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What is the Value of Science?

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Abstract

In our time, scientific research is positively valued as long as, and to the extent that, it has fruitful implications for the development of technology. This is what we may call "the technological assessment of science", or "technologism", for short. I contend that this assessment, so widespread today, stems from a serious error of appreciation, both historically and epistemologically, in ignoring the genuine nature of science—a mistake that can lead, and indeed has been leading for a few decades, to the impoverishment of the scientific spirit and of culture in general.

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Wir müssen wissen, wir werden wissen
("We must know, we will know")
Epitaph on David Hilbert's tomb

Introduction

In our time, the view that the scientific spirit is an important component of human culture that deserves to be valued positively is widely held (at least in those regions of the world that are not yet subjugated by Islamic fanaticism, nor by evangelical fundamentalism). At the same time, however, this positive assessment of science is often subsidiary with respect to the equally positive assessment of technology; that is, scientific research is positively valued as long as, and to the extent that, it has fruitful implications for the development of technology. This is what we may call "the technological assessment of science", or "technologism", for short. I contend that this assessment, so widespread today, stems from a serious error of appreciation, both historically and epistemologically, in ignoring the genuine nature of science—a mistake that can lead, and indeed has been leading for a few decades, to the impoverishment of the scientific spirit and of culture in general.

1. Terminological and Conceptual Precision

Before moving on to developing the argument, it is appropriate to establish some terminological and conceptual precisions to clarify the picture. To begin with, by "science" I mean the totality of scientific disciplines represented in universities and other advanced research institutions. Within academic science today, we may identify the following groups of scientific disciplines: "formal sciences" (logic and mathematics), "natural sciences" (physical-chemical sciences, Earth sciences, life sciences, individual psychology), "social sciences" (social psychology, economics, sociology, ethnology, linguistics, philology, historical sciences), and "interdisciplinary sciences" (especially computer science, certain parts of philosophy, such as the philosophy of science and the philosophy of language, and the cognitive sciences). From a historical point of view, some of these sciences were already consolidated in Hellenistic times (from the 4th century BC on), especially with regard to mathematics, astronomy and some elementary portions of physics and physiology. However, the great boom in the scientific spirit did not occur until the 17th century, first in Western Europe, and later on it developed and expanded across almost the entire planet until the mid-20th century, when a period of lethargy began, to which I will return below.

The other term that requires clarification from the outset is "technology". Nowadays, in current English, the terms "technics" and "technology" are often equated, or else the second is used exclusively to the detriment of the first, but they should be clearly distinguished. "Technics" comes from the Greek "*tekhné*", the art (learned and transmitted from generation to generation) of knowing how to make things or of knowing how to manipulate them. For the Greeks, *tekhné* had nothing to do, neither positively nor negatively, with *epistéme*, which approximately corresponds to our term "science". In this sense, there has been technics since Homo Sapiens appeared on Earth; in fact, the much older Homo Habilis (which for some reason is called so) is likely to have used technics too. However, only since the Neolithic there was an explosion of technical innovations extremely important to Humanity: from the wheel to the printing press, through irrigation systems, the construction of large buildings, sailboats, hourglasses, hoes, gunpowder, and so many others. None of those novelties had anything to do with science. Not even the steam engine, the most revolutionary of the inventions of modernity, qualifies as an example of the benefits of science to technics, as is sometimes assumed: indeed, the branch of science that adequately accounts for the functioning of the steam engine is thermodynamics. However, James Watt invented the definitive model of that machine around 1775, that is, three-quarters of a century before the consolidation of thermodynamics as a scientific discipline (mainly thanks to the theoretical work of Hermann von Helmholtz, Lord Kelvin and Rudolf Clausius in the middle of the 19th century). In sum, the great technical developments that took place over several millennia before the first attempts at a genuine form of science in the Hellenistic era, and even a couple of millennia *after* that time, had nothing to do with the scientific spirit. It is true that the example of Archimedes, in the 3rd century BC, is sometimes mentioned as that of someone who was both a scientific genius (the greatest of antiquity, indeed) and an astonishing inventor of machines; but this actually is a unique example in antiquity, and it is also known that Archimedes himself belittled his technical achievements and wanted to be remembered exclusively for his contributions to *epistéme*—specifically to mathematics and physics (Störig 1957, p. 112).

2. The Scientific Revolution and the Advent of Technology

We therefore find that technics, in a genuine sense, has nothing to do, neither historically, nor conceptually, with the scientific spirit. On the other hand, the cultural form which certainly has a lot to do with science, is *technology*. It is therefore appropriate to distinguish clearly between technics and technology: technology is applied science; or, if one prefers, it is a very special form of technics that presupposes some scientific knowledge.

When and how did technology historically emerge? It is often assumed that this took place in Western Europe with the rebirth of the genuinely scientific spirit. This rebirth occurred after the deep lethargy of more than a thousand years caused by the combined blows of the Christian dogmatism that followed the collapse of the Greco-Roman civilization and the barbarism of the Germanic tribes, blows from which Europe only very gradually revived. This renaissance, which took place in the 17th century (and which is not to be confused with the artistic and literary Renaissance that had flourished more than a century earlier), is often referred to as "the Scientific Revolution". This latter revolution is supposed to have generated great technological advances, in the sense of technology that we have just defined. It is often mentioned that Francis Bacon's publication of his *Novum Organum* in 1620 and the famous motto attributed to him, "*scientia est potentia*", promoted the alliance of the new scientific and the technological spirits. Now, it is worth noticing that Bacon was not a scientist, let alone a technician. He was a politician and a literate, who, by the way, had a great aversion to the sciences of ancient Greece, which he considered useless for the promotion of human well-being. Being vehemently opposed to the spirit of ancient science, he wanted to impersonate the herald of a new era. On the one hand, Bacon certainly had the merit of popularizing the importance of the experimental method in science (although he himself did not conduct any noteworthy experiment); but, on the other hand, he did not understand at all the decisive role of mathematics in the empirical sciences, nor did he realize the revolutionary significance of the discoveries of his genuinely scientific contemporaries, such as Kepler and Galileo. More than the promoter of the new scientific spirit, Bacon was the remote forerunner of what I have called "technologism", as evidenced beyond doubt by his apodictic affirmation: "the true and legitimate goal of science is nothing more than to give human life new inventions and resources" (Störig 1957, p. 223—my translation).

