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Editors' note: bibliometrics and the curators of orthodoxy

MSCS Editorial Board

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Have you ever seen the Citation Indexes (CIs) for the year 1600? At that time, a very active community was working on the reconstruction of planetary movements by means of epicycles. In principle, any ellipse around the Sun may be approximated by sufficiently many epicycles around the Earth. This is a non-trivial geometrical task, especially given the lack of analytical tools (sums of series). And the books and papers of many talented geometers quoted one another. Scientific knowledge, however, was already taking other directions. Science has a certain 'inertia', it is prudent (at times, it has been exceedingly so, mostly for political or metaphysical reasons), but even under the best of conditions, we all know how difficult it is to accept new ideas, to let them blossom in time, away from short-term pressures.

At best, CIs transform this slowness into a tool for judgement. If used unwisely, as is increasingly the case, they discourage people (young ones in particular) right from the outset from daring to think, from exploring new paths: how is it possible to find a job today in the field of science or to get tenure without the inertial consensus of the majority, of the largest research areas, imposed by CIs? So the avalanche effect inhibits or even eliminates variety, which is at the core of culture and science. And the preventive effect against novelty is what we particularly fear.

At Ecole Normale Supérieure, in Paris, the departments of Mathematics, of Physics, and of Computer Science have expressed their firm opposition to the increasing use of CIs as a tool for scientific evaluation, or for characterising scientific laboratories. Note that eight out of the nine Fields Medals obtained in France have been given to former students and/or teachers from this Mathematics department (Grothendieck is the exception: outliers are always to be expected). The Physics department counts two Nobel Awards and has an extraordinary scientific history. In areas that are familiar to the readers of this journal, as well as in many other fields, the relatively young Computer Science department, which originated from the Mathematics department, has an impressive record. We join our colleagues in this institution, as we all believe that the use of CIs, as a spreading international phenomenon, is one step further away from a balanced mix between a 'culture of knowledge' and a 'culture of results' towards a pure culture of results: in the field of science, this is an assured path to having no more results.

Concerning editorial and publishing activities, in addition to the distortions in judgements induced by so-called 'impact factors' for journals (see the ranking quoted below, which is fluctuating because ill-founded), further distortion is caused by having a very small number of self-selected commercial organisations assume the crucial task of deciding just 'what' to index. From the perspective of this well-established journal, we observe that these organisations make it difficult for new journals to get indexed at all. In particular, authors who are consciously trying to break the stranglehold that a few expensive non-academic commercial publishers have on scientific publishing are even more severely disadvantaged by these unreliable and arbitrary numerical evaluations.

Further arguments are exposed in the text below, approved by the ENS Computer Science Department, and in the references therein. In particular, we explain how the discrete charm and the presumed objectivity of the 'numbers' provided by the CIs may divert scientific evaluations. We particularly recommend the document by the International Mathematical Union (Adler *et al.* 2008), where both methodological and technical critiques (concerning the flawed use of statistics) are given.

Let us just add one more comment. For a long time now, citations have been made of, say, Riemann Manifolds, Relativity Theory or Connes' Non-Commutative Geometry without references to writings by the authors. Even worse, the well-known notion, say, of Martin-Löf algorithmic randomness has been quoted and re-defined simply as 'ML-randomness', not only without a citation of the founding paper, but also omitting the originator's name, as being evident for the specialist. Scientific evaluation and promotion is an important and difficult task, as much as refereeing is for a top journal. CIs, increasingly used by managers and administrators, miss out on both novelty and established advances: these are not the tails of a Gaussian of science. They are at the core of scientific construction, and they are what makes science worthwhile and rich with always new, unexpected, heterodox knowledge and technical fall-out.

Excerpts from the DI-ENS document (LIENS 2008)

The use [of CIs] is spreading, to the detriment of motivated and close scientific evaluations. At the same time, ill uses as well as the manipulation of these numbers are increasing, entailing a counter-productive expenditure of energy. We believe that the abuse of such indicators runs counter to the development of knowledge.

