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A Straight-Jacket for Conceptual Breakthroughs: The Appraisal in Science as a Brake on the Progress of Knowledge

Part II. **What to Do to Get out of the Impasse of Neophobia**

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This article is Part 2 of an article published in May 2016, of this Journal, entitled *Why New Ideas Get Dashed to Pieces on the Rocks of Evaluation.*

The analysis of the patterns and procedures of evaluating scientific results, carried out in the first part of the article clearly displays that the current practice of science is by no means directed at encouraging novelties and supporting original ideas. At the same time, it is absolutely obvious that in the epoch of swift changes the world undergoes, insufficiently comprehensive renewal of knowledge emasculates, to a large extent, its practical potential and undermines the opportunities of involving science in solving social and human problems. Therefore, the reality comes into a harsh collision with the needs of society and the interests of its development. This article seeks to answer: How to resolve such conflicts? In what way to transform the practice of evaluating scientific results, so that it favor the emergence and growth of new ideas?

Introductory Remarks

The analysis of the patterns and procedures of evaluating scientific results, carried out in the first part of the article clearly displays that the current practice of science is by no means directed at encouraging novelties and supporting original ideas. At the same time, it is absolutely obvious that in the epoch of swift changes the world undergoes, insufficiently comprehensive renewal of knowledge emasculates, to a large extent, its practical potential and undermines the opportunities of involving science in solving social and human problems. Therefore, the reality comes into a harsh collision with the needs of society and the interests of its development.

How to resolve this conflict? In what way to transform the practice of evaluating scientific results, so that it favor the emergence and growth of new ideas?

If to return to the "standard" formula of the value (quality) of scientific results ($Q = \alpha*T + \beta*O + \lambda*V$, where T is topicality, O – originality, V – validity, and α, β and λ – the weights of the respective components) it is easy to notice that increasing the role of originality requires augmenting the significance of the component βO. In other words, at least one of the two possible operations should be undertaken - "normative" raising the relative weight of pioneering ideas or crediting to their "account" more substantial points than for validity or topicality. However, unfortunately, none of them brings to the wished effect.

If to increase the weighing factor β, that is, the importance of originality, to the detriment of the rest of indicators, it will acquire a hypertrophied character. As a result, the researchers will strive for originality at any cost – independent of whether what they propose has any sense and is bolstered with facts and arguments. The more original is the idea and the farther wanders from the generally accepted knowledge the better, even if it contains no tiny grain of rationality. As regards the increase of the significance of the factor O (higher scores for originality as compared with topicality or validity) it will produce a contrary effect. Nobody will seek for sizable innovations. What to cudgel the brains for, when even a trifling ("microscopic") portion of the new allows of standing on a level with the very topical and well elaborated, valid findings? Consequently, in both cases, the mark is missed – no balanced evaluation can be obtained.

However there is no great problem in that. To reinforce the innovative potential of research, keeping, at that, the existing level of their substantiveness and veracity, it is not necessary to resort to artificial measures. New ideas need neither crutches or leading strings nor indulgences or "quotas". It is quite enough not to give additional preferences to the established knowledge. Owing to its preponderant position, the latter penetrates into all the pores of scientific mind, ousting what comes into conflict with it. And in this struggle, to survive and, all the more, to sprout up, new idea must get a right for voice – albeit not equal with that of "canonical" knowledge, but at least an opportunity to show its worth and – when necessary – to justify itself.

What measures should be undertaken to give the new a chance to breach the shell of the established knowledge and overcome the natural opposition of human mentality? What changes in science policy and the practice of appraisal of ideas advanced could remove the artificial impediments, making easier their way to knowing minds, and application to the problems mankind faces? How to rearrange the interrelations inside science as a social institute, so that they favor fresh winnowings and not inhibit their diffusion and consolidation in the scientific tradition?

1. Openness to World

First of all, it is necessary to overcome the atmosphere of closedness and non-transparency which reigns in the procedures of evaluation and judgment. Sure thing, it has nourishing historical roots and goes back to the times of science's being a professional corporation with quite isolated life. And therefore even the most advanced fields of it bear, up to now, the mark of medieval seclusion and hierarchy. However today when sizable innovations are demanded from science its own inner ambience not only does not favor conceptual breakthroughs but, rather, impedes them. And what above, it turns science itself into an autarkic formation - behind the development of society.

In words, many talk of the openness of scientific institutions and the system of managing them. But in reality, it is no more than a decor. All that can. to some extent, infringe the organizational, financial, or other interests of those occupying visible places in the science hierarchy or having immediate relations to the latter is concealed from the general public under the pretence of secrecy or inaccessibility to laymen. So, Taverne, agreeing that "more openness and transparency are to be encouraged where possible", nonetheless stresses: it is not worth displaying "unthinking subservience to the principle of participation." Elucidating his position, Taverne reminds that in Britain, the participation of victims of rail accidents in making safety policy secured billions of pounds invested. But they save barely about five lives a year, while the number of those dying on British roads every day is twice as high. "The fact is", he proclaims, "that science, like art, is not a democratic activity. You do not decide by referendum whether the Earth goes round the Sun" (Taverne, 2004; 271). Apparently, his belief is that the last exclamation finishes off, once and for all, the supporters of the openness and transparency of science. As other full and partial opponents of involving the general public in its affairs, Taverne is assured that "outsiders" are not merely useless; they unceremoniously break into fine and delicate interrelations of learned heads and bring only turmoil into the orderly mechanism.

Nothing astonishing as well as new, though: the scientific community almost always had evinced a dualism toward the outside world. On one hand, scientists liked to be watched and, at times, admired by laymen but, on the other hand, sided against those persecuting them and disputing their respectability and expertise. At their very first séance, the members of the Royal Society decided to write down in detail their discussions and to retain them in the minutes of meetings. However as soon as polemics began to burst out among intellectuals against the Royal Society, some of its members demanded to toughen, without delay, the policy regarding their privileges and the propagation of knowledge (Eamon, 1985; 342, 346). The enlightenment and democratism promptly disappeared, and the corporate interests came to the forefront.

But there is another stand, too. It implies the intervention of public. Just owing to that, science has not entirely gotten into stagnation, yet. It is enough to recall the loud exposures of the last time: faked dissertations "defended" by high-ranking officials (for instance, by the former German Minister of Defense); journals which are busy tritely selling their pages; institutions granting degrees in exchange for money or service; etc. How such unmasking becomes possible? Only thanks to the media, social networks, and other like external associations and organizations. But for their interference, these phenomena scarcely would get wide publicity. For, those staying within the "big science" are either not interested in resisting the "diseases" or lacking power to do so.

Not in vain, some researchers propose to extend the context of innovations in research and technology by including the general public and mass media which, though not related directly to science as a social institute, are deeply interested in it - both positively (employing the achievements) and negatively (being concerned about possible menaces) - as bearers of culture serving breeding ground and ambience for the growth of knowledge, and the sphere of application of emerging ideas. In addition, the concept of "knowledge democracy" is introduced that implies a parallelism with the development of society and its political organization (Carayannis, Campbell, 2009; 207 - 208). And although it is hard to fancy the search of truth to be arranged through democratic procedures, one cannot but admit that modern science sorely needs closer participation of the public at large in its affairs.

Sure, the shortage of people's involvement in the life of science is conditioned, to a great extent, by their poor understanding of the nature and mechanisms of conducting scientific research. And however much programs aimed at enlightening the general public and acquainting it with science as a social institute may be implemented they would not allow people "from outside" to get at it as well as scientists themselves do. But does it mean no admittance for "trespassers"? Must the scientific community become a kind of sect or semi-religious order? Surely no. As Wilsdon and Willis fairly note, the public should engage in the affairs of science, and "against the current", at that. Its task is to "remove some of the structures that divide the back-stage from the front-stage." It is necessary "to make visible the invisible, to expose to public scrutiny the values, visions and assumptions that usually lie hidden" (Wilsdon, Willis, 2004; 24). Just that is what the public's part is all about.