If Bacon was therefore not the champion of the Scientific Revolution, and not even a valuable assistant, who were its protagonists? Well, they were essentially those men whom Arthur Koestler once called "the sleepwalkers" (Koestler 1959), because, without realizing it, they walked firmly down the right path to reach the right goal. The "sleepwalkers" of the seventeenth century, which Koestler explicitly deals with in his book, are: Johannes Kepler, Galileo Galilei, René Descartes and Isaac Newton. To them we could add other champions of the new scientific spirit in the 17th century, not as popular as those mentioned, but very decisive too, namely: William Harvey (for human physiology), Robert Boyle (for chemistry) and Christiaan Huygens (for optics and mechanics). Besides being a scientist, was any of them a technologist? Only one of them, Huygens, may be described *cum grano salis* as such, because he invented the pendulum clock; however, what he was most interested in was not the measurement of time, but the development of the wave theory of light, as well as the solution of certain mechanical problems (like the right formulation of the laws of collisions and the analysis of centrifugal forces), all of which did not induce him to invent any machine. Of all the other "sleepwalkers" of the Scientific Revolution of the 17th century, there is not one whose name may be associated with a technical invention. Not even Galileo, to whom some texts of scientific popularization still today attribute the invention of the telescope: Galileo did not invent the telescope; what he did was to use the telescope that someone else (it is not known for sure who, probably a Flemish craftsman) had invented a few years earlier. In addition, Galileo used this invention not to improve the human condition, as Bacon would have wanted, but to focus it on the Moon and the stars, and thus discover that the surface of the Moon is comparable to that of the Earth (with its mountains and valleys) and that there were a number of stars far superior to what had previously been assumed. That is, Galileo made an essential contribution to the increase of human knowledge, not to the improvement of human well-being.

So, if it was not in the century of the Scientific Revolution that science and technics mated, was it then in the next century, the 18th century? The answer is equally negative. We have already seen that the greatest invention of the 18th century, the steam engine, had nothing to do with any scientific theory, either contemporary or of earlier date. And of the great scientists of the 18th century, namely the Bernoulli, Euler, Lavoisier, Coulomb, Buffon, etc., none of them can be said to have made a significant contribution to the technics of their time. Only Benjamin Franklin (who, by the way, was not a great scientist) contributed to technology by inventing the lightning rod, but apart from the fact that it was a rather casual invention, Franklin's own electricity theory, the so-called "theory of the two fluids," soon turned out to be entirely mistaken.

The same goes, *mutatis mutandis*, for the first half of the 19th century. Let us ask: what does the railway owe to contemporary or earlier scientific theories? Nothing. And the steamship? Nothing. And the cure of smallpox? Nothing. And, for the great scientists of that time—the Cauchy, Laplace, Dalton, Fourier, Clausius, Helmholtz, Darwin, ...—what machine did they invent or what disease did they cure? None. Only the great mathematician Karl-Friedrich Gauss can be said to have made a timid technical contribution, based on his knowledge of electricity theory: a primitive form of a telegraph, which in practice, however, proved to be useless; actually, we owe the telegraph as we know it today to Samuel Morse, who was not a scientist, but a sculptor.

It is only during the second half of the 19th century that the first attempts at a systematic use of scientific theories for technical developments began. Some entrepreneurs and politicians, who saw in scientific discoveries (at least in certain areas of physics, chemistry and physiology) a possible (indirect) source of benefits, began to take a genuine interest in science. And this is how the alliance of scientists, engineers, doctors, entrepreneurs and even some clairvoyant politicians began to consolidate—and the result of that heterogeneous confluence is what we can genuinely call "technology".

Perhaps the first, or at least the most notorious and influential example of this new spirit of alliance between scientists, engineers and politicians was the deployment of the underwater telegraph between Britain and the United States in 1866 thanks to the scientific advice of William Thompson (later honored with the title of "Lord Kelvin"), who was already renowned for his contributions to a discipline very different from (and independent of) communication technology, namely the foundations of thermodynamics. Thanks to Kelvin's great influence, the decision of the University of Cambridge to establish, in the course of the 1870s, the Cavendish Laboratory, with the explicit purpose of constituting a coalition of scientists, engineers, entrepreneurs and officials for the promotion of applied science as an economic value took place (for more details, see Ball 2019, p. 29). This happened not before the last third of the 19th century. A contemporaneous parallel development took place in Germany, mainly due to the great influence of the pathologist Rudolf Virchow, though not in the area of physics, but in the coalition of the life sciences and medicine.

This is how in the second half of the 19th century, the first successful bases for the cooperation between scientists and technicians (in a broad sense of the term "technician", encompassing all kinds of engineers and medical doctors) began to be settled. It was on these bases that the 20th century turned out to be the first great century of technology. It would be ridiculous to list all the inventions made throughout the 20th century that were inspired by the many scientific theories proposed during that period or before. It suffices to mention only a few of the technological developments which have profoundly transformed the daily lives of humans: from radio to computers, through television, antibiotics and nuclear power plants. None of these inventions could have been conceived and implemented without the background of one or more previous solid scientific theories. This is what technology means, and this is what is characteristic of the 20th century and, perhaps, stretching this fecund period into the past, of the second half of the 19th century, *but of no previous era*.

