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Science and its Constraints (an unfinished story)

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Abstract. It is noticed that in the present day societies the progress of science is too dependent on the mass sociology. Some steps to moderate the phenomenon are briefly discussed.

The science in our world depended always on its economic, political or even religious trends. Enough to mention the Pythagoras theorem, which became the state secret in ancient mini totalitarian regimes. Then the polemics between the geocentric (Ptolemean) and heliocentric doctrines [1]. Also the cosmological ideas about the origin of our universe. One would like to think that the modern societies, at least in the democratic countries, created finally the conditions in which the science can develop without artificial barriers, but it is enough to contemplate our XX and XXI centuries to see that this opinion might be too optimistic.

As it seems, the relatively natural development of science occurred in the first part of XX c. thanks to spontaneous exchange of opinions in the historical congresses (Solvay and others). Their polemics, not limited by an undue 'correctness', were crucial for the advance of some (right or wrong) ideas.

So today, one can enjoy Einstein discovery of special relativity. Then his hypothesis about the light quanta, together with the persistent Millican attempts to defeat the quantum model, an effort which turned quite fertile – though not to reject but rather to understand the idea better [2].

We can also follow the early arguments of Pauli about the nonsense of the electron spin. Yet, after changing his opinion he proposed to describe it by 2×2 matrices, today called the *Pauli spin*.

In the next decades, the most fruitful relations (though not quite innocent!) between the scientists and politicians were typically due to the personal contacts between the scientists and government representatives, creating the support for military and technical projects.

For some time the publications of papers seemed to obey certain natural rules of *submissions, referee opinions and public discussions* described, e.g., in [3] Yet,

the authors paint an almost ideal equilibrium between the individual effort and the public response. In reality, this equilibrium, since several decades, is in danger.

The Profession Expands

Already in 60s the number of scientists was relatively large and soon, the personal contacts became difficult, if not impossible. A class of intermediaries thus appeared and turned out to be increasingly powerful. The administrators, bureaucrats, etc. had to classify the scientists and their work without reading papers. The same about scientific projects. Part of the information could be deduced from some accessible numerical data, such as the number of publications, citations, graduations etc. The rest was left to various levels of referees, chosen by experience or just intuition of the Editors (and/or the Institute directors). Their opinions were quite important but not always correct. Still, the science was progressing reasonably well.

"Publish or Perish"

I did not succeed to check precisely at which moment appeared the famous slogan: "publish or perish". If you search in Google, you can see that this simplified rule is still quite alive. Some recent observations are made by David Colquhoun, neuroscientist from London, who discusses the problem in his (2014) article: "Publish-or-perish: Peer review and the corruption of science" [4]. The scientists, afraid of their reputation (or survival!), started to hurry up and the number of papers was increasing fast, at cost of their quality. The devastating stress to keep publishing affected many areas of science producing almost an ecological disaster.

The Referees Missing!

As an inevitable consequence, there was soon not enough specialists to evaluate the "productive avalanche". The authors of the medium quality papers had therefore to evaluate the other medium quality papers. Even this could not help. A lot of contributions which could be of interest if elaborated patiently, were not indeed completed, aborted rather than published. The masses of "fast papers" could not be consumed by "fast referees".

An exception among the "publication champions" was Peter Higgs, the Edinburgh professor (Nobel 2014). According to Dekka Aitkenhead report in The Guardian, (Dec. 6, 2013), Higgs confesses that before getting his Nobel Prize, he was an embarrassment to his department in all moments of the research reports. When asked about the list of his recent publications, he could only say: "None". He noticed: "Today, I wouldn't get an academic job. It's as simple as that". He also doubts that his breakthrough could be achieved in today's scientific culture, because of obligations to collaborate and keep churring out papers. "It is difficult to imagine how I would ever have enough of peace and quiet (. . .) to do what I did in 1964". Skeptical and unbeliever, he regrets that the particle he identified in 1964 is known as "God particle". In 1999 he turned down his knighthood, considering that too many honors are used for political purposes. . .

Trends and Mainstreams

Of course, the case of Higgs is an exception. What the organism of science must

assure is to create the majority of 'regular workers', i.e., the scientists whose destiny is not necessarily the Nobel prize, but who work in equilibrium between the formal obligations and free moments for some independent ideas. Unfortunately, this equilibrium is also in danger.