The public cannot and must not supersede the inner mechanisms of managing science. It is enough to carefully watch what occurs within science and, from time to time, step in and correct the events when the order in it goes too far beyond the interests of society. Still more important is the choice of priorities and objectives themselves at which science should be directed – especially as for those demanding of great concentration of resources and flared-out forms of organization. The control over this choice may not completely be passed to scientists. Their function consists in proper comprehension of the social orders and their translation into the language of scientific problems (Petrosyan, 1985; 111 - 112; Petrosyan, 1989; 17 – 20). Moreover, these problems and research plans based on them are subject to societal evaluation, what, again, is not an exclusive prerogative of the scientific community (Petrosyan, 1987; 51 – 53). In short, paraphrasing the known words said of militaries, it may be proclaimed that science is a too important and responsible business to trust it to scientists alone.

2. Inner Transparency

The openness to world is only one side of matter. The other – and even more essential – is the inner transparency of science, its perspicuity and accessibility of what occurs in it to the researchers themselves. Meanwhile, the actual practice wanders from the desired even farther than in the case with the general public. Any "engaged member" of a scientific council has a full opportunity to "save" his own candidate and, on the contrary, to sink "strange" one almost without fear to be caught. A complacent and, at that, semi-educated reviewer arrogantly jeers at the results obtained by a pioneer which are not easily accessible to a mediocre mind. The reality of today's science, its actual organizational ambience, is a heavy burden to active scientists and research collectives which are bound to restrain their own creative potential. The atmosphere of "under-the-carpetness" and elusiveness tightly coupled with this practice engenders relations in which, increasingly frequently and to a greater degree, come to the fore such features as superficiality of judgments, irresponsibility of decisions, and arbitrariness in actions. But errors and willful misconducts committed, together with their impunity, threaten to turn science, eventually, into an anachronic organism slowly but surely moving to self-destruction.

One might ask: are the reviewing and expert evaluation fundamentally unfit tools of selection of scientific works? Perhaps it is better to refuse the filtration of publications as it is done in some archive systems? Such a step is scarcely worth making. Unfortunately, the great part of information flow boils down to non-significant or low-grade materials. For the last two decades of the past century, more than 100 citations received only 0.3 percent of papers. At that, to the share of two thirds of them, falls one reference or less (Vinck, 2010; 113). Taking into account that such a prevalent part, by definition, cannot be revolutionary its staying in the shadow may not be explained otherwise than by the flaws in substance. And even if to forget of the necessity to somehow protect young minds against them it must be admitted that the institute of reviewing alone enlists efforts and make the authors perform some primary selection of thoughts and arguments and express them more distinctly.

The problem is related not so much to expert filters as such as to the forms they acquire and to how these are used. The blind, irresponsible reviewing eats away science from inside as rust and not merely lowers the standard of quality of publication. It undermines the motivation of researchers for improving the results and stating them substantially and honestly.

To obtain real unprejudicedness and, at the same time, to support new winnowings it needs to open, as much as possible, the procedure of reviewing. So as not only the author of the material submitted but everyone could get to know who has written the review and, on familiarizing himself with it, appreciate – retrospectively, over a distance of years, as well – how far the reviewer has gone to the bottom of ideas expounded, whether he has understood the arguments and discerned the vistas they open up to science and practice. And above all, the duty of anybody daring to officially evaluate the others' works must be to substantiate each remark concerning the text under review. Not to play the oracle as a preacher, from pulpit but to hold a deferential dialogue with the colleague being in need of competent judgments, not of anonymous and shallow declarations.

The same concerns also the collective decisions made by groups of experts. As in the case with reviewing, here, too, the maximum possible transparency is required. No secret ballot. Be it a matter of councils granting academic degrees, of commissions considering applications for grants or scholarships, or of committees "distributing" scientific awards, everyone taking part in making decisions must not merely vote but distinctly – preferably in writing or at least with record – substantiate his position. At that, the criteria on the basis of which the appraisal is made - for which merits and demerits precisely the work is countenanced or censured – should be clearly delineated. For, the right for voting implies the readiness to bear responsibility.

3. Honest Competition

As the analysis shows, evaluations in principle cannot be objective. It is a fiction they insist on either under delusion or on the basis of selfish motives. Any evaluation is subjective, and the point is only whose subjectivity holds sway, which of a multitude of subjectivities is elevated to the rank of objectivity and, thereby, becomes a standard the others should be adjusted to.

As to independence it has surely certain sense. The matter is only of understanding of its nature. What is meant by independence? Nobody is free of conditions under which he lives and acts. All are influenced by both knowledge and culture they have imbibed in the process of their formation and growth, and by people with which are tied organizationally and communicate in the course of day-to-day work. Well then, whom the evaluator must not depend on?

If it is true that no one can take an objective stand then it needs to accept the multiplicity of (almost) equipollent "judges" (centers of evaluation). Since each of them dwells in the setting of complicate and multi-dimensional dependence on many diverse factors, it is not worth telling of their "free decisions". And the positions they utter and, all the more, the evaluations being made regarding the scientific findings are largely pre-determined by that social and conceptual "charge" with which the experts have approached the carrying out of their task. The only thing in hand is to secure, as higher as possible, the independence of the evaluators from each other. They should not play the same pipe but each his own one. And this polyphony, and sometimes even cacophony, is just that ambience in which the alternative ideas grow ripe more readily and quickly.

But the multiplicity of evaluators in itself is not capable of starting up the engine of the progress of knowledge. If they function quite indifferently to each other there will be no getting away from monopolism. Science will remain under the power of "usurpers" – with the only difference that in place of a monolithic top, come a number of groups within which the previous relations are kept in miniature. Otherwise, the absolute monopolism gives way to a peculiar oligopoly – few separate "judges" (centers of evaluation) whose "independent spirit" does not bother to easily engage in consociation. But that means the alternativeness will not necessarily be converted into the support for radically new and breakthrough ideas.

Unluckily, just such is the basis of today's science. The harm being brought to both social efficiency of science and, in particular, its innovative potential by monopolism and engrossing power and resources in few hands increasingly often becomes subject of deliberation by scientists, what evidences their dissatisfaction with the current state of affairs, and striving to overcome it. Little by little, comes the understanding that comfort in handling with things and people, and deliverance from external pressure turn out to be a break on the growth of knowledge, fetters on its prospects. So, the American chemist Bauer is distressed about that in science, the show is run by "monopolies composed of international and national bureaucracies. Since those same organizations play a large role in the funding of research as well as in the promulgation of findings, these monopolies are at the same time research cartels." They push to sidelines the "minority views", preventing them from publishing in respected journals, and instead of unprejudiced expertise, promote own stands "in order to perpetuate their prestige and privileged positions" (Bauer, 2004; 651). It is very difficult to resist them, especially when the question concerns single scientists. Even the most eminent and authoritative of them can fall into disfavor if they dare to oppose the monopolies. So researchers not merely fail to stand for new ideas but catch heavy troubles when defending their elementary rights. There remains for them only to struggle for survival in the world of science.

Something of the kind happened, for instance, to the retrovirologist Duseberg who made bold to utter doubts about that HIV (Human Immunodeficiency Virus) necessarily calls forth AIDS (Acquired Immunodeficiency Syndrome) and, all the more, is its single cause. The scientist lost funding and had to make intense efforts even for publishing his papers in the National

Academy of Sciences' Proceedings, though he was of its members. Nothing astonishing; in 1991, the letter of a group of his colleagues, including two "Nobelists" in the field of molecular biology, where the rigid linking of HIV to AIDS was called into question, had been discarded by such journals as "Nature", "Science", "Lancet", "New England Journal of Medicine". Duseberg encroached upon not merely the postulates of the "pillars" of the science hierarchy but also the interests of the institutions behind them - the World Health Organization, UNAIDS, the World Bank, the Centers for Disease Control and Prevention, and others. Many governments have channeled for fight against AIDS funds to the amount of billions dollars – and suddenly it comes out that most efforts are applied for nothing (Bauer, 2004; 652). Is it not easier to oust from science a particular – albeit distinguished – academic and his "heretical" ideas than to rebuild a whole complex of research, and above all, to admit the funds spent earlier to be hardly covered to a due extent?