3. Science as Fundamentally Independent of Technology

Now, when focusing on the development of science from the mid-19th century to the present day, we can see a few branches of science that came to extraordinary results, but have little or nothing to do with contemporary or subsequent technical inventions. A notorious case is, of course, that of the formal sciences—logic and mathematics—which since the mid-19th century had a boom incomparably superior to any previous development since the Greeks, but completely oblivious to any technological application. For example, one of the deepest contributions to logic and mathematics in the 20th century were the theorems of the completeness of first order logic and of the incompleteness of arithmetic that Kurt Gödel proved in 1930/31. Now, ninety years later, these famous theorems so far show to be completely irrelevant to any technological application. It is true that shortly after Gödel's proofs, there were some developments in the new logic and the foundations of mathematics that used similar formal techniques and that, in the long run, would lead to technological applications in the area of Artificial Intelligence; the most notorious case is, of course, that of the Turing machines; but Gödel's completeness and incompleteness results as such were irrelevant to these later developments.

The same goes for another discipline located at the opposite end of the range of sciences, far removed from mathematics but equally independent of applied science, namely, philology. Indeed, for the proof that all those languages known as "Indo-European" or "Indo-Germanic" have a common origin in a primal language, the "proto-Indo-European" (a language already lost nowadays, but that undoubtedly existed), the philologists of the second half of the 19th century and early 20th century (especially Franz Bopp and August Schleicher), who obtained this result after long and admirable efforts, did not promote any technical application of their discovery, and it is difficult to imagine to what new technology the identification of the proto-Indo-European could lead.

In the case of those scientific disciplines of which it is traditionally claimed, or simply assumed, that they are closely linked to technology, as is often assumed of the natural sciences, we will encounter so many exceptions that we could not even say that they confirm the rule. One of the best confirmed theories of biology that has deeply marked mankind's self-image is undoubtedly Charles Darwin's theory of evolution. Now, what is the machine or instrument that has been built thanks to this theory? The question is obviously ridiculous for being totally out of place. Only in the field of preventive medicine when dealing with pathogenic microorganisms it may appear that the principle of natural selection could be relevant for certain therapies, but these are rather marginal studies. In any case, to the vast majority of practicing physicians (i. e. technicians devoted to the healing of the sick), the theory of evolution remains completely irrelevant.

Even in physics, a discipline which many people think of when talking about the benefits that science brings to technology, we face more than one good example of irrelevance or very little relevance of science to technological developments. The two most fundamental and best-confirmed physical theories in human history are Albert Einstein's general theory of relativity, on the one hand, and the theory often referred to as "the standard model of particle physics" (in the following abbreviated as "SMPP"), on the other hand, developed in the 1960s primarily by Murray Gell-Mann, Sheldon Glashow, Steven Weinberg and Abdus Salam. Now, with regard to generalized relativity, it should be noted that Einstein formulated his theory in 1915, and very soon (in 1919) it would be brilliantly confirmed and celebrated by the scientific community as a huge scientific advance. However, only 80 years later it would be found that such a theory may have some technological relevance, albeit a very secondary one indeed, by helping to design the GPS satellite location systems. (In fact, GPS systems also may be developed without taking into account the fundamental equation of generalized relativity.) And as far as the SMPP is concerned, 60 years after its conception, we still are waiting for someone to tell us what its technological implications are. There certainly are some notable technological applications of (classical) quantum mechanics, like the laser, but this is a technology which was developed before the advent of the SMPP; also, there is certainly much talk nowadays about the prospects of developing so-called "quantum computers", but leaving aside the fact that they still are rather a promise than a technological fact, they would be an application of classical quantum mechanics and not of the SMPP as such.

In other cases, we can certainly point to very important technological developments based on pure science research, but in such a way that these researches were conducted with complete independence from any objective of technical application long before its technological possibilities were revealed. This is the case of the discovery and study of radioactivity in the late 19th and early 20th centuries by Henri Becquerel, and the couple Marie and Pierre Curie: only several decades after their scientific discoveries it turned out that radioactivity could be technologically relevant (whether for the construction of nuclear weapons or for cancer treatment) after Otto Hahn and Fritz Strassmann discovered the possibility of the nuclear fission of uranium in the late 1930s. It is noteworthy that neither Becquerel nor the Curies would have ever thought of such applications.

In other cases, technical inventions have had some relation to previous scientific inputs but they are so only in a much more indirect way than is usually assumed, and also often not with the theory considered to be the most valid and important in the domain in question. For example, it is true that Thomas A. Edison could not have thought in the late 19th century of making an incandescent electric lamp if he would not have taken into account Ohm's law established at the beginning of the same century. However, the really fundamental theory in this field, namely J. Clerk Maxwell's electrodynamics, published a few years before Edison's invention, served this inventor no good. In other cases, the scientific theory that inspired a technical invention later turns out to be completely false; this was the case already alluded to above of Franklin in the 18th century, who invented the lightning rod inspired by the theory of the two electrical fluids—a theory that would be abandoned soon afterwards...

Let us now summarize what the examples set out above, as well as many others that could be brought forward, show about the supposed linkage of scientific progress with technological progress. In many recognized scientific disciplines there is virtually no link between the two areas; others contain examples of a strong linkage, but also other examples (within the same discipline) of lack of linkage, or of not quite significant linkage, or even of an erroneous linkage between a technical invention and a false theory. It then follows that the essential function of science, at least as the cultural form that Humanity has known since the Hellenistic period, or since the 17th century at the latest, is not to be the advance of knowledge applied to technical developments. Science sometimes lends itself very well to being applied technologically, other times it lends itself only a little, and in still other cases it does not lend itself to it at all. But in any case, applicable or not, applicable to a greater or lesser extent, applicable in the short or in the long term, that which is the main mission of science, and therefore its true value, is not to contribute to technological developments. This is, at best, a side effect of science (welcome to some, disliked by others), but which in any case should not affect our assessment of the scientific theories that are at the basis of such developments. Maxwell's electrodynamics is no more valuable than the general theory of relativity because the former has driven the invention of things like radio and television, and the second has not.

