could hire or consult men of science were few; there was little government support for university research. Men of science often analyzed these trends in terms of a teaching/research dichotomy: men engaged in research "do not on the average devote more than half their time to it", estimated Cattell. [25] Generally speaking, "a man must be regarded as an amateur in work to which he does not devote more than half his time". [26] To Cattell, men of science had no real opportunities that would allow them to devote their time to research and contribute to scientific productivity. "It seems to me", said Cattell, "that scientific men suffer in character because they are employees rather than free men. We are not permitted to follow our chosen leaders, but men are placed in authority over us. We are paid to teach or the like; our scientific work must be done almost clandestinely (…)". [27] To Cattell, these conditions were detrimental to scientific productivity. The advancement of science…and of the scientific profession therefore became Cattell's leitmotif, and statistics his precious tool to this end: "It is surely time for scientific men to apply scientific method [statistics] to determine the circumstances that promote or hinder the advancement of science" (p. 634). [28]

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[23] J. M. Cattell (1922), The Organization of Scientific Men, *The Scientific Monthly*, June, pp. 568-578.

[24] For representatives of this rhetoric, see: S. Newcomb (1874), Exact Science in America, *North American Review*, 119, pp. 286-308. See also: S. Newcomb (1902), Conditions Which Discourage Scientific Work in America, *North American Review*, 543, pp. 145-158; R. A. Millikan (1919), The New Opportunity in Science, *Science*, 50, 1291, September 26, pp. 285-297. For a criticism of the rhetoric, see: N. Reingold (1971), American Indifference to Basic Research: A Reappraisal, in N. Reingold (ed.), *Science: American Style*, New Brunswick and London: Rutgers University Press, 1991, pp. 54-75.

[25] J. M. Cattell (1910), A Further Statistical Study of American Men of Science, *Science*, 32 (827), November 4, p. 633.

[26] J.M. Cattell (1917), Our Psychological Association and Research, *op. cit.*, p. 281

[27] J. M. Cattell (1903), Homo Scientificus Americanus, *op. cit.* p. 570.

[28] J.M. Cattell (1910), A Further Statistical Study of American Men of Science, *op. cit*.

Cattell produced the world's first systematic series of statistics on scientific productivity in science. He published regular statistical analyses for thirty years on the demography, geography, and what he called the performance of scientists. [29] Where he innovated with regard to the measurement of productivity was in extending the measurement from nations to states, cities and institutions: [30] comparing American states, cities and universities in terms of both absolute and relative (per million population) numbers of men of science.

Cattell's first statistical study appeared in 1906. [31] He looked at the geographical origins of scientific men (birthplace) and their present position (residence). He found concentration of origins in a few states and cities. Massachusetts and Boston were identified as the most productive centers of the country. The distribution of men of science by residence revealed the same concentration. Here, Cattell developed a method for evaluating gains and losses of regions based on comparing numbers on birth and numbers on residence: if a state produced 100 men of science (place of birth) but retained only 80 of them (place of residence), then it had lost 20 to other states (mobility). Cattell's estimates showed that large centers like Massachusetts and New York maintain their position (p. 736), and that Washington and California gain (p. 735), but that the South "remains in its lamentable condition of scientific stagnation (p. 736)". Cattell also found concentrations in a few cities: three-fourths of scientific men lived in 39 places. To

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[29] J. M. Cattell (1906), A Statistical Study of American Men of Science: The Selection of a Group of One Thousand Scientific Men, *Science*, 24 (621), November 23, pp. 658-665; J. M. Cattell (1906), A Statistical Study of American Men of Science II: The Measurement of Scientific Merit, *Science*, 24 (622), November 30, pp. 699-707; J. M. Cattell (1906), A Statistical Study of American Men of Science III: The Distribution of American Men of Science, *Science*, 24 (623), December 7, pp. 732-742; J. M. Cattell (1910), A Further Statistical Study of American Men of Science, *Science*, 32 (827), November 4, pp. 633-648; J. M. Cattell (1910), A Further Statistical Study of American Men of Science II, *Science*, 32 (828), November 11, pp. 672-688; J. M. Cattell (1915), Families of American Men of Science: Origin, Heredity and Performance, *Popular Science Monthly*, May; J. M. Cattell (1917), Families of American Men of Science II: Marriages and Number of Children, *Scientific Monthly*, 4 (3), March, pp. 248-262; J. M. Cattell (1917), Families of American Men of Science III: Vital Statistics and the Composition of Families, *Scientific Monthly*, 5 (4), October, pp. 368-377; J. M. Cattell (1922), The Order of Scientific Merit, *Science*, 56 (1454), November 10, pp. 541-547; J. M. Cattell (1927), The Origin and Distribution of Scientific Men, *Science*, 66 (1717), November 25, pp. 513-516; J. M. Cattell (1928), The Scientific Men of Harvard and Columbia, *Science*, 67 (1727), February 3, pp. 136-138; J. M. Cattell (1933), The Distribution of American Men of Science in 1932, *Science*, 77 (1993), March 10, pp. 264-270.

[30] In fact, Cattell never produced numbers for the whole nation because he always selected for his analyses the 1000 best men of science as judged and ranked by peers.

[31] J. M. Cattell (1906), A Statistical Study of American Men of Science III, *op. cit*.

Cattell, “the lack of men of distinction in whole regions and large cities is a serious indictment of our civilization. The existence of cities such as Brooklyn and Buffalo is an intellectual scandal” (p. 738).

The second edition of the directory (1910) allowed Cattell to develop statistical comparisons over time. [32] Cattell reiterated the fact that “we are at present almost wantonly ignorant and careless in regard to the conditions which favor or hinder scientific work. We do not know whether progress is in the main due to a large number of faithful workers or to the genius of a few. We do not know to what extent it may be possible to advance science by increasing the number of scientific positions or how far such an increase might be expected to add to the number of men of genius.” (p. 634). Cattell’s new statistical analysis was entirely placed under an evaluative or moral tone, using terms like gain or loss, success or failure, leadership, deficiency in productivity, progressive centers, sinister and discreditable records. Cattell measured that the states of Massachusetts and Connecticut showed the greatest gains – nearly one-fourth of new men of science reside in these two states, which have just 5% of the US population (p. 641) – that the western states have about maintained their position, while the southern states fell still further behind and big cities were losing to an extent that is “ominous” (p. 640). In general, “the increase in the number of scientific men of standing is only about one-half so large as the increase in the population of the country (...). In no country does there seem to be a group of younger men of genius, ready to fill the places of the great men of the last generation” (p. 645).

This conclusion would be “confirmed” a few years later. Following in Galton’s footsteps, Cattell conducted a survey on families of men of science, and published the results in 1915-1917. [33] He analyzed the nationality and race of men of science’s parents, their occupations, age at marriage and size of family, and arrived at the following result: “The

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[32] J. M. Cattell (1910), A Further Statistical Study of American Men of Science, *op. cit*.

[33] J. M. Cattell (1915), Families of American Men of Science: Origin, Heredity and Performance, *op. cit.*; J. M. Cattell (1917), Families of American Men of Science II, *op. cit.*; J. M. Cattell (1917), Families of American Men of Science III, *op. cit*. A few years later, D. R. Brimhall, co-author of the third edition of Cattell’s directory, analyzed the data further. See: D. R. Brimhall (1922), Family Resemblances Among American Men of Science, a series of four papers published in *The American Naturalist* in 1922-23.

families from which our scientific men come had on average 4.7 children, and those scientific men who are married and whose families are complete have on average 2.3 children" (p. 793).[34] Echoing Galton, Cattell concluded: "It is obvious that the families are not self-perpetuating (…). If the families of the scientific men should increase at the rate of the general population [which they don't], the thousand leading scientific men would have some 6,000 grandchildren instead of fewer than 2,000. These well-endowed and well-placed people would probably have an average economic worth through their performance of not less than $100,000, and the money loss due to their non-existence is thus $400,000,000" (p. 797). To Cattell, society thus has obligations with regard to children of professors. He suggested that universities give scholarships to the sons of men of science, and pay a higher salary to the married professor. This was his suggestion for the reproduction of the "species".

Measuring productivity by the number of men of science, as conceived by Galton and systematically conducted by Cattell, would remain the main statistics on science until the 1960s. Several authors, among them a eugenics sympathizer (R. Pearl), looked at members of academies from a quantitative point of view and compared nations.[35] Historians and sociologists,[36] as well as geographers[37] and official statisticians,[38]

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[34] J. M. Cattell (1917), Families of American Men of Science II: Marriages and Number of Children, *op. cit*.

[35] In addition to de Candolle, see: L. Levi (1869), A Scientific Census, *Nature*, November 25, pp. 99-100; L. Levi (1879), The Scientific Societies in Relation to the Advancement of Science in the United Kingdom, in British Association for the Advancement of Science, *Report of the 49th Meeting*, London: J. Murray, pp. 458-468; E. C. Pickering (1908), Foreign Associates of National Societies, *Popular Science Monthly*, October, pp. 372-379; E. C. Pickering (1909), Foreign Associates of National Societies II, *Popular Science Monthly*, January, pp. 80-83; R. Pearl (1925), Vital Statistics of the National Academy of Sciences, *Proceedings of the National Academy of Sciences*, 11, pp. 752-768; R. Pearl (1926), Vital Statistics of the National Academy of Sciences, *Proceedings of the National Academy of Sciences*, 12, pp. 258-261; A. Schuster (1925), On the Life Statistics of Fellows of the Royal Society, *Proceedings of the Royal Society*, A107, pp. 368-376.

[36] G. Sarton (1923), History of Science, *Cambridge Institution of Washington Yearbook*, 22, pp. 335-337; P. A. Sorokin and R. K. Merton (1935), The Course of Arabian Intellectual Development, 700-1300 A.D.: A Study in Method, *ISIS*, 22, pp. 516-524.