In such an ambience, conceptual innovations and pioneering spirit go to the background. In the limelight are found, ultimately, the sums spent for research. They get recognized as all but the single measure of scientific level. If a university wants to acquire the status of research institution it has to think not so much of how to raise the professional grounding of its academics and the quality of their work or facilitate the ripening, development, and practical employment of new ideas as of whence and in what way to obtain financial backing and thereby to bolster up materially the cherished hopes. Not in vain, as early as in 70s of the past century, the American biochemist Chargaff whose ideas enabled the discovery of the "double helix" of the DNA noted with bitter irony that "in our time" a successful researcher "is not one who solves the riddle, but rather one who gets a lot of money to do so" (Chargaff, 1977: 89). Many university functionaries in order to involve the leading academics in the search for funds have turned it, in essence, into the key condition of career development. Thus, already in 1980s, the Dean of the College of Engineering at Virginia Polytechnic Institute and State University during the meetings of the University Promotion and Tenure Committee was repeatedly stating that those claiming to tenure must obtain about 100 thousand dollars from external sources, and approximately three times more is the amount to be gathered by the contenders for the rank of full professor (Bauer, 2004; 657 - 658). And although it is obvious that funds in themselves get not converted into inventions and discoveries and the accretion of knowledge, their amount increasingly supersede the scientific results proper at evaluating research.

It could not be otherwise. To institutional bureaucracy that more and more turns into the kernel of scientific organizations, just the funds obtained appear to be the most important prerequisite of survival. It is better to have money "earned" without real achievements than great breakthroughs getting no acknowledgment and, consequently, being not rewarded.

Such an atmosphere inevitably urges on to a narrow pragmatism. It communicates an absolute naturalness to the fact that the lion's share of the expenses on science goes to the areas and directions with quite visible return for both the researchers themselves and the institutions they represent, and the funds and agencies supporting them. "Curiosity-driven" researches, that is, the part of basic search with no distinct practical objectives, in actual fact, get no particular support. They are conducted within major mission-oriented projects – sizable industrial, military, or social tasks - as their general "nourishment", or for account of the researchers themselves receiving no additional reward for the efforts applied.

In USA, the federal subsidies for basic research, including both "curiosity-driven" and "oriented", make no more than 10 percent. Sure, in practice it is difficult to separate them from each other. But if somehow - roughly-approximately - to make a distinction between them one would easily see that only few portions of a percent fall on the "curiosity-driven" investigations. At that, the funding of American science is held mainly in several hands. A key role is played by the National Scientific Fund (NNF) – particularly as to university science. About three fourths of federal expenditures on physical sciences come from only three agencies – the Departments of Defense and Energy and NASA (National Aeronautics and Space Agency). Likewise, the vast majority of works in the fields of health-related and life science are initially tied to specific tasks (Brooks, 2006; 42 - 43).

This circumstance alone essentially bounds the possibility of conceptual breakthroughs as the works capable of bringing to them remain without sizable support. And though the lack of funds is partly made up by the curiosity and enthusiasm of particular researchers who devote themselves to laborious and stubborn work, knowing in advance that they would not get a due reward – neither tangible nor intangible, – creative impulses are not enough to produce steadfast outcomes. It is especially noticeable in the case of experimental research requiring expensive equipment, theoretical investigations comprising complicated multistage calculations, or complex works implying the involvement of whole collectives consisting of scientists of diverse specialties with different biases. The organization of the activity of such groups demands considerable forces and funds, but the latter, as a rule, are not granted to those who has reached the edge of comprehension by the experts examining the application or even gone beyond, since it is very hard to explain to “outsiders” what benefit can be obtained from the research a pioneer ventures upon and, all the more, to prove the appropriateness of his claim.

Thus, three dozens of eminent scientists who sharply protested in the beginning of this century against the theory of Big-Bang and referred as their key argument to the lack of empirical evidences in its favor, insisted on that other approaches (plasma cosmology or the model of steady state) representing the Universe as having no beginning and end coped with the main phenomena such as the abundance of light elements, the generation of the large-scale structure, the residual radiation, and the increase with the distance of the red shift of remote galaxies, not worse. The alternative conceptions, in their opinion, have succeeded in foreseeing new phenomena observed afterwards, what the theory of Big-Bang failed to do. Nonetheless the protesters could not but agree with the objection that they, too, were not in a position to cope with all cosmic observations. “But that is scarcely surprising”, they parried, “as their development has been severely hampered by a complete lack of funding” (Lerner, 2004; 20).

The organizational impediments and the fight against dissent within scientific institutions work as before, but they are as if sidelined. In the “big science”, increasingly often and to a greater extent, the center of gravity shifts from censorship and suppression of the ideas out of favor, to prohibiting of their emergence. When some directions of search are not supported they scarcely will beget conceptions one afterwards has to fight against. And if a group keeping to certain view begins to dominate in expert authorities (committees and commissions evaluating the applications for research funding, the scientific results, and the scientists themselves) it is in a position to easily nip in the bud the designs that can jeopardize its beliefs, convictions, and interests. No wonder, instead of opening new vistas, many a researcher pattern their behavior on vogue and what is better sold today, while the scientific community as a whole remains in the gripes of its own hierarchy that sets the problems, relying, for the most part, on the known and the tested. Therethrough, the talent of scientist and his research potential gradually cease to be the key criterion of selection at hiring academics in universities and other scientific institutions.

The American geneticist Pringle, one of those who together with Hartwell has developed the idea of cell cycle deplores that many highly qualified scientists cannot have occupied academic positions. In his words, research organizations employ workers “not on the basis of candidates’ potential for truly creative work". The preference is given to those who "are working in fields perceived to be "hot" and well funded", that is, just there where "less left to be done", and what is "vulnerable to future changes in funding fashion." At the same time, it becomes increasingly more difficult to get support for new basic research - despite the good intentions of the funds to resist this dangerous tendency.

Among the causes, Pringle names, first of all, that a too sizable share of expenses for science goes to hierarchical projects pursuing "overly specific practical goals" or "excessively large (and thus almost inevitably wasteful)." Meanwhile, more compact and directed ("investigator-initiated, small-group") projects which just lead to the truly new discoveries remain in the background (Pringle, 2013; 3283). However Pringle not merely does not indicate the way out of the quag science gets bogged down in, but even does not go deeper into what took it there. Particularly, it remains unclear why research organizations and funds supporting them allow themselves to conduct such a harmful policy and feel, at that, no big difficulties in their activity are not driven out to the sidelines and, as previously, keep in the focus of scientific life.

How to cope with the pressure of monopolies? What measures should be undertaken to get the researcher-innovator out of their gripe?

It is just these questions Bauer tries to answer. The "reforms" he offers are connected, first and foremost, with revision of the order of funding. In his opinion, it should be legislated that a part (say, 10 percent) of funds assigned by the state for science go to those scientists "in opposition" who have formerly excelled as researchers. That would allow of supporting many works though not going into the framework of prevailing tendencies but capable of giving some high practical outcome. Besides a direct effect, such a measure would have also a by-repercussion. It would prompt private funds, too, to support "dissident" research. Further, in the same way, it should be provided for by law that "scientific advisory panels" and "grant reviewing arrangements" comprise as well the adherents of alternative approaches opposing the predominating point of view. This would, according to Bauer, favor the more honest expert appraisals. It is not reasonable to consult only the so-called competent specialists, for in the eyes of the establishment, "dissidents are not competent", and it will strive for "seeking advice only from insiders". Finally, it would be useful to have a Science Court called upon to arbitrate in cases growing from the opposition between "mainstream and variant views". In addition, Bauer finds it pertinent to establish ombudsman offices under scientific journals, private funds, and governmental agencies. Their task is "to investigate charges of misleading claims, unwarranted publication, unsound interpretation, and the like." And what not less important, they "could also provide assistance and protection for whistle-blowers", owing to what all what occurs in scientific organizations would surface and get public, as well as the defects and abuses in their activity would come to light (Bauer, 2004; 653).

These measures, Bauer believes, are capable of curbing the arbitrary rule of scientific monopolies and keep their behavior within an admissible course. But at a closer examination, it is impossible not to notice that all offered by him boils down to pure administrative steps. They not only do not take account of that science is built into the system of market economy but also ignore the peculiarities and the nature of people making it.