4. The Genuine Value of Science

So, if it is not technology that can give meaning and value to scientific knowledge, where does the essential value of science come from—if it has any at all? In the Platonic-Aristotelian tradition, *epistéme*, the historical ancestor of our *scientia*, was characterized as the reasoned and well-justified knowledge of the essence of being. Certainly, today we would use a less metaphysical language, albeit still inspired by the Greek tradition, and we would simply say that *epistéme* or *scientia* is what provides us with a reasoned and well-justified knowledge of what really exists. But leaving aside historical-philological nuances, the purpose of our science is essentially the same as that of the Greeks' *epistéme*; only methods have changed. And even they have not changed drastically: at least since Hellenistic times, the Greeks already knew that mathematics and systematic observation are good tools for achieving solid knowledge. All they still lacked was the idea of controlled experimentation—with some notable exceptions, like the one exemplified by Archimedes. But even experimentation is not absolutely essential for attaining an adequate understanding of the scientific spirit; today, there are still a large number of disciplines considered as genuinely scientific, in which experimentation plays no role at all—from mathematics to linguistics through ethology and ethnology. In fact, our concept of science as the best way to achieve solid knowledge about what the world is like is not so different from Aristotle's. Deep down it is the same. Or at least it has been so until recently, because I must admit that my characterization of what is essential in the scientific spirit comes from a conception less and less shared by those responsible for the scientific policy of supposedly advanced States, by journalists, by those who write reports for ministries, in short, by most people who have some opinion on what science is, or must be. For all these people, science is, instead, nothing more than applied or applicable science. Their paradigm of what should be a scientific achievement is the hackneyed *Big Science* (which is basically nothing but large-scale technology), not the scientific theories as we knew them until the mid-20th century. I will next expand on this subject while documenting what we might consider a dangerous and costly misjudgment.

5. The Menace of Technologism

Technologism is an anti-Aristotelic alternative, a view of science, that, as alluded to above, was originally promoted by Francis Bacon at the beginning of the 17th century. Again, Bacon was not a scientist and, moreover, he was not fully aware of the true meaning of the Scientific Revolution that was taking place at that very time. He was, however, an equivalent of a modern, savvy PR man who greatly influenced the members of the contemporaneous *intelligentsia* and those who followed it. He inspired the phrase "science is power" which in fact meant that science could control Nature, a project that could be extended to human society. However, it is worth noticing that none of the true stars of the Scientific Revolution shared Bacon's view about the purpose of the sciences. For instance, Kepler did not propose to use the laws of the planetary orbits he had discovered to facilitate interplanetary traffic; Galileo did not focus his telescope on the Moon to heal the plight of lunatics; Descartes did not translate geometry into algebra in order to make the job of land-surveyors easier; Huygens did not investigate optical phenomena in order to provide corrective lenses to myopes; and Newton did not apply the law of gravitation he had discovered to tides in order to prevent shipwrecks. Notwithstanding these factual precedents, the Baconian doctrine was successfully adopted even by talented scientists who addressed heads of states, ministers, businesspersons, reporters, philanthropists, and anyone who could be sensitive to the "science is power" fake.

We may see the roots of this misrepresentation of the truly scientific spirit (a misinterpretation endorsed by many scientists themselves) in the fact that science is not practiced in a social vacuum, far from it. Indeed, the practice of theoretical and empirical research is costly. Scientists and their bureaucratic representatives are in need of funds to pay salaries to themselves and their collaborators, to the institutions that host them (the so-called indirect costs), and to purchase consumables and equipment. Consequently, sadly enough, it would seem as if scientists have subconsciously internalized Bacon's views to the point at which the unencumbered scientific goal becomes secondary to the need to maintain afloat the scientific enterprise that allows genuine original science to thrive. Unless corrective action is adopted soon, creative science will likely be reduced to applied science, that is, technology. Under these stressful circumstances, fundamental knowledge, that is the non-utilitarian goal of scientific research, will tend to disappear from our culture. More troublesome, the notion of "science for the sake of science" may become incomprehensible to future generations.

6. The Stagnation of the Genuine Scientific Spirit

Based on an analysis of developments that have taken place during the last one hundred years, we may reach the sad conclusion that the threat represented by technologism replacing science has been intensified in the last decades. Certainly, our perception of a "progressive stagnation of the scientific process" may be regarded by some inside and outside the academic community as just an exaggeration. After all, widespread comments by the specialized press, newspapers and magazines insist in highlighting alleged breakthroughs that have taken place along the length of the 20th and the current centuries. However, if one focuses on momentous discoveries that have taken place during the 20th century and to what has happened as far as scientific breakthroughs during the current century, the picture is rather murky. In fact, unequivocal signs of scientific stagnation are becoming increasingly obvious. To be more precise, the stagnation process in the sciences has become more notorious after the first two thirds of the 20th century have elapsed. Certainly, if we would agree that the 17th century could be considered as a *saeculum mirabilis* for science, a comparable evaluation should be extended to the first two thirds of the 20th century. We may arbitrarily point to 1966 as a conventional temporal limit for exceptional scientific contributions or startling discoveries followed by a mediocre period. And now, let us document this claim.