[37] For twenty-five years (1922-1947), S. S. Visher from Indiana University, geography, published regular statistical analyses of Cattell's directory in many journals, looking at geographical distribution, training, age, birthplace, race, family background and influences on the decision to become a scientist. The studies are collected in S. S. Visher (1947), *Scientists Starred 1903-1943 in American Men of Science*, Baltimore: Johns Hopkins University Press.

produced many quantitative analyses based on counting men of science, until the money spent on research and development (R&D) became the most cherished indicator. Before the appearance of this statistics, however, productivity got a new meaning, and this we owe to psychologists.

Productivity as Output

At the same time as Cattell's very first statistical studies appeared, the meaning of scientific productivity began to change. Already in Cattell's writings, one sees uses of the term in the sense of "output": "The scientific work accomplished in this country is not commensurate with its population and its wealth", claimed Cattell in 1909 (p. 228). [39] Cattell was thinking here of work published, or "productiveness in publication", [40] as he had already measured for psychologists in 1903: "In order to compare our productivity with that of other nations, I have counted up the first thousand references [papers] in the index to the twenty-five volumes of the *Zeitschrift fur Psychologie*" (p. 327). [41] "In a general way, it appears that each of our psychologists has on the average made a contribution of some importance only once in two or three years" (p. 328)." Overall, Germany leads in productivity. "America leads decidedly in experimental contributions to psychology, we are about equal to Great Britain in theoretical contributions, [but] almost doubled by France and Germany, and decidedly inferior to Germany, France, Great Britain, and Italy in contributions of a physiological and pathological character" (pp. 327-28).

What happened to explain this new meaning? As argued above, Cattell used his statistics to contribute to the advancement of science. Psychologists, as a profession, imitated him

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[38] From the late 1930s, national registers on scientific personnel were developed from which statistics were produced, and, from the 1950s onward, surveys of graduates were conducted at both the national and international level.

[39] J. M. Cattell (1909), American Scientific Productivity, *Science*, 29 (736), February 5, pp. 228-229.

[40] J. M. Cattell (1896), Address of the President before the American Psychological Association, 1895, *Psychological Review*, 3 (2), p. 134.

[41] J. M. Cattell (1903), Statistics of American Psychologists, *American Journal of Psychology*, 14, pp. 310-328. Other counting of scientific papers by Cattell can be found in J. M. Cattell (1917), Our Psychological Association and Research, *Science*, 45 (1160), March 23, pp. 275-284; J. M. Cattell (1929), Psychology in America, *Science*, 70 (1815), October 11, pp. 335-347.

with a new kind of statistics: counting scientific papers. Torn between a subject matter supplied by philosophy and the method of the natural sciences, [42] psychology, a very young discipline, had to demonstrate that it already had the status of a scientific discipline. At the beginning of the 1900s, psychologists thus began developing statistics on their discipline specifically to contribute to the advancement of psychology as a science. The rhetoric used was different from Cattell's, however. Here, psychologists did not criticize their conditions as scientists, but rather showed with confidence how psychology was really a science among the sciences. While the yardstick for comparing the scientific profession in America was Europe, reputed for its chairs, laboratories and public support, for the science of psychology it was its status *vis-à-vis* the other sciences, experimental in character, that served as the benchmark.

Statistics on the profession was an integral part of the strategy to make psychology a science. Quantitative evidence was presented on all aspects of the discipline and its institutionalization. Several psychologists developed a rhetoric on progress in psychology in which measures of growth were constructed on psychologists (number, geographical distribution, per million population, status, degrees), departments, curriculums, student enrollment and doctorates conferred, laboratories, activities of the (American Psychological) Association, journals and … publications. Two vehicles carried these numbers. The first was periodic reviews. Some of these were strictly qualitative, but several others included quantitative material. The reviews appeared occasionally, but others were produced more systematically, being part of annual or decennial series. The second vehicle for assessing the progress made in psychology was histories of the Association.

In the course of these efforts, psychologists pioneered the systematic use of bibliometrics (counting papers). This development proceeded in two steps. The first was, to quote one of its very early users, "to take stock of progress" in psychology. [43] Here, productivity was simply the count of papers coming out of a group of researchers. The statistics was

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[42] J. M. Cattell (1898), The Advance of Psychology, *Science*, 8 (199), October 21, p. 535.
[43] E. F. Buchner (1904), Psychological Progress, *Psychological Bulletin*, 1 (3), p. 57.

used for measuring scientific activities and interests. In 1904, E. F. Buchner (of the University of Alabama), founder of the Southern Society for Philosophy and Psychology, started a series of reviews on psychology, entitled *Psychological Progress,* in order to "review "its mode of doing business and of estimating the net results of all the efforts put forth" (p. 57). The series appeared annually in the *Psychological Bulletin* from 1904 to 1913. It included a discussion of recent papers, but also, among other things, figures from Cattell's directory on the number of psychologists, a list of new journals, and statistics on publications. Beginning with the second issue of the review (1905), a table on the percentage distribution of papers appearing in the *Psychological Index*, first published in 1895, was presented. This served to measure the interest of psychologists in certain subjects. To Buchner, publication counts provide "a good measurement of the annual variation of the intensity of interest in the generic topics with which the psychologists are engaged" (p. 97).

In the 1907 edition of the review, Buchner began talking of shifts in interests in terms of gains or losses in "output" (percentage and ranking) with regard to prior years. The concepts "gains" and "losses" were first used by Cattell in his statistical study on men of science, published in 1906. [44] The word "productiveness", also used by Cattell in 1896, made its appearance in Buchner's review of 1908. In the 1912 edition of the review, Buchner calculated that 3,186 papers were published by 2,514 authors. This was more than a 10% decrease from 1908. Buchner concluded "that the science is established beyond all peradventure may be gathered from the striking steadiness of its literary output." (p. 5).

It was S. W. Fernberger of the University of Pennsylvania who would further develop the statistics on publications for measuring scientific productivity. Fernberger is known today for having produced "classics" in the history of psychology. [45] Following Buchner,

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[44] The two terms also appeared in Galton. See F. Galton (1901), The Possible Improvement of the Human Breed under the Existing Conditions of Law and Sentiment, *op. cit*., p. 664.

[45] S. W. Fernberger (1932), The American Psychological Association: a Historical Summary, 1892-1930, *Psychological Bulletin*, 29 (1), pp. 1-89; S. W. Fernberger (1943), The American Psychological Association: a Historical Summary, 1892-1942, *Psychological Review*, 50 (3), pp. 33-60.

Fernberger measured research interests using publication counts. [46] Above all, he published a series of paper entitled *National Trends in Psychology*. [47] Using the *Psychological Index* as a data source, Fernberger conducted international comparisons to study the scientific productivity of nations. The results were published at intervals of ten years from 1917 to 1956. Fernberger documented German supremacy in the first decades of the twentieth century, then a decline; English titles were shown to be on an upward trend, while French titles declined.

From these regular analyses, Fernberger produced two papers on the "political economy" of research, one of them published in *Science*, looking at the effects of world wars, politics and nationalism (publishing in one's own language) on scientific productivity. [48] "It seems of interest to consider certain aspects of these curves as correlated with coincident political and economic events" (p. 84), suggested Fernberger. [49] He discussed how the war, coupled with politics (Nazism, Fascism) and the economic crisis, produced a decrease in the number of publications, but also how other factors like nationalism or the increase in nationalistic sentiment of nations led to an increase in other countries (Italy, Russia, small countries).

The second use psychologists made of bibliometrics was to see "whether or not advance has been satisfactory". [50] Here, a major change occurred. Productivity was no longer measured as a brute quantity of output (N) but defined as a ratio: N/Researchers. We owe this innovation to S. I. Franz, professor at George Washington University (1906-1921), and scientific director (1909-1919) and then director (1919-1924) of the laboratories of

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[46] S. W. Fernberger (1921), Further Statistics of the American Psychological Association, *Psychological Bulletin*, 18 (11), pp. 569-572; S. W. Fernberger (1930), The Publications of American Psychologists, *Psychological Review*, 37 (6), pp. 526-543.

[47] S. W. Fernberger, a series of papers published every ten years from 1917 to 1956 entitled "On the Number of Articles of Psychological Interest Published in the Different Languages", *American Journal of Psychology*, 28 (1), 1917, pp. 141-150; 37 (4), 1926, pp. 578-581; 48 (4), 1936, pp. 680-684; 59 (2), 1946, pp. 284-290; 69 (2), 1956, pp. 304-309.

[48] S. W. Fernberger (1938), Publications, Politics and Economics, *Psychological Bulletin*, 35 (2), pp. 84-90; S. W. Fernberger (1946), Scientific Publications as Affected by War and Politics, *Science*, 104 (2695), August 23, pp. 175-177.

[49] S. W. Fernberger (1938), Publications, Politics and Economics, *op. cit.*

[50] S. I. Franz (1917), The Scientific Productivity of American Professional Psychologists, *Psychological Review*, 24 (3), p. 198.

the Government Hospital for the Insane, or St. Elizabeth Hospital. The same year that Fernberger started his series (1917), Franz produced a study on the scientific productivity of psychologists. [51] “Within the past few years there have appeared reviews of the progress of psychology for different periods of time (…)”, stated Franz. But “we have not been informed by whom the psychological advances have been made, or whether or not in view of the increasing number of professional psychologists there has been a corresponding increase in the number or in the value of the published investigations. In other words, although it is admitted that advance has been made, we are as far from knowing whether or not the advance has been satisfactory and corresponds with the number of psychologists” (pp. 197-198). In a footnote, Franz explained that “the consideration of these matters has been somewhat forced upon me in connection with editorial duties” (recommending those who have exhibited some accomplishment) (p. 200).

