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Scientometrics: the project for a science of science transformed into an industry of measurements

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ABSTRACT

This paper discusses the intellectual justification of scientometrics through the claim that it is part of the quest for a quantitative *science of science*. Initially, I will make a brief description of scientometrics' historical background. Next, I will explain that those disciplines that have been satisfactorily mathematized always contain two distinct basic components: an *axiomatic,* defining the operations that can be realized with the available data, and an *interpretation* of their meaning. Counting papers and citations is a way to collect statistical data about scientific activities, and therefore the axiomatic basis of scientometrics comes from statistics. Regarding the interpretation of scientometrics, I will argue that the meanings attributed to their key concepts are usually borrowed from economics. Then I discuss how the promise of a science of science becomes a too well adjusted historical narrative that apparently justifies the economic concerns of governments and private corporations.

Keywords • Scientometrics. Citation analysis. Quantitative methods. Commoditization.

The significance of science and technology for economic development is currently taken for granted as an indisputable premise of any science policy. A wide range of authors from the nineteenth century already had emphasized the connections between science, technology, and economy, but it was mainly after 1940 that scientific and technological activities began to be systematically accounted by those governments that considered them indispensable requisites for competitive economies. Contemporary quantitative studies on scientific and technological activities make up the field generally called *scientometrics*, which includes a wide variety of subjects encompassing at least four subtypes: "science and technology indicators, information systems on science and technology, the interaction between science and technology, and cognitive as well as socio-organizational structures in science and technology" (Raan, 1997, p. 205). In fact, there is no sharp limit between scientometrics and other empirical studies on science and technology, but it is intuitively recognized that scientometrics primarily aims to *measure* science. The name of the discipline intends to express such idea.

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1 The promise of a *science of science*

The polymath Francis Galton actually made the very first surveys on the number of men of science in England (cf. Godin, 2007a). This initial effort of "measurement of science emerged out of the interest in great men, heredity and eugenics and the contribution of great men to civilization" (Godin, 2008, p. 8). Few decades later, the psychologist James M. Cattell compiled and published a large collection of biographical and statistical information about scientists in the United States (cf. Cattell, 1905), known as the *American men of science*. It is important to notice that those surveys were conducted by isolated scientists who above all intended "to contribute to the advancement of science and of the scientific profession" (Godin, 2008, p.8). Cattell, in particular, was politically engaged and used to make severe critics to Columbia University governance – where he worked from 1891 until being fired in 1917 – which, according to him, had limited academic freedom. He actually used the data from the second edition of the *American men of science* as evidence to argue that universities that adopted democratic methods of appointments and promotions employed a larger number of eminent scientists (cf. Sokal, 2009). By the way, it is noteworthy that a statistical ranking had been part of an argument in favor of academic freedom.

After 1920's onwards, statistical surveys on science were no longer made by isolated scientists but became supported by governmental agencies and international organizations. Ever since, investments in scientific research, technological innovation, patents and expenditures in higher education for improving scientific and technical skills began to be widely employed as indicators of economic development. Thus, around the end of the World War II, economic and geopolitical concerns replaced that somewhat romantic motivation of Galton and Cattell.[1] In the United States, the National Bureau of Economic Research (NBER) begun to collect and publish research and development (R&D) statistical series, and other countries organized their own databases. At that time, however, there was no unified methodology able to provide reliable comparisons between countries. This problem motivated the formulation of a standard methodology for collecting statistics on R&D, and resulted in the *Frascati manual* published in 1963 by the Organization of Economic Co-operation and Development

1 One of the first philosophers concerned with the economic constraints of the scientific activities was the pragmatist Charles S. Peirce. In 1879, Peirce wrote his "Note on the theory of the economy of research" where he proposed a cost-benefit analysis of scientific inquiries. According to Wible (1994, p. 135): "The Note presents an economic model of research project selection in science (...) The Note is also significant for making economic factors a central part of a theory of scientific inference, something which contemporary economic methodologists and philosophers still have not done except for a few notable exceptions". For a detailed examination Peirce´s achievements in such context, see also Rescher (1976).

