On the Origins of Bibliometrics

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Abstract

Among the many statistics on science, counting scientific papers, or bibliometrics, holds a privileged place. Bibliometrics is one of the few subfields concerned with measuring the output side of science. According to most “histories”, bibliometrics owes its systematic development mainly to works from the 1950s (V.V. Nalimov, D.J.D. Price and Eugene Garfield), as founders. The few works conducted earlier are usually relegated to prehistory.

This paper documents how the systematic counting of publications originated with psychologists. In the early 1900s, psychologists began collecting statistics on their discipline. Publications came to be counted in addresses, reviews and histories of psychology for several decades. The aim was to contribute to the advancement of psychology. Far from being a negligible output of a prehistoric type, both the volume and the systematicness of these efforts are witnesses to what should be considered as pioneering work, and their authors considered as forerunners to bibliometrics.

On the Origins of Bibliometrics

Introduction

We owe to American psychologist James McKeen Cattell, editor of *Science* from 1895 to 1944, the first systematic collection of statistics on science. [1] In 1906, Cattell launched the biographical directory *American Men of Science*, published every five years. The directory collected information on thousand of scientists active in research in the United States. From the data, Cattell conducted regular and systematic statistical studies on science until the 1930s. He produced statistics on the number of scientists and their geographical distribution, and ranked scientists according to performance. Cattell can be credited for having launched scientometrics, or the systematic measurement of science.

Cattell introduced two dimensions into the measurement of science, and these two dimensions still define the field today: quantity and quality. Quantity, or productivity as he called it, was simple counting of the number of scientists a nation produces. Quality, or performance, was defined as contributions to the advancement of science and was measured by averaging peer rankings of colleagues.

Cattell's first use of the directory for statistical analysis was concerned with psychologists. In 1903, while the directory was still in progress, he looked at a select group of 200 psychologists and analyzed their "academic origin [institution], course and destination". [2] This study included most of what would define Cattell's work in the years to come: identifying the best men of science, displaying their performance, comparing with other nations, and suggesting courses of action. Cattell classified psychologists into four equal groups based on what he called scientific merit. The results showed that "the

[1] B. Godin (2007), From Eugenics to Scientometrics: Galton, Cattell, and Men of Science, *Social Studies of Science*, forthcoming.
[2] J. M. Cattell (1903), Statistics of American Psychologists, *American Journal of Psychology*, 14, pp. 310-328.

differences are not continuous, but there is a tendency towards the formation of groups or species" (p. 315). Two main groups or types were identified: "there are leaders, and the men of moderate attainments, the leaders being about one-tenth of the whole number. The leaders are again broken into four groups – say, of great genius, of moderate genius, of considerable talent, and of talent" (p. 316).

Having identified a select group of psychologists, Cattell compared their scientific production to other nations, using publication counts: "in order to compare our productivity with that of other nations, I have counted up the first thousand references in the index to the twenty-five volumes of the *Zeitschrift fur Psychologie*" (p. 327). "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 led in productivity. "America leads decidedly in experimental contributions to psychology, we are about equal to Great Britain in theoretical contributions, 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).

Cattell was the first of many psychologists to use data on publications to measure science in the early decades of the twentieth century. He would soon be followed by others. This paper suggests that the systematic use of bibliometrics in history was pioneered by American psychologists. By bibliometrics I mean counting papers (and citations). [3] By systematic I mean the regular use, for analytical purpose and over a continuous period of time, of papers' count. There have been sporadic uses before psychologists, like those of J.S. Billings, surgeon from the US Army, but psychologists made the first systematic use of bibliometrics. This paper explains why psychologists got involved in bibliometrics, and how the statistics were used to contribute to the advancement of psychology.

[3] For a discussion on terms, see: I.N. Sengupta (1992), Bibliometrics, Informetrics, Scientometrics and Librametrics: An Overview, *Libri*, 42 (2), pp. 75-98; W.W. Hood and C.S. Wilson (2001), The Literature of Bibliometrics, Scientometrics, and Informetrics, *Scientometrics*, 52 (2), pp. 291-314.

The first part of this paper documents how psychologists dealt with and defended the status of psychology as a science in the late 1800s. It looks at Cattell as representative of the rhetoric. The second part looks at how several psychologists, from the early 1900s onward, got involved in assessing progress in psychology by way of reviews and histories of the discipline. These reviews and histories included several statistics, among them statistics on publications. Two central figures were E. F. Buchner and S. W. Fernberger. The latter is also a key author when it comes to understanding how publication counts came to serve the rhetoric on the productivity of research. The third part discusses these efforts, including a pioneering study published by S. I. Franz. The last section analyzes the very first psychologists' uses of bibliometrics to study the scientific community as a whole and not only psychological research, and shows how the interest in creativity was the motive behind such developments as conducted by H. C. Lehman and W. Dennis.

This paper is, to a certain extent, a contribution to the history of science. Several authors have documented the efforts of scientists for the institutionalization of science in the 19th Century. While looking at organizations specifically dedicated to the advancement of science, for example, they have analyzed the different strategies used by scientists and the forms that institutionalization took. [4] This paper, together with a previous one, [5] adds the collection of statistics on science to the arsenal of rhetorical resources for the advancement of science. The paper is also a contribution to the literature on boundary-work. Several authors have looked at methods, both experimental and statistical, and their role in demarcating disciplines. [6] Here, it is a specific type of statistics, those on the discipline itself (number of professors, students, publications, etc.) and their role in the advancement of psychology that are documented. As Cattell once suggested, "the subject matters [of psychology] have been supplied by philosophy and the methods by natural

[4] S.G. Kohlstedt (1976), *The Formation of the American Scientific Community: The American Association for ther Advancement of Science, 1848-1860*, Urbana: University of Illinois Press; R. MacLeod and P. Collins (1981), *The Parliament of Science: The British Association for the Advancement of Science, 1831-1981*, Northwood: Science Reviews Ltd; S.G. Kohlstedt, M.M. Sokal and B.V. Lewenstein (1999), *The Establishment of Science in America: 150 Years of the American Association for the Advancement of Science*, London: Rutgers University Press; H. Gispert (2002), *Par la science, pour la patrie: l'Association française pour l'avancement des sciences, 1972-1914*, Rennes: Presses universitaires de Rennes.
[5] B. Godin (2007), From Eugenics to Scientometrics, *op. cit.*
[6] On boundary work and the role of statistics specifically, see: O. Amsterdamska (2005), Demarcating Epidemiology, *Science, Technology, and Human Values*, 30 (1), pp. 17-51.

sciences". [7] This tension was at the heart of psychologists' use of statistics on the performance of psychology as a science.

