TWO CENTURIES-TWO CRISES IN THEORETICAL RESEARCH

LUIGI ACCARDI

Content

I. Introduction

The part of scientific activity devoted to the investigation of issues that, even if solved, would not bring immediate material advantages to the society is called pure research. The fundamental canons that have guided, since the dawn of science, the activity of all scientists who have practiced this type of research, regardless of the importance of their scientific results, are: *the belief that man can distinguish trivial or false scientific statements from profound ones and the determination to pursue fruitful truths*.

These tenets have undergone two crises. One of theoretical and logical nature, which involved not the whole pure research, but only its spearhead: mathematics. This took place right after the first world war and its importance is widely acknowledged among insiders even if its implications for culture in a broad sense (i.e., beyond the technicalities) are still now too weakly perceived. Another, of sociological nature, that we are living now, took place after the second world war, and has its roots in the new features of contemporary science, discussed in Section 1.3 below, which in their turn have their roots in the growing awareness of the importance of this type of research as an essential pillar of a nation's economy and political power.

The focus of the present paper is on the latter crisis, widely exorcised by experts and practically unknown to the non-specialists, but we also briefly summarize the first because, as we will see later, the two are related.

I.I *The first crisis: loss of certainty in mathematics*

For thousands of years mathematicians have believed that a mathematical statement, proved by correct application of the rules of logic, is *true* and that, since the rules of Aristotelian logic include the principle of non-contradiction (*tertium non datur*), given a mathematical statement one can always prove that it is either false or true. In the 1910s, mathematician David Hilbert proposed the program **to prove** this belief be itself true. This is known as Hilbert's program and can be summarized in the following statement:

Prove that, applying correctly the laws of logic, mathematical research can never produce two mutually contradictory statements.

The first step towards the realization of this program was to give a precise, i.e., formal, meaning to the terms false and *true* (otherwise the term *proof* is itself not well defined). In other words, logic itself had to be fully formalized as a mathematical theory. (Formalizing a theory means listing a family of axioms and showing that all statements of the theory can be deduced from those axioms.) This means that logic enters the domain of mathematics.

The first important steps towards this inclusion are due to the mathematician George Boole, in the first half of 1800 and, in the second half of the same century, Glottlob Frege proved (1879) that elementary arithmetic, from which a great part of mathematics can be deduced, can be constructed within an axiomatization of logic, identified through Boole's discovery, with set theory.

Frege's program found an obstruction in the results of Giuseppe Peano and his school, from which the first logical *paradoxes* begun to emerge, in particular the Burali-Forti paradox (1897) a slight variant of which became later known as Russel paradox. The first attempt to construct a formal system free of paradoxes was undertaken by Bertand Russel and Alfred North Whitehead (1910). All these results stimulated the birth of Hilbert's 1910 program. However, contrary to the expectations of the entire scientific world, in 1930 Kurt Godel proved two theorems implying that Hilbert's program cannot be realized (this is the generally accepted interpretation of Godel's results, although some subtle technical arguments have been raised against it). The intuitive contents of Godel's first theorem is that, if a formal system (like mathematics) is coherent (i.e. free from contradictions) and sufficiently rich to contain elementary arithmetic, then it necessarily contains some statement whose falsity or truth cannot be proved within the given formal system (such statements are called *undecidable*). Godel's second theorem states that if a formal system, in the sense specified above, is coherent, then it is impossible to prove its coherence using only axioms of the given system. In a very rough way, one can say that this statement implies that, *even if mathematics is free of contradictions, this cannot be proved.*

It should be emphasized that Godel's first theorem only says that, in a coherent and sufficiently ample formal system (in the sense specified above), there must exist undecidable statements. Since this doesn't rule out the existence of true statements, a mathematician, while accepting the reality of not being able to prove the truth or falsity of every statement, can console himself by thinking that at least he will be able to devote himself to the search

> for those statements that are true and he knows that, if the theory is coherent, there are only three possibilities: *false, true, undecidable.* But here Godel's second theorem enters in the play and puts the mathematician in the difficult condition to recognize that, since he cannot prove the non-contradictory nature of mathematics, he has to admit that he cannot rule out the possibility that, sooner or later, mathematicians will come across some contradiction, i.e. a statement that can be proved true (that is, correctly deduced from the axioms) and such that the negation of this statement has the same property. In other words, the three above listed possibilities become four, namely: *false, true, undecidable, contradictory* (*i.e., simultaneously false, and true*). But a standard theorem of logic says that, if in a (formalized) theory there is a contradiction, then all statements of the theory are simultaneously true and false, i.e., *in such a theory the difference between true and false, hence any pretense of certainty, becomes meaningless*.

In conclusion: from 1930 (the year of Godel's theorems) onwards, mathematicians are aware that *their certainties are local* in the sense that they can be sure that the rules of logic are correctly applied to the deduction of their theorems from the axioms of the theory, but they cannot be sure that such axioms do not lead to contradictions. Let us emphasize that this does not mean that mathematics is contradictory, but only that we cannot prove that it is not. The fact that throughout the history of civilization, no contradiction was discovered supports the optimism of most mathematicians that also in the future no contradiction will appear. *But this is a faith, not unlike faith in God*.

Since for millennia humanity had spoken of mathematical certainties, one can understand why mathematicians remember the early 1930s as the years *of the crisis of the foundations of mathematics*.

But the negative conclusion of Godel's theorems is at the same time a strongly positive achievement: for the first time in mankind's history, *some* limits of what reason can reach can be proved. The emphasis here is on the verb *proved*: many philosophers in different historical periods have argued different limits on what human reason can do, but an opinion is quite different thing than a proof. Opinions can be refuted in the course of time, proofs cannot. *Kline sees in these results the loss for humanity of mathematical certainty* ([Kline80]).

1.2 *The second crisis: scientific truths versus sociological truths*

As we have seen, the first crisis arose from the attempt to give a precise meaning to the notion of *truth* of a statement, at least in a formal, mathematical theory. The second crisis has a more general scope as it involves not only formal theories, like mathematics or logic, but also not fully formalized theories, like physics or information theory. The two are related because undoubtedly the first crisis has weakened scientists' ideal of *scientific truth* in general, paving the way to the dangerous tendencies we are going to discuss. As a matter of fact, the percentage of scientists familiar with the conceptual implications of Godel's theorems, let alone their formal statement and proofs, is negligible.

From this, one might conclude that the weakening of the notion of *scientific truth* is an issue involving only a tiny minority of people.

But history shows that profound ideas, over time, end up spreading themselves in people's consciousness, even if not necessarily explicitly and rationally. So, we can say, with reasonable confidence, that as an experimental proof of a statement cannot be considered certain, from a purely logical standpoint, but only *adequately supported* by experimental evidence, similarly the most that a theoretical assertion can aspire to is to be supported by *adequate* theoretical or experimental verifications.

The term *adequate* presupposes a judgment and a judgment in scientific matters can only be expressed by an individual or a group of individuals. Since most people are neither competent nor interested in scientific matters, traditionally, i.e., since the early beginning of science up to nowadays, there have been only two criteria for the evaluation of scientific results:

the judgment of other scientists and the practical implications of these results:

This system has worked well for millennia, but nowadays the role of science in society, and consequently the sociological structure of the scientific community has undergone drastic changes which require a reconsideration of the aforementioned criteria (see Section 3.1 for a deeper understanding of this important issue).

1.3 **New sociological features of contemporary science**

The main features that distinguish contemporary scientific community from the old one, say before the end of the second world war, are:

1) *Specialization*: the growth of scientific knowledge implies that a researcher in any scientific field must absorb such a quantity of scientific notions and techniques that joint research with colleagues in different fields becomes an extremely hard undertaking.