(cf. OECD, 2002). Christopher Freeman, distinguished English economist, founder and first director of Science and Technology Policy Research Unit at the University of Sussex, prepared the first edition of the *Frascati manual*. According to Godin:

> Christopher Freeman was the ideal person to work on such a manual because he was one of the few people at that time with hands-on experience of designing and analyzing a survey of R&D (...) E. Rudd, from the British Department of Scientific and Industrial Research (DSIR), suggested to the OECD that Freeman be invited as consultant to work on what would become the *Frascati Manual* (...) In the following decades, the manual served as the basis for surveying R&D in member countries, for collecting international data at OECD, and for analyzing trends in science. The manual also gave official statisticians their main indicator on science: Gross Expenditures on R&D, or GERD – the sum of expenditures devoted to R&D [by universities, government, industry and non-profit organizations] (Godin, 2007b, p. 1394).

Ever since OECD has adopted a standard cost-benefit accounting, usually called "input-output model" (cf. Freeman 1962, 1967; Leontief, 1936). Here I do not intend to discuss this model and its limitations, but only to highlight that is an application of traditional econometric statistics on the specific domain of R&D.

The specific term "scientometrics" was originally coined by the Russian mathematician V. V. Nalimov, consisting in the English translation of the title of Nalimov's monograph "Naukometriya" published in 1969 (cf. Garfield, 2007). Nevertheless, the history of scientometrics includes earlier contributions from Alfred Lotka (1926), Samuel C. Bradford (1934), John Desmond Bernal (1939), George K. Zipf (1949), and the dominant figure of Derek de Solla Price.[2] Thus, official (governmental) R&D statisticians and economists were not the only protagonists in the history scientometrics, since this discipline has also encompassed others investigation trends.

From the point of view of its adherents, we usually find out the claim that the discipline was chiefly motivated by two main ideas. First, that making a "science of science" would eventually inform better science policies; second, that only a quantita-

2 A survey on the most cited papers in the history of scientometrics, made by Garfield (2007), includes in the first ten places: 1) Little science, big science (Price, 1963); 2) Networks of scientific papers (Price, 1965a); 3) Science since Babylon (Price, 1961); 4) Scientific community (Hagstrom, 1965); 5) The frequency distribution of scientific productivity (Lotka, 1926); 6) Collaboration in an invisible college (Price & Beaver, 1966); 7) The structure of scientific revolution (Kuhn, 1962); 8) The Matthew effect in science (Merton, 1968); 9) Is technology historically independent of science? A study in statistical historiography (Price, 1965b) and 10) Visibility and the structural bases of awareness of scientific research (Cole & Cole, 1967).

tive approach to the subject would be able to produce objective knowledge about it (cf. Merton & Garfield, 1986; Wouters & Leidersdorff, 1994). The so-called fathers of scientometrics are often presented as devoted pursuers of those two ideals, and due to its form, its content, and the number of citations received — a bibliometric criteria par excellence, however controversial – Price's book *Little science, big science* has been elected the most important work in the history of scientometrics. In the re-edition of this famed book, Merton and Garfield acknowledged Price's theoretical influence on quantitative studies of science and emphasized the policy issues raised by him:

> For in elucidating the social and cognitive arithmetic of science, this book did much to lay the foundations of the field of inquiry given over to the quantitative analysis of science and scientific development – the field that has come to be known as scientometrics, or, at times, bibliometrics. (...) Fired by Price's ample numerical imagination, this book is dedicated to establishing and interpreting the magnitudes of growth in "the size of science": in the numbers of scientists and scientific publications and in the societal resources allocated to the pursuit of science and science-based technology. But, as is emphatically asserted, it is not so much the sheer exponential growth in the size of science – an estimated five orders of magnitude in three centuries – as the logistic character of that growth that calls for special notice. It is argued that the inevitable saturation of science will require freshly formulated science policy: "new and exciting tactics for science" (Merton & Garfield, 1987, p. 73-4).