To Franz, as to Cattell, [52] methods for estimating the value of individuals’ contributions (election to Academies, selection and promotion in universities) all have defects. “We can do something [more] definite by determining that a certain individual has or has not made any published contribution towards psychological advance. This is a comparatively easy method giving positive results. It admits of little or no discussion of a judge’s impartiality, it rests solely upon the admission of published material (…). And there is also the possibility of answering the question: Has the progress, as measured by the number of publications, corresponded with the number of individuals who have become professional psychologists” (p. 200).

From the membership list of the American Psychological Association, Franz chose 84 names from 48 institutions and looked at their publications (as listed in the *Psychological Index*) from 1906 to 1915. He observed a fairly gradual increase in publications over time (p. 203). But the productivity (number of publications by psychologist) varied: “for the past five years about 30% of those who contributed published three or more articles, etc.

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[51] *Ibid*.
[52] J. M. Cattell (1910), A Further Statistical Study of American Men of Science, *op. cit*., p. 665.

each year" (p. 204). To Franz, these numbers on productivity needed qualification because someone may not necessarily be active over the whole period. He thus looked at "the date of the doctorate as the date when publications might reasonably be expected" (p. 204), and compared the number of actual *versus* expected contributors. What he found was that actual contributions in relation to expected contributions decreased (p. 205). Franz checked whether this was true for contributions which are intended to convey new facts or new interpretations (articles and monographs), and found the same.

All the tendencies Franz observed were verified according to age. Franz distinguished two groups of authors: young and old men, defined again by the year in which they were granted their doctorate (before or after 1906). He measured that older men were more productive than younger ones, but that the ratio of actual to expected publications was higher among the younger ones. The same pattern appeared when he constructed a combined index of publications by assigning "arbitrary" values to the six types of contributions to translate the "heterogeneity of the different kinds of publications into a homogeneity". The distribution of the oldest men was more skewed than that for younger men.

To Franz, "it should not be assumed (…) that these men are doing nothing for psychological advance. Some may have editorial duties, some may conceal themselves in the work of their students, and some (like Herbert Spencer) may be reserving their energies for some *magna opera* which will be given to the world in due time. It seems unlikely, however, that as many as 40% of the older group are engaged in the accumulation of material for the development of a cosmology, or of a system of psychology, or of an exhaustive history of the science, or of other large projects which should not be laid aside in favor of the minor contributions such as articles and monographs" (p. 215). "The writer feels that some of the so-called "professional" psychologists should be classed with dilettantes" (p. 216). In conclusion "the attention of the reader is called to the consideration of the wisdom of the action of certain scientific societies which require that a member shall retain membership in them only as long as he continues to show an active interest in the advancement of his science by publication (…)" (p. 219).

Fernberger would continue such analyses of scientific productivity in the 1930s, looking at productivity differences between men and women, academics and non-academics, and documenting the skewed distribution of scientific productivity. [53] By that time, Fernberger was only one of several to use papers as output for measuring scientific productivity, or simply science. Scientific papers were also used as indicator of civilization, for "literature furnishes us with the best mirror of the human mind". [54] In the hands of some psychologists, scientific productivity came to mean creativity, and was measured either by surveying the attitudes of scientists, [55] or counting the age at which scientists publish most and publish their best work, [56] an idea first introduced in the

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[53] S. W. Fernberger (1930), The Publications of American Psychologists, *op. cit.*; S. W. Fernberger (1938), The Scientific Interest and Scientific Publications of the Members of the American Psychological Association, *Psychological Bulletin*, 35 (5), pp. 261-281.

[54] E. W. Hulme (1923), *Statistical Bibliography in Relation to the Growth of Modern Civilization*, London; Butler and Tanner Grafton, p. 9.

[55] The most active psychologists were Anne Roe, Calvin W. Taylor, and Morris I. Stein. See: A. Roe (1951), A Psychological Study of Physical Scientists, *Genetic Psychology Monographs*, 43 *(2)*, May, pp. 121-239; A. Roe (1951), A Psychological Study of Eminent Biologists, *Psychology Monographs*, 65 (14), May, pp. 1-68; A. Roe (1953), A Psychological Study of Eminent Psychologists and Anthropologists, and a Comparison with Biological and Physical Scientists, *Psychology Monographs*, 67 (2), May, pp. 1-55; A. Roe (1952), *The Making of a Scientist*, New York: Dood, Mead & Co.; A. Roe (1952), A Psychologist Examines 64 Eminent Scientists, *Scientific American*, 187 (5), November, pp. 21-25; A. Roe (1961), The Psychology of the Scientists, *Science*, 134 (3477), August 18, pp. 456-459; A. Roe (1963), *Scientific Creativity*, New York: John Wiley; A. Roe (1964), The Psychology of Scientists, in E. Mendelsohn et al. (eds.), *The Management of Scientists*, Boston: Beacon Press, pp. 49-71; A. Roe (1965), *Scientists Revisited*, Harvard Studies in Career Development, no. 38, Graduate School of Education, Boston: Harvard University; C. W. Taylor and F. Barron (eds.) (1963), *Scientific Creativity: Its Recognition and Development*, New York: John Wiley; C. W. Taylor (ed.) (1964), *Creativity: Progress and Potential*, New York: McGraw Hill; C. W. Taylor (ed.) (1964), *Widening Horizons in Creativity*, New York: John Wiley; C. W. Taylor and R. L. Ellison (1967), Biographical Predictors of Scientific Performance, *Science*, 155 (3766), March 3, pp. 1075-1080; M. I. Stein (1953), Creativity and Culture, *Journal of Psychology*, 36, pp. 311-322; B. Meer and M. I. Stein (1955), Measures of Intelligence and Creativity, *Journal of Psychology*, 39, pp. 117-126; M. I. Stein and S. J. Heinze (eds.) (1960), *Creativity and the Individual*, Glencoe: Free Press; M. I. Stein (1962), Creativity and the Scientists, in B. Barber and W. Hirsh (eds.), *Sociology of Science*, New York: Free Press, pp. 329-343.

[56] C. W. Adams (1946), The Age at Which Scientists Do Their Best Work, *ISIS*, 36, pp. 166-169; H. C. Lehman (1953), *Age and Achievement*, Princeton: Princeton University Press; W. Dennis (1954), Bibliographies of Eminent Scientists, *Science*, 79 (3), September, pp. 180-183; W. Dennis (1956), Age and Productivity among Scientists, *Science*, 123 (3200), April 27, pp. 724-725; A. Roe (1965), Changes in Scientific Activities with Age, *Science*, 150 (3694), October 15, pp. 313-318; A. Roe (1972), Patterns in Productivity of Scientists, *Science*, 176 (4037), May 26, pp. 940-941; E. Manniche and G. Falk (1957), Age and the Nobel Prize, *Behavioral Science*, 2, pp. 301-307. For sociologists on age, see: H. Zuckerman and R. K. Merton (1972), Age, Aging, and Age Structure in Science, in M. W. Riley et al. (eds.), *A Sociology of*

context of studies on genius.[57] Then, historians, and histories of science,[58] sociologists[59] and librarians,[60] among them E. Garfield, founder of the Science Citation Index (SCI),[61] started constructing measures of scientific productivity using what they called serials of literature or bibliographical indexes, while others began to formulate laws on scientific

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Age Stratification, New York: Sage; S. Cole (1979), Age and Scientific Performance, *American Journal of Sociology*, 84, pp. 958-977; P. E. Stephan and S. G. Levin (1992), *Striking the Mother Lode in Science: The Importance of Age, Place, and Time*, Oxford: Oxford University Press. For economists, see: J. Schmookler (1956), The Age of Inventors, *Journal fo the Patent Office Society*, April, pp. 223-232.

[57] H. Nelson (1928), The Creative Years, *American Journal of Psychology*, 40, pp. 303-311.

[58] D. J. D. Price (1951), Quantitative Measures of the Development of Science, *Archives internationales d'histoire des sciences*, 5, pp. 85-93; D. J. D. Price (1956), The Exponential Curve of Science, *Discovery*, 17, pp. 240-243; D. J. D. Price (1961), *Science since Babylon*, New Haven: Yale University Press; D. J. D. Price (1963), *Little Science, Big Science*, New York: Columbia University Press. Although not historians, several natural scientists used bibliometrics as early as the mid 1910s, either for historical studies of their own fields or for managerial purposes (library acquisitions). See: H. S. White (1915), Forty Years' Fluctuations in Mathematical Research, *Science*, 42 (1073), July 23, pp. 105-113; F. J. Cole and N. B. Eales (1917), The History of Comparative Anatomy, *Science Progress*, 21, pp. 578-598; P. L. K. Gross and E. M. Gross (1927), College Libraries and Chemical Education, *Science*, 66 (1713), October 28, pp. 385-389; P. L. K. Gross and A. O. Woodford, Serial Literature Used by American Geologists, *Science*, 73 (1903), June 19, pp. 660-664; P. W. Wilson and E. B. Fred (1935), The Growth Curve of a Scientific Literature, *Scientific Monthly*, 41 (3), pp. 240-250; S. Fletcher, S. Boig and P. W. Howerton (1952), History and Development of Chemical Periodicals in the Field of Organic Chemistry, 1877-1949, *Science*, 115 (2976), January 11, pp. 25-31.