Let us start by examining what has happened in the formal sciences, namely, logic and mathematics. Truly revolutionary contributions in these sciences have taken place without exception in the first 2/3 of the 20th century. In 1901, Bertrand Russell discovered the paradox that carries his name that shook the foundations of logic and mathematics; next, between 1910 and 1913, again Russell and Alfred N. Whitehead published the *Principia Mathematica*, a monumental exposition of the new logic and its application to the foundations of mathematics. Then, from the beginning of the 20th century to the 1930s, Ernst Zermelo, John von Neumann and a few others axiomatized set theory as we know it today. In the 1920s, David Hilbert and his disciples developed proof theory, exceedingly important for the foundations of mathematics. In the early 1930s, Gödel proved his famous theorems, probably the deepest contribution to the understanding of the nature of logic and mathematics. In 1940, again Gödel showed the consistency of the so-called "continuum hypothesis" with the other axioms of set theory. Between the 1940s and 1950s, the self-described "N. Bourbaki" group reconstructed all of mathematics in a unified fashion based on set theory. In the 1950s, the theory of categories was developed as a general alternative to set theory. In 1963, Paul Cohen proved that the continuum hypothesis is independent of the other axioms of set theory, a truly intriguing result. In the 1950s and 1960s, Alexander Grothendieck, who many consider the greatest mathematician of the 20th century, published his most revolutionary works on algebraic geometry and topology, which earned him the Fields Medal just in 1966, our "hinged year"; it is symptomatic that, after this date, Grothendieck's contributions became less numerous and less significant, and that he soon after voluntarily withdrew from active research... And now, let us ask ourselves, what fundamental contributions have been made in mathematics since the 1970s? Undoubtedly, some interesting specific results have been obtained such as the proof of Fermat's theorem, or some further developments in category theory; however, none of this is comparable to the accomplishments that took place during the two first thirds of the century.

Let us now move on to the contributions in the physical-chemical sciences. In this field, the contrast between what can be considered as significant contributions in the third part of the 20th century plus the two decades of the 21st century and the first two thirds of the 20th century has been even more spectacular. Absolutely all the fundamental theories about space, time and matter that have revolutionized our understanding of the Universe were proposed *and confirmed* during the first two thirds of the century. In 1905, Albert Einstein enunciated the special theory of relativity; next, in 1915, Einstein again proposed the general theory of relativity that was verified in 1919 by Arthur Eddington and his group through careful astronomic observations. In astrophysics, based on Einstein's general theory of relativity, Georges Lemaître formulated in the 1920s the Big Bang hypothesis that was empirically confirmed by Edwin Hubble in 1929.

Moving on to a completely different branch of physics, namely, quantum physics, it can be noticed that the first version of quantum mechanics was due to the contribution of Max Planck in 1900; the definitive versions of this theory, namely, the matrices mechanics of Werner Heisenberg and the undulatory mechanics of Erwin Schrödinger were independently and simultaneously built at the end of the 1920s. Then, in the 1930s, P.A.M. Dirac established the basis of quantum electrodynamics which allowed the unification of quantum mechanics and the special theory of relativity. Later, the Standard Model of Particle Physics (SMPP), a genuine fundamental theory (and not just "a model"), considered as the most successful theory ever in physics, was gradually constructed beginning in 1961 when Gell-Mann introduced the notion of weak interaction. Shortly thereafter, Glashow unified the electrodynamic phenomena with the weak interaction and Gell-Mann formulated the quark hypothesis. Finally, Steven Weinberg and Abdus Salam published in 1967 (only one year after our arbitrary selection of 1966 as the end of the great scientific contributions in the 20th century) the synthesis of the three great types of interactions, namely electromagnetism, the weak and the strong interactions (for more details about these last developments, see Moulines 2016, pp. 955-956). It is worth calling attention at this point that, after the unification of the three mentioned interactions within the frame of the SMPP, most physicists thought that one more step could be promptly made, namely, the unification of these three basic interactions with the oldest one known, i.e., gravitation, which is dealt with by another (ontologically and methodologically) quite different theory, namely, the general theory of relativity. The expectation by physicists during the last third of the 20th century to find a way to unify both theories either by showing that the general theory of relativity could be "reduced" to a slightly modified version of the SMPP or, alternatively, that a providential untapped genius or a group of geniuses would be able to formulate a novel Great Theory (the famous "theory of everything") that would encompass the SMPP and the general theory of relativity as special cases—a new great theory able to be empirically verified—did not materialize despite the concerted efforts invested in this direction. Indeed, unifying theories such as the various versions of the so-called "string theory" and the notion of the "multiverses", starting in the 1970s, as a matter of principle may not be tested empirically, a fate recognized even by their own originators. Thus, it would appear as if, during this period, at least a group of mathematical physicists would have become exalted metaphysicians using rigorous mathematics indeed, but remaining nevertheless hard-nosed metaphysicians with no connection with empirically testable facts. This alternative has nothing to do anymore with physics as an empirical science, at least as judged from what we have learned from Archimedes, and later on from the developments that took place during the 17th century.

Always within the physical-chemical sciences, but essentially independent of relativist and of quantum physics, there is a branch that deals with irreversible processes, namely what is usually called "non-reversible thermodynamics". It is essentially devoted to the study of chemical and biochemical processes. It originated in the 1930s with the so-called "reciprocity relations" of Lars Onsager which were later refined by Ilya Prigogine's significant contributions in the 1940s and 1950s. No new important theoretical breakthrough in non-equilibrium thermodynamics has occurred after those introduced by the pioneering contributions of Prigogine and his disciples.

In sum, no highly significant theoretical advance has been recorded in physics and chemistry in the last third of the 20th century and during the two decades of the current one. Admittedly, a few noteworthy discoveries did take place in this period such as the detection of the Higgs boson in 2012, which definitely confirmed the SMPP, and the first more or less direct observations of black holes between 2016 and 2019. It should be noted, however, that none of these late discoveries are comparable to the breath, depth and innovative significance of those mentioned above that took place in the first two thirds of the 20th century.