Let us put aside the old idea of Science Court that has been advanced by a number of scientists. It was already subject to criticism, and its flimsiness has been plainly shown (Petrosyan, 2015; 166 - 167). As regards the ombudsmen in science they appear as a subsidiary instance adding nothing substantial to the existing relations and procedures. Unless to provide for that the ombudsman alone decides on the issues vested in him, it must be admitted that they come back to the same expert groups, committees and boards which, as it is believed, act so preconceivedly and ineffectively. As to investigating "charges" it looks a utopia at all, for the progress is hampered just because of the lack of criteria of demarcation without which it comes only to the escalation of control and the preponderance of supervisors. Some will keep watch over the others, and nobody will have enough time and force to conduct research proper. Not to mention that in such an ambience, there is a great danger of spreading the culture of snitching within which the purport of science – the creation of new knowledge – definitely loses any purport.

Nothing will be brought also by the involvement in the activity of expert groups making decisions, of representatives of the "opposition". Can it help in promoting the alternative approaches? Apparently, very little. Clearly, "dissidents" scarcely will take part in their work on a parity basis. At the best, the role of junior partners has been prepared for them. And consequently, two options are at their disposal. The first: "dissidents" persistently proceed, remaining an "outboard" opposition, and the majority simply quells them by means of voting. And the second: those of them who have been bestowed with the honor to take part in official decision-makings accept the rules of the game and turn into an institutional opposition. Then, they are given special quotes and allowed certain, though rather modest, opportunities to promote their own ideas. However these privileges will apply only to "honorable dissidents" themselves and will not tell on other minorities. That is, they will have decided partially their own problems but, of course, will not have changed the system of interrelations in science. Expert evaluations will remain much the same, preconceived and tendentious, – with the only difference that now the composition of "insiders" will be somewhat expanded for account of minorities loyal to the majority.

Especially noteworthy is the Bauer's proposition related to quoting the funds. The 10-percent share earmarked for "dissidents", he offers to use not for breakthrough research or for development of alternative conceptions proper. It should be granted to particular scientists who already have somehow demonstrated their ability to make contribution to science. That is quite understandable. Will a search bring to a breakthrough or a failure is not known. And how much fruitful will have turned out to be the idea advanced? The case with scientists seems to be simpler – their potential is more or less known, and when it is large enough they may be trusted with some funds even if their stand is not shared by others.

But in reality, the risk in providing scientists with funds is not a bit less than when financing ideas. Is it guaranteed that the person who has behind him serious achievements does not mistake in the given case or he copes with the problem set, without fail? The answer hangs in the air. But even more important is another thing. At such an approach, not what is fundamentally new gets support but simply what is alternative - instead of unknown, another "reading" of something by and large known. Of course, this is necessary, too, but does not open vistas. And above all, there remains virtually no room for young talents there.

Meanwhile, staking on focused and systematic self-renewal of science implies not merely autonomy and alternativeness of "judges" (centers of evaluation) but also their contention. Only when rivaling each other (for resources, for skilled specialists, for acknowledgment) they begin to prioritize innovations, and innovating becomes a key leverage for raising their own authority. But it means that each of evaluators should have its own particular interest in performing quality appraisal, that is, profit by passing competent, substantial, and not "committed" judgments. For, only taking high advantage from the success in competition, they will address voluntarily difficult tasks and, instead of imitation of supporting new ideas, will actually open the gates before them.

The collision of structures (institutes) independent from each other must permeate all the appraisal component of the management of science. It applies to preliminary "weighing" of the lines and themes (planning research and allocating of funds), qualifying evaluation of scientists (awarding degrees and upgrading in rank), and recognizing the results obtained (publishing in journals, giving prizes, and so on). Any monopoly over truth and evaluative judgments is fraught with restraining the striving for the new. and slowing down the growth of knowledge. But left to bureaucratic hierarchy, it gets increased manifold and entirely distorts the function of evaluation, emasculating its content.

Table 1. The mechanism of evaluation of scientific results (actual and required)

Guiding principles	Purpose	Reality	Changes to be introduced	Principles to be guided by
Objectivity	Approaching the evaluation unprejudicedly and unbiasedly, so that it not to be contaminated by particular interests or personal emotions.	Completely infeasible – since humans are fundamentally subjective, they cannot give assessments free of the seal of their mind. Therefore, objectivity appears to be only a fiction camouflaging one else's subjectivity which substitutes for itself another subjectivity.	Instead of putting forward a concocted objectivity of mind, one should admit the fundamental multiplicity of subjective evaluators. The single demand placed on their minds must be competence in the field they act in this capacity.	***Multiplicity of subjective evaluators***
Abstract (ideal) independence of evaluators (from all influences)	Delivering the evaluator from any external influences which can tell on evaluation and distort the final judgment	Impossible in practice – at least in full, all the more under hierarchy reigning in modern organizations – incompatible with it. All evaluators are tied with thousands of threads with numerous phenomena and processes occurring around them	Absolute independence is surely impossible, but it is not necessary. It is enough for evaluators to be separated from each other and pass their own judgments irrelatively of those opposing them	***Feasible independence of evaluators (from each other)***
Competitiveness (amongst the subjects under evaluation)	Securing the adversary character of evaluation, which allegedly allows of selecting what has proved to be really the best as applied to the criteria established	Actually absent – mainly imitative because there is in practice no real basis for it. The lack of manifold evaluators and their engagedness in complicated and multi-level relations with universities, industry, and government do not allow them to rival honestly but, rather, goad them into conspiring with each other and third parties on diverse issues	The multiplicity of evaluators independent of each other is that necessary ground on which competition can have shot. The only thing to be added to it is their having own particular interest in performing quality appraisal: they should take high advantage from the success in competition	***Rivalry (between evaluators)***

4. Personal "Lifts"

An important complement to the institutional mechanism of lowering the resistance to new ideas, could serve individual "lifts". It implies trusting, in certain cases, particular outstanding scientists with a right to decide, on their own and promptly (without excessive complicacies) but openly and publicly, the fate of new ideas (within those fields where they have proved to be experts). Understandably, the risk of misunderstanding and rejecting the new on groundless or even farfetched reasons still remains rather grave. However the innovators get another – alternative – way "to the top", and, thereby, more chance to break out from the gripe of formalism and collective irresponsibility.

Thus, it is known that the most original ideas carved their way not due to broad backing on the part of the boards of journals but at their almost total resistance. Under these conditions, only a resolute decision of an influential reviewer or editor could reverse the situation and overcome the inertia of the "sluggish bulk". And frequently, just to such a "non-conformism" we owe the papers expounding conceptions little accessible even to deep connoisseurs of the subject and, therefore, being met without rapture. At that, those who enable, in spite of the resistance of milieu, the material to appear do it usually not since clearly realize its merits but, rather, because of a vaguely-intuitive anticipation of a remote resonance in future.

Higgs, on getting the refusal from "Physics Letters" regarding his note on the "Nobelian" mechanism of the emergence of mass, did not droop. He recast and elaborated on the material and submitted it to another journal – "Physical Review Letters" - which issued the paper almost at once (Higgs, 1964). What is the matter? Why what had aroused no interest in the first outlet, in the second had been recognized worthy of note?

It is commonly thought that the point is related to "extra paragraphs" with gauge bosons, which are a "sales talk" for the idea of the source of mass and therefore favor making sense of and accepting it (Griggs, 2008; 17). But even if they played a role it was not of deciding nature. Such a demonstration of the explaining force of idea could not produce a proper impression on colleagues, for the construal seemed to be probably more shaky and obscure than what was to be proven with it. Rather, the author was lucky with the reviewer Nambu, a brilliant and profoundly thinking physicist sensitive to the problem and able to extend a thread from it to the allied fields of research. He not merely grasped the prospects of the idea suggested by Higgs but also asked him to utter his attitude toward the work of Belgian scientists Englert and Brout who had just published in the same journal. Therethrough, the context in which Higgs was developing his model had been broadened. It would be an exaggeration to deem that Nambu distinctly understood its meaning and vistas it was opening. Where it comes to insights, much more important is the ability to condense in mind a vague pre-thought than a skill of decomposing a thought into its logical components.

Sometimes, making the decision, perspicacious reviewers and editors proceed from seemingly completely down-to-earth considerations. Nonetheless, even in so cases, they not rarely succeed in hitting the "bull's-eye", promoting the innovatory ideas which afterwards prove to have large heuristic potential. It is not of great importance what precisely urges to showing thought for. The key factors are here the research flair and the scientific taste that scarcely are amenable to standardization and, all the more, formalization.