[59] B. N. Meltzer (1949), The Productivity of Social Scientists, *American Journal of Sociology*, 40, pp. 25-29; B. N. Meltzer (1956), Scientific Productivity in Organizational Settings, *Journal of Social Issues*, 12, pp. 32-40; J. G. Manis (1951), Some Academic Influences upon Publication Productivity, *Social Forces*, 29, pp. 267-272; J. Ben-David (1960), Scientific Productivity and Academic Organization in Nineteenth-Century Medicine, in J. Freudenthal (ed.), *Scientific Growth: Essays in the Social Organization and Ethos of Science*, Berkeley: University of California Press, 1991, pp. 103-124; J. Ben-David and L. Aran (1966), Socialization and Career Patterns as Determinants of Productivity of Medical Researchers, in J. Freudenthal (ed.), *Scientific Growth: Essays in the Social Organization and Ethos of Science*, Berkeley: University of California Press, 1991, pp. 71-89; D. Crane (1965), Scientists at Major and Minor Universities: A Study of Productivity and Recognition, *American Journal of Sociology*, 30 (5), pp. 699-714; S. Cole and J. R. Cole (1967), Scientific Output and Recognition: A Study in the Operation of the Reward System in Science, *American Sociological Review*, 32 (3), pp. 377-390; J. R. Cole and S. Cole (1973), *Social Stratification in Science*, Chicago: University of Chicago Press; P. D. Allison and J. A. Stewart (1974), Productivity Differences among Scientists: Evidence for Accumulative Advantage, *American Sociological Review*, 39, pp. 596-606; P. D. Allison, J. S. Long and T. K. Krauze (1982), Cumulative Advantage and Inequality in Science, *American Sociological Review*, 47, pp. 615-625; P. D. Allison and J. S. Long (1990), Departmental Effects on Scientific Productivity, *American Sociological Review*, 55, pp. 469-478; B. F. Reskin (1977), Scientific Productivity and the Reward Structure of Science, *American Sociological Review*, 42, pp. 491-503; J. S. Scott (1978), Productivity and Academic Position in the Scientific Career, *American Sociological Review*, 43 (6), pp. 889-908; J. S. Scott (1981), Organizational Context and Scientific Productivity, *American Sociological Review*, 46 (4), pp. 422-442.

[60] E. Brodman (1944), Choosing Physiology Journals, *Bulletin of the Medical Library Association*, 32, pp. 479-483; H.H. Fussler (1949), Characteristics of the Research Literature Used by Chemists and Physicists in the United States, *Library Quarterly*, 19, pp. 19-35 and 119-145.

[61] E. Garfield (1955), Citation Indexes for Science: A New Dimension in Documentation Through Association of Ideas, *Science*, 122 (3159), July 15, pp. 108-111; E. Garfield and I. H. Sher (1963), *Science Citation Index*, Philadelphia: Institute for Scientific Information; E. Garfield (1964), Science Citation Index: A New Dimension in Indexing, *Science*, 144 (3619), May 8, pp. 649-654.

productivity or on growth of scientific literature. [62] Bibliometrics also came to be used as a tool for managing science and increasing scientific productivity in organizations, [63] and as an indicator of scientific activities in national statistical offices. [64]

Counting scientific papers was only one of the measurements of output that emerged in the early twentieth century. A second measurement was counting inventions. The measurement of invention as an output to science, or research, we owe mainly to the voluminous information from the Patent Offices, particularly in the United States. [65] Again, the motive behind the very early uses was understanding genius [66] and measuring culture or civilization – or national inventiveness. [67] However, the statistics soon came to serve other theoretical purposes, like understanding growth [68] (and decline) [69] in

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[62] B. Weinberg (1923), Sur les lois d'évolution de la pensée humaine, *Revue générale des sciences*, 30 octobre, pp. 565-569; A. J. Lotka (1926), The Frequency Distribution of Scientific Productivity, *Journal of the Washington Academy of Sciences*, 16 (12), pp. 317-323; S. C. Bradford (1934), Sources of Information on Specific Subjects, *Engineering*, 137, pp. 85-86.

[63] W. Shochley (1954), On the Statistics of Individual Variations of Productivity in Research Laboratories, *Proceedings of the IRE*, March, pp. 279-290; M. H. Hodge (1963), Rate Your Company's Research Productivity, *Harvard Business Review*, November, pp. 109-122; I. H. Sher and E. Garfield (1966), New Tools for Improving and Evaluating the Effectiveness of Research, in M. C. Yovits et al. (eds.), *Research Program Effectiveness*, New York: Gordon and Breach, pp. 136-146; H. M. Vollmer (1966), Evaluating Two Aspects of Quality in Research Program Effectiveness, in *Ibid*, pp. 148-167; C. W. Taylor and R. L. Ellison (1967), Biographical Predictors of Scientific Performance, *op. cit.*; N. Wade (1975), Citation Analysis: A New Tool for Science Administrators, *Science*, 188 (4187), May 2, pp. 429-432.

[64] National Science Board (1975), *Science Indicators 1974*, Washington: National Science Foundation, pp. 71s.

[65] One measurement that did not last very long was counting scientific discoveries. See: B. Weiberg (1926), Les lois d'évolution des découvertes de l'humanité, *Revue générale des sciences*, 37, pp. 43-47; T. J. Rainoff (1929), Wave-Like Fluctuations of Creative Productivity in the Development of West-European Physics in the Eighteenth and Nineteenth Centuries, *ISIS*, 12, pp. 287-319. See also Gilfillan's Index of Inventive Effort that combined several indicators: S. C. Gilfillan (1960), An Attempt to Measure the Rise of American Inventing and the Decline of Patenting, *Technology and Culture*, 1 (3), pp. 201-214. For a serious criticism of Gilfillan's Index, see: J. Schmookler (1960), An Economist Takes Issue, *Technology and Culture*, 1 (3), pp. 214-220.

[66] W. I. Wyman (1919), Age of Production in Invention and Other Fields, *Journal of the Patent Office Society*, 1, pp. 439-446.

[67] M. Jefferson (1911), The Culture of Nations, *Bulletin of the American Geographical Society*, 43 (4), pp. 241-265; S. C. Gilfillan (1930), Inventiveness by Nation: A Note on Statistical Treatment, *Geographical Review*, 20 (2), pp. 301-304; S.C. Gilfillan (1951), Measuring Russian Inventiveness, *Journal of the Patent Office Society*, 33, pp. 328-332.

[68] R. K. Merton (1935), Fluctuations in the Rate of Industrial Inventions, *Quarterly Journal of Economics*, 49 (3), pp. 454-474; B. S. Sanders (1936), The Course of Inventions, *Journal of the Patent Office Society*, October, pp. 666-684; E. Graue (1943), Inventions and Production, *Review of Economics and Statistics*, 25 (4), pp. 221-223; J. Schmookler (1950), The Interpretation of Patent Statistics, *Journal of the Patent Office Society*, 32 (2):, pp. 123-146; J. Schmookler (1953), The Utility of Patent Statistics, *Journal of the Patent Office Society*, 34 (6), pp. 407-412; J. Schmookler (1953), Patent Application Statistics as an Index of

technology and technology's role in industrial development, and contributing to the sociology of science [70] and the study of productivity of inventors. [71] In the 1960-70s, counting inventions, their origin and diffusion became the favorite measure of innovation, [72] before statisticians turned to defining innovation as activity rather than output. [73]

By the end of the 1950s, everything was in place for the "modern" conception of productivity to be applied to science: a ratio of input (men of science) to output (publications, inventions).

Productivity as Efficiency

By the 1920s, academics were no longer alone in conducting research. Firms as well as government departments were increasingly involved in research, and statistics came to be collected on these organizations to document the fact. First, the US National Research

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Inventive Activity, *Journal of the Patent Office Society*, 35 (7), pp. 539-550; J. Schmookler (1954), The Level of Inventive Activity, *Review of Economics and Statistics*, pp. 183-190.

[69] S. C. Gilfillan (1935), The Decline of Patenting, and Recommendations, in *The Sociology of Invention*, Chicago: Follett Publishing, pp. 109-130; A. B. Stafford (1952), Is the Rate of Invention Declining?, *American Journal of Sociology*, 42 (6), pp. 539-545. S. C. Gilfillan (1960), An Attempt to Measure the Rise of American Inventing and the Decline of Patenting, *op. cit*.

[70] W. F. Ogburn and D. Thomas (1922), Are Inventions Inevitable? A Note on Social Evolution, *Political Science Quarterly*, 37 (1), pp. 83-98; H. Hart et al. (1927), Preliminary Conclusions from a Study of Inventors, in E. W. Burgess (ed.), *The Progress of Sociology*, Chicago: University of Chicago Press, pp. 191-194; L. J. Carr (1929), A Study of 137 Typical Inventors, in American Sociological Association, *The Rural Community*, Chicago: University of Chicago Press, pp. 204-206; J. Rossman (1931), The Motives of Inventors, *Quarterly Journal of Economics*, 45 (3), pp. 522-528; S. Winston (1937), Bio-Social Characteristics of American Inventors, *American Sociological Review*, 3, pp. 837-849.

[71] L. J. Carr (1932), The Patenting Performance of 1,000 Inventors During Ten Years, *American Journal of Sociology*, 37, pp. 569-580; J. Schmookler (1956), The Age of Inventors, *op. cit*.

[72] C. F. Carter and B. R. Williams (1957), *Industry and Technical Progress: Factors Governing the Speed of Application of Science*, London: Oxford University Press; C. F. Carter and B. R. Williams (1958), *Investment in Innovation*, London: Oxford University Press; OECD (1970), *Gaps in Technology: Comparisons Between Countries in Education, R&D, Technological Innovation, International Economic Exchanges*, Paris; C. Freeman (1971), *The Role of Small Firms in Innovation in the United Kingdom*, Report to the Bolton Committee of Enquiry on Small Firms, HMSO; SAPPHO Project (1972), *Success and Failure in Industrial Innovation: A Summary of Project SAPPHO*, London: Centre for the Study of Industrial Innovation; C. Freeman (1974), Project SAPPHO, in *The Economics of Industrial Innovation*, Manchester: Penguin Books, pp. 171s; R. Rothwell et al. (1974), SAPPHO updated: Project SAPPHO Phase II, *Research Policy*, pp. 258-291.

[73] B. Godin (2005), *Measurement and Statistics on Science and Technology: 1920 to the Present*, London: Routledge, chapter 8.