Regarding the earth sciences, their fundamental theoretical paradigm continues to be the continental sliding slabs theory formulated in 1912 by Alfred Wegener, which was acknowledged to be reliable shortly after the end of WW II. No significant new development in this field has been recorded after this momentous event took place.

Let us now move on to crucial developments that occurred during the last 120 years in the life sciences with the purpose of determining whether they offer the same diachronic pattern seen in mathematics and in physics. Without entering into details, suffice it to remember that a reliable formulation of Mendelian genetics and its empirical confirmation took place during the first two decades of the 20th century with the theoretical work of, among others, William Bateson and Hugo De Vries, and empirically by Thomas H. Morgan and his collaborators around WW I. Later on, in the 1930s and 1940s, a combination of genetics and evolutionary biology opened the way for population genetics thanks to the far-reaching theoretical and empirical contributions due to Theodor Dobzhansky, J.B.S. Haldane, Robert Fisher, Ernst Mayr and George Simpson, who generated the so-called evolutionary modern synthesis. Also, in the 1930s, ethology was created thanks to the leadership of Konrad Lorenz in Vienna. And finally, after the crucial identification of DNA as the carrier of the genetic material by Oswald Avery's group in 1944, it was in the 1950s that Rosalind Franklin, Francis Crick and James Watson developed the bases for the so-called Molecular Biology Revolution by describing the correct double helix structure of the DNA molecule. Decades later, this branch of biology culminated in a technological bonanza that is currently applied to the fields of medicine (diagnostics, vaccines, etc.), agriculture (nutrition, etc.) and other domains. Next, it can be considered that Conrad Waddington's introduction of epigenesis in the field of development in the 1950s and 1960s qualifies as a significant seminal contribution. Realistically, however, has it been any conceptual contribution in the life sciences since the 1960s that could be recognized as earth-shattering like the previous ones?

In the field of psychology, psychoanalysis already flourished before WW I and the behaviorist paradigm emerged shortly thereafter. Now, regarding the subject of cognitive psychology, it is generally acknowledged that it has its roots in the pioneering contribution by Warren McCulloch and Walter Pitt who in 1943 introduced the neuronal network theory which was later on enriched by the initial developments of artificial intelligence by John von Neumann, Norbert Wiener and others toward the end of the 1940s. These days, claims about a grandiose new cognitive paradigm tend to ignore that the basic elements of cognitive science were already in place well before 1966, our arbitrarily designated limit for truly revolutionary contributions in the sciences at large. It would probably be more realistic to consider that ever since the pioneering contributions generated before 1966 in cognitive science, a process of confirmation and data refinement took place thanks to the incorporation of the novel technological marvels of brain imagery. In his recently published book *The Idea of the Brain*, the neurobiologist and science historian Matthew Cobb summarizes the situation in the cognitive sciences by concluding: "No major conceptual innovation has been made in our overall understanding of how the brain works for over half a century" (quoted by Philip Ball in Ball 2019, p. 31). This harsh judgement may certainly appear to be a bit too exaggerated, but it seems to me that it responds to a widespread feeling among the specialists in this area.

Let us consider now the social sciences. In order to reflect about presumably significant developments in these disciplines, it might be useful to recall the ideas advanced by Thomas S. Kuhn in his influential book *The Structure of Scientific Revolutions*, whose first edition was published in 1962, that is, just before our 1966 limit settled above. According to Kuhn's views at that time, the social sciences were in a "pre-paradigmatic" stage because the respective scientific communities were not yet unified in acknowledging which were the fundamental concepts and principles in each one of the relevant fields, which were the basic questions to be answered and which were the methods that could tentatively shed light on those questions. It is worth recalling that in the 1960s, Kuhn's views cautiously implied that at least some of the branches of the social sciences would soon reach a true paradigmatic stage by agreeing on the three elements just mentioned. Realistically, however, it might be fair to recognize that 60 years after such optimistic prediction no such change has been generally acknowledged in the social sciences. Admittedly, at some point, Noam Chomsky's model of generative-transformational grammars in linguistics seemed to reach the desired paradigmatic stage. However, as of today, Kuhn's prediction has not materialized even for linguistics if one realizes that a multitude of well-regarded linguists from all over the world do not relish listening to generative-transformational grammars.

Equally questionable are unsubstantiated claims that, during the last decades, the so-called economic sciences have reached a paradigmatic stage. One may seriously consider this claim if one narrows it down to the developments in microeconomy, and more specifically, in the combination of decision theory with game theory. (Incidentally, these theories were proposed already in the 1950s.) However, if we keep in mind developments in macroeconomy (which is what people normally think about when referring to theories of economics), it should be acknowledged that for decades now there has been an implacable competition among at least three alleged paradigms or general views, namely, the classical neo-liberal of Friedrich Hayek and others, the Keynesian, and the (crypto)Marxist of Thomas Piketty, for example. Clearly, they all originated in approaches dated from before 1966. Altogether, it could be safely concluded that no successful paradigm in any of the social sciences has materialized since their premature anticipation by Thomas Kuhn in the 1960s.