In 1966, a young biologist Margulis suggested an innovatory idea of the origin of eukaryotic cells – all except bacteria. But before she succeeded in publishing the paper expounding that idea it had been rejected by a dozen and a half of scientific journals because of its insuperable foibles. Although from the moment the author had finished the article to its acceptance for publication not so much time elapsed, she herself went through many events, including the marriage and change of name to Sagan (Sagan, 1967). So, it might be said almost without exaggeration that little had remained of the initial author.

It looks astonishing, on the surface, that the sole edition benevolent towards the new idea turned out to be the reputable "Journal of Theoretical Biology" which had sheltered the parvenu's "figment of imagination". However everything falls into place as soon as the facts of the matter come to light. The paper got to the journal not through an ordinary sifter but owing to that the editor Danielli himself got interested in it (Brockman, 1995; 135). Taking into account that the researcher was, by then, the wife of the prominent biologist Sagan, it would be very sound to suppose personal contacts to have played the decisive role. In other words, the article reached Danielli, in a sense, through a pull.

The luck was not merely that the material had been looked through by a competent and authoritative scientist the possibility of publication depended on. Not less – if not more – important was that he took an interested view of the text. The editor looked for pretexts for publication there, not reasons for declining it. Should Margulis go on with submitting the paper to journals, her Odyssey could continue for long.

What are the main merits of such an institute of individual "judges" acting along with major centers of evaluation?

On one hand, personal responsibility, together with the openness and necessity of distinct substantiation of the decision made, essentially levels the possible risks. And on the other hand, individual "judges", propping up major centers of evaluation, inevitably intensify the competition between them and, at the same time, widen the net of channels through which new ideas get promoted and thereby secure some chances even to the not quite enterprising creative persons. That places at the disposal of innovators additional "lifts" operated personally by outstanding minds possessing a taste for the new, flair for prospect, and rich experience of generating ideas. Hence, the total resistance to new ideas must essentially decrease, just as the emergence on market, along with major monopolies, of a multitude of small enterprises not only stimulates to introduce novelties but also urges the big business to encourage innovative decisions

But, despite the competition engages the institutional and personal evaluators in a single field of interaction, the areas of their influence by no means coincide. They, rather, merely intersect, and each group retains, besides common functions, its peculiar ones it can perform better than the rival party. So, institutional centers of evaluation might focus on mature researchers, while personal "lifts' on beginners. The former's prerogative should be funding and awarding, while the latter's the aid in publishing, and involving in research teams. Institutions would be more successful in providing researchers with material and organizational opportunities, while personalities are stronger in live communication (table 2). Therefore, it would be fairer to say that the both kinds of evaluators not merely emulate each other but can be mutually complementary. And together, they are capable of constituting a more flexible and dynamic mechanism of evaluating scientific results than clumsy and unconcerned monopolists having gotten stuck in catering for their own interests far from the progress of knowledge proper and eaten away by institutional bureaucracy, which run the show in today's science.

Table 2. Comparative features of institutional and personal evaluators as regards their tasks

Parameters	Institutional evaluators	Personal evaluators
Circle of researchers	Predominantly mature	Predominantly young (beginners)
Purpose of evaluation	Mainly funding, prize awarding, promotion to higher positions	Mainly publication and appointment to a research job
Timeframe	On continuing basis	From time to time
Character of relations	Chiefly formal	Chiefly informal
Kind of aid	Material and organizational opportunities for supporting research and promoting ideas	Channel of live communication for developing and polishing ideas and of entering into the scientific community

Thus, if to summarize these recommendations and express them by one phrase, the key-note of changes must sound as follows: “From impassable monopolism to honest competition”. It is just that leverage with which one can make science more strong and fruitful and restructure it along the lines of innovativeness.

5. Ideas instead of Accounts

The formalization of organizational life is of achievements of human civilization, and it is not so simple to get rid of it. But it is scarcely worth striving for that. For the complete abolition of standards and regulations is fraught with mess and loss of control. Under ordinary conditions – when solving routine problems, - formalisms are utterly useful. They simplify the activity, retrench the efforts, and save on resources. But where a fresh view of things, an innovatory approach to the matter, is required they become a break on the progress. Hence, although there is no need for eradicating formalisms, it is absolutely necessary to mitigate and counterpoise them.

Why, when assessing the contribution to science or the creative potential of a researcher, the list of his publications is taken as the basis? It is implied that once the papers are published they have gotten some approval from colleagues who are experts in the field. And the longer is the list the better, for the more fecund is the author and the more efforts he makes to increase knowledge.

Frequently, the size of papers is taken to notice, too. It is believed to show how thoroughly the researcher approaches the matter, and how broadly and deeply he substantiates his stand. The larger the paper the more serious is its author. At that, the size is considered not only a quantitative but also a qualitative characteristic. If the researcher is given an opportunity to expound his ideas so explicitly then some respect and a readiness to hear is shown to him. That indirectly evidences a rather high evaluation of his work.

Finally, as another – and more direct – indicator of quality serves just in what editions the researcher publishes his papers. The more reputable and prestigious are the journals and collections they come out in, the higher are valued the scientific results and the more he himself is trusted. By their "weight" (as defined according to the formula: $IF_{(JY)}=(C_1+C_2)/(S_1+S_2)$, where $IF_{(J)}$ is the impact-factor of the journal J for the year Y, S_1 – the number of source items published in the year Y – 1, S_2 – the same in the year Y – 2, C_1 – the number of citations received by the items published in the year Y - 1, and C_2 - the same for the Y - 2), journals sometimes are divided into "tiers" each of which allegedly comprises the editions of roughly the same level, what actually predetermines the status of papers presented there. So the index "developed to help librarians make subscription decisions, has de facto been repurposed by researchers, journals, administrators, and funding and hiring committees as a proxy for the quality and importance of research publications." And now, "researchers are judged by where their articles are published rather than by the content of their publications" (Bertuzzi, Drubin, 2013; 1505).

Quite often the demands on the participants of a competition for offices, scholarships, or grants comprise the publication of papers in top-tier journals. Furthermore, "some scientists", as Johnston, an experienced editor of a scientific journal, notes, "list in their CVs (and will tell you in conversation) the impact factor of each journal in which their articles were published." Do they not notice the substitution of criteria? But why it is not stopped? All is very simple, Johnston explains, researchers "also recognize that hiring and promotion and grant evaluation committees put weight on the journal impact factor" (Johnston, 2013; 792). No wonder that, deciding on where to submit the paper, authors put not in the last place what is the "weight" of the edition, how the publication in it will be taken from the stand of career development (by current and future employers, colleagues, and even editors of other journals).

Of course, there is some logic in such a system of evaluation. The scientist publishing many articles of large sizes in "reputable" journals is more likely to be a true connoisseur in his field and, the most probably, is held in the scientific world in high esteem. However the trouble is that, on one hand, there is no automatic connection there, and there are a lot of exceptions when the aura of recognition is created around such a person artificially (for instance, the English psychologist Burt); and on the other hand – and that is much more important, – a scientist can be a fecund and highly reputed researcher but a retrograde and obscurant in science and a brake on radically new ideas (the notorious Soviet biologist Lysenko). As to the respectability of edition it is quite delusive and frequently turns out to be "blown-up" – particularly when it is identified by formal indexes.

The impact factor of the journal "Acta Crystallographica-Section A" increased during a year almost 25 times – from 2,051 in 2008 to 49,926 in the next year. In 2009, it has acquired the highest impact factor among the editions indexed by the system "Journal Citation Reports". Where this wonder came from? It has been begotten by a single review article that appeared in the issue devoted to the 60th anniversary of the journal, in which the development of the SHELX system of computer programs since 1976 was traced (Sheldrick, 2008). There is nothing astonishing in that it has drawn a wide response. Its matter at issue was a software package initially designed for punched cards and computers tens of thousands times slower than modern ones but retaining its capacity and 30 years later being widely used in crystallographic research over the world when determining structures, despite the availability of much more advanced counterparts. That is, the paper was from the very outset virtually doomed to receiving a great number of references (Jain, 2011; 87). And that was, to a large extent, independent of the profundity of analysis and the quality of text. Consequently, the brilliancy of the paper itself in the flashes of citing was, rather, a reflected light. Nevertheless, it raised the journal where has been placed to the top of scientific influence.