Council became the source of several statistics on science in the United States, continuing and extending the series published in *Science* by Cattell on graduates and grants for scientific research. [74] The council's most influential data, however, were on industrial laboratories. From 1920, the organization systematically collected information on firms conducting research and the number of industrial "men of science", or scientific and technical personnel. The lists of laboratories were used by many public organizations for conducting surveys, and the data for analyzing industrial research. What was most wanted, however, was information on money spent on research, more difficult to obtain from firms for confidentiality, accounting and methodological reasons. [75] This type of information came in the late 1930s and early 1940s. Following a pioneering National Research Council study, [76] firms began to be surveyed on their expenditures on R&D by several industrial organizations, then government departments and statistical offices. [77] At

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[74] National Research Council, *Research Laboratories in Industrial Establishments of the United States of America*, Bulletin of the NRC, vol. 1, part 2, March 1920; National Research Council, *Doctorates Conferred in the Sciences in 1920 by American Universities*, Reprint and Circular Series, November 1920; National Research Council, *Funds Available in 1920 in the United States of America for the Encouragement of Scientific Research*, Bulletin of the NRC, vol. 2, part I, no. 9, 1921; National Research Council, *Fellowships and scholarships for Advanced Work in Science and Technology*, Bulletin of the NRC, November 1923; National Research Council, *Handbook of Scientific and Technical Societies and Institutions of the United States and Canada*, Bulletin of the NRC, no. 58, May 1927.

[75] One had to develop estimates based on personnel, as did J. V. Sherman (1941), Research as a Growth Factor in Industry, in National Research Council (1941), *Research: A National Resource (II): Industrial Research*, National Resources Planning Board, Washington: USGPO, pp. 120-123.

[76] M. Holland and W. Spraragen (1933), *Research in Hard Times*, Division of Engineering and Industrial Research, National Research Council, Washington.

[77] For the United States, see: National Research Council (1941), *Research: A National Resource (II): Industrial Research*, National Resources Planning Board, Washington: USGPO; US National Association of Manufacturers (1949), *Trends in Industrial Research and Patent Practices*, Washington: National Association of Manufacturers; D. C. Dearborn, R. W. Kneznek and R. N. Anthony (1953), *Spending for Industrial Research, 1951-1952*, Report to the Department of Defense, Division of Research, Graduate School of Business Administration, Harvard University; US Department of Labor, Bureau of Labor Statistics, Department of Defense (1953), *Scientific R&D in American Industry: A Study of Manpower and Costs*, Bulletin no. 1148, Washington; NSF (1956), *Science and Engineering in American Industry: Final Report on a 1953-1954 Survey*, Bureau of Labor Statistics, NSF 56-16. For Great Britain, see: Federation of British Industries (1943), *Industry and Research*, London; Federation of British Industries (1947), *Scientific and Technical Research in* British Industry, London; Federation of British Industries (1952), *Research and Development in British Industry*, London; DSIR (1958), *Estimates of Resources Devoted to Scientific and Engineering R&D in British Manufacturing Industry, 1955*, London. For Canada, see: Dominion Bureau of Statistics (1941), *Survey of Scientific and Industrial Laboratories in Canada*, Ottawa; Dominion Bureau of Statistics (1956), *Industrial Research-Development Expenditures in Canada, 1955*, Ottawa.

the same time, public institutions, above all those concerned with planning, [78] started compiling statistics on government activities with regard to both scientific and technical personnel and money devoted to research. [79] This series of measurements on public research was launched by E. B. Rosa, chief physicist at the US Bureau of Standards, who compiled, for the first time in American history, a government budget for "research-education-development" in 1920. [80]

By the mid 1950s, in Anglo-Saxon countries at least, all sectors of the economy were systematically surveyed on their scientific and technical personnel and their expenditures on R&D: industry, government, university, and non-profit organizations. [81] The data gave rise to the concept of a national budget on science, first imagined by the British scientist J. D. Bernal [82] (and developed by the US National Science Foundation), [83] or Gross Expenditures on R&D (GERD), the sum of expenditures in the above four economic sectors. [84]

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[78] In the United States: US National Resources Committee, US National Resources Planning Board, US Works Progress Administration, US President's Scientific Research Board. In Great Britain: Advisory Council on Science Policy. In Canada: Department of Reconstruction and Supply.

[79] For the United States, see: National Resources Committee (1938), *Research: A National Resource (I): Relation of the Federal Government to Research*, Washington: USGPO; V. Bush (1945), *Science: The Endless Frontier*, North Stratford: Ayer Co. Publishers, 1995; H. M. Kilgore (1945), *The Government's Wartime Research and Development, 1940-44: Survey of Government Agencies*, Subcommittee on War Mobilization, Committee on Military Affairs, Washington. OSRD (1947), *Cost Analysis of R&D Work and Related Fiscal Information*, Budget and Finance Office, Washington; J. R. Steelman (1947), *Science and Public Policy*, President's Scientific Research Board, Washington: USGPO; Bureau of Budget (1950), *R&D Estimated Obligations and Expenditures*, 1951 Budget (9 January 1950), Washington; National Science Foundation (1953), *Federal Funds for Science: Federal Funds for Scientific R&D at Nonprofit Institutions 1950-1951 and 1951-1952*, Washington. For Great Britain, see the *Annual Reports of the Advisory Council on Science Policy* from 1956-57 to 1963-64, London: HMSO. For Canada, see: Department of Reconstruction and Supply (1947), *Research and Scientific Activity: Canadian Federal Expenditures 1938-1946*, Government of Canada: Ottawa; Dominion Bureau of Statistics (1960), *Federal Government Expenditures on Scientific Activities, Fiscal Year 1958-1959*, Ottawa.

[80] E. B. Rosa (1921), Expenditures and Revenues of the Federal Government, *Annals of the American Academy of Political and Social Sciences*, 95, May, pp. 26-33. See also: E. B. Rosa (1920), Scientific Research: The Economic Importance of the Scientific Work of the Government, *Journal of the Washington Academy of Sciences*, 10 (12), pp. 341-382.

[81] B. Godin (2002), The Number Makers: Fifty Years of Science and Technology Official Statistics, *Minerva*, 40 (4), pp. 375-397.

[82] J. D. Bernal (1939), *The Social Function of Science*, Cambridge (Mass.): MIT Press, 1973.

[83] National Science Foundation (1959), *Methodological Aspects of Statistics on R&D: Costs and Manpower*, NSF 59-36, Washington.

[84] OECD (1962), *The Measurement of Scientific and Technical Activities: Proposed Standard Practice for Surveys of Research and Development*, Paris.

What soon came to characterize the statistics on science, no longer collected by scientists like Cattell but by statistical offices and government departments, was the framework into which they were developed and used: an accounting framework. The model used to collect and analyze the newly-conceived data on science was framed in terms of input and output. Inputs were defined as investments in the resources necessary to conduct scientific activities, like money and scientific and technical personnel, and outputs as what comes out of these activities: knowledge and inventions. A very simple framework defined the relationship between input and output as follows:

Input ? Research activities ? Output

The first edition of the OECD Frascati manual (1962) set the stage for the input-output approach as a framework for statistics on science. [85] The manual was entirely concerned with proposing standards to official statisticians for the measurement of inputs. Despite this focus, the manual discussed output, and inserted a chapter (section) specifically dedicated to its measurement because "in order really to assess R&D efficiency, some measures of output should be found" (p. 11). From its very first edition, the Frascati manual suggested that a complete set of statistics and indicators, covering both input and output, was necessary to properly measure science. From the two inputs suggested (scientific and technical personnel and money spent on R&D), money would become the main indicator on science for the next decades. With regard to outputs, the indicators suggested in 1962 were patents and payments for patents, licensing and technical know-how. [86] From 1981 on, the manual discussed five output indicators, all of a technological and economic type: innovation, patents, technological receipts and payments, high-technology trade, and productivity. The OECD also started publishing methodological

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[85] *Ibid*.

[86] An early statistical analysis of two indicators was conducted by the director of the OCED statistical unit and presented at the Frascati meeting in 1963. See: Y. Fabian (1963), *Note on the Measurement of the Output of R&D Activities*, OECD, DAS/PD/63.48.

manuals on output [87] and regular analytical and statistical series that collected both series of statistics under one roof. [88]

This accounting orientation of official statistics on science we owe to science policy. From its very beginning, science policy was definitively oriented toward technological innovation. As early as 1962, for example, the OECD Committee for Scientific Research recommended that the Secretariat "give considerable emphasis in its future program to the economic aspects of scientific research and technology". [89] This orientation was in line with the 50% economic-growth target advocated by the OECD for the decade. [90] In fact, to policy-makers, innovation was always considered to be the final output of the science system. It is no surprise, then, that the OECD Frascati manual was contracted to an economist, C. Freeman from the National Institute of Economics and Social Research (London).

Freeman would conduct statistical studies linking input to output in the early 1960s, [91] and remained a fervent advocate of the input-output framework for decades. [92] As consultant to various organizations, Freeman believed that "it is only by measuring innovations (…) that the efficiency of the [science] system (…) can be assessed" (p. 25).

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[87] OECD (1990), *The Measurement of Scientific and Technical Activities: Proposed Standard Practice for the Collection and Interpretation of Data on the Technological Balance of Payments*, Paris; OECD (1992), *The Measurement of Scientific and Technological Activities: Proposed Guidelines for Collecting and Interpreting Technological Innovation Data*, Paris; OECD (1994), *The Measurement of Scientific and Technical Activities: Data on Patents and Their Utilization as Science and Technology Indicators*, Paris; OECD (1995), *Manual on the Measurement of Human Resources Devoted to Science and Technology*, Paris.

[88] The two series are *Main Science and Technology Indicators* and *Science, Technology and Industry Scoreboard*.

[89] OECD (1962), *Economics of Research and Technology*, SR (62) 15, p. 1.

[90] OECD (1962), *The 50 Per Cent Growth Target*, CES/62.08.