2) *Massification*: it is known that the number of scientists currently living is greater than the sum of all scientists in all previous eras.

3) *Industrialization*: in advanced countries science, and more generally education, has become an essential pillar of economy due to the rapid contraction of the times from discovery to applications. This has two implications: (i) Nowadays we can speak of the industry of education as one of the largest sectors (both in terms of the number of people involved and of budget) in the economy of any advanced country. (ii) Scientific research has become an intrinsic pillar of advanced technology, not only in the sense of applications, but also in the converse sense that some fundamental scientific discoveries more and more often come from industrial research centers (for example the discovery of super-conductivity al high temperatures took place in an IBM laboratory).

4) *Politicization*: In advanced countries, the funds invested in pure science have no comparison with those invested in the same sector before the 1950s. The control of the distribution of these funds stimulates the birth of scientific lobbies in the following simply called *corporations*.

5) *Globalization*: the power of contemporary media allows aggregations and collaborations of scientists acting in different countries, even very distant from each other, that 50 years ago were simply inconceivable.

These new features should not be exorcized because they are intrinsic to our society. Each of them includes positive and negative aspects but, if the social forces arising from them are completely left to themselves, they can have deleterious effects. It's like to what happens with the social forces arising from the markets: now everybody agrees that some state control is needed to prevent disastrous effects. The difference between liberals and communists on this issue is only quantitative, not clear cut as it was little more than one century ago.

The main goal of the analysis that follows is to rise the problem and to propose some possible solutions.

We will use the term *contemporary science* to refer to those scientific communities that possess the 5 features listed above and *old science* to refer to science before the appearance of these features.

The mutual interaction among these new features has serious implications on the sociological structure of the scientific community and the main objective of the analysis that follows is to make some of them explicit.

Specialization creates many islands, i.e., micro-, and macro-sectors of science, scarcely or not at all communicating among them. The purely theoretical nature of the research in these sectors favors the fragmentation of groups and self-referentiality, whereby islands proliferate with a process like cell division.

Massification of theoretical research, and the fact that creativity is not a mass phenomenon in our era (and probably will not be in the future), implies that most researchers in a micro-sector stick to a common micro-context and their scientific activity consists in solving problems and elaborating variants of notions within this micro-context (see Section 3.2 for more details). This kind of activity can be synthetically called normal science following a terminology introduced by T. Kuhn [Kuhn62].

Normal science is, for some respects, like the activity of the workers in an industry, but with a big difference. In industry each worker realizes a task well defined and prescribed from the above following a general plan aimed at a global goal (for example the production of cars, planes, or of various kinds of services). Moreover, those who realize the given plan are defined a priori: the employees of the given industry. In normal science the global goal is vague (e.g., to better understand a specific and well delimited topic), local tasks are chosen by single researchers with the only constraint of being within the given micro-context and interactions among different tasks are also object of individual choices. A single micro-context defines a *sociological community* whose members recognize each other and, due to globalization, typically they belong to different countries.

Politics, in the academic sense, comes into play in two ways: control of research funds and control of jobs (which is a different way of controlling research funds). The control of ideas is a consequence of these two. Typically, the distribution of jobs takes place at local level, i.e., involving the single universities, but some nations, like Italy, have a centralized system which attributes the highest academic titles (professor and associate professor), thus creating a pool of potential candidates from which individual universities can choose. But, as we will see in Section 3.3, the local choices are more and more influenced by factors outside the control of local academic powers like the various types of impact factors i.e., indices of the degree of influence of the various journals, the hierarchical subdivision of scientific journals in different disciplines into categories which classify these journals into categories according to their importance. Some of the criteria, on which this multiplicity of indices is based, are public some are not. But, since these indices are produced by relatively few organizations, even in the cases when the criteria are public, there is ample space for manipulation in the concrete calculation of them.

In the distribution of research funds, alongside the traditional national centers, various international institutions have been created in the last decades and their role is constantly growing. This happens for example in Europe with the funds of the various programs of the European Community. In some countries, like the USA, also the army supports theoretical and technological research. In all cases these organizations must rely on experts, and this creates a competition among the various scientific corporations aimed at influencing these organizations. The methods used in this competition and their effects are briefly discussed in the following section.

1.4 *Alaska's gold*

The need to influence funds or legislation or the organizations producing the different indices of scientific relevance, creates a push towards the creation of scientific corporations. This is a relatively recent fact because, even if corporations already existed in ancient Rome and, in 1200, they flourished in Europe under different names, they did not include scientists (probably since they were too few). Nowadays corporations have evolved into trade unions, professional registers, professional associations, …, but in science the feeling of belonging to the same group is psychological and many of the collective behaviors of scientific corporations are not directly related to national or international associations.

Only few, among the sociological communities corresponding to various micro-contexts, manage to achieve the critical size and the level of internal organization that allows access to control of funds or jobs distribution. Contrarily to what happens in industry, scientific corporations are not clearly defined because of their informal nature and because the aggregation criteria are multiple and dynamical, combining interests of single macro or micro scientific sectors with national or local interests. The result is that, inside the scientific community some groups have formed, membership to which is not formal, such as for Freemasons or for scientific associations, but that represent the strong powers in this community and whose role is essentially to guarantee the permanence of the existing equilibria and increase the influence of the single groups on society. In the following we will refer to the complex of these groups using the neologism *buro-academy* to distinguish them from those whose interests are concentrated in research.

Due to the size of contemporary research community (massification), the control a single scientific corporation can achieve can only be partial, (in fact very partial with respect to the size of the whole research community) but in some cases (as will be seen in Section 2) it can achieve a global reach, involving the whole world.

An historical analogy, concerning macro-politics rather than academic politics can help to understand the mechanisms that allow the occurrence of such a thing.

After having purchased Alaska, the USA government let the rumor filter through that in Alaska there was gold. The political interest in populating the new US state with Americans resonated positively with economic interests and this rumor was highly amplified by the press. At the same time, the idea tickled the spirit of adventure inherent in Americans since their nation's origins. The result of the combinations of all these factors was a large immigration to Alaska. There was some gold in Alaska, but the economic wealth created by the sudden arrival of a large population, in terms of infrastructures, houses, businesses, …, was incomparably greater than that created by gold.

This is a good example of how a half-truth, shrewdly propagated through the media by political and economic forces, can end up to the benefit of society. From this example three natural questions arise:

1) Have similar techniques of mass manipulation been applied to scientific research?

2) Are there instances where such techniques have been used to propagate unwarranted (or even false) claims with scientific pretensions?

3) If the answer to the above questions is yes, can we

claim that these manipulations have globally benefited science? Of course, some sectors were benefitted because they got large funds and high prestige, but this question refers to science as a whole.

In the following section, to explain why the answer to the first two questions above is yes, we will discuss a case study that has all the qualities to become a textbook example: the saga of the quantum computer.

Starting from Section 3.1 we will restrict our attention to mathematics and to those parts of theoretical physics nearest to it (theoretical research). We will argue that those negative features, which in the quantum computer case emerge in extreme form, are present in the sociological structure of contemporary theoretical research albeit in milder, but not less dangerous, form. This fact raises grim concerns about the relationship between society and scientific corporations. Section 4 outlines a possible strategy to contrast these dangerous trends.

2. A case study: the saga of the quantum computer

The revolution brought about by electronic computers in all human activities is there for all to see. Computers were invented by mathematicians in their attempts to automatize elementary arithmetic operations (addition, subtraction, multiplication, division): the name *Computer* keeps track of this history even though the role of today's computers is to process any type of information and calculations are no longer the most important of their performances. The logical structure of the computers used today is still the one proposed by the mathematicians Herman H. Goldstine and John von Neumann in the 1950's (readable and accurate accounts of the history of classical computers are [Goldstine72] and [MetrHowlRota80]).