Well then, is such a determination of the journal's authoritativeness rightful? Obviously no. There is a great distance between the citability of an edition and its respectability. And the point is even not the grotesqueness of the case. Some console themselves with that it is barely an excess which is not to be overstated. Thus, Sen notes that "the high impact factor of the journal will exist for two successive years. In the second year the impact factor will be more, and in the third year, the impact factor will significantly decline" (Sen, 2012; 290). That is, soon all will come back, and the short-run triumph of the "blown-up" journal will look barely as a local "gumboil". As to the global hierarchy of editions there little thing will change. However that is not merely an oversimplification but also a misunderstanding of the marrow of the phenomenon which consists in artificial endowing the authors with the influence that does not relate to them anyway and, moreover, does not exist at all.

For a start, the influence made out of thin air disappears not so quickly. Its steep drop is possible only in the case the rise in the journal status is not accompanied with an inflow of other materials capable of "generating" references. Further, a key question arises: what publications maximally favor the rise of the impact factor? The same Sen complaining that it is always difficult to predict the type of papers with most large potential of citability admits, nevertheless, an exception – review ones. On a whole, they are certainly mentioned more often than any others. And consequently, most references are drawn by publications which, by definition, do not contain something fundamentally new. Thereby, it is confirmed once more: new ideas cannot, in principle, be the leaders of influence or the models of quality if to judge on them by mentioning in literature. Finally, there is a "technical" flaw in calculating the impact factor. It is determined for two years, while that period embraces the phase of "hot" response. During such a short time, it is feasible to grasp and take in only what is comprehensible in main even without getting down to the bottom of matter. All essentially new is difficult to learn and demands of efforts and time, resonating with a fairly good delay. Hence, the real influence of a journal not on the routine life of science but on the progress of knowledge, in principle, cannot be revealed by its impact factor. All the more, the reputation of edition should not be extrapolated on texts come out in it. That is why the list of published works in itself, however impressive it may seem to be, expresses adequately neither the contribution to science nor the research potential of scientist.

First, too often, researchers - even the most outstanding - publish run-of-the-mill, insipid works that are made "to order" or simply "for an appearance". Such papers bear little meaning "charge" and, all the more, do not contain anything new. Some of them can be used with the purpose of enlightenment; others perhaps have a polemical value; there are also such ones which chew over the ideas and arguments known since long ago, helping increase the number of their adherents. And even if to put aside that most of such texts does not perform so much as these functions, it must be admitted that they by no means concern with the accretion of knowledge.

Second, even those scientists who come forth with new ideas frequently publish a great numbers of papers rehashing them in different ways. Saving nuances and tinges, it is hard to find out significant distinctions between these publications. Sure, they can be of use, too. And sometimes, such "elaborations" even suggest new ideas to other authors. But there is no accretion of knowledge there, as well. Nonetheless, they make the list of publications longer not worse than the texts full of breakthrough ideas do.

And third, even if to take into account only those works which advance and prove new ideas one cannot disagree that their value for science is far from being identical. Some of them comprise a whole series of original ideas, while in others a single one is scarcely traced. Several works propose sizable and breakthrough solutions, whereas the great bulk confine themselves to small improvements. Then, is it admissible to put on the same level so diverse papers? It is unreasonable, indeed, to believe that the author of several dozens of articles which discuss variations of an experimental procedure or critically examine the inferences from a theory, gets, by significance, far ahead of Mendel with his very modest list of publications on units of heredity. Furthermore, few will argue against that even if Einstein had written nothing but the paper explaining the photoelectric effect, which became, in fact, the prelude to quantum mechanics his contribution to physics in that case, too, would be incomparably higher than of many those whose "record of service" numbers hundreds of items.

True, in the today's practice of science, the authors of numerous "pointless" texts coming out in "respectable" editions not rarely are valued higher than the geniuses making conceptual breakthroughs (such as the mathematician Perelman). And sometimes, to the forefront get pushed quite outright charlatans feeling no aversion to falsifying the results and cynically rising up the ladder of advancement. Thus, in the beginning of 80s of the last century, in Harvard - the heart of American biomedicine – the story of Darsee, the pupil of Braunwald who was one of the leading cardiologists and physician-in-chief for two of the most prestigious hospitals, had rang out. The young scientist had published during two years nearly a hundred of articles and abstracts. Thereby he not only urged his mentor to press for setting up a separate laboratory for the talent but also aroused jealousy in the colleagues which, fairly not believing that such an amount of works can be performed within so short period of time, got an eye on him, and he had been, at last, caught at forging the data. True, owing to Braunwald, the incident had been hushed up and Darsee, ousted from office, was continuing to work in the laboratory and conducting experiments, particularly within the framework of a project funded by the National Institute of Health (to the sum of nearly three quarters of a million dollars), and, just as if nothing had happened, publishing his papers. But then, suspicions came to those representing the grant-giver, and they sounded an alarm. A committee appointed by the Harvard Medical School confirmed that he manipulated his studies (Broad, Wade, 1982; 13 - 15).

So, unluckily to Darsee and despite the protection of one of the highest "hierarchs" of science, the fraud had been discovered pretty quickly. However it is not a typical case. The protégé behaved defiantly and impudently, not only not being afraid of exposure but virtually neglecting all the precautions. His "achievements" were striking eyes, while excessive self-confidence made him vulnerable. Otherwise he most probably would have continued his ascent and become a pace-setter in science. For, in the atmosphere of formal exactingness – as more as possible publications and accounts, and actual connivance – subject to protection on the part of the "pillars" of a scientific monopoly, fraud and imitation become imprescriptible attributes of organizational life in science, increasing the "penetration force" of ones and undermining the motivation of others.

Not in vain, the Nobel laureate Higgs has been distressed by that in the academic culture which took shape in the "big science" with its rubber-stamping the publications, he could not succeed in what brilliantly performed in 1964. The scientist admitted that when the system of the evaluation of research had been introduced in his department he became an embarrassment. As he was not enough fecund, to the question about his last publications had to answer: "None". More than that, as it has been revealed, the folk at the top would get rid of him as early as in 1980 if he had not been nominated for Nobel Prize (Aitkenhead; 2013). Nevertheless even so obstinate administrators having no need for a scientists who has published since making his main discovery less than 10 papers must have a completely clear notion of that between the number and respectability of publications, on one hand, and the contribution to science, on the other hand, lies a chasm.

Little is added to the list of published works by their citing. Sure, there is certain correlation between the references and the other forms of recognition on the part of colleagues and the scientific community as a whole. That makes the illusion that high citability in itself is a sign of the quality of publication. However a more profound analysis of the data distinctly shows that the quality, and, all the more, the originality of findings, is by no means the single or at least the chief reason for citation.

In the mid of 60s of the past century, a study has been undertaken concerning the interrelation of the quantity of publications and their quality as measured by the number of references, and the acknowledgment of the authors as expressed in their awards, renown, and the position being occupied by them in reputable institutions. 120 American physicists had been taken and divided into 4 categories – depending on how much works they published and how often were cited. Into the first group had been put "fruitful" authors (many publications and high frequency of citation), into the second – "mass producers" (a lot of published works but few references to them), into the third – "perfectionists" (less papers published, almost each of them getting wide response), and into the fourth – "silent" ones rarely writing and still more seldom being mentioned.

It appeared that in the first and third groups, 90 or slightly more percent of scientists had received awards, while in the rest two, respectively 64 and 57 percent. Further, essentially more than a half of the representatives of the first and third groups (68 and 55 percent) were well known in scientific circles, while of the physicists of the second group, despite the abundance of publications, only 29 percent were familiar to colleagues across the country, not to mention that those from the fourth group were staying in shadow (5 percent). It should seem that the more is the number of published works and especially of references to them the higher the value of the results obtained and, hence, the contribution to science of the researcher himself.

But is it the case? Apparently, no. First and foremost, the approach to the analysis was not quite correct from the methodological point of view. Particularly, the authors who had published not less than 30 works were regarded as fecund, and the publications which received not less than 60 references as of high quality. Accordingly, everyone who had not lasted out a little till this level (say, that having published 29 works, or mentioned 59 times) was automatically recognized an underproductive and not enough "conditioned" researcher. But that is not the main point. Not so important were, as well, the doubts tormenting the authors and casting shadow on the analysis. For instance, they noted that the reward system acted not identically in different research departments. To boot, by their observation, "sheer quantity of publications is more likely to be used as a criterion of promotion in the less prestigious departments", while "quality research is more often rewarded when it is produced by physicists in high ranking departments" (Cole, Cole, 1967; 390). But the weightiest argument against their general conclusion, not noticed by them comes from the confrontation of the series of data with the offices of those who had fallen into the sample.