It is noteworthy that the most emphatic appraisals to Price's works came from adherents of functionalist schools of social sciences, in particular Merton and his followers whose social model of science depends on the notion that it works as a complex rewarding system. Price and Merton clearly converged in at least two specific points: "the fact that it is excluded, for a practitioner of scientometrics, the possibility to discuss the content of science (...) and the postulate according to which science is reducible to an autonomous entity that can be studied objectively" (cf. Shinn & Ragouet, 2008, p. 43). Nevertheless, most affinities between Mertonian functionalists and scientometricians have concerned, above all, their belief that citation analysis could explicit the mechanisms of social recognition in the scientific community. For instance, Eugene Garfield, the founder of the *Institute for Scientific Information* (ISI), usually claimed that "a lot of people are passed over in the informal reward system of science... citation analysis became for me a vehicle to transform an informal system of recognition into an explicit reward system for science" (Cawkell & Garfield, 2001, p. 150). This claim, by the way, echoes Cattell's earlier criticism to American universities' administration, whose salaries and promotions policies were hardly informed by objective criteria (cf. Cattell, 1913).

In the meantime, one could wonder whether those high expectations were actually met or not. As matter of fact, no one can deny that in the last three decades the extraordinary amount of quantitative measurements has influenced science policies around the world. However, what about the alleged scientific character of such enterprise? "Price's dream" (Wouters & Leydersdorff, 1994) was that scientometrics would eventually overcome the supposed limitations of qualitative approaches. Did it come true?

Price used to describe "the development of scientometrics as the emergence of a "relatively hard" social science" (Wouters & Leydersdorff, 1994, p. 194). According to Price's (1970) analysis of 162 journals, the literature in the research fronts of science – particularly in physics and biochemistry – exhibits an "immediacy effect" due to their fast and vigorous development. In order to show this immediacy effect, Price compared different areas proportion of citations in the last five years – a measure known as *Price's Index*. For him, the fundamental difference between "hard" disciplines and "soft" ones is that the former exhibits a short-temporal pattern of citation frequency (thus a higher Price's Index) while the latter does not.

In summary, the *signature* of research fronts would consist in some type of "citations freshness", which could be detected by Price's Index. Wouters and Leydersdorff (1994) applied this criterion to examine whether scientometric studies actually exhibits high immediacy effect, but the results showed a citation temporal-pattern not significantly different from traditional social studies on science. Therefore, although scientometric studies make intensive use of descriptive statistics, they are far from being that hard science envisioned by Price. Looking retrospectively, the promise of a *science of science* sounds as a beautiful narrative starring remarkable protagonists engaged in a somewhat promethean adventure. However, one may wonder whether it does not sound a too well adjusted narrative.

2 Formalism and interpretations: the case of the impact factor

Practitioners of scientometrics usually claim that their quantitative methods provide greater objectivity in comparison with other qualitative approaches to scientific practices. Despite the controversial character of such a claim – which I will discuss further – it is important to remember that all disciplines that have been satisfactorily mathematized always contain two distinct basic components: a formalism, defining the operations that can be realized with the available data, and an interpretation of their meaning. As matter of illustration, let us examine the measure known as *arithmetic mean*. This is a purely formal operation used to estimate the central tendency of a collection of numbers, and defined as the sum of the numbers divided by the size of the collection. So, if we wish to know the average weight of a given population (for instance, the students of a college), we sum the weight of each person and divide by the total number of persons. By the way, a medical researcher could interpret this average weight as a health "indicator" regarding the eating habits of the students. Now, in case we wish to know the average number of cigarettes smoked per day by the same population, the arithmetic mean would not be interpreted as a useful "indicator" because the subpopulations of smokers and non-smokers differ absolutely in regard to the habit of smoking. The mathematical formula would be the same, but the first interpretation sounds reliable, while the second does not.

