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GENERAL AND APPLIED PHYSICS

The Myth of Academic Excellence and Scientific Curiosity

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Abstract We try to substantiate, using physics as an example, the following statement: the quality of scientific production cannot be measured by any numbers or indices whatsoever.

In an important book [1], Mathias Binswanger alerts to the danger of staging "artificial competitions" in fields of human activity where, technically speaking, no market exists. Examples are science and education, because of the impossibility of classifying the participants on the basis of quantitatively measurable indicators. In science, the one presently used is "the number of publications in certain journals." In Binswanger's opinion, "such competitions lead the participants to the useless production of 'measurable' achievements and products which results, in the long term, in waste of time and reserves. The victim lying along the path is the intrinsic motivation and, with it, the pleasure of working, both essential to the real scientific performance, which does not lend itself to exact measurements."

On the basis of criteria such as the one mentioned above, a well-accepted concept of "elite" and "excellence" was formed, which provides orientation to universities and research foundations in the whole world for the distribution of funds. The journals which we will call top A, of high "impact factor" (a rather controversial number analyzed in detail in [1]), boast high rejection indices, which reach 95 %, encouraging referees to recommend refusal in the almost totality of cases in order to justify this so important "measure of quality." Thus, a major objective of these journals became the search for justifications to substantiate refusal, resulting in a one-sided, and therefore unfair and unrealistic, view of the refereeing process. Due to lack of time of the referees, the papers are not read in any detail and are rejected with arguments which are difficult to refute, e.g., "it is new and interesting, but the methods are standard" or "there are no sufficiently new ideas to justify publication in top A." Sometimes, depending on the journal and on how influential in the community the author is, harder approaches are used, such as very long delays in turning in a report. Finally, the editor frequently acts to reinforce the main idea of the system, for instance, by choosing to refuse a paper whose publication is not too emphatically recommended by the referees, or resting upon a negative remark by a referee who recommended publication, without checking it even superficially, either by lack of time or interest, or both. There is great power attached to such positions, because no appeal of the final decision is possible.

It must, however, be remarked that a few high level, but not top A, publications do take their roles seriously, assigning referees who actually read the papers and even offer constructive remarks and/or corrections. In these exceptional cases, paraphrasing Matthews and Salam, the paper satisfies, finally, the condition of having been read by at least two people, one of whom may be author, and some scientific communication has been achieved, even if dissemination remains more restricted. I recall in this context a conversation with a colleague, who observed: "Walter, everybody knows what our 'business' is: Nature, Science, Physical Review Letters (PRL)." It came then to my mind what a distinguished deceased colleague, Professor Daniel Henry, from the Mathematical Institute of the University of

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Sao Paulo, would have said about this, because, while collaborating on a paper, I asked him if he had an idea where to send it for publication. I never forgot his answer: “Forget the damn publication, we want to understand!” In fact, this is the most important message of this note, which I shall try to elaborate on. Incidentally, the paper was finally published in *Communications in Mathematical Physics*, a top A journal in the area, which today, unfortunately, follows the same pattern of the others.

One of the most problematic features of this process of evaluating excellence is that perverse behavioral patterns are generated [1]: (a) a strategy of praising citations, (b) absence of deviations from established theories, (c) “salami tactic,” (d) increase of the number of authors per article, (d) increasing specialization, and (e) falsification and fraud. The salami tactic is the well-known subdivision of the work done into thin slices, as in a salami, in order to maximize the number of publications. We shall be concerned mostly with (b), (e), and (f) in what follows: in the case of (f), with the most subtle types of fraud favored by the system. Our main objective is to analyze some of the most serious consequences of this framework using physics as an example, but the generality of the phenomena should render the discussion interesting to all fields of academic activity.

The physicist Arthur Schawlow, who received the Nobel Prize for the theory of the laser, used to say: “The scientists of greatest success are not, frequently, the most talented ones, but those who are moved by curiosity, who simply must know the answer.” Obstacles to this search for understanding due to pure and simple scientific curiosity (perhaps the most noble task of any researcher, the one Dan Henry was referring to) have existed for a long time, today intensified by the connection with the excitement of competition and the idea of a market, which has been created around all activities, including academic and educational, by the media, politicians, and general public. Thereby, a profound mistrust of fundamental research at universities, situated in “ivory towers,” was established because society in general does not know what this research means and what it is “good for.”