The Status of Psychological Science

Cattell's use of data from *American Men of Science* was aimed at the advancement of science. To him, the conditions facing science in America were detrimental to research. Men of science were occupied more with teaching than with research, and salaries of performing researchers were too low. Men engaged in research, estimated Cattell, "do not on the average devote more than half their time to it" (p. 633). [8] Generally speaking, "a man must be regarded as an amateur in work to which he does not devote more than half his time". [9] To Cattell, "eminent men are lacking and this we must attribute to changes in the social environment" (p. 646): the growing complexity of science, educational methods, lack of fellowships and assistantships as well as prizes, teaching load, and low salary. "The salaries and rewards are not adjusted to performance", unlike Germany, Great Britain and France where the "exceptional men have received exceptional honors (…). Methods should be devised by which scientific work will be rewarded in some direct proportion to its value to society - and this not in the interest of the investigator, but in the interest of society" (p. 648). For 30 years, Cattell published statistics on the demography and geography of scientists in order to demonstrate this state of affairs and to contribute to the advancement of science. [10]

Statistics on psychological science were specifically developed to serve the same goal: to participate to the advancement of psychology. The rhetoric used was different, however. Here, Cattell and other 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,

[7] J. M. Cattell (1898), The Advance of Psychology, *Science*, 8 (199), October 21: 533-54, p. 535.
[8] J. M. Cattell (1910), A Further Statistical Study of American Men of Science, *Science*, 32 (827), November 4, pp. 633-648.
[9] J. M. Cattell (1917), Our Psychological Association and Research, *Science*, 45 (1160), March 23: 275-284, p. 281.
[10] B. Godin (2007), From Eugenics to Scientometrics, *op. cit.*

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.

In several addresses to his peers, Cattell gave himself the task of documenting "the development of psychology into a science rivaling in activity and fruitfulness the other great sciences" (p. 541). [11] The first such address, given before the American Psychological Association, appeared in 1896. [12] "In the struggle for existence that obtains among the sciences psychology is continually gaining ground" (p. 134), claimed Cattell. "The academic growth of psychology in America during the past few years is almost without precedent" (p. 134). To Cattell, this progress was due to methods: "Measurements have just the same place in psychology as in the material sciences (…). Measurements are a part of description, and by far the most exact, general and economical method of description hitherto devised" (p. 140). He was here thinking of statistics, averages and statistical errors: "The theory of probabilities, enabling us to measure both our knowledge and our ignorance, is one of the great achievement of the human intellect, and is equally applicable in sciences attaining varying degrees of exactness" (p. 142).

Two years later, in a second address (before the AAAS), Cattell was still more confident, even arrogant. [13] He first discussed how, in fifty years, "psychology has shown great vitality [and has become] a university discipline" (p. 534). As evidence, he discussed the works of distinguished American psychologists like W. James, G. C. Hall and G. T. Ladd, and talked about laboratories, departments, journals and the work of the American Psychological Association. The whole address was aimed at putting psychology on top of the hierarchy of the disciplines. Already in 1896, Cattell suggested that experimental psychology had practical applications in "education, medicine, fine arts, political economy and, indeed, in the whole conduct of life" (p. 144) and, because of its method of

[11] J. M. Cattell (1898), The Advance of Psychology, *op. cit.*
[12] J. M. Cattell (1896), Address of the President before the American Psychological Association, 1895, *Psychological Review*, 3 (2), pp. 134-148.
[13] J. M. Cattell (1898), The Advance of Psychology, *op. cit.*

observations and experiments that sets "a standard of carefulness and objectivity" (p. 145), had made relations with other sciences in physics, physiology, biology (p. 147-148). Now, psychology was the mother of all sciences. "Psychology is not a new science", claimed Cattell. "It should be regarded as one of the oldest of the sciences" (p. 535). "Compared with psychology, a science such as astronomy may almost be regarded as naïve. The entire known performance of the solar system and of the fixed stars since the time of the Chaldaean is less complicated than the play of a child in its nursery for a single day (…). Atoms and molecules are so invisible, the ether is so intangible, we know after all so little about them, that it is easy to invent hypotheses" (p. 538). And he continued: "The two greatest scientific generalizations of the present century are the conservation of energy and evolution by survival of the fit. Now, if consciousness alters, however slightly, the position of molecules in the brain the fundamental concept of physical science must be abandoned. If consciousness has no concern in the actions of the individual we have one of the most complex results of evolution developed apart from the survival of useful variations, and the Darwinian theory has failed (…). The world is one world; every part of it is in relation to every other part, and each part consists in these relations" (p. 540).

To Cattell, "there is no department of knowledge or activity which does not have an aspect that concerns psychology, and while hitherto it is psychology which has learned from sciences preceding it in their development, the time will come, and perhaps has now come, when every science must take into account the facts and theories of psychology" (pp. 538-539). He then cited examples of problems in which psychology merges with mathematics, mechanics, physics, physiology, chemistry, geology, geography, biology, anthropology, literature and fine arts. "Errors of observation, the personal equation, the relation between mental and physical intensity, are subjects where the investigations of the psychological laboratory must be applied in astronomy and the other physical sciences" (p. 539). Overall, "psychology has become an integral part of modern science (…). Its position in the body scientific is henceforth secure" (p. 540). [14]

[14] More than thirty years later, Cattell still held the same type of discourse. In a paper on "the economic value of psychology [and] its usefulness to our modern civilization", Cattell depicted the object of

Cattell's addresses made use of some statistics, but it was B. G. Miner from the University of Illinois who would present more systematic numbers. [15] In a paper published in *Science* in 1904, Miner announced that: "in the following pages the writer collects certain facts which bear upon the recent development of psychology in American institutions of higher learning, with the hope of giving a more adequate means for judging the present status of this science" (pp. 299-300). The data were taken from the catalogues of 150 colleges and information furnished by the directors of 34 prominent laboratories. Miner presented statistics on laboratories, their number and their value divided into three classes according to equipment and apparatus. He also presented numbers on incomes and space devoted to psychology in universities. He looked at chairs, departments and their sub-divisions or specialties, professors, courses and education. He estimated that 20% of total university enrollment was in psychology, and "60% of the graduates from the larger universities in America today have taken at least an introductory course in psychology" (p. 302). He also tried a sort of ranking of universities, an idea Cattell would develop further, based on their numbers of doctorates, and discussed the strength of quantitative methods in psychology by way of enrollment in laboratory courses and research in residence in laboratories. To Miner, "few other sciences can show an equal record, and certainly no other country approaches the United States in the number [63] occupied in zealous psychological research" (p. 303).

To both Cattell and Miner, psychology, although a very young discipline, already had the status of a scientific discipline. Statistics on the profession was an integral part of the strategy to make psychology a science. Quantitative evidence was presented on all

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psychology as "the control of the behavior of individuals" (p. 286). To Cattell, "it is for psychology to determine what does in fact benefit the human race": selecting, training and directing men (p. 286). "The study and practice of medicine and engineering will be greatly advanced when we realize the extent to which they should be based on understanding and controlling behavior" (p. 286). "The control of thoughts, emotions and behavior has been undertaken by the churches, the schools, the laws and the rest in order to accomplish definite results that are regarded as desirable, but they have largely failed because it is difficult to change human nature" (p. 286-287). "Psychology can do more by placing individuals in surroundings where they will act in the way that is wanted than by attempting to change individuals" (p. 286). See: J. M. Cattell (1930), The Usefulness of Psychology, *Science*, 72 (1864), September 19, pp. 284-287.