In the field of scientometrics, we use the arithmetic mean to calculate the so-called *impact factor* of scientific journals. In this case, the formula consists in the number of citations of a journal during a period of three years divided by the number of citable articles published by the journal in the same period. The *meaning* attributed to such metrics, however, depends on the *interpretation* about the components of the formula. Firstly, we have to accept the assumption that the number of citations somehow represents a measure of the quality of the papers published by journals. This keystone assumption – that scientists' citing behavior is correlative to a definite value attributed to cited works – is very controversial because "not enough is known about the 'citation behavior' of authors - why the author makes citations, why he makes his particular citations, and how they reflect or do not reflect his actual research and use of the literature" (Smith, 1981 *apud* Wouters, 1998, p. 230). Secondly, why choosing a period of three years, rather than four or five? Is it purely arbitrary choice? Indeed, it comes from Solla Price's immediacy index. The choice of three years instead of five can be seen as arbitrary; however, it preserves the original idea that innovative scientific areas have short-temporal citation pattern.

Even if we disregard the questions above, there is much criticism to the adequacy of impact factor because this "refers to the average number of citations per paper, but this is not a normal distribution. It is rather a Bradford distribution[3] (…) Being an arithmetic mean, the impact factor therefore is not a valid representation of this distribution and unfit for citation evaluation" (Tamilselven & Balasubramanian, 2012, p. 11). For example, the editors of Nature "have analyzed the citations of individual

3 The Bradford distribution (or Bradford´s Law) is a negative exponential distribution described by Samuel Bradford, in 1934, according to which there are a few very productive journals, a larger number of moderate producers, and a still larger number of constantly diminishing productivity.

papers in Nature and found that 89% of last year's figure [of citations] was generated by just 25% of our papers" (Nature, 2005, p. 1003-4). This result definitely shows that arithmetic mean is not a reliable indicator since a small portion of cited papers induces most part of citations (compare it with the "average number of cigarettes" example discussed above).

Finally, there is a strong semantic bias in the term *impact factor*. Its correct name should be "average citation in the last three years" but this unpretentious name probably would be neither seductive, nor merchantable. Indeed, *impact factor* is a fantasy name for an ISI product, published in the Journal of Citation Report. I do not intend to discuss this marketing strategy, but let me highlight one important detail. We call something a "factor" as it works as a "cause" or "input" that influences something else. However, the number of citations is not a cause but an "effect" or "output" resulted from the credits attributed by other authors. There is an inversion operating here because to publish in journals that exhibits high *impact* factors *seems* to be a warranty of being read and cited. As noticed above, however, just a minority of papers are highly cited.

3 Economic interpretations of scientometrics

Scientometric reports (e.g. UNESCO, 2010; OECD, 2011) usually appear as a kind of *accounting of science* based on the supposition that we can evaluate scientific development like the increased production of a commodity (cf. Raan, 1997; Gibbons & Wittrock, 1985). In a general sense, a commodity is any marketable item; but in a narrower sense, it is a marketable item that exhibits *fungibility*, i.e., the property of a good or service whose individual units permit mutual substitution. Units of goods like soybeans are evaluated by the markets as equivalents and, in accordance, if we want to compare the production of such a commodity by distinct producers, it is enough to compare the number of units produced. However, there are marketable goods that are not fungible: diamonds, for instance, are not fungible because their varying colors, sizes, carats and cuts make difficult to establish any standard value, so it is not reasonable to exchange equal quantities of diamonds because their values can vary dramatically.