Now suppose that somebody says that there are theoretical arguments proving that if one could build a computer based on the principles of quantum mechanics, then such an hypothetical machine let us call it *quantum computer*, would be able to do things that not only contemporary computers, but also the computers that one can reasonably expect to build in the next two or three decades would be able to do.

Clearly such a statement is very intriguing: it promises nothing less than a qualitative leap in a technology essential to modern society. This is something everybody dreams.

But dreams are one thing and reality another and common sense, even before scientific method, would react to such a triumphal affirmation with some simple and natural questions, for example:

(i) in which tasks does the quantum computer outperforms the classical ones?

(ii) which are the theoretical arguments that prove this superiority?

(iii) have these theoretical arguments solid scientific bases?

The answer to question (i) is simple: in more than forty years quantum computer enthusiasts have managed to produce only one example of situation in which the quantum computer is claimed to perform better than the classical one. To answer the remaining two questions, recall that, intuitively, an *algorithm* for solving a problem is a list of elementary steps (i.e., that cannot be broken down into further steps) which, if applied iteratively starting from an initial step (called *input*), lead to the solution of the given problem in a finite number of iterations. It is intuitively clear that, an algorithm with fewer steps is better than one that, with the same input and the same purpose, requires a larger number. With this premise, the theoretical argument, mentioned in question (ii) and widely advertised in the apologetic literature on quantum computer, is the following:

> *there exists one (single) mathematical problem that, the quantum computer can solve much faster than any known mathematical method (thus any classical computer, because they can only implement such methods*).

Following this literature, we will call this claim the *quantum supremacy argument.*

For completeness it should be added that there exist a few other quantum algorithms, but they are never mentioned in the media advertisements extolling the qualities of quantum computers because even proponents of quantum supremacy recognize that they have so many weaknesses that they are unsuitable for any serious application. Furthermore, since the main criticism to the quantum supremacy argument (see Section 2.1 below) has been circulating publicly for more than 10 years, in the past few years, many advertisements on quantum computer try to put emphasis on different *possible* applications. However, at the current time the theoretical foundations of these applications are rather obscure, and their practical realizations are either absent or they have nothing to do with computing, for example quantum networks have to do with telecommunications. We will therefore focus our analysis on the only application whose theoretical formulation is clearly and exhaustively described in the literature and is not reduced to purely verbal statements. It should be emphasized that all the hype on which the launch of quantum computing was built was based on this single application.

The mathematical problem mentioned in the quantum supremacy argument is known as the *integer factorization problem*. It is irrelevant, for the non-expert reader, to understand what this problem is about. The only important thing to be understood are the arguments used by the supporters of quantum computer to convince the public, and in particular the research fund providers that this is an important problem for society.

To achieve such an understanding, it is sufficient to know that there is a special family of mathematical algorithms, the *cryptographic algorithms*, which are used to protect our credit cards, our communications with banks, our privacy, our identity, to open our cars, …, in other words they are ubiquitous. Since their role is to protect the digital aspects of our privacy, they must be able to resist hacker attacks aimed at violating it. Algorithms with this property are called *secure*. In the light of these clarifications, the quantum supremacy argument can be rephrased saying that *there exists one (single) cryptographic algorithm (called the RSA algorithm) whose security is based on the fact that the integer factorization problem is a very difficult mathematical problem.*

This algorithm is effectively important for contemporary society because it has been adopted as a standard by the American government and then by many countries. It is used by banks, by credit cards issuers, …. So, quantum computer supporters argue, a nation who could realize quantum computer would be able to decipher many classified data of many other nations, hence acquiring a huge power.

A careful reader will easily understand why, even if the quantum supremacy argument were right (and in a moment we will see it is not), the deduction from it that the quantum computer is important for society would be totally unwarranted from the logical point of view. In fact, as just explained, the social relevance of the integer factorization problem (as opposed to its importance for pure mathematics, which is very high) comes from the fact that the RSA algorithm has been adopted as a standard by several nations. But the adoption of a standard is a technical convention, and such conventions can be changed, in fact this routinely happens in our society. Furthermore, since decades there exist classical cryptographic algorithms which achieve the same purpose of the RSA algorithm, with comparable or higher levels of security and of performances, and which are not based on the integer factorization problem. Therefore, even if the quantum supremacy argument were right (and we reiterate that this is not the case), it would be sufficient to replace as a standard the use of the RSA algorithm by one of the just mentioned algorithms and this would nullify the touted threat posed by the still hypothetical realization (in realistic form) of quantum computers. The paradoxical thing is that this trivial remark was, in a stroke of publicity genius, turned into yet another propaganda item for the quantum computer. In fact, to describe those algorithms whose security is not based on the integer factorization problem, the proponents of the quantum computer invented the label *post-quantum algorithms*. This label was immediately embraced both by the buro-academy and by some big industries who saw in it the possibility to sell old algorithms or variants of them as big innovations, and in fact this is what is happening. When new ideas are lacking, people invent new names (this unfortunately is becoming a current practice of contemporary science independently of quantum computer). The advertising brilliance of the new label lies in the fact that, to talk about post-quantum algorithms as we speak of post-Newtonian physics, reinforces the belief, in the psychology of uninformed people, that the quantum computer has effectively made a breakthrough in science, which is completely false.

An additional remark, not of evidentiary character but nevertheless indicative, is that in the history of science a scientific innovation has never been based on a single result but on the contrary it has always produced, in a short time after the initial steps, a multiplicity of additional discoveries (this happened for example in the first half of the 19-th century with the development of classical computers see for example [Goldstine72J] and [MetrHowlRota80]). In almost 40 years, nothing similar has happened in the case of quantum computer, what we have called the *quantum supremacy argument* is still the *only* argument existing so far in favor of this thesis. This fact should be at least a source of doubts on the enthusiastic claims of the supporters of this line of research.

Many more arguments can be adducted to prove the weakness of the quantum supremacy thesis, but it is better to postpone this discussion in order not to distract the reader s attention from the main argument against this thesis, that we are going to discuss in the following section.

2.1 *The main objection to quantum supremacy*

The conclusion of the quantum supremacy thesis is correct only if the answer to question (iii) above is yes, i.e., if the proof of the superior performances of the quantum computer is correct.

Let us see why this is not the case. It is not necessary to be mathematician or an expert of quantum theory or of information theory to understand why the *proof of the superior performances of the quantum Computer is not correct. Common sense is sufficient.*

The point is that there are two types of algorithms: deterministic, which always end up with a solution of the problem, and stochastic, which give a solution not surely, but only with certain probability. It is intuitively clear that the deterministic algorithms, giving more information, are more complex and require more steps. At the light of this distinction, the only argument adduced in support of the quantum supremacy can be rephrased, more precisely, as follows:

the quantum algorithm can solve the integer factorization problem much faster than any classical deterministic algorithm.

It is not necessary for a reader, willing to understand the situation, to know the details of the quantum algorithm or of the classical ones. To this goal there are only two things one should know, namely:

(I) The quantum factoring algorithm is a stochastic algorithm, not a deterministic one: it doesn't always give the answer, but only with a certain probability.

(II) There exist classical stochastic factoring algorithms.

That being the situation, any reader with a modicum of common sense will wonder:

> *since the quantum factoring algorithm is a stochastic algorithm, why advocates of quantum supremacy compare it with a classical deterministic one and not with the existing classical stochastic ones?*

It's like saying: *my horse runs faster than all the horses I've compared it to*, and then it turns out that only lame horses were used in the comparison. Similarly, proponents of quantum computers do not compare the performances of their algorithm to the classical ones of its own category (which, as we have seen, exist), but fraudulently choose to compare it to others in a notoriously underperforming category.