7. John Horgan's View on the Stagnation of Science

Within the context of this essay, it is legitimate to ask whether the tendency toward a progressive stagnation of the genuine scientific spirit is a temporary, fleeting phenomenon, or does it have profound historical and social roots? Despite clear evidence for the patent science stagnation phenomenon, it is puzzling to notice how rare has been a rigorous analysis of it by scholars in the field. Perhaps, the exception in this regard has been the systematic analysis of the subject by the scientific commentator and historian of science John Horgan who in 1996 published a book provocatively entitled *The End of Science: Facing the Limits of Knowledge in the Twilight of the Scientific Age*. Essentially, he explained the stagnation of the sciences as a result of the combination of two endogenous processes. One of them relates to the assumption that in certain areas, such as particle physics and molecular biology, the fundamental laws that have already been uncovered fulfill all the explanatory requirements of the subject. The argument follows that those disciplines have reached an optimum of consolidation and confirmation *ad vitam aeternam*, and that, therefore, what remains unknown are just little complimentary details, that could be translated pejoratively as "mop-up operations". In addition to this depressing interpretation, Horgan also entertains the notion that the scientific enterprise in general has reached a degree of sophistication that prevents the human intellect to surpass the natural limits of human cognition. In other words, until a few decades ago, difficult but not insoluble problems could have been resolved when a single genius, or a group of collaborating geniuses, could propose and verify a highly complex theory. However, according to Horgan, in the recent past, the complexity of the problems faced by scientists is such that explanations of those subjects are beyond the intellectual capacities of humans. Thus, in our times it would be unimaginable the arrival of a Darwin, a Hilbert, an Einstein, or a School of Copenhagen capable to resolve them successfully.

Historians of science, practicing scientists and the educated public already know about arguments like the one Horgan advanced regarding the limitations of the human intellect either to make further substantial progress, or else to resolve yet to be explained scientific issues that have become too complex for the human mind. As is widely known, a comparable view arose toward the last third of the 19th century triggered by physicists who prematurely considered that the fundamental laws of physics had already been proposed and verified, and that only unimportant details were still to be resolved. It is well-known that the German professor of physics Philipp von Jolly emphatically recommended his young pupil Max Planck not to devote his career to physics, since, supposedly, no interesting new developments could be expected in this discipline (Planck 1950). And again, the argument that science had reached its intrinsic human limit became popular among European intellectuals and scientists, initially in German speaking countries and later even more acutely in France. More specifically, when the famous Swiss physiologist Emil Du Bois-Reymond examined basic questions regarding the essence of matter, life and conscience, he was quoted as stating the famous phrase "*Ignoramus et ignorabimus*" ("we ignore and we will ignore") (Du Bois-Reymond 1872). During those years, other intellectuals and scientists, especially in France, also stated that science in general was bankrupt (Otero 2011). Shockingly, however, only a few years later, at the beginning of the 20th century, very important theoretical developments in science took place such as the introduction of mathematical logic, the strengthening of set theory, as well as the creation and confirmation of the relativity theories and of quantum physics in the basic sciences. Meanwhile, the development of genetics revolutionized the field of biology. On the one hand, one wonders whether the pessimistic current views of John Horgan are a re-edition of the myopic views of Phillip von Jolly, Du Bois-Reymond and the French "bankruptists" of the 1890s. On the other hand, leaving Horgan's pessimistic views aside for the moment, we should give him deserved credit for having diagnosed early on the stagnation of the sciences in the last decades. We might differ however on identifying the etiology of the phenomenon. Neither the current stagnation nor the one diagnosed by Jolly, Du Bois-Reymond or the "bankruptists" of the end of the 19th century were due to an inherent (and unavoidable) evolution of the scientific spirit. Further, it seems unlikely that at the end of the 19th century or today, simultaneously, all the sciences may have ended up in an intellectual cul de sac. Due to what metahistorical and/or metascientific miraculous coincidence sciences that have nothing or little in common, from mathematics to ethology, going through physics, chemistry, geology, and biology, may have reached their explanatory limits at the same time? Moreover, each branch of these sciences has shown to develop following a very unique historical process. Indeed, mathematics as a scientific discipline dates back to the 6th century BC (that is, 25 centuries ago), scientific astronomy started developing in the 4th century BC (23 centuries ago), physics developed starting in the 3rd century BC (22 centuries ago), chemistry began developing in the 17th century (just 4 centuries ago), biology began as a science starting in the last third of the 18th century, that is two and a half centuries ago, and finally, scientific psychology developed in earnest toward the end of the 19th century (a little more than a century ago). It is, therefore, highly unlikely that these varied scientific disciplines might have imploded by having reached simultaneously the same intellectual obstructing wall.

8. Toward an Externalist Explanation for the Stagnation of the Sciences

Summarizing Horgan's thesis, an explanation for the current scientific stagnation suggests that it is due to factors inherent to the respective scientific disciplines. This represents an "internalist" explanation. However, the previous discussion of the historical and methodological data at hand suggests that Horgan's thesis is not plausible at all. It is preferable to consider, instead, first and foremost the external factors (social factors, that is) that might more realistically explain the stagnation that we both agree currently affects the sciences. By blaming external factors for the stagnation of the sciences, we may offer a tentative optimistic alternative in the sense that, once those factors identified, they may be susceptible of being corrected. In this regard, shortly before the beginning of WW II, J. B. S. Haldane, a widely praised physiologist and geneticist, anticipated that something undesirable was becoming evident about how public opinion was perceiving the role of the sciences in society. Here is an excerpt of his worrying premonitions:

> It is quite possible, I think, that as the ideals of pure science become more and more remote from those of the general public, science will tend to degenerate more and more into medical & engineering technology, just as art may degenerate into illustration and religion into ritual, when they lose the vital spark. (Haldane 1937, p. 119)

I share Haldane's diagnosis of the crisis that the sciences are now going through formulated more than eight decades ago, and I prefer it over the one Horgan advanced less than three decades ago. Moreover, I propose to consider two important independent factors that, from an epistemic perspective, relate to the practice of the sciences and the social context in which the sciences are perceived; though they are methodologically independent, they mutually reinforce themselves. These two factors are, on the one hand, the above referred technologism that has overtaken the practice of the sciences, and on the other hand, what can be characterized as the competitive spirit under which the sciences are currently conducted.