[15] B. G. Miner (1904), The Changing Attitude of American Universities Toward Psychology, *Science*, 20 (505), September 2, pp. 299-307.

aspects of the discipline and its institutionalization: professors, departments, curriculum, student enrollment and graduates, laboratories, journals and the work of the Association.

First Steps toward the
Institutionalization of Psychology in the United States

First laboratory	Johns Hopkins University (Hall)	1883
First journal	American Journal of Psychology	1887
First department/Chair	University of Pennsylvania (Cattell)	1888
Association	American Psychological Association	1892
First Index	Psychological Index	1895

Taking Stock of Progress

This kind of rhetoric served as a model for other psychologists and their use of statistics. In the following decades, 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), curriculums, doctorates conferred, laboratories, journals and … publications. Two vehicles carried these numbers. The first was periodic reviews. Some of these were strictly qualitative, [16] but several others included quantitative material. The reviews appeared occasionally (this was the case with Cattell, E. G. Boring, C. A. Ruckmich, and C. R. Griffith), but others were produced more systematically, being part of annual (E. F. Buchner) or decennial series (S. W. Fernberger). The second vehicle for assessing the progress made in psychology was histories of the Association (Fernberger).

Founder of the Southern Society for Philosophy and Psychology, E. F. Buchner of the University of Alabama started the first series of review on psychology in 1904. One year earlier, he had already laid out his rationale as follows. [17] Fifty years ago, wrote Buchner, "psychology was well regarded as a waif; it was not received by the students of facts; and it was gingerly given a berth by the great chasers after world categories. The revolution which has given us "scientific" psychology, the historian will have to say, proceeded in two directions" (p. 194). The first and foremost of these directions was method: "It developed a general type of method, which wrought the great change from speculative defense of the application of certain theoretical interpretations of every variety of inner experience, to a factual, inductive, measurable, experimental mode of approach (…)" (p. 194). To Buchner, "in the past, American psychology sailed under the terms "mental and moral philosophy" (…). It, too, was molded chiefly by the theologians" (p. 194). Now, American psychology "renounces our former mode of intellectual dependence upon some foreign system, or upon some old-world thinker" (pp. 194-195).

Then Buchner reviewed the work of the American Psychological Association and its influences, mainly laboratories, chairs, and the systematic literature of its members. He also devoted a large section to the organization of psychology, namely the Association and its work from 1892 to 1901. He looked at members and meetings: attendance, presidential addresses, papers presented. He classified the 283 papers presented over the decade under two classifications: 1) the categories and subcategories of the *Psychological Index*, created in 1895, 2) topics such as methods, interests and results. Buchner also calculated number of papers per member, and observed a trend that, much later, came to be formalized into a law: [18] "89 members have been the total contributors, of whom

[16] For example, see: E. B. Titchener (1905), The Problems of Experimental Psychology, *American Journal of Psychology*, 16 (2), pp. 208-224; E. B. Titchener (1910), The Past Decade in Experimental Psychology, *American Journal of Psychology*, 21 (3), pp. 404-421.
[17] E. F. Buchner (1903), Ten Years of American Psychology, 1892-1902, *Science*, 18 (450), August 14, pp. 193-204, and 18 (451), August 21, pp. 233-241.
[18] 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, 26 January, pp. 85-86. J.M. Cattell also discussed such a distribution as regards the "performance" of men of science. See: J. M. Cattell (1903), Statistics of American Psychologists, *op. cit.*, p. 315; J. M. Cattell (1906), A Statistical Study of American Men of Science II: The Measurement of Scientific Merit, *Science*, 24 (622), November 30, p. 707.

thirty-four have presented one unit, as paper, report, etc. each; twenty-three have presented two units each; ten have presented three each; eight have presented four each; five have presented five each; three have presented six each; two have presented fourteen, one seventeen, one nineteen and one twenty-three units. The remaining fifty-nine members have been inactive, silently paying their annual dues. It is, indeed, a serious question whether the association can hasten its realizations by carrying forty percent empty baggage (…)" (p. 204).

In 1904, then, Buchner started his series of reviews on psychology, entitled *Psychological Progress*, in order "to take stock of our progress", that is, "reviewing 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 discussion of recent papers, but also figures from Cattell's directory on the number of psychologists in the country and from *Science*'s series on doctorates conferred, 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* was presented. This served to measure the interests 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). Here, like many psychologists who would produce such statistics in the coming years, Buchner was proud to measure the progress made in experimental methods: "When we come to look for the features of the psychology which has come to be among us, we find them to have developed through a devotion to measurement, enumeration, and comparison, as the efficient methods of ascertaining the elemental facts of the inner life and their relations" (p. 406). [19]

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 (p. 8). The concepts "gains" and "losses" were first used by Cattell in his statistical study on men of

[19] E. F. Buchner (1903), A Quarter Century of Psychology in America, 1878-1903, *American Journal of Psychology*, July-October, pp. 402-416.

science, published in 1906. [20] The word "productiveness", first used by Cattell in 1896, [21] also made its appearance in Buchner's review of 1908 (p. 10). 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. The growth of the *Index* is approaching the limits" (p. 5).

Psychologists continued Buchner's reviews of progress in the following years. In 1912, C. A. Ruckmich of Cornell University published a review of 25 years of psychology that was full of statistics. [22] The source of the data was catalogues from universities and a questionnaire sent to 39 institutions. Laboratories, courses, departments and their conditions, such as affiliation with other departments, were measured. The paper also dealt with the standing of psychology among the sciences. Psychology was compared (and ranked) to six other disciplines in terms of the number of professors, academic hours, registrations and appropriations. These disciplines were political economy, education, physiology, physics, philosophy and zoology. A combined index was computed to aggregate the diverse statistics, but without much success. "On the whole", concluded Ruckmich from his numbers, "psychology foots the lists more often than any other discipline" (p. 529).