The reaction of the science organism were "trends", and "mainstreams". Associated with high reputation journals they create the international power groups. By a natural mechanism of sociology, the authors 'navigating' in the mainstreams discuss mostly with themselves, cite mostly works of their groups. The referees privilege the papers of their "friends in trends", sometimes converting themselves into the subconscious trend guardians, rejecting the outsiders or critical articles. This did not moderate, but focused only the publication efforts, an effect clearly observed in theoretical or mathematical physics, existing also in natural sciences.

As remembered by one colleague, in certain epoch it was rather difficult to publish a work in elementary particle physics without mentioning the Regge poles.

Later on, the similar "magic subject" became *strings*. In several scientific centers it became almost an obligation to cultivate the subject (even with a damage for the rest) "because it is the only game in town". After his 20 years of work on strings the known physicist Lee Smolin published the book "Trouble with physics" [5]. But the trouble persists. To mention that some of your results confirm (or seem to confirm) some string predictions immediately assures the positive interest of a large number of referees.

The price of trends is high; it consists in producing the large numbers of secondary papers which not even the most dedicated archaeologist of XXII century will have patience to read! To dissent or to criticize the mainstream could be rather difficult.

The situation reminds inevitably an aphorism of Jean de La Fontaine from XVII c: "All minds of the world are impotent against any stupidity which became fashionable" [6]. Indeed. Even if not stupidity, the minds are still helpless...

The trends and mainstreams, have some true achievements by focusing the efforts and accelerating the progress in some particular areas. This may be justified if contributes to the training (and graduations) of the young students, but even so, some mass phenomena, like the huge numbers of authors of one publication, can make quite difficult to appreciate the individual merits.

The Editorial Empires

Unable to protect the market from the avalanche of publications the Editors of prestigious journals, quite frequently, take the problem into their hands: they don't even worry to send a paper to a referee, but just decide themselves. I happen to know about a paper submitted to the Physical Review Letters to which the Editor answered: "Sorry, but this is not interesting to our community." Given the situation, this is not an offence, though on margin of this humorous incident the question arises, what is exactly "our community"?

The illustrations are many. On one occasion one of our colleagues submitted to the Physical Review A an article about the operational techniques of controlling

the charged particle by the $\delta(t)$ potential pulses in an ion trap. The referee decided that even if the mathematical result were true, the $\delta(t)$ pulses cannot be experimentally achieved, so the subject is outside the areas of the Physical Review A, which traditionally takes care to be close to the experiment. Yet, at this moment, the Physical Review Letters receives gladly the articles on "*bam-bam* control" of quantum evolution, the *bam's* meaning the δ-pulses, though now they are called "separators" or "interruptors", published, by the authors who already had a large number of papers in the Physical Review Letters, Physical Review A, etc., typically from the highly prestigious centers like, e.g., Harvard, MIT, etc., though, till now they do not seem to lead to any efficient experimental methods (even in "our community"?). Of course, these remarks may be premature, since the practical applications of theoretical results are unpredictable and almost never immediate. So, maybe, in some 10 years we shall see a technological bam-bam revolution? On other occasion, I could detect two essential critical articles against a leading trend, one of them waiting about one and a half years and the other about two years before they could be published. This was no longer the symptom of a light disease...

An excessive power of some elite groups certainly is not limited to the *Physical Review Letters* et al. The same phenomenon can be noticed in *Nature, Cells, Science*, etc., criticized recently by the known scientist. Thus, in his article "How journals like *Nature*, *Cell* and *Science* are damaging science" published in The Guardian, Monday, Dec. 9 2013, 19:30 GMT, the known specialist Randy Schekman (one day before receiving his Nobel prize!) wrote: "... These luxury journals are supposed to be the epitomes of quality, publishing only the best research. Because founding and appointment panels often use the place of publication as a proxy for quality of science, appearing in these titles often leads to grants and professorships. But the big journals reputations are only partly warranted..." And later on: "It builds the bubbles of fashionable fields where researchers can make the bold claims these journals want, while discouraging other important work..." (remember Jean de La Fontaine?!),

The criticism of Schekman is just a detail compared with much wider discussions in biological sciences, concerning the evolution of life. For the groups of colleagues sympathizing with the leftist ideas, the Charles Darwin theory of the natural selection [8] is almost sacred. Yet, Darwin never claimed that his theory explains *everything*. In fact, attempts of modifying Darwin appeared in the Soviet regime in form of Michurin and Lysenko doctrines that the living organisms "can learn" to modify themselves (however, forbidding the genetics of Mendel as an antisocialist intrigue). The evolution steps difficult to explain by Darwin's theory indeed exist and were recently explored by the opposite current of the religion sympathizers who launched the idea of the "intelligent design". As a result, in some modern educative centers the theory of Darwin is unwelcome, as offending religious dogmas. It thus seems, that the role of religions in science is not over. In fact, the religious problems of our world are visibly increasing. Curiously, in many modern cultures an obligatory principle seems to be "the respect for religious feelings". How nice! But then, what about any other feelings?