2.2 *Conclusions from a scientifically correct comparison*

If one takes the trouble to make the scientifically correct comparison (i.e., stochastic with stochastic and not with deterministic) one arrives to the following conclusion:

the quantum factoring algorithm and the classical stochastic factoring algorithm are equivalent.

The term *equivalent* means that in both cases, for the same input, the orders of magnitude of the number of steps, of the probability of error and of the error itself are the same.

The reason of this equivalence is that the quantum factoring algorithm is not a new algorithm, it is only the translation in quantum language of a classical stochastic factoring algorithm, well known to anybody with some experience in classical factorization algorithms decades before that people were talking about quantum computers. There is no new invention, simply a translation from classical to quantum language. The more technically oriented reader will be able to check the validity of the above statements examining the vast literature on classical factorization algorithms (deterministic or stochastic), for example [Bressoud89], [Crandall-Pomerance00], [Riesel94].

Given that, apart from the different language corresponding to different physical realizations, the theoretical steps of the two algorithms are exactly the same, it should not be surprising for anybody that they are equivalent in the sense define above. In other words: it is intuitively clear that the quantum algorithm cannot do better than the classical algorithm of which it is a translation.

2.3 *Additional problems with the quantum algorithm*

In the previous section we have seen that the quantum algorithm is equivalent to a classical one from the theoretical point of view. But there is a big difference due to the fact that the *quantum computer is an analog computer*, i.e., the binary strings on which the classical computer operates are coded in quantum states. These states evolve according to the laws of quantum physics and the process is concluded by a quantum measurement.

It follows that a full equivalence between the two types of computers holds only if one omits from the comparison all the additional complexities coming from the physical realization of the quantum algorithm.

If all these additional complexities are taken into consideration, as required by a serious application of the scientific method, then the conclusion is that:

the overall performance of the quantum factoring algorithm is worse than that of the classical stochastic factoring algorithm.

The proof that it is actually worse depends on technical arguments involving the mathematical description of quantum systems and the quantum theory of measurement. These arguments will not be discussed here (for these technical details see [Accardi10]).

Summarizing:

– The advocates of quantum supremacy have only one argument, of theoretical nature, in support of their thesis.

– This argument is based on a misleading comparison of two algorithms belonging to different classes (stochastic and deterministic).

– The correct comparison, i.e., *comparison of algorithms in the same class*, leads to the conclusion that the quantum algorithm has at most equivalent (but in reality worse) performances than the classical ones, which is exactly the opposite of what the quantum supremacy argument claims.

2.4 *Can we talk about a quantum computer fraud?*

The term *fraud* presupposes someone's will to defraud someone else.

For example, if a person *A* sells a counterfeit coin to another person *B* pretending that it is an ancient and precious coin, we naturally think of a fraud. A person willing to defend *A* might argue that *A* accidentally found the coin and sincerely believed that the coin was ancient and valuable. But if *A* is an antique dealer, then ethical and professional standards would require that, before telling *B* that the coin was ancient and valuable, *A* had done an accurate and exhaustive investigation about the truth of this statement. In absence of such an investigation, the boundary between fraud and self-conviction becomes blurred to say the least.

The situation is similar in the case of the quantum computer with the antiquarian replaced by a scientist (or several groups of scientists), the coin by the quantum algorithm and *B* by the citizens who are paying for research on quantum computers with their tax money.

Out of metaphor, one could ask oneself: *in the case of quantum computer, has contemporary scientific world acted as the honest antiquarian or as the self-convincing one?* In the rest of this section, we will try to answer this question.

As we have seen, the distinction between deterministic and stochastic algorithms was widely known to anyone with the slightest familiarity with algorithms and the classical factoring algorithm was known to number theorists years before its quantum translation.

So how could it happen that for decades *not a single voice from the scientific world* was raised to explain to the public those simple critical considerations that we discussed in the previous section?

This has surely to do with the *Alaska's gold effect* described in section 1.4: people are inclined to believe in what they strongly desire.

Some of the economic interests that come into play in the case of quantum computer are clear. Theoretical physicists are aware of the fact that the role played by their discipline, as leading science of the 20-th century, is going to be taken in the 21-st by biophysics. This creates the need of a new paradigm (again in the sense of Kuhn [Kuhn62]) in which their discipline played a key role, and which could be perceived as a fundamental innovation in a field of crucial importance for contemporary society. The combination of quantum theory with computer science was an ideal candidate.

The mathematicians were happy that the only argument used to support the new paradigm was the translation of a theorem in number theory. Some national scientific schools in mathematics, aware of the fact that, in the deepest conceptual revolution of the second half of the 20-th century in this discipline, their role has not been so dominating as they would have liked, felt the need to participate in the launch of an alternative paradigm in which they could claim a relevant role of those parts of the discipline that are most dear to them, at the same time diverting attention from the innovations coming from environments that they consider extraneous.

For information theory scholars, it was clear that the success of quantum computer would have boosted their role, already important, in contemporary society. So, the attention of the members of many scientific groups had focused more on the benefits that would come to their corporations convincing society that this new paradigm was relevant for it, than on ascertaining if this new paradigm was scientifically warranted or not.

What actually happened is that, in a relatively short time, the numerical majority of the scientific world and the politicians, appropriately guided by an effective mass media campaign, accepted the new paradigm and agreed to invest in it. Once this goal was achieved, it would have been natural, as it actually happened, that those groups that had launched the new paradigm would have had, at the same time, the control of those funds that the political class had been persuaded to invest and the prestige coming from the fact of being the directors of this new and fundamental line of research.

There are well-founded reasons to believe that some Western governments have done much more than economic investments for the affirmation of the new paradigm for example using the power of their own secret services to spread it and contain the *deviationists*. However, for obvious reasons, it is very difficult to find objective facts in support of this statement. Therefore, this topic is best left to future historians of science to whom, perhaps, some documents, rather than just clues might be available.

In any case, in less than a couple of decades after the early 1980s, a common consensus involving important scientific groups and representatives of the political world was essentially achieved. The ground was then ripe for the massive media launch of the new paradigm and the media pressure soon reached also those governments that had been less involved in the construction of it. Since all the important scientific sectors publicly endorsed the validity of the acclaimed discovery, governments all over the world became afraid of arriving unprepared to an important technological development that was deemed to be within reach. Consequently, they decided to invest in the new paradigm quantities of money proportional to their political ambitions. The same motivation, accompanied by the attraction of government funds, pushed industry, from large and world-renowned companies such as IBM to individual investors, to start building such computers.

Concerning the construction of these machines, there is one fact worth noting. The message filtered through the media is that *quantum computers have been built.* But, as usual with quantum computer, information is surrounded by a thick fog. First of all, it is not specified if these are real computers, i.e., multi-purpose machines, or *dedicated machines*, i.e., machines which can perform a *single task* (in the case of quantum computer: integer factorization). It is not easy to know if these machines can perform the minimum that one may require to a computer, that is the elementary arithmetic operations (addition, subtraction, multiplication, division) and, if so, how many of these operations per second are they able to perform. Experts can retrieve information on the size of integers these machines are able to factorize, on the time needed, on the energy required to achieve the goal, …, however, it is a fact that the media skip this information. The legitimate suspicion arises that such information is omitted because, since it concerns the reality of the quantum computer, it could distract people's attention from the dreams, carefully nurtured, about it.