Now, by analogy, as we make bibliometric comparisons, are we comparing diamonds or soybeans? According to our analysis, if we compare, for instance, the raw number of publications of the producer A with the raw number of publications of the producer B, we are assuming that a publication is a fungible commodity. In the same line, academic rankings that evaluate academic productivity by comparing raw numbers of publications are making such economic interpretation of the data. Nevertheless, it is manifest that such comparisons do not sound adequate if we consider that scientific works are not fungible. Let us examine another analogy: Joseph Haydn had one hundred and six symphonies published, while Beethoven had nine; is there any sense in saying that Haydn was more productive than Beethoven? It is a good analogy because sheet music is *published* material likewise scientific papers. In case we interpret "published symphonies" as fungible commodities, the answer is yes; but if we consider that musical works are not fungible commodities, that question is nonsensical.

In order to avoid the question above, scientometricians have proposed citation data provided by *ISI* as the best way of estimating the value of publications, under the controversial assumption that the number of citations is a reliable, objective indicator of scientific works' quality (cf. Leydesdorff, 1998; Wesel *et al.*, 2013; Wouters, 1998). One might argue that the presumed objectivity is indeed a value judgment held by managers of scientific institutions and Mertonian sociologists of science without a reliable "theory of citation" that could explain and predict citing behavior. This assumption, in addition, cannot itself be supported on the basis of exclusively quantitative evidence, for it could only be tested in the light of quantitative data if there were an independent criterion of quality. Nevertheless, the development of citations indexes demands this assumption because:

> The basic function of the Science Citation Index (and similar devices) is to turn an enormous amount of lists of references upside down. Instead of organizing these references according to the articles they belong to, they are organized according to the articles they point at (...) This rather innocently looking inversion has important consequences. By creating a different typographical format of the lists of references - by organizing the references not according to the texts they belong to, but according to the texts they point at, they become attributes of the cited instead of the original, citing, texts (Wouters, 1998, p. 232-3).

By considering citation as an attribute of the cited texts, instead of the original citing ones, citation indexes reify the notion that scientific publications have some sort of value that could be quantitatively measured. Alternatively, one might say that ISI has created a type official accounting of scientific capital – as to use Bourdieu´s (1976) terminology. Not finding a better expression for it rather than an analogy, I say a paper seems to be a "deposit" made by a producer, and citations appear as "interests" paid by other producers, so that the total symbolic capital of a given producer is the sum of the number of published papers plus the number of citations received.

> Not only can the citation frequency of a certain article be measured, this frequency can also be summed at higher levels of aggregation to obtain the citation

> frequencies of institutions, journals, countries and even of disciplines and scholarly fields as a whole. Much of descriptive scientometrics is based on this. Consequently, the citation seems to have a universal quality. Since the citation frequency of every article can be measured — if a publication is not cited it still has a citation frequency: with a value of zero — every article can be compared with any other (...) The role of the citation might also be compared with that of money, especially if we take the evaluative use of scientometrics into account. When the value of an article is expressed in its citation frequency, the citation is the unit of currency of science by which every article can be compared with any other, no matter what the subject or field is (Wouters, 1998, p. 235-6).

Citation analysis avoids the question about the non-fungibility of scientific works by taking the number of citations as a measure for comparing their values – which means that papers are considered partially fungible. Nevertheless, citation analysis interprets citations themselves as fungible because "all citations are equal" (Smith, 1991 *apud* Wouters, 1998, p. 235), a very controversial assumption that has motivated the quest for a citation theory (cf. Leydersdorff, 1998). Here I wish to stress as "the role of the citation might also be compared with that of money (...) the unit of currency of science" (Wouters, 1998, p.235). Incidentally, in a critical discussion on the role played by ISI in the last decades, Adam (2002) have called it "the undisputed king of the counting house of contemporary science" (p. 728).