All that are, perhaps, just anecdotes hardly sufficient to reflect the magnitude of the desperate scientists' battle to legitimize themselves by streams of "creativity". Since the numbers of the scientists and their works are still growing (according to the statistics performed by D. Colquhoun [4] about 1.3 millions of papers published in 23.750 journals in 2006), it was inevitable that the social organism had to find some new channels to expand.

The Open Access, Paid-Journals

The answer were the new type journals, accessible *on line* to anybody, in which the authors must however pay for their publications. It is assumed, that the papers submitted to those journals are carefully revised, but it can be easily understood that the main reason to accept an article is money paid by the author rather than the article quality. While in physics few of these journals already gained some positive reputation, the most of them are just business enterprises, caring basically to earn money. The socio-economical study of this phenomenon was recently made by the workers of the (criticized) *Science* by submitting some senseless articles to a huge number of the on-line paid journals. This is what one of them reports [7]:

> On 4 July, good news arrived in the inbox of Ocorrafoo Cobange, a biologist at the Wassee Institute of Medicine in Asmara. It was the official letter of acceptance of the paper he submitted 2 months earlier to the *Journal of Natural Pharmaceuticals*, describing the anticancer properties of a chemical that Cobange extracted from a lichen. In fact, it should have been promptly rejected (. . .) Its experiments are so hopelessly flawed that the results are meaningless. I know because I wrote the paper. Ocorrafoo Cobange does not exist, nor does the Wassee Institute of Medicine. Over the past 10 months, I have submitted 304 versions of the wonder drug paper to open access journals. More than half of the journals accepted the paper, failing to notice its fatal flaws. Beyond this headline result, the data from this sting operation reveal the contours of an emerging Wild West in academic publishing.

The business of the open access journals is certainly worth of careful attention. According to *Science* report, the journal described above is one of 270 in one of the largest open access publishers, with 2 millions of its articles downloaded by the researchers every month. In 2011 it was bought by Wolters Kluver Netherlands (the company with the annual revenues of nearly $ 5 billion). Published in Science [7].

The Support for Scientific Projects?

On this complicated background, specially in the crisis time, the increasingly difficult task is to choose, which scientific projects of the Universities and other science institutes should obtain the financial support. Despite the fact that the funds for the science go recently down, the state organs controlling them don't shrink but rather systematically grow: they need more regulations and personnel to spend their money truly well! (remember Parkinson's law?)

About three years ago I heard a curious story about a group of our colleagues in Prague working hard to formulate an ambitious research project with participation of well-known specialists from several UE countries. In order not to make errors in presenting the 'formats', they asked help of a colleague who was recently working in the European Evaluation Commission. The formats were filled carefully under the expert control and presented to the Commission. As required, they contained the original together with a high quality copy. Yet, one week later, they received the answer that the project was rejected, with no possibility of reclamation. The reason was that the signature on the original was in the blue color and on the xerox copy it was black. No revisions accepted!

Something from my personal observations (as the referee on several occasions). It is of course natural that the project's authors should inform the commissions about their expected work topics, with some approximate timing, also about their planned experiments and measuring devices, some hopes about the results, etc. The phenomenon which I have seen, however, were the data of the kind: after the first year of the project, we shall publish 4 papers, after the second year, again 4 papers and after the third year, the 5 papers... Quite shocking! The authors, after one or more months of working hard on the project, were so afraid that their effort may be in vain, that they were blind to the idiocy of reporting the exact numbers of their future publications (and, as somebody told: blind also to their own blindness!)

Several years ago I had an occasion to ask Bill Phillips (Nobel in physics 1997): "What do you think about the short, two or three years long projects, whose authors are asked to present from the beginning the 'calendar of activities' including even the future publications?". "Yes, we have them too" he answered, "but the power of US is, that we have also a large variety of institutions which can support the science without this kind of nonsense!". (I just wonder whether it is still true?). This is, however, not the end of the story.

Science AND *Technology?*

One of the reasons of so high interest in science are the technical applications which changed completely our lives (for good and for bad!). However, the relations between the scientific and technical progress are seldom free of conflicts.