[91] C. Freeman (1962), Research and Development: A Comparison Between British and American Industry, *National Institute Economics Review*, 20, May, pp. 21-39; C. Freeman, R. Poignant and I. Svennilson (1963), *Science, Economic Growth and Government Policy*, Paris: OECD.

[92] C. Freeman and A. Young (1965), *The Research and Development Effort in Western Europe, North America and the Soviet Union*, Paris: OECD; C. Freeman (1967), Research Comparisons, *Science*, 158 (3800), October 27, pp. 463-468; C. Freeman (1969), *Measurement of Output of Research and Experimental Development*, UNESCO, ST/S/16; C. Freeman (1974), *The Economics of Industrial Innovation*, London: Penguin Books; C. Freeman (1982), *Recent Developments in Science and Technology Indicators: A Review*, mimeo, Brighton: SPRU.

[93] “The output of all stages of R&D activity is a flow of information and the final output of the whole system is innovations – new products, processes and systems” (p. 27).

To Freeman, “the argument that the whole output of R&D is in principle not definable is unacceptable (…). If we cannot measure all of it because of a variety of practical difficulties, this does not mean that it may not be useful to measure part of it. The GNP does not measure the whole of the production activity of any country, largely because of the practical difficulties of measuring certain types of work. The measurement of R&D inputs omits important areas of research and inventive activity. But this does not mean than GNP or R&D input measures are useless” (pp. 10-11). And what about the relationship between input and output? “The argument that the input/output relationship is too arbitrary and uncertain in R&D activity to justify any attempts to improve efficiency or effectiveness (…) rests largely on the view that unpredictable accidents are so characteristic of the process that rationality in management is impossible to attain (…). The logical fallacy lies in assuming that, because accidental features are present in individual cases, it is therefore impossible to make useful statistical generalizations about a class of phenomena” (p. 11).

The historical source of the input/output framework is twofold. The first is management of industrial research and the control of costs. Establishing a relationship between input and output at the national level, that is the level that interests governments most, is in fact the analogue to the firms’ ratio on “returns on investment” (ROI). For decades, managers have constructed such ratios in order to evaluate their investments. [94] Very early on, the ratios came to be applied to R&D activities. By the 1950s, most companies calculated ratios like R&D as a percentage of earnings, as a percentage of sales, or as a percentage

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[93] C. Freeman (1969), Measurement of Output of Research and Experimental Development, *op. cit.*

[94] A. D. Chandler (1977), *The Visible Hand: The Managerial Revolution in American Business*, Cambridge: Belknap Press; H.T. Johnson (1978), Management Accounting in an Early Multidivisional Organization: General Motors in the 1920s, *Business History Review*, 52 (4), pp. 490-517; H. T. Johnson and R. S. Kaplan (1987), *Relevance Lost: The Rise and Fall of Management Accounting*, Boston: Harvard Business School Press; D. A. Hounshell and J. K. Smith (1988), *Science and Corporate Strategy: Du Pont R&D*, 1902-1980, Cambridge: Cambridge University Press.

of value-added, [95] and a whole “industry” developed around studying the “effectiveness” of research. [96] Very few administrative decisions really relied automatically on metrics, [97] but it was not long before the ratios came to be applied to aggregated statistics on industrial R&D [98] and national R&D expenditures. [99] In the latter case, GDP served as denominator and gave the famous GERD/GDP ratio as the objective of science policies. Then economists came on the scene and developed methods for estimating social rates of return. [100]

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[95] F. Olsen (1948), Evaluating the Results of Research, in C. C. Furnas (ed.) (1948), *Research in Industry: Its Organization and Management*, Princeton: D. Van Nostrand, pp. 402-415; A. Abrams (1951), Contribution to the Session on Measuring the Returns from Research, in Engineering Research Institute, *Proceedings of the Fourth Annual Conference on the Administration of Research*, University of Michigan, September 11-13, 1950, University of Michigan, pp. 22-24; R. N. Anthony and J. S. Day (1952), *Management Controls in Industrial Research Organizations*, Boston: Harvard University, pp. 286-300; J. B. Quinn (1960), How to Evaluate Research Output, *Harvard Business Review*, March-April, pp. 69-80.

[96] R. M. Hogan (1950), Productivity in Research and Development, *Science*, 112 (2917), November 24, pp. 613-616; D. C. Pelz (1956), Some Social Factors Related to Performance in a Research Organization, *Administrative Science Quarterly*, 1, pp. 310-325; J. B. Quinn (1959), *Yardsticks for Industrial Research: The Evaluation of Research and Development Output*, New York: Ronald Press; N. Kaplan (1960), Some Organizational Factors Affecting Creativity, *IEEE Transactions of Engineering Management*, 30, pp. 24-30; The Institution of Chemical Engineers (1963), *Productivity in Research*, Proceedings of a Symposium held in London on 11-12 December 1963, London; B.-A. Lipetz (1965), *The Measurement of Efficiency of Scientific Research*, Carlisle: Intermedia; R. E. Seiler (1965), *Improving the Effectiveness of Research* and Development, New York: McGraw Hill; M. C. Yovits et al. (eds.) (1966), *Research Program Effectiveness*, New York: Gordon and Breach; D. C. Pelz and F. M. Andrews (1966), *Scientists in Organizations: Productive Climate for Research and Development*, New York: John Wiley; B. V. Dean (1968), *Evaluating, Selecting, and Controlling R&D Projects*, American Management Association.

[97] For evidence, see: NSF (1956), Science and Engineering in American Industry, *op. cit*, Washington; A. H. Rubenstein (1957), Setting Criteria for R&D, *Harvard Business Review*, January-February, pp. 95-104; N. C. Seeber (1964), Decision-Making on R&D in the Business Firm, *Reviews of Data on R&D*, 44, February, NSF 64-6, Washington: NSF.

[98] J. V. Sherman (1941), Research as a Growth Factor in Industry, *op. cit*.; K. T. Compton (1941), Industrial Research Expenditures, in *Ibid*, pp. 124-125; US National Association of Manufacturers (1949), Trends in Industrial Research and Patent Practices, *op. cit*.; US Bureau of Labor Statistics (1953), *Industrial R&D: A Preliminary Report*, Department of Labor and Department of Defense; Bureau of Labor Statistics (1953), *Scientific R&D in American Industry: A Study of Manpower and Costs*, Bulletin no. 1148, Washington; D. C. Dearborn, R. W. Kneznek and R. N. Anthony (1953), Spending for Industrial Research, 1951-1952, *op. cit*.; NSF (1956), Science and Engineering in American Industry, *op. cit*.; NSF (1960), *Funds for R&D in Industry: 1957*, NSF 60-49, Washington. For Great Britain, see: Federation of British Industries (1952), *R&D in British Industry*, London: FBI; DSIR (1958), Estimates of Resources Devoted to Scientific and Engineering R&D in British Manufacturing, *op. cit.* For Canada, see: Dominion Bureau of Statistics (1956), *Industrial R&D Expenditures in Canada, 1955*, Reference paper no. 75, Ottawa.

[99] J. D. Bernal (1939), The Social Function of Science, *op. cit*.; R. H. Ewell (1955), Role of Research in Economic Growth, *Chemical and Engineering News*, 18 July, pp. 2980-2985; NSF (1956), Expenditures for R&D in the United States: 1953, *Reviews of Data on R&D*, 1, NSF 56-28, Washington.

[100] Z. Griliches (1958), Research Costs and Social Return: Hybrid Corn and Related Innovations, *Journal of Political Economy*, 66 (5), pp. 419-431; E. Mansfield (1965), Rates of Return from Industrial R&D, *American Economic Review*, 55 (2), pp. 310-32; J. R. Minasian (1969), R&D, Production Functions, and Rates of Return, *American Economic Review*, 59 (2), pp. 80-85; E. Mansfield et al. (1977), Social and Private Rates of Return From Industrial Innovations, *Quarterly Journal of Economics*, May, pp. 221-240.

The second source of the input/output framework in statistics on science is economics and the production function. The production function is an equation, or econometric "model" suggested in the late 1920s, [101] that links the quantity produced of a good (output) to quantities of input: P = f (L, K). There are, at any given time, or so argue economists, 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 economist E. Mansfield explained: "The production function shows, for a given level of technology, the maximum output rate which can be obtained from a given amount of inputs". [102]

The production function was the first "model" used to integrate science into economic analyses. J. Schumpeter [103] and R. M. Solow [104] are emblematic representatives of its early use. Then, in 1960, in collaboration with the US Social Science Research Council, the US National Bureau of Economic Research (NBER) organized an important conference on the economics of science. It was attended by senior economists like Z. Griliches, S. Kutznets, F. Machlup, J. Schmookler and researchers from RAND, [105] among others. The conference was the first time the production function was extensively discussed for studying science. In fact, most of the papers were concerned with an input-output framework. 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". [106] Equally, to F. Machlup, "the analysis of the supply

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[101] C. W. Cobb and P. H. Douglas (1928), A Theory of Production, *American Economic Review*, 18, March, pp. 139-165.

[102] E. Mansfield (1968), *The Economics of Technological Change*, New York: Norton, p. 13.

[103] J. Schumpeter (1939), *Business Cycles: A Theoretical, Historical, and Statistical Analysis of the Capitalist Process*, New York: McGraw-Hill, Volume 1.

[104] R. M. Solow (1957), Technical Change and the Aggregate Production Function, *Review of Economics and Statistics*, 39, August, pp. 312-320.

[105] K. J. Arrow, C. J. Hitch, B. H. Klein, A. W. Marchall, W. H. Meckling, J. R. Minasian, and R. R. Nelson.