— Firstly, the depth and the originality of a scientific publication do not correlate with the expediency with which it is quoted, given that certain trends momentarily emerge and then fall into oblivion (a citation is taken as evidence of "impact" for a journal only when made within two years following the publication of the cited article).

— Each index ranking and each purveyor of bibliographic information presents its own aberrations, providing very approximate measurements: coverage varies widely according to the discipline and within a discipline. Very few conference proceedings are covered (in computer science, the absence of the major conferences is absurd), as well as very few books. This gives the fluctuating classification of journals according to the index ranking being used: "The first journal according to ISI (...) is the 195th according to CiteSeer; the 2nd according to ISI does not appear in CiteSeer; the 6th for ISI is 958th for CiteSeer... Conversely, the 1st for CiteSeer (...) is 26th for ISI; the 4th for CiteSeer (...) is 122nd for ISI" (Kermarrec *et al.* 2007).

— The formal correctness and the semantics of the software used is rather dubious; in particular, could an index ranking calculated today compare with the same index ranking calculated in two or ten years? The Harzing "Publish or Perish" and Google Scholar software is not free (FLOSS) and can evolve at any moment; the updating of the databases is beyond any form of control.

— *"A systematic study of the CIs of four internationally renowned INRIA researchers shows that the bias and shortcomings observed in the indicators are not exceptions but, rather, the rule – at least in terms of computer science in its broadest sense."* (Kermarrec *et al.* 2007).

Despite these known shortcomings, the importance of these indicators in evaluations, be they individual, by team or by laboratory, is growing, and often replaces or reduces the role of true evaluation (which we consider a relevant component of scientific work). We are led to believe that these numbers will never be more than an element among others, but the discrete charm, even the objectivity of the number, is incomparable. The temptation is great to calculate these numbers 'just to see' and then, because it is easy to do so, to use them to discriminate cases which at first glance may appear to have a comparable standing. In fact, numerous examples demonstrate that these excesses are already occurring, sometimes systematically so – see page 10 of Adler *et al.* (2008). They reduce the responsibility of every scientist to take a stand at his or her own risk, explaining in a jury that such and such is profound and original. Such notions are not conveyed by numerical indicators.

The growing importance of these indicators is therefore contrary to the advancement of knowledge, because it constitutes a hindrance to risk-taking, to originality, to interdisciplinarity and innovation, aspects that are constitutive of scientific progress and research. *"In addition to the fact that it is possible to significantly 'defraud' the values used for indicators in this way, the ever-increasing use of these indicators in the assessment of researchers has damaging consequences for science and innovation. Given the bias from which their calculation suffers, an exaggerated consideration of indicators may push young researchers into obtaining quick results, to the detriment of more long-term research and thereby slowing down innovation and penalising the formation of small communities in emerging fields."* (INRIA 2008).

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This document has been signed by all members of the Editorial Board.

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Science, Problem Solving and Bibliometrics[1]

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Problems and Theories

The head of a prestigious scientific institution recently said, by paraphrasing a famous quotation: "we solve problems that are posed, not that we pose". This view totally misses the history and role of human knowledge construction and prepares wrong ways for evaluating it.

Science is not problem solving, it is theory building. Any relevant, difficult problem requires the construction of a new theoretical frame to deal with the problem in an original and effective way. Moreover, problems follow from the proposal of a theory.

Animals continually solve problems that are posed to them by events. We, the humans, by language, in our communicating community, we looked at the Moon, at the Stars, which pose no problem, and invented Myths and Theories, and derived from them countless problems. We also looked at inert matter, a stone, some sand on a Greek beach, and proposed the atomistic theory. Science originated by these attempts to organize the world by concepts and theories. Later, it was radically renewed by looking again at planets, but from a different perspective: from the point of view of the Sun, on the grounds of a different metaphysics, which lead to a theoretical revolution. It was also renewed by looking at two falling stones in an original way and at physical trajectories as inertial, at the infinite limit of a non-existing frictionless movement.