Already in the decades of the 1980s and 1990s, the “politically correct” idea was launched: that universities should create “applied knowledge” (reminding one of Faraday’s remark, “What is the use of a new-born baby?”). This remains quite timely: in order to comply with this mistrust, there is a pressure of politicians and media to create competitions, necessarily of artificial nature, because the corresponding market does not exist (what would be the market for cosmology, for instance?), in such a way that only the “best” have their turn. I recall in this context an old “list of unproductive professors,” published some two decades ago by a leading newspaper in São Paulo, in which, surprisingly, many distinguished researchers appeared, or a recent internal list which appeared recently in the Institute of Physics of the University of São Paulo, classifying members of the faculty by the number of PRLs they had published. These best become spokesmen for excellence, frequently alerting to the fact that society pays their salaries and the taxpayer has the right to demand “production of new knowledge.” It happens, however, that the normal taxpayer does not have the necessary education to understand, and therefore to get interested in, fundamental research, and—a somewhat biting detail—the “new knowledge” may be wrong, i.e., present contradictions with the fundamental principles of physics. This last aspect, which involves even physicists awarded with the Nobel Prize, has assumed great importance in recent times, to the point of representing a menace to the actual meaning of scientific investigation, namely, the search for truth; as we shall see, it turns out to relate to Binswanger’s point (f).

There are several reasons behind the above-mentioned syndrome. One of them relates to Binswanger’s point (e), the increasing specialization, which led to a significant reduction in the level of general knowledge required from a physicist. This generates not only demotivation, due to the impossibility of viewing phenomena in a more general context, but also, of course, pure and simple ignorance. To give one example, it is not rare nowadays that an elementary particle physicist knows very little quantum field theory (QFT), which forms the foundation of the theory. As a consequence, the famous “standard model” of elementary particles, which recently became popular in the media as a consequence of the search for the Higgs particle, is commonly presented as a panacea for all interactions in physics, excepting gravitation. For the so-called strong interactions, which are responsible for the binding of protons and neutrons in the atomic nucleus, the model is, however, innocuous, for simple reasons connected with the size of the adimensional coupling constant (of the order of 10) and the elementary theory of asymptotic series, with which any graduate student who knows the gamma function has some familiarity. For this reason, there is as yet no theory for the proton–neutron mass difference or for the magnetic moment of the neutron, which plays a role in nuclear magnetic resonance. It is a problem of gigantic proportions, comparable in importance to a theory of quantum gravity, but the majority ignores or pretends to ignore this fact: the standard model is a set of extremely difficult open problems (in the electroweak theory, there is the mysterious global nature, i.e., not perturbative, of the theory, which worried great physicists such as Feynman and Landau, and in electrodynamics is connected to the so-called Landau pole).

Another direction appeared around the 1980s with the advent of string theory. It becomes soon clear, however, to most specialists in QFT that several gaps of principle were intrinsic to the theory (see the discussion pp. 299–300 in [2] as part of the reminiscences of Rudolf Haag, one of the greatest physicists in the field). Haag recalls that there was a suggestion that, within the scheme of string theory, only one theory would be possible, the famous theory of everything and Hans Joos, an eminent particle physicist, observed in this connection: "This may bring a revolution in our ideas in the years to come or it will quickly disappear again." As observed by Haag, "unfortunately neither one of the alternatives was realized."

The motives leading to this last fact were not analyzed by Haag, but, according to Charlton [3], "In contrast to the ideal of impartial and objective analysis, in the real world it looks more like most scientists are quite willing to pursue wrong ideas, so long as they are rewarded for doing so with a better chance of achieving more grants, publications and status." Charlton calls this phenomenon "Zombie science," a "science which is dead but will not lie down, it has no life of its own, being animated and moved by the incessant pumping of funds." In fact, it is not only funds that keep it moving, but an extraordinary lobby of power and influences. In the case of string theory, eminent mathematicians such as Sir Michael Atiyah provided large support, because they "saw in string theory at last one range of ideas in physics which could stimulate developments of new mathematics. But for physics this was negative because it drew the approach more and more into speculative areas in which contact with questions subject to experimental test got lost" [2]. This last point is important, because a theory is, in general, rejected, when it fails in the prediction of "reality."

It may, however, happen that a theory actually fails in the prediction of one or of a series of experiments it proposed to describe ab initio and, in spite of that, remains unshattered. This occurs with the BCS theory of superconductivity, which awarded the 1972 Nobel Prize to J. Bardeen, L. N. Cooper, and J. R. Schrieffer. Since the very beginning (see [4] and the references given there or [5] for a short summary of the situation), it became clear that the theory did not fulfill a basic physical principle, viz., local gauge symmetry. It is the latter that guarantees the existence of a current (electric in the case) that is locally conserved, meaning that charges may not be created or destroyed even locally; that is, it is impossible that a charge gradually vanishes at some point of space, even if it is simultaneously recreated at the same rate at a different spatial point (see the Appendix for a brief explanation of this point and references). Just this current is one of the central observables of the theory, because it is measured experimentally and, in special superconducting structures (rings), flows without dissipation for long periods of time. Subsequently, it was verified that the BCS theory did not explain some of the most fundamental effects of the theory, including the famous Meissner effect, which is directly related to the current, and later suggested important analogs in QFT and particle physics, related to mass generation (the Higgs effect) and confinement.