[106] Z. Griliches (1962), Comment on W. R. Mueller's paper, in NBER, *The Rate and Direction of Inventive Activity*, Princeton: Princeton University Press, p. 347.

of inventions divides itself logically into three sections": input, input-output relationship (the transformation of inventive labour into useful inventions), output. [107]

The conferees discussed at length how to define and measure inputs and outputs, and what the relationship is between the two. Although some concluded for causality, like R. Minasian, [108] important doubts were expressed about the relationship. "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. [109] "No doubt there is a direct relationship of some kind, but we have no evidence that this relationship does not change" (p. 55). Griliches asked the participants "whether an increase in inputs in the knowledge producing industry would lead to more output" (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" (p. 153). To Griliches, "none of [the] studies comes anywhere near supplying us with a production function for inventions", and when they establish a relationship between input and output, these relationships "are not very strong or clear" (p. 350).

To Machlup, the production function was "only an abstract construction designed to characterize some quantitative relationships which are regarded as empirically relevant" (p. 155). In its place, he soon suggested collecting indicators. In 1962, Machlup published what was the first collection of multiple statistics on science, or knowledge, as he called it: education, R&D, communication, information. [110] In his chapter on R&D, he

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[107] F. Machlup (1962), The Supply of Inventors and Inventions, in NBER, The Rate and Direction of Inventive Activity, *op. cit*. p. 143.

[108] J. R. Minasian (1962), The Economics of Research and Development, in NBER, The Rate and Direction of Inventive Activity, *op. cit*. p. 95.

[109] B. S. Sanders (1962), Some Difficulties in Measuring Inventive Activity, in NBER, The Rate and Direction of Inventive Activity, *op. cit*. p. 55. For a highly lucid analysis on the same topic at about the same time, see: W. H. Shapley (1959), Problems of Definition, Concept, and Interpretation of Research and Development Statistics, in NSF, *Methodological Aspects of Statistics on Research and Development: Costs and Manpower*, NSF 59-36, Washington

[110] For a very early collection of several statistics on science (patents, inventions, discoveries) used for measuring knowledge (*sic*) and its growth, see W.F. Ogburn and S.C. Gilfillan (1933), The Influence of

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 to whether they were tangible or intangible, and measurable. [111] Machlup's table marked a transition here. From a theoretical and "abstract construct", the production function became a "practical" tool as well: official statisticians would follow Machlup and adapt the accounting framework to their efforts at measuring science, first among them the OECD and the US National Science Foundation. [112]

By the end of the 1960s, few traces of the production function remained in statistics on science, except in econometric studies on productivity (see next section). The input-output framework now had a life of its own, being part of the cult of efficiency. [113] D. J. D. Price, an historian of science and one of the founders of scientometrics and bibliometrics, was an influential person here. He generally collected several indicators to measure science as a system, presented them in an input-output framework, and suggested all sorts of input-output ratios. [114] In the following decades, most researchers would use an input-output framework to conduct "accounting" or evaluation exercises of investments in science. [115]

Invention and Discovery, in *Recent Social Trends in the United States*, Report of the President's Research Committee on Social Trends, New York: McGraw Hill, p. 126.

[111] F. Machlup (1962), *The Production and Distribution of Knowledge in the United States*, Princeton: Princeton University Press, p. 178-179.

[112] National Science Board (1973), *Science Indicators*, Washington: NSF.

[113] For a reflection on the cult of efficiency at this time, see: D. Bell (1965), Work and its Discontents: The Cult of Efficiency in America, in *The End of Ideologies: On the Exhaustion of Political Ideas in the Fifties*, Boston (Mass.): Beacon Press.

[114] See, for example: D. J. D. Price (1967), Nations can Publish or Perish, *Science and Technology*, October, pp. 84-90; D. J. D. Price (1967), Research on Research, in D. L. Arm (ed.), *Journeys in Science*, University of New Mexico Press, pp. 1-21; D. J. D. Price (1978), Toward a Model for Science Indicators, in Y. Elkana et al., *Towards a Metric of Science: The Advent of Science Indicators*, New York: Wiley & Sons, pp. 69-95; D. J. D. Price (1980), *Towards a Comprehensive System of Science Indicators*, Paper presented to the Conference on "Evaluation in Science and Technology: Theory and Practice", Dubrovnik, July; and to the "Quality Indicators Seminar", MIT, October; D. J. D. Price (1980), A Theoretical Basis for Input-Output Analysis of National R&D Policies, in D. Sahal (ed.), *Research Development and Technological Innovation*, D. C. Heath and Co., pp. 251-260.

[115] On very early evaluation exercises using a costs-benefits framework, see: T.M. Porter (1995), *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life*, Princeton: Princeton University Press.

Productivity as Outcome

Productivity as efficiency ("getting value for money") emerged from considerations about the science system itself and its output: how the system performs, that is, do men of science produce the expected output of a scientific type? As the last section documented, the concept slowly drifted toward looking at output of a technological type. The next step was measuring productivity as outcome. In fact, the production function gave economists and statisticians not only a framework for measuring the productivity of the science system itself, but also a tool for measuring the impact or outcome of the system on (economic) productivity.

Interests in measuring the impact of science, or technology, on productivity can be traced back to the years following the Great Depression, when the bicentennial debate on the role of mechanization on employment reemerged. [116] There were optimists, like US sociologist W. F. Ogburn and economic advisers, and pessimists. In the 1930s, efforts began to be invested into measuring the "dark prophecies" on technological unemployment, as economist David Weintraub called it. [117] The US Bureau of Labor Statistics, [118] the National Bureau of Economic Research, [119] the US Work Projects Administration, [120] and the US Department of Agriculture, [121] concerned with the

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[116] E. Fano (1991), A "Wastage of Men": Technological Progress and Unemployment in the United States, *Technology and Culture*, 32 (2), pp. 264-292; A. S. Bix (2000), *Inventing Ourselves Out of Jobs? America's Debate over Technological Unemployment, 1929-1981*, Baltimore: Johns Hopkins University Press.

[117] D. Weintraub (1937), Unemployment and Increasing Productivity, in National Research Council, *Technological Trends and National Policy*, Washington, pp. 67-87.

[118] US Bureau of Labor Statistics (1931), Mechanization of Agriculture as a Factor in Labor Displacement, *Monthly Labor Review*, 33 (4), pp. 749-783; US Bureau of Labor Statistics (1932), Digest of Material on Technological Changes, Productivity of Labor, and Labor Displacement, *Monthly Labor Review*, 35 (5), November, pp. 1031-1057.

[119] D. Weintraub (1932), The Displacement of Workers Through Increases in Efficiency and their Absorption by Industry, 1920-1931, *Journal of the American Statistical Association*, 27 (180), pp. 383-400; F.C. Mills (1932), *Economic Tendencies in the United States*, New York: National Bureau of Economic Research; H. Jerome (1934), *Mechanization in Industry*, New York: National Bureau of Economic Research; F.C. Mills (1936), *Prices in Recession and Recovery*, New York: National Bureau of Economic Research; F.C. Mills (1938), *Employment Opportunities in Manufacturing Industries of the United States*, Bulletin no. 70, New York: National Bureau of Economic Research.

[120] D. Weintraub (1937), Unemployment and Increasing Productivity, *op. cit.*; D. Weintraub and I. Kaplan (1938), *National Research Project on Reemployment Opportunities and Recent Changes in Industrial Techniques: Summary of Findings to Date*, Work Projects Administration, Philadelphia; H. Magdoff, I. H.

declining share of agriculture (as opposed to manufacturing) in the economy, were forerunners.

The Work Projects Administration has been quite influential. With over sixty projects conducted between 1938 and 1940, among them one on the impact of the depression on industrial laboratories, [122] Weintraub, as director of the project on Reemployment Opportunities and Recent Change in Industrial Techniques, thought, in line with a study he conducted for the National Bureau of Economic Research in 1932, that measuring labor 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 popularly as that of technological unemployment”. [123] To Weintraub, “the unit-labor-requirement ratio indicates changes in man-years employed per unit of total output” (p. 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. Indeed, Weintraub found a disparity between production and employment: “while production in 1935 was 14 percent above 1920, the productivity of hired workers was 39 percent higher or the unit labor requirement was 28 percent lower (…). While 146 units of the Nation’s output were being produced in 1929 for every 100 in 1920, only 16 percent more man-years were employed in 1929” (pp. 71-72).

Weintraub admitted, however, that his ratio of labor productivity “cannot be interpreted as measures of the extent of technological advance” (p. 78). “To measure the full effect

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Siegel and M. B. Davis (1938), *Production, Employment and Productivity in 59 Manufacturing Industries*, Works Projects Administration, Philadelphia; C. Gill (1940), *Unemployment and Technological Change*, Work Projects Administration, Philadelphia.

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[122] G. Perazich and P. M. Field (1940), *Industrial Research and Changing Technology*, Work Projects Administration, National Research Project, report no. M-4, Pennsylvania: Philadelphia.

of even a single technological change on displacement or absorption would necessitate the virtually impossible task of tracing it through the innumerable factors which bear on the total volume of production and employment” (p. 80). “The effect of strictly technological change on employment in a single industry or even a single plant cannot be isolated (…). Over-all productivity ratios (…) reflect a variety of factors in addition to the mechanical improvements” (p. 79). In the end, Weintraub concluded that the productivity ratios “can be regarded as indicative of the effects of technological changes only in the broadest sense” (p. 79).

Despite these caveats, measuring labor productivity became the main statistics on the outcomes of science and technology. Using the production function, economists began interpreting movements in the curve as technological change (the substitution of capital for labor), [124] while others equated labor productivity with science (technological change is likely to result, all other things being equal, in labor productivity), [125] and still others correlated R&D with productivity measures. The National Bureau of Economic Research has been quite active in this kind of study. [126]

What became influential as method was measuring multifactor productivity (MFP). Until the 1950s, economic growth was explained as a function of capital and labour – the Cobb-Douglas function. [127] Science and technology came to be added to this function by R. M. Solow. In 1957, Solow formalized early 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”. [128] Integrating science and technology into the economic equation was thus not a

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[123] D. Weintraub (1937), Unemployment and Increasing Productivity, *op. cit.*, p. 67.