As a matter of fact, science is not the progressive occupation of reality by known tools, it is instead the definition of the very objects of knowledge, the construction of new perspectives and of new conceptual frames. Problems follow from these active constructions of knowledge, interact with it. Relevant problems, posed within a given theory, require a new insight, a change of perspective, often a new theory. And in the history of science, theories can be hardly distinguished from philosophical thinking. This may be implicit, but further novelties and critical reflections are enhanced by explicit philosophical frames, sometimes also in interaction with the arts and their proper knowledge content and expression (Weyl, 1952; Angelini, Lupacchini, 2013; Longo, 2011). This interplay is at the core of the history of mathematics, physics, biology; it reached a very high intensity in some of the most productive moments of our cultural and scientific invention, the VI - IV centuries BC in Greece, the Renaissance and during the decades of formation of XXth-century mathematics, physics and biology, bridging the last two centuries.

In contrast to this, one prominent physicist stated once that “the philosophy of science is about as useful to scientists as ornithology is to birds”. And birds are very good at solving their problems. Yet, can one set apart the scientific work by Darwin, Riemann, Poincaré, Bohr, Einstein, Schrödinger … from their philosophy of knowledge and of science? As a matter of fact, in the mind of most managers of science, this critique of philosophy covers also the theoretical aspects of science, as they always border each other. So, government's policies in financing science must be justified by their role in solving the country's problems and by their

1 *Invited Lecture*, Academia Europaea Conference on “**Use and Abuse of Bibliometrics**”, Stockholm, May 2013. Proceedings, Wim Blockmans et al. (eds), Portland Press, 2014.

accountable economic fall-out - stated the French *Cour des Comptes* (the constitutional Accounting Agency) a few years ago. In either case, science, with no philosophy, is viewed as applied problem solving, with immediate or short term economic results. This misses the actual role and history of culture and science, which radically modified the human condition. Science and culture crucially contributed, often by “enabling” in a highly unpredictable way and in changing economic and social contexts, the dynamics of our societies.

Going back to birds, ornithology is the science of bird life and evolution; it is then analog to knowledge and reflections on human condition and history. Then, the difference between birds and humans is exactly that birds do not have ornithology, while we have "humanology", that is humanities and a theory of human evolution (or natural sciences, more generally)

Bibliometrics and Democracy

Managers continually solve problems that are posed to them, of any kind. They have a general training that teaches them how to solve problems in any context, by referring to a unique, universal theory: the "common sense" theory.

Today, managers stepped into science by solving a fundamental problem: how to evaluate science? how to finance it ? So they used the common sense theory: by asking the vote of the majority of scientists, in each discipline. This vote is expressed by the number of quotations and by the impact factors of journals, based on the (average) number of quotations in the two years following publication. Isn't this a undebatable and effective use of democracy? Since this poll, in comparative evaluations, is directly and indirectly expressed by counting quotations, it is, allegedly, a rigorous, expression of a majority consensus on scientific content. It is objective.

Now, democracy is grounded on two fundamental principles: the government by a majority and the possibility for a minority to propose alternative policies, to explore new or different ways of being together.

The formation of scientific thinking is a delicate process. Science is the interplay between these two fundamental aspects of democracy. When some major theory becomes common sense, then the novelty will pop out against this common sense framework, by a disagreement with the main frame theory. This has been so since the formation of Greek science, then with the modern scientific revolution and further on with the XXth-century radical changes of perspective, in Physics, Mathematics, Biology. The formation of scientific knowledge is always against "common sense", (Bachelard, 1940).

Also in everyday work and in relation to existing theories, a scientific thinker always starts by an "unsatisfaction". In mathematics, say, facing a problem, the relevant solution comes from saying first: the mathematical structures that are currently used for this or that are not the good ones, this is not the right theoretical approach, these are not the right tools ... . Then, the mathematician looks at matters from a, maybe slightly, but different perspective, in a new frame. Unsatisfaction helps in "making a step aside", reflecting critically on the current approaches, inventing new mathematical structures, maybe by minor variants of existing ones.