A known historical example was the collaboration of the famous US inventor, Thomas Edison with his colleague, the equally famous Nicola Tesla [10]. The son of the Serbian family, Tesla, already as a youngster showed exceptional talents. He was specially fascinated by electricity. The electricity generators at this time were producing only the constant voltage (direct current), which could be sent by cables at small distances. In his moment of inspiration Tesla predicted the generators of alternating currents, but in the conservative Austro-Hungary nobody was interested. Tesla therefore decided to go to the U.S.A. He was accepted in the laboratory of T. Edison, who promised him the reward of $ 50,000 if he succeeded to construct a better electric generator. As it seems, Edison did not expect any great success. He already invested millions into an electric plant in the Pearl Str.

which could only send with difficulty the direct current at the 1.5 km distance to Manhattan. Meanwhile, Tesla worked intensely and soon he created the first generator of the alternating current. Edison, however, was completely absorbed in his competition for the better generation of the direct current (the challenge was again of $ millions) Some journals report that he just fired Tesla without paying him the promised reward of $ 50,000. According to a more exact study [10] this was not so. Anyhow, Edison blocked further work on alternating currents. Furious Tesla abandoned the Edison's laboratory and patented his discovery, which was bought by an American millionaire for $ 60,000. Tesla did not even suspect that he lost an enormous fortune for a miserable price.

In this old story Tesla represented the scientific idea and Edison the technology. It can be noticed that some elements of their antagonism were repeated in the future. In fact, after the golden time of historical collaboration, some products of the science were inconvenient for industry: by simplifying the production they could cause the looses if not ruin of the already developed production patterns. A notable phenomenon was that the industrial enterprises were buying the interesting patents, not to apply, but inversely, to avoid their applications! The phenomenon does not seem limited to the past.

Another trouble is the different dynamics of the science and its applications.

It seems essential that the application of the scientific discoveries can be unpredictable; on some occasions it happens by pure accident, not by any carefully elaborated project. The historical examples are multiple. Just to mention one, the discovery of penicillin was not the success but rather an obstacle of a microbiological research program. If Alexander Fleming continued his planned experiments without worrying about his spoiled microbe cultures, he would probably ended up with some routine results without even suspecting what he had lost!

It must be also remembered that the most important technical applications must take their time. The discovery of the electrodynamics of Maxwell and Faraday was seen with extreme skepticism by lords of the British Parliament. "What use there can be out of some partial differential equations?", they asked. According to the existing memories, Faraday answered: "And what use of a child?". In fact, the child was growing. Not immediately, but after about 50 years, the radio and then the radar were invented. Later on the use of optical fibers. The internet seemed a little practical trick, not even patented, but within about 30 years it turned on the most powerful revolution, causing the rebellions and governments fall. Our today civilization is based on fibers. The question arises, could all this be accelerated? Apparently not by bureaucratic pressures.

In physics, and other exact sciences, while the mountains of publications were growing fast, the increase of valuable applications was much slower. Some state administrations, helped by the industry leaders invented the concept of "Science AND Technology". The hopes were that it would convince the successful industries to invest into the science programs. At the beginning it was almost true (just remember the NASA investment into the International Conferences). Soon, however, the dependence was inverted.

In 1989 after the change of regimes in the Eastern Europe, the hope was that the scientific institutes finally will obtain enough grants to develop ambitious research projects. Yet it was not exactly so. In the first years of the "new deal" in Poland, the funds for the universities and research centers did not satisfy the initial hopes. A group of scientist from various institutes, decided to examine the situation. The result was shaking: several million dollars missing! The delegation of scientists visited the Science Ministry demanding the information. "No trouble, nothing disappeared", they were told. "In the framework of "Science and technology", several large enterprises also presented their scientific projects which the Ministry considered of interest. Then, they reported quite satisfactory results...

Today, the neologism "Science AND Technology", in spite of the entire optimism, works as a linguistic tumor which sucks funds out of the scientific work, for enterprises which have never enough. The cases are abundant. About three years ago, our colleagues checked that an enormous part of the "science support funds" all over the world are consumed by industrial establishments which (specially in the crisis time) care only about the short term profits. A shaking example is the sequence of industrial projects with costs notably higher than the customary university projects, approved by one of the world science ministries, including quite costly scientific project of Volkswagen factory. Thanks to this kind of mechanisms, many state bureaucracies can report, e.g.: "We spend around 0.4% of national income to develop the science". Yes... but of this quantity, how much were the souvenirs to the great (transnational) enterprises, which had nothing against simply consuming the funds and then, no difficulty in presenting the satisfactory reports? The Journal "El Pais" in Spain some weeks ago asked: "And where is the promised 1%?" An extremely naive question. It would be very fortunate if some part of 1% was really invested into the basic science in any world country.