Ruckmich published a second review in 1916 and included statistics on publications to look at "the productivity of the science as an index of its stability and growth" (p. 112). [23] He selected six journals between 1905 and 1915 and counted the number of papers, the number of pages covered by each article, and the kind of method used. Papers were

[20] 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.
[21] In his 1896 address, Cattell used the term "productiveness" to talk of the scientific production of psychologists: the American Psychological Association publishes two journals, the psychologists contribute to many journals in general sciences, and "books written by members of the Association stand well to the front among American contributions to science". J. M. Cattell (1896), Address of the President before the American Psychological Association, *op. cit.*, p. 135.
[22] C. A. Ruckmich (1912), The History and Status of Psychology in the United States, *American Journal of Psychology*, 23 (4), pp. 517-531.
[23] C. A. Ruckmich (1916), The Last Decade of Psychology in Review, *Psychological Bulletin*, 13 (3), pp. 109-120.

classified under the 78 headings and subheadings of the *Psychological Index*. Unfortunately, Ruckmich gave few statistics in his paper. He counted over 800 publications covering more than 20,000 pages, and presented a distribution of publications and number of pages by method (experimental, non-experimental, speculative-theoretical), but that was all. To Ruckmich, the numbers reflected an "increasing productivity" (p. 120).

It was S. W. Fernberger of the University of Pennsylvania who would further develop the statistics on publications. Fernberger is well known today for having produced "classics" in the history of psychology, one in 1932, the other in 1943. [24] He looked at the evolution of membership and at the increasing emphasis placed on publishing as a criterion for eligibility, and discussed finances, journals of the Association, organization and meetings. He charted the number of papers presented at each meeting since 1892, looked at the "productivity" of universities at these meetings, measuring that 19 universities produced 53% of all papers (p. 55), and looked at what he called the consistency of publication (or regularity over time: number of years in which members published) and the fields of interest, or research interest.

As a preliminary step to his review series, Fernberger looked at the American Psychological Association, following in E. G. Boring's footsteps. In 1920, Boring (of Clark University), as Secretary of the American Psychological Association (1919-1922) and a historian of psychology, [25] published a geographical analysis of Association members. [26] He used data from the 1920 Year Book and produced tables on subjects of instruction by geographical sections of the census, number of psychologists per million population, fields of research, academic rank and academic degrees. All the data were broken down by sex. [27]

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[24] 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.
[25] E. G. Boring (1929), *A History of Experimental Psychology*, New York: Appleton-Century-Crofts.
[26] E. G. Boring (1920), Statistics on the American Psychological Association in 1920, *Psychological Bulletin*, 17 (8), pp. 271-278.
[27] For another statistical study of the time, see: C. R. Griffith (1922), Contributions to the History of Psychology, 1916-1921, *Psychological Bulletin*, 19 (8), pp. 411-428.

Fernberger continued this analysis in the following years. His analyses of the Association's progress began in 1921. [28] Here he extended Boring's analysis to one more dimension, looking at the places where American psychologists were trained, a subject extensively studied by Cattell since 1906 for the scientific profession as a whole. He found a relative concentration: five institutions (Columbia, Chicago, Harvard, Clark and Cornell) had together granted 69.8% of all the doctorates, and eight institutions had granted 85.0%. In 1928, Fernberger updated Boring's numbers and made comparisons for 1920-1928. [29] He looked at the geographical distribution of psychologists by state, place of training, mobility (or inbreeding) by comparing place of training with place of work, positions held, subjects of instruction, and number of psychologists per million population. He also looked at research "interests" or fields of research as reported by psychologists themselves in the Year Book since 1918. [30]

Then, in 1930, Fernberger began looking specifically at publications for a measure of research interest. [31] In a "former study we have analysed only the Year-Book data or, in other words, what the psychologists have *said* about themselves. The present study attempts to rectify this possible source of error by studying what these individuals have actually *done* and published of a research nature" (p. 526). Fernberger now had in hand a more "objective" source. What happened? Before looking at Fernberger's data, we need to discuss the two factors responsible for this development: the concept of productivity, and the *Index* as a measure of science.

[28] S. W. Fernberger (1921), Further Statistics of the American Psychological Association, Psychological Bulletin, 18 (11), pp. 569-572.
[29] S. W. Fernberger (1928), Statistical Analyses of the Members and Associates of the American Psychological Association Inc. in 1928, *Psychological Review*, 35 (6), pp. 447-465.
[30] See also: S.W. Fernberger (1929), Research Interests of American Psychologists, *American Journal of Psychology*, 412 (1), pp. 163-164.
[31] S. W. Fernberger (1930), The Publications of American Psychologists, *Psychological Review*, 37 (6), pp. 526-543.

Whether or not the Advance Has Been Satisfactory

In 1917, S. I. Franz, professor at George Washington University (1906-1921), and scientific director (1909-1919) and then director (1919-1924) of the laboratories of the Government Hospital for the Insane, or St. Elizabeth Hospital, produced a study on the scientific productivity of psychologists. [32] "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, methods for estimating the value of individuals' contributions (elections 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*

[32] S. I. Franz (1917), The Scientific Productivity of American Professional Psychologists, *Psychological Review*, 24 (3), pp. 197-219.

Index) from 1906 to 1915. Six types of contributions were retained: monographs, articles, discussions, books, reviews and reports of meetings. To Franz, such a source of data was ideal: “the failure to list all the psychological publications of any individual rests solely with that individual” (footnote, p. 201).

Franz 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. 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 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 the ratio of actual to expected publications was higher among the younger ones. The same pattern appeared when he constructed a combined index of publication 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).

Interest in productivity was only the first factor behind the measurement of publications as an indicator of scientific progress. The other concerned data sources. While American psychological science was, until now, defined by the members of the Association and their scientific productions, from 1917 the *Psychological Index* itself came to define (or represent) what psychological science was: [33] psychological science was no longer strictly an Association affair, nor an American phenomenon, but an international business. And psychology was fortunate enough to have an *Index* published since 1895 that collected (almost) all (important) titles published yearly worldwide in psychology. The *Index* could be used as a measure of psychological science. [34]

In the same year as Franz (1917), Fernberger turned to international comparisons using the *Psychological Index* as a source of data. He started a series of papers on the scientific production of nations entitled *National Trends in Psychology*. [35] These were published at intervals of ten years from 1917 to 1956. Because the *Index* did not include the place of residence of the authors or their addresses, Fernberger use language as a proxy for the country origin of the titles. His categories were:

[33] On how measuring science using a bibliographical index gives a particular picture of science, see: P. Wouters (1997), The Signs of Science, in B. C. Peritz and L. Egghe (eds.), *Proceedings of the Sixth Conference of the International Society for Scientometrics and Infometrics*, Jerusalem: School of Library, pp. 491-504.

[34] For the period 1895-1936, Fernberger has estimated that the publication indexed 156,861 titles.

[35] 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.

- German (including Austria and some of Switzerland).
- French (including Belgium and some of Switzerland).
- Italian.
- English (broken down into countries from 1940). [36]
- Russian (from 1936).
- Others (broken down into 18 languages from 1946).