Critical thinking is at the core of scientific theoretizing: one has to step aside and look at the very principles of knowledge construction, as grounding the dominating way of thinking. And change fuels the history of science. We have to be constantly mobile, plastic, adaptive, able to get away from the dominating frame. But also an engineer, who has had a good theoretical and critical training, may face a technical problem posed to him/her, by proposing a new point a view, by approaching it in a new way, away from the intended applied frame or theory and, by this, he/she may invent an unexpected solution. On the theoretical side, a way for enhancing a critique of leading knowledge principles and exploring new scientific perspectives, may be obtained by crossing boundaries, comparing foundations and by an explicit philosophical commitment in natural sciences (Bailly, Longo, 2011; Longo, Montévil, 2013).

Critical thinking is the fundamental component of minority thinking: it implies disagreement with respect to the mainframe theory, the common sense theory. This forces science to relate to democracy by relying first on the minority side, by the proposal of new ways of understanding, of acting, of moving ahead. And this is so also in the ordinary research activity, possibly by minor changes of perspective; otherwise it is not scientific research. Sometimes, rarely, changes are revolutionary; always, they enrich knowledge and prepare revolutions.

Of course, one may work in the "majority theory", but the novelty, the new idea, even within that theory, will always require a change of insight that will bring the proponent on a critical side, possibly a new minority side, more or less away from the main frame. History of science teaches us that the opinion of the majority has always been on the wrong side, at each moment of the formation of new scientific thinking. One does not need to refer only to the most quoted turning points, such as the modern scientific revolution, as it was so also for the early approaches to biological evolution (Buffon, Lamarck), or for differential geometry and the various branches of physics invented in the XIXth century (thermodynamics, electromagnetism, statistical physics). Gauss was "scared" to present his ideas on non-euclidean geometry and did not make them public for decades. Riemann and Helmholtz were litterally insulted by award winner E. Dühring, elected by influential majorities in 1872, about 20 years after Riemann's fundamental writings on differential geometry. Poincaré's geometry of non-linear systems was largely ignored for about 60 years, till the 1950s, when theories of deterministic chaos were brought to the limelight by Kolmogorof and Lorentz. Some work I recently studied, Turing's seminal paper on morphogenesis (1952), had little or no followers for about 20 years! An early revitalization can be found in (Fox-Keller, Segel, 1970).

These are not exceptions: this is how scientific thought is formed. The exception is when an innovative theory is quickly accepted: Einstein's Relativity is probably the unique case of a rapid success and diffusion of a novel approach. I am not expressing by this the romantic myth of the isolated, revolutionary scientist. These revolutions or novelties are always made possible by and within strong scientific schools. The modern scientific revolution matured in the intellectually very lively context of Italian Renaissance. It crossed the invention of the perspective in painting, a new organization of human space, including, later, the spaces of astronomy (van Fraassen, 1970; Angelini, Lupacchini, 2013; Longo, 2011). Naturalism originated then by a new way of looking at phenomena and at our humanity, by inventing a new metaphysics, from Leonardo's drawings to Nicolas Cusanus's proposal of an "infinite universe" (Zellini, 2005). These processes always required a change of viewpoint, with respect to the official theory, also within an excellent school, yet against that very school.

Galileo, in his youth, worked on the "physics of Hell", (Galilei, 1588), a possible path towards the "naturalization" of a religious ontology and, by this, of knowledge. As a matter of fact, a common fashion, in the XVIth century, was, for excellent physicists and mathematicians, the heirs of Pacioli, Cardano and Bombelli, to solve the many problems posed by the material structure of Hell. Galileo turned one of these problems into a seminal theory, that is into science[2].