The technologism factor has already been addressed above. Let us next deal with the second factor. In my view, this second factor is grounded on a mischaracterization of how the sciences should currently be appreciated and practiced. Namely, instead of classically considering science as a collaborative enterprise among scientists in search of truth, or at least an approximation to it, the current rationale to assure success in science considers that scientific progress will materialize only as a result of a ruthless competition among scientists. This competition could be exercised among separate individual scientists or between small groups with the aim of achieve prestige and/or financial support from governmental, philanthropic or big industrial funders. The necessary goals to obtain the prestige and the funds to initiate or to continue doing research do not in themselves have much to do with pursuing the search for truth or the objective knowledge of Nature. Instead, those goals are: 1) the number of papers published yearly in prestigious peer-reviewed periodicals (preferably in the Anglo-Saxon countries) by the scientist or the group of scientists considered, and 2) the number of times a publication by the scientist(s) in question is cited in the periodicals referred to above. The first criterion of scientific recognition has increased exponentially in the last decades while the second one, that we may baptize as *citalogics*, is increasing significantly as well. Actually, citalogics has become a recognized branch of the sub-discipline of sociology of science destined to assess the worth of scientists for governmental or business funding sources.

Citalogics, as a metascientific discipline, began in the 1970s/80s, but it became very influential after Internet and the Web of Science would turn to be the evaluators of records of scientists (at least in the so-called paradigmatic sciences). In 2005, Jorge Hirsch, a physicist, coined what became the *h index* aimed at quantitatively evaluate with objectivity the productivity of any scientist based on the number of her publications and the number of citations her publications accumulated over time. Ever since, the h index has increased its popularity and thus it has been used with increased frequency in university settings, and in industry and commerce. It has become obvious that under these circumstances, scientists in constant competition with their colleagues in the same area of research aim to increase their respective h index. This attitude prevents them from considering their colleagues as welcomed collaborators, as originally conceived by traditional science. Instead, fellow scientists in the same area of research become dangerous competitors. It then follows that in order to increase their respective h index researchers will tend to publish as many articles as possible on popular subjects susceptible to impress publication reviewers and those in funding "study sections". As a result of this mismanagement of values, it is not surprising that young researchers would avoid selecting difficult and/or esoteric research subjects where sure short-term success is problematic and chancy. Under these dangerous conditions, it is unlikely that young investigators would take the luxury of waiting two decades to publish their research efforts as Newton, Darwin and others did in the past to convince themselves of the solid quality of their results. As David Chavalarias and Philippe Huneman recently argued: "the perverse effect of the incitement to the race to publish leads almost mechanically to a decline of the quality of scientific production" (Chavalarias & Huneman 2020, p. 4—my translation).

On top of the pervasive influence of the two external factors referred to above that have decisively contributed to the current stagnation of the sciences, one may notice an additional serious detrimental outcome. Having to "sell" their research projects to their own competitors sitting in judgment in arbitrarily selected, conflicted "study sections", researchers are encouraged to oversell the merits of the areas of research they choose and promise improbable outcomes. The sad realities faced by researchers who apply for funds foster the adoption of a cynical attitude toward a situation in which applicants and funders (direct and indirect ones) accept the odious situation where each participant plays a role in a drama that is just a farce. This is hardly the way to do creative science.

Until a few decades ago, the dictum "Science for the sake of science", which derived from the previous one dated from Classical Antiquity positing "Knowledge for the sake of knowledge", was accepted by any minimally educated person. Despite repeated statements in the same sense, current public opinion appears increasingly dubious of such claims. Paradoxically, however, two other structurally analogous dictums, namely, "Art for the sake of art" and "Sport for the sake of sport", coined in the 19th and 20th century, respectively, enjoy a higher popularity than the much older one about science. As sketched in this essay, we may attribute this unfortunate development to technologism, on the one hand, and to the misguided competitive attitude prevailing in established research institutions, on the other. In a cynical twist, one may recognize a sort of late revenge by Francis Bacon. The situation that the principle "science for the sake of science" faces now is due to complex factors that prevent the fulfillment of the stated goals of scientists who decades ago explicitly understood and abided by the contract between scientists and the public who funded basic research. Current realities in the practice of science, in the political discourse, in the short-termism of the electorate, of the public opinion and of the media do not help much in restoring the tradition dating from the 17th century that would provide the basic seeds for "science for the sake of science" to restore its original intrinsic creativity.

Conclusions: is Here a Problem to be Fixed? If yes, by Whom?

The sciences have been one of the most important contributors to the development of humanity on planet Earth. Now, a number of arguments have been advanced in this essay that indicate that the sciences are facing short and long-term serious threats that question their viability in the midst of a decades-old period of crisis. These threats are generated by the same protagonists who have been and are still responsible for their perceived success, namely, humans. Simultaneously, humanity at large is also facing comparable threats to its viability in the form of climate change, pollution, and over-population. It is not an exaggeration to qualify these threats to human viability as real crises. The resolution of this wide-range threat will require the adoption of remedies that should address current shortcomings affecting all aspects of human activities. The sciences and the scientists should volunteer to play crucial roles in advancing theories, reliably collecting and interpreting data aimed to resolving intellectual unknowns on a long-term basis. The narrative just offered here implies that during the last half-a-century the virtues of academic scientific research have been replaced by the pursue of technological feats that do not address the sustainability of the heterogeneous components of humankind living in a biodiverse environment. A resumption of creative science may not by itself resolve the complex crisis humankind is facing. However, if the sciences could help in a communitarian effort in such direction, this will only take place in an atmosphere in which scientists are given the opportunity and the tools to generate knowledge without financial "strings attached".

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