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. To Fernberger, "perhaps the most striking point brought out by this study is the extreme necessity for the student of psychology, - no matter of what nationality he may be, - to have a facile and critical reading knowledge of both German and English". [37]

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). [38] "It seems of interest to consider certain aspects of these curves as correlated with coincident political and economic events" (p. 84), suggested Fernberger. [39] 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 increase in nationalistic sentiment of nations led to an increase in other countries (Italy, Russia, small countries). To Fernberger, "political and economic factors (…) have a major influence on the magnitude of scientific publication (…). On the whole, as one would expect, war and

[36] The method used was as follows: "The method employed in the present study was to first examine the name of the author in the hope that he could be recognized and classified as to country. If the name was not recognized, search was made in the membership lists of the American and British Psychological Associations. If still unidentified, *Who's Who*, the *International Blue Book*, *Who's Who in Education* (American) and similar biographical references were consulted. If still unidentified, one sought to find the country of origin of the author by internal evidence in the title, the abstract or the publication in which the article appeared", S. W. Fernberger (1940), A National Analysis of the Psychological Articles Published in 1939, *American Journal of Psychology*, 54 (2), p. 296.

[37] S. W. Fernberger (1917), On the Number of Articles of Psychological Interest Published in the Different Languages, *American Journal of Psychology*, 28 (1): 141-150, p. 150.

[38] 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.

periods of economic depression tend to decrease the volume of scientific output. On the other hand, the presence of a new and crystallized political ideal and of a strong centralized government which tends to encourage and even to subsidize research markedly increases the volume of scientific publication, as in the case of Italy and Russia" (p. 90). And he continued: "But if there is the presence of new political ideals and a strong centralized government which does not particularly encourage and subsidize research, as in Germany, there is apparently a decline in the volume of scientific output. Finally, the growth of a strong nationalistic sentiment markedly tends toward publication in the national language even though such publication, for the smaller countries, must inevitably reduce the size of the audience to which the publication may appeal" (p. 90).

This kind of analysis of the *Index* was extended to the study of the scientific productivity of American psychologists in two more papers. In 1930, Fernberger looked at the publications listed in the *Psychological Index* over a ten-year period (1919-1928). The *Index* listed 3,768 research contributions from 482 psychologists. [40] Fernberger found sex differences in what he called "productivity": an "increase in the number of titles for men for successive years but no appreciable increase in the number of titles for women. In the case of neither sex is the increase in titles comparable to the increase in membership over the same period of time" (p. 527). The numbers were as follows: "the average number of contributions over the ten-year period are 7.77 for men and only 4.09 for women. The men Members of the Association have been almost twice as productive, on the average, as the women" (p. 528). "Almost 14 times as many men as women produced 10 articles or more over the same period" (p. 528). Moreover, men displayed a much broader interest (number of fields in which individuals work). [41]

[39] S. W. Fernberger (1938), Publications, Politics and Economics, *op. cit*.
[40] S. W. Fernberger (1930), The Publications of American Psychologists, *Psychological Review*, 37 (6), pp. 526-543.
[41] On the measurement of women in psychology, see: S.W. Fernberger (1939), Academic Psychology as a Career for Women, *Psychological Bulletin*, 36 (4), pp. 390-394; A.I. Bryan and E.G. Boring (1944), Women in American Psychology: Prolegomenon, *Psychological Bulletin*, 41 (6), pp. 447-456; A.I. Bryan and E.G. Boring (1946), Women in American Psychology: Statistics from the OPP Questionnaire, *American Psychologist*, 1 (12), pp. 71-79.

Fernberger then looked at several variables and their effects on productivity: geographical distribution and consistency of publication, date of degree ("on the whole, the results show greater productivity for the individuals of more recent degree but the differences are very slight", p. 534), place of degree (an average of 6.91 papers per year, which was surpassed by Cornell, Harvard and Chicago, p. 537) and size of town: "On the whole, the towns with a population greater than 250,000 are responsible for the greatest productivity (…)" (p. 540). But productive research [more than 10 papers] may be carried on irrespective of the size of the town in which the research worker is located" (p. 541).

Again in 1938, Fernberger measured the scientific productivity of American psychologists. [42] To Fernberger, "a study of published titles can be the only objective method of obtaining this information" (p. 262). He looked at publications indexed in the *Psychological Abstracts* for 1932-1936. [43] He found 587 individuals responsible for 3,963 titles, or an average of 6.75 per member (p. 269). He also found a wide range of "variability of frequency of publication": "22% of the members published nothing at all and one individual published 49 titles or an average of almost 10 titles each year" (p. 269). He compared the volume of publications of academics and non-academics and found the former two and one-half times more productive: an average of 7.8 titles for academics versus 3.1 (p. 277). With regard to the consistency of publication, he measured that 22% of psychologists published nothing, and that 40% of the members "missed publication only one year or not at all". He tried to compare the research interests as classified in the *Abstracts* to those indicated by the members in the Year-Book for 1918-1937, but the classifications were too different to allow a meaningful comparisons (p. 274).

Fernberger's work has been quite original. Following Franz, he started measuring scientific productivity systematically by counting publications. This he did using the

[42] 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.
[43] From 1936 onward, Fernberger used the *Psychological Abstracts* as the successor to the now-discontinued *Psychological Index*.

Index. [44] He also started looking at productivity from an international perspective. What remained to be done was to apply publication counts to the community of scientists as a whole. This he left to others.

Man's Most Creative Years

In 1928, a paper appeared in the *American Journal of Psychology* under the pseudonym Helen Nelson. [45] The author criticized the view, expressed by R. S. Woodworth of Columbia University, among others, that "the period from twenty years up to forty seems to be the most favorable for inventiveness". To Nelson, "that the years between twenty and forty are productive ones for genius there can be no doubt. But that the manifestations of genius are mainly confined to this score of years, genius itself disproves" (p. 303).

This publication launched a whole series of studies by psychologists in the following decades on the creativity of scientists. Psychologists began looking at scientists from all disciplines, not only their colleagues in the discipline of psychology. They thus entered the field of "science studies". Counting papers was one of the methods used to measure creativity (the other was questionnaires and performance tests). H. C. Lehman, from Ohio University, was the most active author on this subject for over twenty years. [46] He was particularly interested in the following question "At what age are men likely to do their most outstanding work?". [47] He looked at the arts, literature, philosophy and the sciences. He used histories, dictionaries, biographies and source books to measure outstanding contributions (publications, inventions). In all his studies, Lehman confirmed

[44] For other studies of the time using the *Index* (or *Abstract*) for studying research interests, see: F.L. Goodenough (1934), Trends in Modern Psychology, *Psychological Bulletin*, 31 (2), pp. 81-97; W.S. Hunter (1941), Research Interest in Psychology, *American Journal of Psychology*, 54 (3), pp. 606-607. Still other studies on research interests used a sample of the "best" journals, as ranked by researchers; see: G.W. Allport (1940), The Psychologist's Frame of Reference, *Psychological Bulletin*, 37 (1), pp. 1-28; J.S. Bruner and G.W. Allport (1940), Fifty Years of Change in American Psychology, *Psychological Bulletin*, 37 (10), pp. 757-776.