This was alerted by the distinguished experimental physicist Bernd Matthias between 1960 and 1970 (see [6] and references cited there). Alternative models appeared, in particular those of Hirsch [6], but, in spite of their claim of better explaining the experiments by a different mechanism, were never accepted by top A journals, such as PRL, and for this reason were hardly noticed by the large community of physicists. Quoting Hirsch: "The most prestigious and mainstream physics publications are completely silent about the possibility of the BCS theory being wrong, and papers submitted to these journals casting doubt on the validity of the BCS theory to explain conventional superconductors are not accepted for publication." The interested reader will find in [6] a detailed confirmation of the scheme of zombie science denounced by Charlton: we have thus illustrated item (b) in Binswanger (note that all this is independent of the scientific value of Hirsch's contribution, which is by no means our issue here).

We see that the main obstacle to the truth becoming known was the false concept of excellence associated to peer review in top A journals. A rejection to publish there leads to publication in a "non-top-A," but this is equivalent to imparting a label: the author did not succeed in publishing in top A; therefore, the contribution is not first rate. Theories frequently superior to those well-established ones or those supported by influential scientists (who also frequently belong to the editorial committee of top A journals) are in this way effectively blocked. We have thus arrived at item (f) in Binswanger, in the case that the blockade is, as in the case of Hirsch and many others, with the objective of preventing dissemination of ideas contrary to those of a group ("enlightened self-interest" [3]).

In essence, this procedure derives from the enormous decline of values observed in the present society, in which absolute pragmatism prevails. We are very far from the Bohr's description of Dirac as a "pure soul," as well as from Einstein's Weltbild: in these days, the most influential personalities were known by their total intellectual integrity. Only a great abyss between the professional and ethical upbringing is able to explain this surrealistic picture.

It is urgent and necessary that foundations supporting research and universities become conscious of how serious the problem is and accept that academic and scientific quality does not lend itself to measurement by objective numbers. As Binswanger remarks [1], those who carry the responsibility must assume subjective decisions, using if necessary the help of third parties in order to avoid arbitrariness. General one-sided schemes such as of top A journals previously described should be abandoned, and referees should be encouraged to decide solely on the basis of their view of the scientific value or merit of the contribution, and editors should also have the courage of assuming subjective decisions in case they do not find the referee reports satisfactory, but again, of course, on the basis of their independent judgment of the material, and not with the primary aim of reinforcing a general scheme as described before.

These editors are the present version of the "mandarins who control the job market," mentioned by Dyson [7, p. 161]. They should pay special attention to the "unfashionable pursuits" which are the subject of chapter 14 of Dyson's book [7], because, even if they do not reach the level of Sophus Lie, Hermann Grassmann, or Hermann Weyl discussed there, they may still bring extremely valuable contributions to science: this often relates to item (b) of Binswanger. To quote Dyson once again [7], "The problems which we face as guardians of scientific progress are how to recognize the fruitful unfashionable idea, and how to support it." If the mandarins do not have the courage to drastically change the ship's course, we run the serious risk of, in the name of a grotesque concept of excellence, emptying our original motivation: scientific curiosity.

Appendix

It is not pointed out, even in the very best textbooks, that the BCS theory violates the covariance of the dynamics with respect to the gauge transformations of the second kind

$\vec{A} \to \vec{A} + \nabla\chi$ together with $\Psi \to \Psi \exp(-ie\chi/\hbar c)$

where $\vec{A}$ denotes the vector potential and Ψ the second-quantized Fermi field (see [4] for a comprehensive formulation). It is, of course, the invariance of the Hamiltonian under (1) that leads to the total charge conservation and well-known explicit formulas for the charge, electric current, and magnetic polarization in terms of these fundamental fields (see, e.g., [4]). The well-known expression for the interacting pair potential in many-body theory, as well as the electron–phonon interaction Hamiltonian, is trivially invariant under (1), but the point is that the BCS theory is based on *truncation* of such a fully gauge-invariant model.

It is not obvious that the truncation has this effect, but it was noticed already in the early days [8]. Early attempts to cure it were based on uncontrolled approximation schemes (i.e., it is not possible to evaluate the error involved in the approximation), see [9–11]. More recent ones (see [6] and references given there) rely on entirely different formulations, but they have not been seriously analyzed or discussed by the experts and/or the physics community for the reasons explained in the text.

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