At a closer examination, it gets evident that the greater share of persons holding positions in reputable organizations are among the representatives of the third group (77 percent). No wonder that their voice is well heard. Although there are essentially less "known" persons in this group than in the first – 55 percent versus 68 – they are cited no less than their more fecund colleagues. Why? Because they are "perfectionists", or, rather, in view of their higher rank? Is it not simpler to suppose that it is a manifestation of social conditions and orientations of the citing – from diverse forms and degrees of dependence to striving for enlisting the support of the strong of this world? Quite natural, the little cited are several times less familiar to the general public in science, just as few know the "silent" ones. However it seems to be very noteworthy that among the "mass producers" publishing frequently but being rarely mentioned, the quantity of the known is precisely equal to the number of those high-ranking. At that, those from "top" institutions yield to none either in awards or in "reputational visibility", that is, in the number of those who have heard of them but are not familiar with their published works (table 1). Not to mention that the rank of institution an author represents correlates with their "citability" roughly half as strong again as with quantity (Cole, Cole, 1967; 385).

Table 3. Corresponding figures of 4 groups of physicists as to diverse forms of their recognition (adapted from: Cole, Cole, 1967; 385 – 387).

Types of physicists	Quantity (number) of works published	"Quality" of works (frequency of citation)	Share of the awarded	Share of the prominent[1]	Share of those from top institutions	Share of those with reputational visibility[2]
I - "Fruitful"	High	High	90	68	58	42
II - "Mass producers"	High	Low	64	29	29	21
III - "Perfectionists"	Low	High	91	55	77	41
IV - "Silent"	Low	Low	57	5	27	7

[1] At least fifty percent of fellows are familiar with their works.

[2] Number of colleagues who heard of them/number of those not familiar with their works.

This circumstance allows of looking at citing in somewhat different way. One has to admit that references characterize not only the work mentioned but also the purposes and style of the person who makes them. Some deem at all, that, mentioning a source, the author not so much evinces what precisely has influenced the course of his thought as tries to find solid arguments in favor of his own stand, for the purport of publication is "to sell a product" (MacRoberts, MacRoberts, 1996; 440 - 441). Others go still farther, likening the choice of citations to "packaging a product for market" (Law, Williams, 1982; 543). That is, the references express the marrow of what is expounded no more than a bit of paper a sweetmeat has been wrapped in conveys its taste. And hence, they have no direct relation to the contents of the text but, rather, are called upon to reconcile readers with it.

Sure, it would be an exaggeration to think that citations are chosen arbitrarily. A simple semantic analysis displays the "kinship relations" between the text and the works it refers to. Thus, Song and Galardi marked out in a scientific database 21 articles which had received multiple references, and measured their likeness to the publications where they had been mentioned. Thereupon, the degree of the semblance discovered was compared with that being observed in the absence of citation. It had been revealed that in the first case unlike to the second, a statistically significant semantic relation was registered (Song, Galardi, 2001). Consequently, at all the freedom of choice the author enjoys, the range of possible references is fundamentally bounded and exactly cannot be of any kind. Furthermore, the demand of convincingness itself which should be obtained through citing implies that between the text and the works being cited must exist, in the general case, some rather close conceptual relation. Elsewise the references scarcely could serve arguments bolstering the author's stand.

However there are great doubts as to that the mention of a work in a publication is an equivalent to the acknowledgment of its contribution to making and developing the idea put forward in that publication. Certainly, it is an oversimplification to believe that, citing recognized authorities, every author tries to have hung on their fame and therethrough to impart some more persuasiveness to his own text. Garfield had composed a cumulative index of scientific citation for the years 1975 - 1979. It appeared that out of 10 million 641 thousand papers which had been mentioned at least 1 time, only 6.3 percent had been vouchsafed no less than 10 references, 1.5 percent – no less than 25, and 0.4 – no less than 50. As regards those works which had been cited over 100 times their share made barely 0.1 percent (Garfield, 1985; 406). Hence one may conclude that there is virtually no "canonical code" of works being mentioned, without fail, by nearly all who enter into a field of research. But does it mean that the appeal to authority is not a dominant at the choice of citation?

Zuckerman maintains that at such a distribution of references which has been revealed by Garfield, it is not worth considering them a means of persuasion. Otherwise, in her opinion, the shares on which fall a great number of references would be much higher (Zuckerman, 1987; 334). But no one insists that hanging on authorities or imparting convincingness to a text are the only purposes of citation. There are many other motives urging the authors to citing – from "minute" and "extraneous" to quite solid and immediately pertaining to the case. Among them one can mark out, for instance, pleasing the editor, scientific advisor, reviewer, or colleague; striving to find for the idea historical parallels or allusions; emphasizing the belonging to a school or a circle of like-minded scientists; etc. On the other hand, it is well known that the vast majority of publications glide absolutely unnoticed. Even if they are mentioned it is done by either the authors themselves or their pupils, subordinates, and other dependant persons who proceed from quite specific considerations. Therefore, the top of the pyramid of references is virtually doomed to be narrow. One should wonder not at that somewhat more than 6 percent of works have gathered no less than 10 references but, rather, at the quantity of those fallen into this category. It came up to 670 thousand. And even the number of those mentioned during 5 years over 100 times reached 10.5 thousand. Taking into account that authority is a "piece good", it is difficult to get rid of the thought that just the authorities make the core of the last group of mentions.

Thus, citation evidences merely the popularity, authoritativeness, or – at the best – "neededness" of the work or author, their being in demand on the part of the rest of researchers. But does it tell of their originality or profoundness? By no means so. The very fact of quick recognition of an idea is an indirect sign of that its originality does not reach a sizeable extent, for no truly breakthrough notion can get some wide support at once. For that, certain rearrangement of the thought of colleagues is required, what takes a rather long stretch of time. Say, during all the remaining third of the XIX century after the Mendel's paper on the "units of heredity" had come out only 4 works referring to it, none of them directly touching on the idea advanced (Petrosyan, Petrosyan; 2006; 182). Consequently, the evaluation of scientific findings on the basis of the indexes of citation which perhaps has a sense as to well known and established ideas loses any significance as soon as it draws nearer to the frontier of the "unexplored". Standards and formalisms turn out to be impotent where the primary importance should be assigned to non-ordinary approaches and unexpected insights.

Then, what to do?

The main thing needed is rejecting the exceeding schematization of evaluations and judgments and shifting the emphasis from the formal criteria onto substantive ones. It is necessary to ask not only and not so much of the list of publications by the researcher or their citation in other papers as of the ideas suggested by him, their validity and possible applications. How much texts are written by the scientist, in which editions they have published, who and in what connection has cited them? – answers to these questions are not of utmost importance. Much more fundamental and decisive for making evaluation are the points of the accretion of knowledge he claims to, and in what way and with what exactly he bolsters them.

Such an approach has at least two advantages. On one hand, it tears off the tinsel from many considerable lists of publications where, behind the long train of diverse titles, the scarcity of ideas and the lack of originality are hidden, and from the other hand, allows the innovators to enter a competition with those who prefers to move with small steps and refit the edifice of science without sizeable rearrangement. It means not in the least that the breakthrough ideas are henceforth given "green light". However, clearly, it will be much more difficult to dismiss them out of hand. And thereby, they will obtain more opportunity to get to scientific mind. And one more wicket will be half-open through which original ideas can penetrate into the body of recognized knowledge.

6. The Vivifying Doubt

No less important factor of assimilation of new ideas is the receptivity of scientific community, its inner readiness to making sense of and applying non-familiar and non-standard views, which rises in the ambience of constant exchange of notions. That is why in organizations and communities, as much as possible centers of live communication should be created. It is not necessary that they be face-to-face, though direct contacts stimulate conjectures and insights best of all. The key condition is the presence of institutional mechanisms allowing the innovators to enter a dialogue with those working in allied fields - and not only with potential supporters but also with overt critics. Elsewise it will be infeasible to bring up to scratch even the most prospective idea.