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2 Hell is a cone of a 60° base angle, whose vertex is at the center of the Earth. This poses a major challenge, dear to the Church's and Universities' managers of the time, who wanted scientists to solve problems and claimed to be opened to the new sciences: how thick must be the Earth's arch covering the Hell as a dome? In order to obtain an estimate of this value, Galileo referred to the structural properties of Brunelleschi's dome of Santa Maria del Fiore. But he did not use its ratio of sizes, instead he made an original computation with an intuition of scaling effects: while he obtains, as for the thickness of the Hell's roof, one height of the Earth radius, he observes that a small dome of 30 "braccia" (arm length) may need only one or even one-half braccia, (Galilei, 1588). Galileo was also puzzled by the scaling of the Devil, a further challenge – she is 1,200 meters tall, with the same proportions of a human – impossible (see (Lévy-Leblond, 2006 and 2008) for a historical discussion

This juvenile work gave Galileo a sufficient bibliometric index to get tenure in Pisa in 1589, when he stopped working on the Hell and, some time later, got in touch with Kepler. Tenure is fundamental to allow free thinking, even though, in some historical contexts, it may be insufficient to protect this freedom, when the novel theoretical proposal is too audacious, too much against the main stream - and minority thinking, thus scientific thinking, is not allowed to go beyond certain metaphysical or political limits.

In this case and in all the others I mentioned above, the new theoretical frame does emerge within a strong scientific school and a relatively free debate - it is allowed to emerge as long as the novelty does not contradict a dominating metaphysics. Yet, even within a school, the further change is due to a few who dare to go further, or, more precisely, to think differently. It is the school that produces the possibility of thinking deeply and differently; it is not a matter of isolated individualities.

We have to promote schools, but their strength will reside also or mostly in the amount of freedom they allow to side-track approaches. No one could think freely in Soviet Union, except in Mathematics and in Theoretical Physics (but not in Biology) within the Academy of Sciences. And remarkable and original work, in Mathematics and Physics, was produced in that singular context. Some, local, space of dissent may suffice for science, if circumstances allow (for example, the social privileges accorded to scientists in the SU). But dissent is needed for science.

Bibliometrics is the apparently "democratic" analog of the Church's dominating metaphysics in the XVIIth century or the Party's truth in the SU. These rulers were not elected, but other majority rulers were elected, such as Hitler or Salazar. It suffices then to kill the opposing ideas and democracy looses its meaning - and science disappears, like in Germany after 1933. The majority vote, per se, is not democracy. Democracy requires also and crucially the enablement or even the promotion of a thinking and active minority. Bibliometrics forbids minority thinking, where new scientific ideas always occur by definition, as history teaches us. If a scientist has to write on top of his/her CV his/her bibliometric indices, that is the evaluation by the majority of scientists of his/her work, and present it in all occasions, this will prevent the search for a different approach, to dare to explore a new path that may require 60, 20 or 10 years to be quoted, as in the examples I gave above. And he/she is constantly pushed to develop as much as possible technical tools in a familiar and well established theoretical frame, as they may allow others to write more papers, where the technique may be quoted.

We all need to be evaluated in science and severely so. But a new idea, an apparently absurd exploration may be accepted by a majority of two or three in a committee of three or five colleagues giving tenure. The success may require several applications, but the candidate with too original ideas may finally encounter a small group of open minded colleagues, who do not look a priori at the bibliometric index, but dare to understand and evaluate contents. This also applies to publishing in good journals. If the editor does not care of the expected impact factor of the journal (a "next two years" quotation criterion!), but is able to find open minded referees, an apparently strange, non-sense or non-common sense idea may find its way to publication. So, after six or more attempts, even the 1971 seminal paper by Ruelle and Takens on chaotic dynamics could find a publisher, and, after several years of failures, in the 1990s, Gallese's, Rizzolati's and collaborators' unexpected results on “Mirror Neurons” were

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and a possible solution for the now widely accepted “Devil's violation of scaling equations”). This problem opened the way to Galileo's seminal work on scaling and its fundamental equations, 50 years later, which extends also to biology: the section of bones gives their strength, it must thus grow like the cube of their length, not as the square, since the animal's weight grows like the cube (Galilei, 1638; Longo, Montévil, 2013, chapter 2). The paths of knowledge construction are unpredictable and may even pass through the Hell, (Lévy-Leblond, 2008), if a scientist is allowed to think theoretically and with sufficient freedom, that is to deal with a problem by theory building, in full scientific generality.

at last published (see references)[3]. Both papers were too original to be immediately accepted, yet a couple of audacious editors finally dared to publish them.