A table with the performances of existing quantum computers and of their improvements during the years would be easily understandable by a large audience, but this would allow the public to realize how small and insignificant these results and progresses have been. For the same reason, the public will not find on the media the comparison between the historical development of quantum computer and that of the classical one. In fact, in a few years the classical computer evolved from mechanical to analog and from analog to digital (see [Goldstine72], [MetrHowlRota80]) and in the same period its performance grew enormously. On the contrary, in decades, the developments of the performances of the quantum computer, which is analog by definition, when compared to those of the classical computer in intervals of time of the same length, appear to be irrelevant.

It is worth emphasizing that the early classical computers were analog machines, and that this technology was replaced, already in the 1940s, by the digital one. So, quantum computer is intrinsically based on an old, obsolete technology, abandoned by engineers several decades ago because inefficient, expensive and requiring bulky machines (just like the quantum computers so far).

In old science the acceptation of a new paradigm was subordinated to a thorough critical analysis where arguments in favor and against the new idea were carefully confronted. Exactly the opposite took place with quantum computer where the acceptation of the new scientific paradigm took place before any critical analysis of its theoretical or experimental solidity.

To summarize, let us return to the comparison with the antiquarian of dubious morality and the coin of dubious antiquity. In this comparison the role of the coin is played by quantum computer and that of the antiquarian by the scientific community that accepted to believe in quantum computer instead of following the dictates of the scientific method which requires thorough due diligence before accepting a conviction. The economic convenience, in the case of the antiquarian, is accompanied, in the case of the quantum computer, by additional factors such as prestige, self-satisfaction deriving from the feeling of participating in a scientific revolution, ….

For the reasons explained in Section 2, a due diligence by the scientific community, i.e., a critical analysis required by the standard canons of scientific practice, would have totally debunked these feeling and highly reduced the expectations of other advantages.

But no such critical analysis was done by the scientific community and the reasons why this happened, which are deeply related to the 5 new features of contemporary science formulated in Section 1.3, have been shortly outlined in this section.

Sooner or later the scientific community will have to cope with the ethical and professional implications of the absence of this due diligence in the case of quantum computer, but this will be rather later than sooner for the reasons discussed in the rest of this article.

In conclusion, the quantum computer is a perfect demonstration of Voltaire s statement:

Fool mortal being! How well you do apprehend
to repeat what you do not comprehend! [Voltaire]

3. Sociological aspect of the second crisis: a necessary premise

What follows is a critical analysis aimed at highlighting some aspects of the contemporary scientific research which should be of serious concern to our society. It is important to underline that the criticism concerns the system, not individuals. On the other hand, it is known that a good individual inserted in a bad system can make actions that are objectively negative from an ethical point of view or can simply be silent in the face of such actions done by others. This phenomenon has been studied in connection with many personalities who had joined, in good faith and convinced of being right, to the Fascist or Nazi regime or with the self-justifications of their actions by many criminals. There are institutions born from noble ideals and with a high tradition of realizing these, which also have been or are subject to periods of decline in which such ideals, albeit verbally acclaimed, they are effectively denied. For example, the Christian Church has experienced such a period of decline that lasted several centuries.

The thesis of this second part of the article is that:

1) Contemporary theoretical research is experiencing a similar moment of decline that is destroying the great tradition of this part of science (and in fact the similarities with the experience of the Christian Church are more than one could think).

2) This tradition needs to be re-established and this can only be done starting from the bottom up, that is, by spreading awareness of these problems and relying on the fact that such awareness stimulates the ethical sense of the majority of individual researchers, that is high, and pushes them to overcome that network of small interests that the decadence of the system has grafted and sustains.

3.1 *Sociological trends in contemporary theoretical research*

In this section we show that the scheme that brought quantum computer to sociological (as opposed to scientific) success, is not an exception, but rather a trend that is pervading more and more sectors of contemporary theoretical science.

We have seen, in Section 2, how the scientific community has been able to convince nearly everyone in the world to believe in a false scientific statement (namely that, at least in theory, a quantum computer outperforms a classical one in the solution of the integer factorization problem). At the moment such an extreme situation in theoretical science is not known (even if bad and successful examples propagate with high velocity, so one cannot exclude that similar situations will happen in the future) but the methods of leveraging people's psychology to spread an opinion defending the interests of very particular groups are the same.

We will distinguish between *pure research and theoretical research*. In fact, also in experimental scientific activities, pure research plays a fundamental role, but in this case, the existence of a direct confrontation with experiments or with concrete applications provides reasonably objective criteria to evaluate the relevance of a given scientific result thereby severely limiting the possibility of the buro-academy to create undeserved reputations and to propagate alleged scientific innovations.

On the contrary, in contemporary pure mathematics and in some parts of theoretical physics, the absence of such a direct confrontation with experiments or with applications has had two effects:

(i) a growing self-referentiality in each micro-sector of these disciplines.

(ii) A shift of the academic competition from the truth of scientific statements to their *importance*.

For example, in mathematics self-referentiality, combined with massification and industrialization, induced a slow decay of the average level of the professional profile of a mathematician.

Until the early decades of 1900 it was common among mathematicians, to be able to give relevant contributions not only to different sectors of mathematics, but also to other disciplines, mainly physics and engineering, but since ancient times we know bright examples of mathematicians giving substantial contributions to astronomy, agronomy, computer science, technology, economy, biology and even social sciences (in 1700). This tradition stems from a time when the differentiation among different scientific disciplines was far from being realized: in the Middle Ages the term mathematician was used to designate a person expert in mathematics, physics, astronomy and even astrology.

The evolution of mathematics in the last century can be better understood bearing in mind the parable of the Kantian dove which, since as she flew higher, she felt less resistance from the air, she was led to believe that in a vacuum she would be free from all resistance. Similarly, Mathematicians felt freer when pursuing tasks purely inside mathematics and, over time, they gradually cut all ties with external stimuli thus completely overturning their glorious old tradition. In old times, the more universal a mathematician was, the more he was appreciated by his community; now the opposite takes place: a person who has made contributions to various branches of science is considered a traitor by his official group and an invader by others. Mathematicians are trained to prove theorems born inside their sociological groups whose contents is, in most cases, incomprehensible even for mathematicians working in different fields, not to mention scientists in other disciplines. A handful of them, typically after having received high recognitions from their specialistic group, try to connect themselves with the old tradition producing papers in different disciplines, but too late. They did not train themselves to conceptually interact with other disciplines, usually they are masters in application of some techniques, and they produce more or less elegant applications of these techniques. But the main value of the old tradition lies precisely in the fact that the solution of problems outside mathematics often led to the discovery of new fundamental mathematical structures or techniques and, to obtain such results, it is necessary to educate one's scientific taste, from the very first steps of one's scientific education, according to the old tradition (see further discussion of the consequence of abandoning this tradition in Section 3.4).

The problem with effect (ii) mentioned above is that the notion of importance has a highly subjective contents and this opens the way to manipulations by scientific corporations. Let us see how.

In Section 1.3, we have seen that, as a consequence of specialization, massification and industrialization, theoretical science has become an archipelago of communities, of different sizes depending on the discipline, which speak different languages, have difficulties in mutual communications and the members of one community have scarce knowledge or interest on what those of another community do. So, there is a growing isolation among corporations cultivating different theoretical sectors. Since a scientist's career, in 99% of cases, begins, and ends within one of these corporations, this creates an ever-increasing dependence of the individual on the group and a dangerous psychological shift in the minds of researchers: from the first days of their introduction into the scientific world their attention is focused on the problem of joining the right group that will guarantee them a career. How fast and how brilliant this career will be, depends on personal skills and commitment, because competition inside single groups can be high, but absorption of the group values and adherence to the group paradigm are necessary conditions. A first consequence of this is that the average theoretical scientist loses the taste to understand and appreciate results outside the paradigm within which he has chosen to work. A second consequence is what one might call *the effect of cancer cells*: cells of the human body tend to reproduce themselves, but in a healthy body there is a mechanism of global equilibrium that prevents arbitrary endless reproduction of a single type of cells. On the contrary, cancer cells lose touch with the overall needs of the body and continue their reproduction regardless of these needs thus causing eventual decay of the body itself.