Instead – the scientific communities all over the world are now incessantly bombarded by marketing announcement, how to make, how to organize our own enterprise, in spite of the world crisis... (Should I cite hectares of promising announcements?) In UE e-mail boxes, some of the business proposals are quite difficult to remove: by trying to cancel, the announcement responds by sending you to some new links, which neither want to disappear. To cancel the entire sequence and return to your e-mail, you need some additional computer tricks. All this is no longer an innocent marketing, but a heavy parasitism!... One would like to think that this is the last unpleasant problem, but it isn't!

The Far East Catastrophe

As if it was not enough, a new challenge is now developing in China. The article "China's Publication Bazaar" (*Science* [9]) reports the existence of the new lucrative commerce which permits the young desperate scientists to buy the authorship of papers already accepted for publication. The report is so shaking, that I permit myself to quote some fragments.

> The *Science* investigation has uncovered smorgasbord of questionable practices including paying for author's slots on papers written by other scientists and buying papers from online brokers (. . .).
>
> "There are some authors who don't have much use for their papers after they're published and they can be transferred to you" a sales agent told to a Science reporter posing as a scientist. (. . .) The company would sell the title of first co-author on the cancer paper for 90,000 yuan ($ 14,800) Adding two names, first co-author and corresponding co-author, would run $ 26,300, with a deposit due upon acceptance and the rest on publication.
>
> (. . .) On 6 July, a few weeks after our conversation (. . .) the paper appeared online in the International Journal of Biochemistry & Cell Biology. The printed version followed in September, roughly when the agent said it would. The title and the abstract had undergone minor revisions from the e-mail solicitation. But the list of authors was transformed. On the published paper, two first authors shared the honor. (Our reporter did not pay for authorship). (. . .) Following an inquiry from *Science*, an investigation by the International Journal of Biochemistry & Cell Biology found that a total of four authors had been added and two dropped (. . .)
>
> *Science* documented authorship fees ranging from $ 1600 to $ 26,300. At the high end fees exceed the annual salary of some Chinese assistant professors. But SCI papers – particularly those published in journals with a high impact factor – are so critical for getting promotions that researchers shell out.

The section "Paper-pushers" quotes the Chinese dealer: . . . 'Several agencies claim they collaborate with specific journals indexed in SCI to guarantee publication, The representative for one company (. . .) was blunt about the collaboration: "We rely on our guanxi" – a Chinese concept evoking relationships often deepened by exchanging gifts. "To put it simply, we give them money". At least three companies offer to assist the scientists who have written a paper and want to ensure the publication. Other firms claim to purchase a number of pages in journals. Several agencies specified both the journal and issue in which a paper would appear – even though the paper had yet to be written.'

The article quotes an opinion of one of ex-editors that the phenomenon is not too abundant, but "it completely destroys the academic environment". Let us add: already damaged by the "publish or perish". Moreover, the Chinese "Publication bazaar" can be so prosperous only because its brokers have accomplices in the Editorial Boards of some world SCI journals *outside of China*! The corruptive process illustrated here has an almost cancerous mechanism!

Certain uneasy **Conclusions** must follow.

1. The bureaucratic pressure of "publish or perish" must disappear. The scientific results cannot be estimated by numbers of publications.

2. Enough of the scientific projects which promise the number of publications. The scientific research is needed only if the results cannot be predicted. If authors can predict the number of their papers, it means that their work is unnecessary.
3. Be careful with numerical rankings of the scientific institutions. Their significance can be very misleading.
4. Careful with the linguistic tumor of "Science AND Technology". Shouldn't these two concepts be at least partly separated to grant some modest contribution to the basic science?
5. An investigation of the corrupt activities on the Chinese Publication Bazaar as well as their partners in all world journals is urgently needed.
6. Enough of ideologies, religions and 'political correctness'. The scientist should not offend his colleagues, but has no obligation to care that his results won't antagonize anybody.
7. Enough of the trend, mainstreams and obligatory worship of the "excellence groups". Yet, in some near future, we might offer our friendly patience to the "luxury journals" and their leaders (of course, not without some friendly critiques!).

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