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

[46] Several of Lehman's papers are collected in H. C. Lehman (1953), *Age and Achievement*, Princeton: Princeton University Press.

Woodworth's judgment. But he also went further. In *Man's Most Creative Years*, published in 1944, Lehman asked whether quantity was related to quality, as measured by his sources. [48] The answer was no. Using diverse sources again, he charted age versus productivity in eleven fields from science and invention to poetry and music, concluding: "Quality of output and quantity of output are not necessarily correlated, output of the very highest merit tending to fall off at an earlier age level than does output of lesser merit" (p. 392).

In a similar vein, W. Dennis (Brooklyn College) looked at eminent scientists and their scientific production. [49] Using the *Biographical Memoirs* of the US National Academy of Sciences, Dennis retained 41 men whose names appeared between 1943 and 1952 and who reached the age of 70. From the biographies, he calculated that these men have been responsible for 8,332 papers, or 203 per year on average. Then, Dennis looked at the 25 most eminent scientists of the nineteenth century, as selected from histories on the basis of space devoted to them in encyclopedias and dictionaries of biography, a method called historiometry. [50] He determined their publications using the *Catalogue of Scientific Literature, 1800-1900*, published by the Royal Society of London. He found opposite results to Lehman's, namely a "definite relationship between productivity and eminence in science (…). The greater the number of pieces of scientific work done by a given man, the greater the likelihood that one or more of them will prove to be important" (p. 182). "In science, quantity and quality are correlated" (p. 183).

In a second study, Dennis looked at the age at which scientists produce most. Again, he calculated numbers different from Lehman's, concentrating on all types of publications, not just the best. [51] From the *Webster's New International Encyclopedia* (1930), he

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[47] H. C. Lehman (1936), The Creative Years in Science and Literature, *The Scientific Monthly*, 43 (2), August, pp. 151-162.
[48] H. C. Lehman (1944), Man's Most Creative Years: Quality versus Quantity of Output, *The Scientific Monthly*, 59 (5), November, pp. 384-393.
[49] W. Dennis (1954), Bibliographies of Eminent Scientists, *Science*, 79 (3), September, pp. 180-183.
[50] "Historiometry is to history what biometry is to biology": the statistical study of men through dictionaries and biographies. F. A. Woods (1909), A New Name for a New Science, *Science*, 30 (777), November 19, pp. 703-704; F. A. Woods (1911), Historiometry as an Exact Science, *Science*, 33 (850), April 14, pp. 568-574.
[51] W. Dennis (1956), Age and Productivity among Scientists, *Science*, 123 (3200), April 27, pp. 724-725.

selected the 156 scientists who lived to age 70 or beyond between 1800 and 1900, and counted the number of their publications as listed in the *Catalog of Scientific Literature*. He calculated that "in the 30s a high average rate of productivity is reached [2 publications per year], and this rate is maintained for three decades" (p. 724). A. Roe, a prolific psychologist who published several studies on the psychology of scientists in the 1950s and 1960s, confirmed such a persistence of publication over many years in the case of eminent scientists. [52]

This fascination with the ideal age at which scientists produced their most important work is an old and recurrent theme in the social sciences. [53] However, this fascination was only one of the reasons for studying age. The other was practical, namely the reproduction of the "species". Among the first statistics on science produced in history, many counted members of scientific societies, their age at election and the death rate. This was motivated by establishing or revising the rules of Academies with regard to membership and the election procedures. [54]

In the 1950s and 1960s, American psychological studies on scientists, their creativity and its measurement exploded. [55] These studies were often conducted under contracts from

[52] 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.

[53] See, for example: W. I. Wyman (1919), Age of Production in Invention and Other Fields, *Journal of the Patent Office Society*, 1, pp. 439-446; C. W. Adams (1946), The Age at Which Scientists Do Their Best Work, *ISIS*, 36, pp. 166-169; J. Schmookler (1956), The Age of Inventors, *Journal fo the Patent Office Society*, April, pp. 223-232; E. Manniche and G. Falk (1957), Age and the Nobel Prize, *Behavioral Science*, 2, pp. 301-307; H. Zuckerman and R. K. Merton (1972), Age, Aging, and Age Structure in Science, in M. W. Riley et al. (eds.), *A Sociology of 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.

[54] A. Schuster (1925), On the Life Statistics of Fellows of the Royal Society, *Proceedings of the Royal Society*, A107, pp. 368-376; 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.

[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

departments like the Office of Naval Research, the Air Force, NASA and the National Institute of Health, or industries and their associations (such as the Industrial Research Institute). At the same time, the idea of creativity came to be associated with or simply transformed into that of "productivity": sheer volume of papers or author/paper ratios. The management of research, the performances of organizations and the factors responsible for efficiency, and accounting were responsible for this metamorphosis. [56] Psychologist D. C. Pelz of the University of Michigan was among the influential authors responsible for this movement. The book written with F. M. Andrews and entitled *Scientists in Organizations* became a classic in the management of technology for decades. [57] Extending studies initiated in the 1950s, [58] the authors studied the factors, conditions and environments that were most conducive to creativity or productivity in research and innovation – freedom, communication, diversity, dedication, motivation, satisfaction, groups and…age [59] – and developed performance measures. Thereafter, scientific productivity, defined as the production of papers, came to be measured by

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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, *Sociology of Science*, New York: Free Press, pp. 329-343.

[56] R. M. Hogan (1950), Productivity in Research and Development, *Science*, 112 (2917), November 24, pp. 613-616; 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; B. V. Dean (1968), *Evaluating, Selecting, and Controlling R&D Projects*, American Management Association.

[57] D. C. Pelz and F. M. Andrews (1966), *Scientists in Organizations: Productive Climate for Research and Development*, New York: John Wiley.

[58] D. C. Pelz (1956), Some Social Factors Related to Performance in a Research Organization, *Administrative Science Quarterly*, 1, pp. 310-325.

[59] Pelz confirmed Lehman's results that the 30s was a productive period, but he found a bimodal curve: there is a second period of productivity in the 50s.

sociologists, [60] increasingly by way of the newly created *Science Citation Index* (1963), [61] and a whole community of specialists, called bibliometricians, developed.

Conclusion

Currently, the community of bibliometricians defines itself with reference to the work of scholars published in the 1950s, for example V.V. Nalimov, [62] E. Garfield [63] and, above all, D.J.D. Price. [64] As a matter of fact, Price looked at several disciplines, instead of just one like psychologists. He was interested in measuring science to study the growth of knowledge and its laws, a much-discussed topic of the day, [65] particularly among librarians [66] "charged with the management of their monster". [67] His systematic writings

[60] 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. S. 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.

[61] E. Garfield and I. H. Sher (1963), *Science Citation Index*, Philadelphia: Institute for Scientific Information).