If, instead, each evaluation refers to a “global” majority vote, that is to the opinion expressed by the largest number of quotations or expected quotations (the short term impact factor) by all scientists in the discipline on Earth, science is at the end. Or we will have a new form of techno-science, the one managers can easily judge and finance: short term problem solving and techniques within clearly established frames, the problems that the majority in a discipline can easily understand, that even managers can grasp. But no radically new theory will ever pose its own, internal problems that *cannot even be seen* from the dominating perspective.

Networks, diversity and “the norm”

Computer networks give us a tool comparable to writing, another of our extraordinary inventions. They were both motivated by metaphysics and philosophy. In Mesopotamia, five thousands years ago, humans made visible the invisible, language and thought, in a dialogue with the Gods, (Herrenschmidt, 2007). The human interaction was suddenly enriched by this new tool and by the magic of the permanent sign, thus the explicitly symbolic transmission of myths, history and knowledge. A new form of exchange modified our communicating community.

In the last century, Hilbert's philosophical questions, originating from his theory on the Foundations of Mathematics, were answered by Gödel, Church, Kleene and Turing by proposing Computability Theory and abstract Logical Computing Machines (Turing). Later, our interacting humanity connected concrete computing machines in networks and started a search for suitable theories of this new level of communication. Networks, today's computer networks in particular, allow mankind to access knowledge and memory of mankind, an extraordinary enhancement of our interactive thinking. We can access to diversity, collaborate at distance, appreciate differences, enrich cultures by endless hybridisations.

Yet, these networks may also be used also for "normalizing" humanity. They may be used for averaging everybody. Just force a unique criterion for "excellence"; replace the network structure by a totally ordered line of values, a uniform scale of points, the same for everybody. Then the networks' richness in confronting diversity may be used to forbid the variance from the imposed norm. Transform the network of exchange of University or of researchers into a total order, on the grounds of a few (often perfectly stupid or managerial) criteria, and diversity is lost.

Hybridisation and contaminations are at the origin of most novelties in evolution, both biological and human or cultural evolution. But no hybridisation nor contamination is possible in absence of diversity, including the "hopeful monsters", the wrong paths continually explored by phylogenesis, (Goldschmidt, 1940; Gould, 1989). We have to accommodate errors, wrong paths, if we want diversity and, by it and within it, the novelty of science.

Self-appointed agencies of managers propose criteria and technical tools for averaging the world of knowledge, to normalize thinking according to common sense values. We should oppose to this unique scale of values some sort of "index of diversity". They are already used by biologists to assess the dynamics of an ecosystem: when diversity decreases, the situation in general worsens, major extinctions happen or are expected. Diversity guarantees the ever changing dynamics that is essential to life and to human cultures. By normalizing evaluations, forcing identity of aims, of metrics and, thus, of cultural contents, we are killing the permanent "variations on themes" as well as the radical changes in perspective that constitute the ever changing path of scientific knowledge.

3 David Ruelle mentioned this story in several lectures; Gallese's personal communication.

Networks allow collaborations, today as never before. Yet, they may be used to force mainly competition on the grounds of fixed values and observables, by accounting criteria with no content. Competition is much easier, in science, than collaboration. It may even be based on cheating, on announcing false results, declaring non existing experimental protocols, on stealing results, organizing networks of reciprocal, yet fake quotations. Collaboration instead is very hard: good scientists are very selective in accepting collaborators and diversity makes the dialogue difficult, while producing the most relevant novelties. A research activity mainly based on competing for projects and prizes, on competitive evaluations, destroys the chances for open collaborations, closes the mind to the others. Occasionally, we may need to compete for a job, a grant. The point is to avoid turning this inevitable fact of life into the main attitude in scientific work, that is to make competition and normalizing evaluations the driving force and the guidelines of our scientific activity, which instead should be based on collaborating diversities.

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