[124] J. Schumpeter (1939), Business Cycles, *op. cit.*; S. Valavanis-Vail (1955), An Econometric Model of Growth: USA, 1869-1953, *American Economic Review*, pp. 208-227.

[125]

[126] G. S. Stigler (1947), *Trends in Output and Employment*, New York: NBER; S. Fabricant (1954), *Economic Progress and Economic Change*, New York: NBER; J. W. Kendrick (1961), *Productivity Trends in the United States*, Princeton: Princeton University Press.

[127] R. R. Solow (1956), A Contribution to the Economic Theory of Economic Growth, *Quarterly Journal of Economics*, 70 (1), pp. 65-94.

[128] R. M. Solow (1957), Technical Change and the Aggregate Production Function, *op. cit*.

deliberate initiative, but it soon became a fruitful one. In the following years, researchers began adding variables (factors) into the equation in order to better isolate science and technology [129], and adjusting the input and capital factors to capture quality changes in output. [130] Since these first calculations, the literature on measuring science and productivity has grown exponentially, becoming an "industry".

Very early on, the mathematics behind the models was qualified as "not strong enough to permit very accurate estimates (…). At best, the available estimates are rough guidelines" wrote E. Mansfield in a review article published in 1972. [131] Solow himself admitted in 1978: "No way has been found to measure directly the contribution of technological progress to the growth of output (…). The usual routine, in the absence of anything better, is to treat technological progress as the ultimate residual. One identifies as many of the components of economic growth as one can, and what is left provides at least an upper limit to the contribution of technological change". [132] Twenty years later, Z. Griliches concluded that "the quantitative basis for these convictions [links between investments in science and economic growth] is rather thin", and pleaded for realism. [133] Echoing Weintraub, [134] the recent OECD manual on measuring productivity summarized the problems as follows:

> When labour and capital are carefully measured, taking into account their heterogeneity and quality change, the effects of embodied technical change and of improved human capital should be fully reflected in the measured contribution of each factor of production (…). More often than not [however], data and resource constraints do not permit a careful differentiation and full coverage of all labour and capital inputs (…). Some of the embodiment effects of technological change and some or all of the changes in skill composition of labour input are

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[129] E. F. Denison (1962), *The Sources of Economic Growth in the United States and the Alternatives Before Us*, Committee for Economic Development, New York; E. F. Denison (1967), *Why Growth Rates Differ*, Washington: Brookings Institution.

[130] D. W. Jorgenson and Z. Griliches (1967), The Explanation of Productivity Change, *Review of Economic Studies*, 34 (3), pp. 249-283.

[131] E. Mansfield (1972), Contribution of R&D to Economic Growth in the United States, *Science*, 175 (4021), p. 478.

[132] R. M. Solow and P. Temin (1978), Introduction: The Inputs for Growth, in P. Mathias and M. M. Postan (eds.), *The Cambridge Economic History of Europe*, vol. 7, Part I, Cambridge: Cambridge University Press, p. 26.

[133] Z. Griliches (1998), R&D and Productivity: Econometric Results and Measurement Issues, in Z. Griliches, *R&D and Productivity: The Econometric Evidence*, Chicago: University of Chicago Press, pp. 52-89.

[134] D. Weintraub (1937), Unemployment and Increasing Productivity, *op. cit.*, p. 79.

> picked up by the MFP residual (…). [But] MFP is not necessarily technology [it also includes the impact of other factors], nor does technological change exclusively translate into changes in MFP. [135]

Nonetheless, measuring multifactor productivity has occupied economists ever since. Economic growth and productivity have, above all, entirely defined officials' understanding and measurements of the outcomes of science on society. Such outcomes are many (organizational, cultural, social, environmental, economic), but economists have focused, by definition, on economic ones, particularly productivity. [136] Governments followed. This happened from the very beginning of science policy in the 1960s, [137] and acquired increased importance with new growth theories and discourses on the new economy in the 1990s. [138] This focus on productivity as outcome of science we owe largely to the accounting framework used for measuring science. To "accounting", the economics is what is significant, what is rendered visible and what becomes imperative for action. The social is the residual and is relegated to the periphery. [139]

Conclusion

The concept of scientific productivity arose out of issues on the decline of civilization. Men of high ability had to reproduce themselves to maintain the progress of civilization.

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[135] OECD (2001), *Measuring Productivity: Measurement of Aggregate and Industry-Level Productivity Growth* (Productivity Manual), Paris, p. 115-117.

[136] Admittedly, sociologists like W. F. Ogburn and S. C. Gilfillan in their chapter in *Recent Social Trends in the United States* (1933) looked at the outcomes of technology from another perspective, but very few numbers were produced. See also: S. McKee Rosen and L. Rosen (1941), *Technology and Society: the Influence of Machines in the United States*, New York: Macmillan.

[137] Discussions on productivity in Europe we owe largely to the United States and the Marshall Plan. After World War II, the European Productivity Agency, part of the newly created Organization for European Economic Co-Operation (the predecessor to the OECD), devoted considerable efforts to this task. As a result, science policy in Europe came to be defined as a "tool" for increasing productivity. See: B. Godin (2002), Technological Gaps: an Important Episode in the Construction of Science and Technology Statistics, *Technology in Society*, 24, pp. 387-413. On productivity issues in Europe before the 1950s, see: J. Tomlinson (1994), The Politics of Economic Measurement: the Rise of the Productivity Problem in the 1940s, in A. G. Hopwood and P. Miller (eds.), *Accounting as Social and Institutional Practice*, Cambridge: Cambridge University Press, pp. 168-188.

[138] B. Godin (2004), The New Economy: What the Concept Owes to the OECD, *Research Policy*, 33 (5), pp. 679-690.

[139] A. Hopwood (1984), Accounting and the Pursuit of Efficiency, in A. G. Hopwood and C. Tomkins (eds.), *Issues in Public Sector Accounting*, Oxford: Philip Allan, pp. 167-187.

In science, the issue took the form of promoting the advancement of science. To Cattell, the advancement of science meant, first of all, more scientists. The United States was not productive enough of scientists because the conditions of work in this country, among them the little time allocated to research, were detrimental to science.

One way to demonstrate this state of affairs was to estimate gaps in publications or scientific output between the United States and European countries. To psychologists, however, there was no such gap in their discipline. Psychological science was flourishing, and statistics on scientific papers served to document the case. In the next decades, the statistics became the main way of measuring scientists' output.

For firms and governments, however, such arguments were not enough. Funding scientific research required that a firm earn profits, and funding scientific research from public money, that the system of science proves how it was efficient, and this places onus on technological output. To account for economic progress and "manage" the research system accordingly, a series of statistics were conceived built into an accounting framework. The hypothesis, or expectation, was that more money should lead to more output. The ultimate output, or outcome, was profits, economic growth and productivity.

This is not the end of the story. In recent years, all sorts of scoreboards of indicators have appeared that measure nations on different dimensions of science, technology and innovation. The idea behind a scoreboard is that of "performance". But how does one distinguish performance from productivity? In the literature, the two concepts often appear together, or are used interchangeably. Looking at statistics again helps answer the question. What define a performing country in scoreboards are simple comparisons with other countries on any of the indicators: the higher the numbers, the more a country is performing. Productivity is part of the toolkit of these indicators, but it defines one aspect of performance only. It is that aspect, however, that explains or is explained by other aspects measured, that aspect towards which other aspects are or should be directed, that aspect which gets priority on the political agenda.

Conceptions of Scientific Productivity

<table>
<tr><th></th><th colspan="2">Reproduction</th><th colspan="2">Output</th><th>Efficiency</th><th>Outcome</th></tr>
<tr><td>Pioneers</td><td>Galton</td><td>Cattell</td><td colspan="2">Franz,
Fernberger</td><td>Firms (accountants)
Machlup, Freeman,
OECD</td><td>Weintraub,
Solow</td></tr>
<tr><td>Followers</td><td>Race and Genius studies</td><td>Science studies
Scientometrics</td><td colspan="2">Science studies
Bibliometrics
Research evaluation</td><td>Economists
Governments
Research management
Research evaluation</td><td>Economists
Governments</td></tr>
<tr><td>Aims</td><td>Progress of civilization</td><td colspan="2">Advancement of science</td><td colspan="2">"Accounting"</td><td>Economic growth</td></tr>
<tr><td>Indicators</td><td>Eminent men</td><td>Men of science</td><td colspan="2">Papers</td><td>Money (R&D)
Returns</td><td>Labor productivity
MFP</td></tr>
<tr><td>Statistics</td><td colspan="4">N
N/population (or group, kinship, race, nation, researchers)</td><td>I/O ratios</td><td>Growth rates</td></tr>
</table>

Actually, the field of science and technology studies, particularly the policy-oriented subfield, has fully endorsed the productivity issue rather than criticizing it. Every conceptual framework developed over the last fifty years is concerned with accounting and efficiency in the broadest sense. Whether one looks at the linear model of innovation, input-output analyses, the information economy or society, the national system of innovation, the knowledge-based economy, or the new economy, the most central issue and the statistics of the frameworks are economic, among which is the concept of productivity. Either one measures the productivity of the science system itself, or scientific productivity (academic papers), or the contribution of science to economic growth and productivity.

There are at least three reasons that explain this orientation. One is the basic unit of science policy and analyses. Whereas early studies of science, particularly sociological studies, were concerned with people and the varied impacts of science on people’s lives, current studies focus entirely on organizations and their efficiency. Growth rather that quality drives policies. Second, and methodologically, economic growth and productivity are easier to measure than the social and cultural aspects or impacts of science, for example. For this reason, many researchers prefer to use data sources that are easily available and standardized rather than develop specific surveys. Third, most studies are conducted by economists or, for purposes of “emulation”, by researchers using an economic-type framework. These, then, are three factors that automatically suggest three *loci* for changing frameworks and statistics: the policy issues, the data, and … the researchers.