Academic corporations behave like cancer cells: they become self-referential, they lose sight of the global needs of theoretical science and concentrate their efforts in increasing their numerical size and influence in different sectors of society, which means jobs and research funds.

Innovative new ideas, which would require the emergence of new aggregation groups, are seen as enemies whose growth must be prevented with all means, in particular cutting funds and access to jobs for younger theoretical researchers. But in science, as in biology, mutations are essential for evolution. Some more illuminated governments are aware of this danger, but they can do very little because in theoretical science the main tool to evaluate research activity is the so-called *peer review system*. This worked generally well in old science, but the industrialization of science has made this system, in its present scheme, obsolete because it is well known that those who have the power to choose the peer reviewer can, by appropriate choice, decide the fate of an application for funds, of an article submitted for publication, of a competition for a position in a university or in a research institution.

It is not surprising that after a few decades of this practice, *the temptation to use this power to define rather than understanding* what is important and what is not begins to develop within scientific corporations. The quantum computer saga is an extreme example of this, but we should not forget that it grew up from the above described general and worrying trend of contemporary theoretical science.

Let's consider an example, unrealistic if taken literally (because governments are well aware of the danger of potential conflicts of interest), but which unfortunately captures a very realistic aspect of what the struggle for influence in theoretical research has become.

Imagine that the Japanese government launches a competition to finance the creation of a new model of electric car. The stake is very high and all Japanese car makers participate in the race. Now imagine that the Japanese government selects representatives of Renault as peer reviewers. It is known that, between Renault and Nissan, there is a more than ten-year alliance, so for the representatives of Renault to address the financial support towards Nissan is a way to strengthen their group. So, it is probable that, with these premises, the decision will be in favor of Nissan.

Changing names and situations, the above unrealistic example perfectly fits the situation of the competitions for the distribution of funds for theoretical research in the European Community in the past twenty years. When these competitions begun, in the early 1980s, the situation was completely different: the European Commission was supporting networks among European universities. This means that aggregations were born from below, corresponding to real scientific collaborations among different research groups and that a relatively small amount of money was creating a huge flow of exchanges of European researchers among a large number of European universities. The large number of people involved, and the great enthusiasm generated by the fact that these spontaneous aggregations reflected real common scientific interests, was beginning to create a new generation of researchers who were taking Europe rather than their own countries as landmark.

But precisely these characteristics, which brought the realization of the genuine European ideal closer, aroused great concern in the buro-academy, accustomed to the old national academic balances and which saw the birth of new genuinely European aggregations as a threat. In particular, the fact that the funds for each European network were not so large compared with those available, implied that a large number of evaluating commissions was needed, hence an even larger number of members of these commissions: even for the power of the buro-academy it was impossible to control them all.

With the creeping and climbing techniques proper to it, in less than 20 years the buro-academy managed to conquer two important victories on the European Commission convincing it that:

1) The distribution of funds to a multiplicity of networks was a useless dispersion to be suppressed.

2) It was much better to concentrate on a few *excellent* theoretical researchers a sum of money of the order of 10 times what was early given to a whole network of universities and correspondingly reduce an order of 10 times the number of available grants.

The combination of buro-academic pressures with the national interests of a few nations with a tradition of preponderance in theoretical research managed to push the European Commission to take a decision that killed in the bud the birth of a new European generation of theoretical researchers educated to think in European terms, rather than in terms of their single nations. The *European dream* that had led to spontaneous aggregations of multinational groups of researchers has been replaced by the usual narrow-minded academic games.

Since the control of the evaluating commissions of a few grants is much easier that the control of many commissions, the buro-academy had an easy time seizing control of these few commissions. From that moment the ethical level of the whole system of grants distribution has plummeted. Tedious and redundant statements on ethical issues are currently being used as a fog to cover up the material violation of these issues in various circumstances. One concrete example of these violations is the following. In some of the above-mentioned grants a list of *objective criteria* was published inducing applicants to believe that these criteria would have been followed by the evaluators. The minimal ethical requirement, when you propose a list of objective criteria in a competition, would be to bind the evaluators to strictly link their reports to these criteria, for example by scoring each candidate for each of these criteria and then selecting those with the highest scores. In this way the evaluators, willing to exclude some candidate with a high score in the objective criteria including others with minor scores, would have been obliged to explain in great detail the motivations of their choice. But the European Commission did not follow this minimal ethical requirement *because the evaluation reports were not obliged to explicitly link these criteria with their decisions*. In this way, it has emptied its list of objective criteria of all content, deceiving those candidates who naively felt guaranteed by it. To understand if some evaluating commission has effectively abused of this right, it would take a historian of mathematics brave enough to verify if there were any cases in which researchers who met all criteria were rejected and others, who did not meet a single one of them, were admitted.

So, the winners of these grants are divided into two categories: a restricted one including the lines of research the buro-academy wants to advertise and another including those lines of research considered harmlessness in the sense that the recipients should not constitute a threat for the established academic equilibria.

Through the control of the distribution of European funds the buro-academy not only acquired the power to define what is important and what not in European theoretical research, but also that of gradually destroying those research lines considered harmful for the equilibria they want to defend because the innovative contents of these researches is so high that they have the potentiality to alter these equilibria creating new scientific aggregations.

Ethical violations like the above mentioned one are neither exceptional nor restricted to the European Commission: situations like that of the Renault-Nissan example have become a standard in all kinds of evaluation of theoretical research and strong groups balance their interests by careful distribution of recognitions to several micro-groups, satisfying the harmlessness condition, in order to minimize discontent and counting on the old saying of the Mafia: *whoever took gets caught*.

The replacement of the qualities of wisdom, moral integrity and feeling of responsibility towards society, required by any serious evaluation, has been replaced by feeling of responsibility towards narrow group interests who, depending on the circumstances, fight among themselves or join forces, but are unanimous in their determination to prevent any evolution in the existing academic balance.

This upheaval of the ethics of scientific evaluation greatly harms society because nowadays the peer review system is extended, from the control of the access to scientific journals to a much wider horizon, involving the evaluation of universities or of single departments, the career of individuals, the distribution of prizes, jobs, research funds, …. Not many years ago scientific recognitions were proportional to objective scientific discoveries, today it is proportional to the degree of acceptance by a scientific community and strong groups grant such acceptance only to their allies or those considered harmless.

For the evaluation of scientific research, there is no alternative to the peer review system, yet society should be aware of the degeneration that has taken place in this system and think seriously about the search for corrective mechanisms.

3.2 *Creators, elaborators, and sellers*

In this section we discuss how the power to decide from above and to impose lines of theoretical research, coupled with that of stifling unwanted innovative theoretical ideas, manages to influence the contemporary way of doing theoretical scientific research.

The massification of science creates the need to give sociological recognition to an increasing number of people. Typically, these recognitions take the form of prizes, funding, jobs, … *As we have seen, the industrialization of science has moved the competition from scientific power to make an important theoretical discovery to academic power to define which theoretical discoveries to publicize with the tools listed above.* In theoretical research this shift has been facilitated by the fact that there are no patents or copyrights for theoretical discoveries.

A first consequence of this shift has been that plagiarism has become common practice in theoretical research: when a new useful idea or technique comes out from an individual or a group not affiliated to a strong corporation, it is quite common that different strong groups appropriate it ignoring the original discoverer. This creates pathological situations such as that the same mathematical object is used with different names from various poorly communicating sociological groups. In fact, change the name of new theoretical ideas is one of the first signs of intellectual misappropriation.