[62] *Scientometrics* (2001), V.V. Nalimov: Memorial Issue, 52 (2).

[63] B. Cronin and H. B. Atkins (eds.) (2000), *The Web of Knowledge: A Festschrift in Honor of Eugene Garfield*, Medford: ASIS Monograph Series; *Current Science* (2005), Fifty Years of Citation Indexing, 89 (9-10), pp. 1502-1554.

[64] *Scientometrics* (1985), Derek John de Solla Price: Memorial Issue, 7 (3-6); J. Furner (2003), Little Book, Big Book: Before and After Little Science, Big Science: A Review Article, *Journal of Librarianship and Information Science*, 35 (2), pp. 115-125 and 35 (3), pp. 189-201.

[65] H.C. Lehman (1947), The Exponential Increase of Man's Cultural Output, *Social Forces*, 25, pp. 281-290; P. Weiss (1960), Knowledge: A Growth Process, *Science*, 131 (3415), June 10, pp. 1716-1719; G. Holton (1962), Scientific Research and Scholarship: Notes toward the Design of Proper Scales, *Daedalus*, 91, pp. 362-399.

[66] S. Herner (1956), Technical Information: Too Much or Too Little, *Science*, 83 (2), August, pp. 82-86.

were clearly linked to the emerging field of science studies. For all these reasons, Price has to be considered as one of the founders of bibliometrics (as well as scientometrics), in J. Ben-David's sense. [68] But there were forerunners. [69] Who were they?

Several "histories" of bibliometrics cite early works by librarians, [70] or argue that the chemical sciences were "at the vanguard of these profound changes" in information science. [71] Others are a little more exhaustive, [72] but a complete history of bibliometrics remains to be written. In general, the very early works of the early 1900s are forgotten, or mentioned very rapidly and selectively as prehistory. [73] The systematic use of bibliometrics by psychologists is never mentioned. In fact, the only psychologists' works on bibliometrics discussed in the literature are two occasional studies based on or containing an analysis of citations. [74] Why? Because authors generally argue that the "real" history starts with Garfield's *Science Citation Index* and its influential innovation in the systematic indexing of citations. As Garfield himself suggested: "Such an "impact factor" may be much more indicative than an absolute count of the number of scientists' publications, which was used by Lehman and Dennis" (p. 109). [75]

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[67] D.J.D. Price (1961), *Science since Babylon*, New Haven: Yale University Press, p. 104.

[68] Ben-David distinguished between forerunners and founders, the latter having "students". J. Ben-David and R. Collins (1966), Social Factors in the Origins of a New Science, reprinted in G. Freudenthal (ed.) (1991), *Scientific Growth; Essays on the Social Organization and Ethos of Science*, Berkeley: University of California Press, pp. 49-70.

[69] As regards scientometrics, Cattell is a founder, not a forerunner. See: B. Godin (2007), From Eugenics to Statistics on Science, *op. cit*.

[70] See, for example: R. N. Broadus (1987), Early Approaches to Bibliometrics, *Journal of the American Society for Information Science*, 38 (2), pp. 127-129; P. Wouters (1999), *The Citation Culture*, Doctoral dissertation, University of Amsterdam.

[71] A. Thackray and D. C. Brock (2000), Eugene Garfield: History, Scientific Information, and Chemical Endeavor, in B. Cronin and H. B. Atkins (eds.), The Web of Knowledge: A Festschrift in Honor of Eugene Garfield, *op. cit.*, pp. 11-23.

[72] J. Meadows (2000), The Growth of Journal Literature: a Historical Perspective, in B. Cronin and H. B. Atkins (eds.), The Web of Knowledge, *op. cit.*, pp. 87-107.

[73] For a comprehensive bibliography, see: A.G. Pritchard and G.R. Wittig (1981), *Bibliometrics: a Bibliography and Index*, Watford, Hertfordshire: ALLM Books.

[74] H. Cason and M. Lubotsky (1936), The Influence and Dependence of Psychological Journals on Each Other, *Psychological Bulletin*, 33 (1), pp. 95-103; K. E. Clark (1957), *America's Psychologists: A Survey of a Growing Profession*, Washington: American Psychological Association.

[75] E. Garfield (1955), Citation Indexes for Science: A New Dimension in Documentation Through Association of Ideas, *Science*, 122 (3159), July 15, pp. 108-111.

Certainly, the *Science Citation Index* was "the first really serious attempt at universal bibliographical control of science literature since the turn of the century" (p. 649). [76] But we have to distinguish two uses of bibliometrics here. The first is counting papers, and here psychologists were definitely forerunners. Although limited to their own discipline at first, the *systematic* use of publications as an indicator for science can be traced back to psychologists. The other use of bibliometrics is citation analysis. Several authors conducted this kind of analysis starting in the late twenties, including psychologists. [77] It is to Garfield and the *Science Citation Index* that we owe the tool that would allow the systematic studies of citations.

Psychologists were not methodologists in bibliometrics. However, they were consciously aware of the limitations of statistics on publications. Fernberger knew of the partial coverage of the *Psychological Index*, which badly indexed some countries' literature and did not index gray literature at all. [78] He also mentioned the possibility of (negligible) discrepancies because of lags in indexing (he counted the papers from the date of publication in the *Index* rather than from the actual year of publication of the papers). [79] Above all, he knew that the "value" of papers differs and that he was not measuring "the value of the contributions of the different countries" but "the interest in psychology". [80] Similarly, Cason and Lubotsky's early study of citations interpreted citations as measuring influence, not quality: cross-references between psychological journals is a "quantitative measure of the extent to which each psychological field influences and is influenced by each other psychological field". [81] Finally, one can mention Franz's

[76] E. Garfield (1964), Science Citation Index: A New Dimension in Indexing, *Science*, 144 (3619), May 8, pp. 649-654.
[77] H. Cason and M. Lubotsky (1936), The Influence and Dependence of Psychological Journals on Each Other, *op. cit*.
[78] S. W. Fernberger (1917), On the Number of Articles of Psychological Interest Published in the Different Languages, *American Journal of Psychology*, 28 (1): 141-150, pp. 144-145.
[79] *Ibidem*.
[80] S. W. Fernberger (1926), On the Number of Articles of Psychological Interest Published in the Different Languages, *American Journal of Psychology* 37 (4): 578-581, p. 578.
[81] H. Cason and M. Lubotsky (1936), The Influence and Dependence of Psychological Journals on Each Other, *op. cit*., p. 95.

discussion of fractioning of multiple-authors papers and their decision to assign "a joint article to both individuals, and given in each case its full value". [82]

Despite the limitations of their sources, psychologists have been quite imaginative. While Boring was publishing one of the first network analyses (or genealogies) of psychology, [83] Fernberger conducted a survey of graduates to measure "the prestige and impact of various psychologists". [84] He asked 2,288 students in psychology "to check a list of names, indicating by one check the familiar names, and by two checks the names of those whose special fields of competency were known to them" (p. 288). He compared the judgments of students (ranks) to objective measures like "productivity" in publications, as listed in the *Psychological Register* and the *Psychological Abstract*, and to starred psychologists (among the thousand best scientists from *American Men of Science*). [85] He came to the conclusion that "only relatively few individuals seem to have made a really lasting impression (…). Among the highest ranking psychologists, in this study, are those whose major contributions are recent, as well as those who are historical figures" (p. 298). To Fernberger, age did not matter. He identified three factors responsible for impact and prestige: amount of publications, quality of the published publication, and kind of contribution (like opening a new field, contributions to a number of fields, or development of a new technique).

