Science, problem-solving and bibliometrics

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Problems and theories

The head of a prestigious scientific institution recently said, 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, humans, using language of our communicating society, looked at the Moon and 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 and some sand on a Greek beach, and proposed the atomistic theory. Science emerged from 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 led 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 new conceptual frames. Problems that 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 and often a new theory. And in the history of science, theories can hardly be 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 [1–3]. This interplay is at the core of the history of mathematics, physics and biology; it reached a very high intensity in some of the most productive moments of our cultural and scientific invention, the 6th–4th Centuries BCE in Greece, the Italian Renaissance and during the decades of formation of 20th Century mathematics, physics and biology, bridging the last two centuries.

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In contrast with this, one prominent physicist once stated 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 philosophy of knowledge and of science from the theoretical ideas of Darwin, Riemann, Poincaré, Bohr, Einstein, Schrödinger etc.? As a matter of fact, in the minds of most managers of science, this critique of philosophy also covers the theoretical aspects of science, as they always border on each other. So governments' policies in financing science must be justified by their role in solving the country's problems and by their accountable economic fall-out, as 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 analogous to knowledge and reflections on the human condition and history. Subsequently, the difference between birds and humans is exactly that birds do not have ornithology, whereas 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 whatever 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 have stepped into science by solving a fundamental problem: how to evaluate science? How to finance it? So they have 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 citations and by the impact factors of journals, based on the (average) number of citations in the 2 years following publication. Isn't this an unquestionable 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. It is objective.

At present, 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 novelty will pop out against this common sense framework, by a disagreement with the mainframe theory. This has been so since the formation of Greek science, then with the modern scientific revolution and further on with the 20th Century radical changes of perspective, in physics, mathematics and biology. The formation of scientific knowledge is always against 'common sense' [4].

Also, in everyday work and in relation to existing theories, a scientific thinker always starts with a 'dissatisfaction'. 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 good ones, this is not the right theoretical approach, these are not the right tools. Then, the mathematician looks at matters from an ever so slightly different perspective, in a new frame. Dissatisfaction helps in 'taking a step to the side', reflecting critically on the current approaches, inventing new mathematical structures, maybe minor variants of existing ones.

Critical thinking is at the core of scientific theorizing: 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 and able to get away from the dominating frame. But also an engineer who has had good theoretical and critical training may face a technical problem posed to him/her, by proposing a new point of 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 involve the crossing of boundaries, comparing foundations and an explicit philosophical commitment in natural sciences [5,6].

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, acting and moving forward. This is so also in ordinary research activity, possibly through 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 place the proponent on a critical side, possibly a new minority side, more or less away from the mainframe. 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 also for the early approaches to biological evolution (Buffon and Lamarck), or for differential geometry and the various branches of physics invented in the 19th Century (thermodynamics, electromagnetism and statistical physics). Gauss was 'afraid' to present his ideas on non-Euclidean geometry and did not make them public for decades. Riemann and Helmholtz were literally insulted by the 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, until the 1950s, when theories of deterministic chaos were brought to the limelight by Kolmogoroff and Lorentz. Some work I recently studied, Turing's seminal paper on morphogenesis [7], had little or no followers for about 20 years! An early revitalization can be found in the paper by Fox-Keller and Segel [8].

These are not exceptions: this is how scientific thought is formed. The exception is when an innovative theory is quickly accepted: Einstein's relativity theory is probably the unique case of a rapid success and diffusion of a novel approach. I am not expressing this as 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 the Italian renaissance. It crossed the invention of perspective in painting, a new organization of human space, including, later, the spaces of astronomy [2,3,9]. Naturalism originated then in 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' [10]. 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' [11], 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 16th 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. Note that Hell is a cone of a 60° base angle, whose vertex is at the centre 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 the Earth's arch be to cover 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 using his intuition on the scaling effects. While he obtains, as for the thickness of the Hell's roof, one height of the Earth's radius, he observes that a small dome of 30 'braccia' (arm length) may be only one or even one-half braccio [11]. Galileo was also puzzled by the scaling of the Devil, a further challenge, as she is 1200 metres tall, with the same proportions of a human, and thus impossible (for a historical discussion and a possible solution to the now widely accepted 'Devil's violation of scaling equations', see [12,13]). 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 [14] (also see Chapter 2 in [6]). The paths of knowledge construction are unpredictable and may even pass through Hell [13] 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.

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

In this case and in all of the others I have mentioned above, the new theoretical frame emerges 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, further change is ascribable 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 in the amount of freedom they grant to side-track approaches. No one could think freely in the Soviet Union, except in mathematics and in theoretical physics (but not in biology) within the Academy of Sciences. Yet, 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 Soviet Union). But dissent is necessary for science.

Bibliometrics is the apparently 'democratic' analogue of the Church's dominating metaphysics in the 17th Century or the Party's truth in the Soviet Union. 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 loses its meaning, and science disappears, as in Germany after 1933. The majority vote *per se* is not democracy. Democracy also crucially requires 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 his/her bibliometric indices on top of his/her CV, that is, the evaluation by the majority of scientists of his/her work, and present it on all occasions, this will prevent the search for a different approach, the courage 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 technical tools in a familiar and well-established theoretical frame as much as possible, as they may allow others to write more papers, where the technique may be quoted.

We all need to be evaluated in science, and ruthlessly. 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. Success may require several applications, but the candidate with overly 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 2 years' quotation criterion!), but is able to find open-minded referees, an apparently strange nonsense or non-common-sense idea may find its way to publication. So, even after six or more attempts, the 1971 seminal paper by Ruelle and Takens [15] on chaotic dynamics could find a publisher, and after several years of failures, in the 1990s, unexpected results on 'mirror neurons' by Gallese, Rizzolati and collaborators[3] were at last published [16,17]. 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 doomed. Or we will have a new form of techno-science, the kind that managers can easily judge

[3]David Ruelle mentioned this story in several lectures. Regarding Vittorio Gallese, this was personal communication.

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 with writing, another of our extraordinary inventions. They were both motivated by metaphysics and philosophy. In Mesopotamia, 5000 years ago, humans made visible the invisible, language and thought, in a dialogue with the Gods [18]. 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 in their proposal of 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, present day computer networks in particular, allow mankind to access knowledge and memory of mankind, an extraordinary enhancement of our interactive thinking. We can access diversity, collaborate at a distance, appreciate differences, enrich cultures by endless hybridizations.

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 any variance from the imposed norm. Transform the network of exchange of universities or of researchers into a total order, on the grounds of a few (often perfectly stupid or managerial) criteria, and diversity is lost.

Hybridization and contamination are at the origin of most novelties in evolution, both biological and human or cultural evolution. But no hybridization or contamination is possible in the absence of diversity, including the 'hopeful monsters', the wrong paths continually explored by phylogenesis [19,20]. 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. Many of us signed strong documents against forms of evaluation of scientific work handled by these methods (for an example of one of them, see [21]). We should oppose the proposal of a 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 generally 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.

Networks allow collaborations, today as never before. Yet, they may be used to force competition mainly on the grounds of fixed values and observables, by accounting criteria with no content. Competition within science is much easier 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 or 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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