Another consequence, in opposite direction, is that strong groups routinely exploit the acknowledged delay between theoretical discovery and emergence of the first applications, obscuring the absence of scientific results by magnifying the potential for future discoveries or applications: as quantum computer teaches, selling dreams is much easier and more convenient than producing scientific results.

3.3 *Elaborators*

The solution of specific problems has always been and will be a cornerstone of education in any theoretical science. The best students are considered those who manage to solve more difficult problems or given the same difficulty, more quickly. In the past 50 years, the situation has evolved from students to researchers. In each branch of any theoretical research sector there are problems that are considered interesting by different groups of people. 99% of the contemporary research activity is devoted to these types of problems. Typical examples of problems of this kind, in mathematics, are *generalizations and classifications*. Often, a great deal of technical ability and of ingenuity is required to solve some of these problems in specific contexts. In them the pattern to follow is in some sense preassigned in the sense that the problem is clearly posed, and one knows a priori the techniques needed to solve it, the main issue is to skillfully apply these techniques or produce variants that lead to the solution. For these reasons, in the following, those whose research activity is exclusively dedicated to these problems will be called elaborators. This kind of research activity is essential for the development of theoretical science and any scientist devotes a significant fraction of one's time to it. It is therefore quite natural that, in the majority of researchers, this fraction becomes in fact the totality. What concretely happens is that single groups of scientists identify a sector deemed of interest and concentrate their activity on it. The relevance of the results obtained is decided within the group itself.

This kind of research activity perfectly fits with all the 5 features mentioned in Section 1.3 as characteristic of contemporary science.

3.4 *Sellers*

The category of *sellers* is typical of contemporary science. Their activity is mainly focused on academic politics: entering influential commissions, preventing undesired groups to do the same, magnifying the results of their or allied groups and discrediting much more innovative, but potentially dangerous results. One could say that this is *academics as usual*, but what is new is, from one side the dimension of the phenomenon and on the other side the intrinsic connections between elaborators and sellers and the common diffidence against creators. Within the *scientific-industrial* complex, the sellers' role is marketing and political connections with outside groups. Obviously, we are not speaking of official, codified, roles but the division of labor even if not codified explicitly is clearly perceived by all actors. This category has a different degree of development in different disciplines. Physics is surely the most advanced discipline in this direction, its industrialization begun with the large particle physics laboratories (which gave origin to the term *big science*) and continued to propagate till to reach the theoretical levels from which the saga of quantum computer was born.

In mathematics this trend is much less developed although with some exceptions like the fashion, exploded a few decades ago around catastrophe theory and now forgotten: its promises of applications to every sort of issues, including social ones, and the heavy emphasis the media had on it (at that time the internet was not so developed) present some similarities with quantum computer, even if on much smaller scale. This is related to the self-referentiality issue, discussed in Section 3.1 and the consequent cut of all bonds of pure mathematics with problems outside itself which has greatly impoverished ideally and culturally this discipline. It has been created an artificial distinction between pure and applied mathematics which belies an age-old tradition. Pure mathematics has expelled from its culture the interest in problems outside itself and relegated them to the sector of applied mathematics: two islands with scant scientific interactions. A young person wanting to do research in mathematics is obliged to make a drastic choice between applications and so-called pure mathematics. The applied mathematician is trained to apply sectorial techniques corresponding to the different sectors of mathematics. But the most interesting problems for applications rarely require a single type of technique for their solutions. It is rather required to acquire the ability to understand which techniques are needed for a given problem, to quickly absorb those parts which are needed and to coordinate them, refraining from the impossible ambition to become, in a short time, an expert in different sectors of science. This goal can be achieved, but it requires decades of intensive work and constant application of the methodology described above.

3.5 *Creators*

The three classes of researchers described above are not disjoint, on the contrary every researcher has experience of all three activities, albeit to different degrees: the distinction captures the dominant aspect, which is never exclusive. On the other end, important discoveries usually come from single individuals or small groups and the history of science teaches us that the greatest theoretical revolutions have practically never come from the solution of clearly posed problems. Research work can be compared to extraction of diamonds from a mine: you know that the probability to find a diamond is high in that place, but if you do not develop techniques to guess the places with highest probability of success, the tenacity to persist, the capacity of working hard, it is unlikely that you will find a diamond.

If however your plan is not to extract diamonds from a known mine, but to discover a new one, your stake his higher and proportionally your risks are; you need all the qualities listed above but they are not sufficient. Likewise in creative research the problem is almost never well posed, people are guided by intuition to look in a certain direction, you have to master many techniques because you don't know a priori if the right one there will be only one or if the discovery will come from the fusion of techniques and notions from different fields through unusual combinations.

One can say that some of the deepest theoretical discoveries come from a *change in the way of looking at a known topic with a* process that can be compared to the switch in our mind that takes place looking at the images produced by the *gestalt psychology* among which the most famous one is probably the one in which the same image can be interpreted either as the profile of a vase or as the profile of two faces looking at each other (an interesting collection of such images can be found in the book [Falletta90]). The discovery of special relativity followed this pattern. All the most important formulas of this theory were already present in the literature in the framework of classical electrodynamics. Einstein changed this point of view and showed how the interpretation of the same formulas can be extended to the domain of classical mechanics. This intuition proved to be extremely fruitful, leading to the discovery of atomic and nuclear energy power. The discovery of quantum mechanics followed a different pattern: the attempts to construct a mathematical model of the atom fitting the available experimental data lead to the emergence of a completely new mathematical formalism. Contrarily to the case of classical mechanics, this new formalism *was far from the intuition* (as Heisenberg once said [Heisenberg58]) and its origins were quite obscure. The new formalism was used for almost one century because it worked so well, and practically all modern electronic technology is based on its use. However, the mystery surrounding its origins persisted for all this time and only recently it has been understood that it is a special manifestation of a deeper level of classical probability.

In mathematics one meets a similar situation with the birth of non-Euclidean geometries. Starting from 1500, the geographical discoveries stimulated the need of more and more precise maps and the construction of the first globes.

This led to the discovery of many mathematical results concerning the geometry of the sphere. A geographical map is the projection of the earth or of a part of it (i.e., a 3-dimensional object) into a 2-dimensional object like a sheet of paper. This, and the perspective studies of early Renaissance painters, stimulated the development of projective geometry, which achieved high levels in 1600. Most of these results can be interpreted, with hindsight, as results in non-Euclidean geometry (that of the sphere or the projective space), but historically they were still interpreted within the conceptual frame of traditional Euclidean geometry. It still took 2 centuries before Gauss had the intuition of the possibility that the laws of space can have different mathematical models (geometries). Less than a century after him, Einstein completed the picture with the intuition that the presence of large masses creates a curvature in space, thus giving a physical meaning to non-Euclidean geometries. In the 19-th century a similar change of point of view has occurred, with quantum probability, for the laws of chance.

For these kinds of discoveries technical skill, although necessary, is not sufficient and there is no rule that drives to them. Because of this, they fit badly with most of the 5 features mentioned in Section 1.3 as characteristic of contemporary science: with massification, because they necessarily involve a tiny minority of scientists, with industrialization, because they cannot be codified in a set of transmissible rules, with politicization because the people involved in them have little time left devote to academic policy issues. Globalization is the only one of these features fully compatible with them, and in fact beneficial to them since these kinds of discoveries are the culminating moment of several ideas, problems and contributions coming from scientists acting in different parts of the world and, more and more frequently, they come from collaborations between scientists with complementary skills and intuitions.