4 The counting house

By taking advantage of the remarkable development of information technologies in 1960s and 1970s – which provided means for application bibliometric tools in a scale hardly imaginable few decades before – Garfield found out the Ariadne's thread of the living labyrinth of scientific literature: citation analysis. Additionally, Garfield succeeded in establishing one of the most successful companies of the informational economy (Beira, 2010). Garfield begun his career as a chemist, but very early became interested in library science. Nevertheless, his most impressive personal trait was to be an entrepreneur with a strong business acumen (cf. Cawkell & Garfield, 2001, p. 154-5). He founded his first company in 1955, named Eugene Garfield Associates Inc. However, "the name Eugene Garfield Associates Inc., was changed to The Institute for Scientific Information in 1960. The reason for the adoption of this name was to create a more equal ground in competing with non-profit organizations" (Cawkell & Garfield, 2001, p. 153). This business strategy of changing the name of the company intending to sound "non-profit" has been very successful and even today most people seldom realize that ISI is not a non-profit organization.

The main ISI's product – the Science Citation Index (SCI) – was launched in 1963, and it is important to emphasize that SCI was intended to be much more than a mere academic database; indeed, it was the development of Garfield's ideas into a product economically viable. According to Cawkell and Garfield (2001, p. 156): "the SCI database represents the company's most valuable asset because of the many ways its data can be processed to form a new product". In 1992, the Thomson Reuters acquired the ISI, by approximate 210 million USD (cf. Beira 2010), and thenceforth controls the largest database on publications in almost all areas of knowledge. It is important to notice, by the way, that citation analysis depends on massive databases covering large number of publications. Nowadays the global market of Science, Technical and Medical (STM) publishing is composed by "companies such as Elsevier, Wiley, Springer, and Taylor & Francis (...) These companies hold more than 50% of the global market of STM publishing, which is estimated as between 7 and 11 billion dollars" (Hengl, Gould & Gerritsma, 2012, p. 43). The incorporation of ISI by Thomson Reuters means that the same company both publish scientific works and account their eventual recognition. In fact, the expansion of bibliometric studies have been connected with STM global publishers' interests, and it is not surprising that those companies now control the most important databases required for citation analysis.[4]

Conclusion

The rising of scientometrics happened during the post- and cold war, so we cannot disregard all the constellation of extra-scientific interests of that milieu. In fact, governments and international organizations like OECD supported the first official scientometric studies on R&D, following the guidelines proposed by econometricians like Cristopher Freeman. Not long after, the extraordinary development of information technologies provided the ground for private enterprises like the Institute of Scientific Information (ISI), headed by Eugene Garfield. Despite of this industry-like side of scientometrics, it is also true that distinguished scientists, such as Price and Merton, used to furnish some type of theoretical justification for scientometrics by emphasizing the supposed advantages of quantitative methodologies. In the end, geopolitical, economic and scientific concerns have converged into an amalgam of interests mutu-

4 For instance, the Web of Science is maintained by ISI-Thomson Reuters, Elsevier maintains the Scopus, and the Crossref is a non-profit organization that receives support and approval from Wiley and Springer.

ally reinforcing, and have laid the ground for a *scientifically-justified* – in the mind of its adherents – global industry of measurements.

To conclude, I wish to rejoin two related small questions. First, is it useful to collect statistical data and to analyze them using a mathematical formalism? In my opinion, yes, it is, because statistical data could inform science policies. I am not saying here that scientometric studies are actually informing *better* science policies. Most of scientometrics reports are productivist, that is, positive evaluations are assigned to scientific fields according to the increased quantity produced, resulting in some type of "taylorization" of research activities (cf. Oliveira, 2008). However, there is no impediment to make valuable diagnosis about scientific practices based on the scarcity of productions. For instance, the scarcity of papers and books on certain subject – e.g. agro-ecology and other alternative agricultural practices, such as Lacey's (2008, 2010) investigations on the controversy of transgenic seeds – could be interpreted as indicator for more investment in the field, in particular due to the social values embodied by such scarce researches. Second, should scientometric research be so much dependent on the database owned by a single company? The answer is no, because although ISI's business strategy may be legitimate, the existence of alternative databases (private and public) eventually enlarges the repository of available data. Thus the remedy against business-oriented biases in scientometrics is a more democratic scientometrics, otherwise scientific information will continue being controlled by half-dozen global corporations.

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