[82] S. I. Franz (1917), The Scientific Productivity of American Professional Psychologists, *op. cit*., footnote, p. 202.
[83] M. D. Boring and E. G. Boring (1948), Masters and Pupils Among the American Psychologists, *American Journal of Psychology*, 61 (4), pp. 527-534.
[84] S. W. Fernberger (1954), The Prestige and Impact of Various Psychologists on Psychology in America, *American Journal of Psychology*, 67 (2), pp. 288-298. On a very first study (by questionnaire) on the prestige of psychologists, see: M.A. Tinker, B.D. Thuma and P.R. Farnsworth (1927), The Rating of Psychologists, *American Journal of Psychology*, 38 (3), pp. 453-455.
[85] A few years later, another psychologist, K. E. Clark, used a similar method, comparing citations received with judgments of experts and other indices of eminence. See K. E. Clark (1957), America's Psychologists: A Survey of a Growing Profession, *op. cit*.

These were only some of psychologists' studies in bibliometrics. [86] Why did psychologists get involved in measuring science, including scientific productivity? One factor has to do with their background. Experimental psychology is an empirical science. In the very early psychological laboratories, psychologists were reading instruments and measuring time reactions or sensory judgments, like brightness or size. [87] Then, statistics came to be used in a large number of specialties in psychology: questionnaires were developed to measure personality, and performance tests and attitudes scales were constructed, giving rise to what came to be named psychometry. [88] Measuring the psychological discipline itself was therefore a small and easy step to many psychologists. But the factor that contributed most to the psychologists' use of publication counts was the crusade for the advancement of psychology as a science. And here, psychologists measured the progress of the discipline with the support of an experimental and "quantitativist" colleague who served the cause of the advancement of all the sciences with statistics for over thirty years and launched the field of scientometrics in 1906: J. M. Cattell.

Certainly, Cattell has not participated much in bibliometrics. After his 1903 paper, [89] he counted publications only twice. The first appeared in an address given on the occasion of the celebration of the 25th anniversary of the American Psychological Association in 1916. Here, Cattell used bibliometrics minimally: he constructed a chart of the papers

[86] Psychologists also measured the publication of bibliographies, the distribution of journals by countries, the number of chapters and pages devoted to various subjects in books and textbooks, and words in titles. See: E.G. Boring (1928), Do American Psychologists Read European Psychology?, *American Journal of Psychology*, 40 (4), pp. 674-675; R.A. Davis and S.E. Gould (1929), Changing Tendencies in General Psychology, *Psychological Review*, 36 (4), pp. 320-331; C.M. Louttit (1929), The Use of Bibliographies in Psychology, *Psychological Review*, 36 (4), pp. 341-347; C.M. Louttit (1931), Psychological Journals: A Minor Contribution to the History of Psychology, *Psychological Review*, 38 (5), pp. 455-460; A.R. Lauer (1931), Why Not Re-Christen the "Psycho-Galvanic Reflex"?, *Psychological Review*, 38 (4), pp. 369-374.

[87] On early lists and statistics on American laboratories, see: E.-B. Delabarre (1894), Les laboratories de psychologie en Amérique, *L'Année psychologique*, 1, pp. 209-255; J.M. Cattell (1898), The Psychological Laboratory, *Psychological Review*, 5, pp. 655-658; C.A. Ruckmich (1926), Development of Laboratory Equipment in Psychology in the United States, *American Journal of Psychology*, 37 (4), pp. 582-592; J.M. Cattell (1928), Early Psychological Laboratories, *Science*, 67 (1744), June 1, pp. 543-548; C.R. Garvey (1929), List of American Psychology Laboratories, *Psychological Bulletin*, 26 (11), pp. 652-660.

[88] G. H. Hornstein (1988), Quantifying Psychological Phenomena: Debates, Dilemmas, and Implications, in J. G. Morawski (ed.), *The Rise of Experimentation in American Psychology*, New Haven: Yale University Press, pp. 1-34; K. Danziger (1990), *Constructing the Subject: Historical Origins of Psychological Research*, Cambridge: Cambridge University Press.

[89] J. M. Cattell (1903), Statistics of American Psychologists, *op. cit.*

presented at the 25 meetings of the Association, and showed a great decrease in historical, philosophical, analytical and introspective papers, and a great increase in papers concerned with the measurement of individual differences in behavior. [90] He did not, however, answer his introductory question: "We may wonder whether the importance of the work accomplished in this country for psychology has increased in the same ratio as the number of those engaged in it" (p. 279). The second use of bibliometrics Cattell made was presented as an address to the 9th International Congress of Psychology, Yale University, New Haven, in 1929. Cattell simply reproduced Fernberger's data, updating them for one year. [91] Cattell's contribution to statistics on science then is not bibliometrics. Rather, it was with his many reviews of the psychological profession and his numerous statistical analyses of the scientific community published on the basis of his directory *American Men of Science* [92] that Cattell "taught" his peers how to use statistics to promote the advancement of the profession: "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), regularly suggested Cattell. [93] Figures "show the advantage of statistics over general impressions" (p. 688). [94] It is an "objective method". [95] Psychologists listened to the point of pioneering the systematic use of bibliometrics.

[90] J. M. Cattell (1917), Our Psychological Association and Research, *op. cit*.
[91] J. M. Cattell (1929), Psychology in America, *Science*, 70 (1815), October 11, pp. 335-347.
[92] For the first time, a study of the directory was conducted on psychologists in 1939 by geographer S. S. Visher. See: S. S. Visher (1939), Distribution of the Psychologists Starred in the Six Editions of American Men of Science, *American Journal of Psychology*, 52, pp. 278-292. One finds also a table furnished by Cattell in H.D. Kitson (1926), A Preliminary Personnel Study of Psychologists, *Psychological Review*, 33 (4), pp. 315-323.
[93] J.M. Cattell (1910), A Further Statistical Study of American Men of Science, *op. cit*.
[94] J. M. Cattell (1910), A Further Statistical Study of American Men of Science II, *Science*, 32 (828), November 11, pp. 672-688.
[95] J. M. Cattell (1922), The Order of Scientific Merit, *Science*, 56 (1454), November 10, p. 547.