Another reason why creative research activity is cultivated by less and less people is because in it the discovery of the right formulation of the problem is part of the problem itself. Therefore, younger people feel insecure with it and prefer to attack those problems that are clearly formulated, while older scientists have formed their intuition and their taste in other directions, so they have a deep and sincere appreciation for any step forward in one of these directions, while they feel suspicious with respect to any discovery involving ideas, techniques, or notions with which they are not familiar.

4. *Conclusion: A new interaction between science and humanities*

We have seen, in a multiplicity of examples, that, in contemporary theoretical science, the power to impose one's own narrative of the facts is routinely used by the strong groups and in the long run it has given rise to the degenerations described above, like the quantum computer saga described in Section 2, the cancer cells effect described in Section 3.1, the group-centered world vision of many scientists described in Section 3.3, ….

These mechanisms are not special to science: they emerged long before in various social structures like politics, the military or religious structure, industry. However contemporary society, at least in non-autocratic regimes, has developed methods to prevent degenerations of these mechanisms. These methods never work completely, and they don't work automatically, in the sense that they must be continuously adapted to a continuously evolving society, but at least they manage to keep alive the dream of an *open society*. We hope that similar mechanisms will also develop in theoretical science. With respect to these mechanisms, theoretical science is a newcomer, and it is for this reason that, for it, social mechanisms of prevention of degenerations are rare and concern small groups or individuals.

In the development of these prevention mechanisms, the role of humanists such as historians of science, sociologists, journalists, philosophers of science, …, who are interested not only to the facts, but also to those sociological and political mechanisms that increasingly accompany the development of scientific disciplines, could be relevant.

In the preceding pages, the testimony of future historians of science has often been invoked, hoping that, by reestablishing certain historical truths, they can limit the manipulations of the buro-academy.

At the moment there exist many historians of science, but the problem is that in most cases they trained as researchers inside a specific discipline and later their interests moved in the direction of the history of that discipline, thus in them the *group loyalty* imprint discussed in Section 3.3 is present. This cultural imprint is responsible of two characteristics, present in most contemporary studies on the subject: high specialization and the fact that many of these studies can be better classified into the category of *hagiography* rather than in that of history of science. To the second category belongs a large part of the articles appearing in popular science magazines (here again the quantum computer is a textbook example).

This statement would require a deeper analysis with many concrete examples, but the restrictions on the dimensions of this article prevent this.

A common feature of all these studies is the absence of any attempt towards *conceptualization*, i.e., the effort to abstract from the formal language of mathematics or the technical jargon of a scientific sector the leading new ideas that distinguish technical achievements from deep innovations.

The humanities could play an important role in overcoming these limitations. Within these disciplines a new generation of scholars should be educated, free from the aforementioned cultural imprints and capable of interacting with the scientific world and to describe to a wider public the new dynamics of this world which developed after the second world war and that we tried to describe in this paper. A first benefit of this interaction could be that the conceptualization efforts, essential for a deeper level of communications between science and society and nowadays ostracized and restricted to an extreme minority of scientists, becomes widespread and accepted. A second, equally important, benefit could come from the inclusion of people coming from humanities into those organizations, local or global, that produce the criteria used to evaluate single scientists, scientific journals or institutions such as research centers, universities, …. The presence of these *neutral observers*, free from loyalty bonds to the strong corporations, would limit their ability to manipulate these criteria in their favor. Such a program will be hard to realize, in a world where the isolation of different cultural sectors as far as decision mechanisms are concerned has become a reality, but governments, as well as supranational institutions, should understand that remaining subservient of the buro-academy damages the development of an essential sector for the society, such as that of scientific research.

Exactly in the directions discussed in this article go the ideas exposed in the paper [Weisberg-Muldoon09] which demonstrates the advantage, for science as a whole, of including in the funding and distribution of jobs also those groups of researchers that the authors define as *mavericks* (and that we called *creators*). This is exactly the opposite of what the Italian government does, requiring, for a group's access to public research funds, a numerical dimension which is far from what an avant-garde group can achieve. The conclusions of the article just cited are confirmed by the work [Shahar14].

Governments and supra-national institutions should rationally address the problem of criteria for allocating public research funds (which include jobs) taking as a basis the literature on the subject to which the two papers cited belong.

These criteria should first of all be public and based on objective criteria, such as those mentioned in Section 3.1, accompanied by the obligation for evaluators to assign scores to these criteria and to explain in detail the reasons for possible discrepancies between scores and decisions made. Furthermore, they should be dynamic, in the sense that they should be accompanied by accurate *ex post* evaluations of both the work of the assessment commissions themselves and of the results achieved by the funded groups, comparing them to those achieved by some of the unfunded. Both types of assessment are currently absent from the evaluation procedures of both the Italian government and the European institutions.

Reason for optimism is the belief that there will always exist a tiny minority of scientists who believe that the importance of scientific discoveries cannot be decided on the basis of numerical majorities or of academic power and consequently devote their efforts to educating one's scientific taste to recognize and appreciate profound scientific discoveries regardless of the group or individual who realized them.

Historical experience shows that true science survived extremely disadvantaged situations like barbaric invasions, religious oppressions, autocratic regimes like Nazism or Stalinism, … There is therefore a well-founded hope that it will also survive the buro-academy. This struggle however will be more difficult because, for the first time in history, the threat comes not from outside the scientific world, but from the inside.

LUIGI ACCARDI
(accardi@volterra.uniroma2.it)

References

[Accardi10] Accardi Luigi:
Complexity considerations on quantum computation,
in: Quantum Bio-Informatics V, Eds. L. Accardi, W. Freudenberg, M. Ohya: World Scientific (2013) 1-13 Invited talk to the: Fifth International Conference on Quantum Bio-Informatics, Tokyo University of Scien-

ce, 7-12 March 2011,
Previously published electronically in: Proceedings of the International Symposium on Applied Sciences in Biomedical and Communication Technologies (ISABEL), Rome, 9-11-2010 electronic proceedings: www.isabel-conference.com

[Bressoud89] David M. Bressoud,
Factorization and Primality Testing, Springer (1989)
[Crandall-Pomerance00] Richard Crandall, Carl Pomerance,
Prime Numbers, A Computational Perspective, Springer (2000)
[Falletta90] Falletta, Nichilas The Paradixicon, Wiley & Sons (1990)
[Goldstine72] Goldstine Herman H.
The computer, from Pascal to von Neumann, Princeton University Press (1972)
[Heisenberg58] Heisenberg W.:
Physics and philosophy; the revolution in modern science, New York, Harper (1958)
[Kline80] Kline Morris:
Mathematics: The loss of certainty, Oxford University Press (1980)
[Kuhn62] Kuhn, Thomas S.: The structure of scientific revolutions., University of Chicago Press (1962) (1st ed.)
[MetrHowlRota80] Metropolis M., Howlett J., Rota GC (eds.):
A history of Computing in the Twentieth Century,
Academic Press (1980)
[Riesel94] Hans Riesel,
Prime Numbers and Computer Methods for Factorization,
Second Edition, Birkhauser (1994) (p.156)
[Shahar14] Shahar Avin:
Breaking the Grant Cycle: On the Rational Allocation of Public Resources to Scientific Research Projects Dissertation is submitted to the University of Cambridge, for the degree of Doctor of Philosophy August 2014, available on the Web
[Voltaire] F.M. Arouet De Voltaire:
La Pulcella d Orleans, Tradotta da Vincenzo Monti, a cura di GiulioNatali,con disegni di Giuseppe Mazzoni. Classici del ridere N. 22 A.F. Formiggini Editore in Genova (1914) Canto 15-th, strophe XXV (p. 206):
[Weisberg-Muldoon09] Weisberg, Michael, Muldoon, Ryan:
Epistemic Landscapes and the Division of Cognitive Labor, Departmental Papers(Philosophy)(2009)7. http://repository.upenn.edu/philosophy_papers/7