The chief factor in such a communication is the innovator's opportunity to answer to his critics, adduce counter-arguments, and demonstrate that his stand is not so weak and shaky as it could seem to be if to proceed from superficial evaluations given by opponents. That would make the critics formulate their judgments on the new idea from the very outset more responsibly, and, therethrough, mitigate the resistance coming from the tradition. To boot, the author of the new idea would get a real chance - not by way of exception but within the normal procedures – defend his position and bring to the opponents' notice some additional arguments. And besides, it would allow of overcoming, at least partly, the main enemy of new ideas – the conspiracy of silence which usually grows around them. As the most simple and effective way of fighting with those is not disproving but ignoring them, one needs no more to resort to criticism and adduce counter-arguments; moreover, he can avoid at all the risk to pass for an obscurant barring the path of intellectual progress.

The lesson given to the Persian Satrap in the Asia Minor in the connection of his order to forget Herostratus has been well learnt by his followers. The first they do when meeting with a disliked novelty is not criticizing or debunking but hushing it up. And only after it becomes evident that the novelty inexorably gets noised abroad, they begin to act overtly. That is why a secured opportunity to present the position and freely discuss the ideas advanced and as much unalterable right to publicly answer to criticism are of the most important prerequisites of curbing the deaf resistance to the new.

Like mechanisms formerly were present in the scientific community. One can recall, for instance, the heated controversies that were taking place in private correspondence or public debates on the pages of books and journals. Now, such precedents are met with very rarely. The criticism itself has become petty and shallow. It claims to be politically correct and, therefore, looks largely formal. The parties stand not so much for ideas, as for their place in the sun within the professional community. Consequently, when outside, extraconceptual factors are not touched, they try not to hammer each other, bearing in mind that themselves can draw fire from adversaries whereupon their image and social status must suffer.

A semblance of scientific dispute is yet kept where the vulnerability of participants does not reach the critical level or they have solid arguments bolstering their position. Otherwise prudence and evasiveness reign in reasoning. To be at a distance and to avoid conflicts – this is the motto of ordinary scientists. More or less weighty refutation or critical remarks are encountered chiefly in authors from "advanced" (acknowledged) research centers and key natural sciences (physics, chemistry, biology, etc.). As regards the representatives of humanities and liberal arts (psychology, sociology, anthropology, etc.) or of the "science periphery" they think not so much of promoting their ideas or debunking the opposing conceptions as of right selling themselves. These researchers mostly have no need for subverting the authorities – rather, they find it necessary to enlist their support.

26 researchers from an American province (University of Iowa) were asked to fill out the form about what motives they were guiding by when deciding on the references in the last published works. It came out that the main thing was to impart some persuasiveness to their texts. At that, only in 2 percent of cases, they mentioned the publications of colleagues in a critical manner (Brooks, 1985). That is quite natural; when self-displaying in a positive light becomes the key priority, few think of refutations, and, to boot, seldom. A similar picture has been observed also in the study of psychologists' motivation at citing. The analysis of the journal papers of 310 authors revealed that they only in very rare instances resort to negative appraisal of the colleagues' publications (Shadish, 1995). Can one talk of disputes in such a context?

What today is named debate is, in most cases, far of being such. And even these largely truncate and emasculated discussions rarely touch upon the radically new. They are conducted usually on familiar topics being of interest to the bulk and, therefore, not pregnant, by definition, with conceptual breakthroughs. To table a new peculiar idea in a reputable edition the researcher must have carved out a name in science. But radically new ideas seldom come to those who already have it acquired. They more readily occur to "no-names" (young and "hungry") who have quite a lot to prove to the world around as yet. However these "immatures" hardly will be allowed to open a dispute on a "strange and confused" theme on the pages and forums purposed for reputed persons. That is why it may be asserted that true innovators in most cases are doomed to "unrequitedness", and until they can break the silence and find their tongue, the resistance to the new will scarcely be driven into reasonable bounds.

By Way of Conclusion: A General Recipe

How to help new ideas carve their way and give them a chance to be heard? And how to correct the guidelines of science policy, so that to provide more support for radical innovations in knowledge? To put aside details and minor factors and formulate briefly and embossedly, at least a series of necessary steps directed at removing artificial barriers from the way of new ideas and mitigating the opposition to them must be accomplished.

1. Science should be open to world as much as possible. The general public has the right to be in the know as to the progress of knowledge and to trace its tendencies and prospects. Sure, it cannot supersede the inner mechanisms of managing science. But that is not requisite, though. It would be enough to carefully watch what occurs within science and, from time to time, step in and correct the events when the order in it goes too far beyond the interests of society.

The point is not only that scientists should not be out of control. Still more important is the choice of priorities and objectives at which science is aimed – especially as for those demanding of great concentration of resources and flared-out forms of organization. The control over this choice may not completely be passed to scientists. Their function consists in proper comprehension of "social orders" and their translation into the language of scientific problems. Moreover, these problems and research plans based on them are subject to societal evaluation which again is not a prerogative of the scientific community alone.

2. A special significance should be attached to the inner transparency of science. Each researcher needs a distinct understanding how and by which criteria scientific findings are evaluated, in what way and on what ground the appointment to the offices are made, and what underlies the awards and distribution of funds. Only substantiated judgments being announced openly and publicly and personal responsibility for them on the part of evaluators can somewhat restrain the arbitrariness and oust selfish considerations from decision-making.

No "blind" reviewing. The names of reviewers must not be hidden from people. Anyone should have an opportunity to familiarize himself with the reviews, confront them with the subject under evaluation, and pass his own judgment on how exact and deep they reveal its merits and demerits. No secrecy of ballot. Vote should be not merely open but also reasoned and justified. Those voting in committees and commissions must back their stands with arguments – preferably in writing, or at least explicitly registered in the minutes.

3. Any monopoly – all the more on truth – is fraught with stagnation and slowdown of self-renewal. But left to bureaucratic hierarchy, it gets increased manifold and entirely distorts the function of appraisal, emasculating its content. And therefore it is necessary to maintain the multiplicity of autonomous and equipollent centers of evaluation (funds allocating resources; associations and unions granting licenses and accreditations; committees awarding prizes or decorations; journals deciding on publication; universities conferring academic degrees; etc.). But since they are in complicate and multi-level dependence on the forces acting around them there can be no "free decision" in full sense. The only thing in hand is to secure, as higher as possible, the independence of evaluators from each other. But to keep up the engine of the growth of knowledge the multiplicity and alternativeness are to be combined with the institutions' own particular interest in performing quality appraisal. Only taking high advantage from the success in competition, they will begin to prioritize innovations, and so innovating will become a key leverage for raising their authority.

The evaluation of scientific results should be "flatten" as much as possible, and the role of hierarchical structures reduced. Sure, it is hardly possible to get rid of hierarchy at all, for just on its base any full-fledged management of collective work is built. However science is of those few spheres of activity with high uncertainty of outcomes and central role of creative initiative where hierarchy, may be, more hampers than aids. Consequently, to the golden rules of the management in science must be added the motto: as few as possible levels in the hierarchy and as easy as possible passage from one to another, and above all, as simple and efficient as possible communication between them.

4. The institutional centers of evaluation should be complemented with personal ones – through endowing the most outstanding scientists with a right to single-handedly, though openly and publicly, appraise, within the bounds of distinctly outlined power granted to them, the scientific findings and the potential of researchers. These peculiar "lifts" will allow the innovators, especially young ones, to evade bureaucratic impediments and get to scientific audience swifter and to introduce into the practice of their interrelations with the machinery of "big science", a personality dimension. In the issue, original ideas will obtain one more springboard.

On one hand, the personal responsibility, together with the openness and necessity of distinct substantiation of decisions made, essentially levels the possible risks. And on the other hand, individual "judges", propping up major centers of evaluation, inevitably intensify the competition between them and, at the same time, widen the net of channels through which new ideas get promoted and thereby secure some chances even to the not quite enterprising creative persons. But despite the rivalry, the areas of influence of institutional and personal evaluators do not coincide. They, rather, merely intersect, and each group retains, besides common functions, its peculiar ones it performs better than the other party does (particularly, material and organizational support on the side of institutions, and guidance and live communication on the side of persons), and therefore can be mutually complementary. That is why taken together, they are capable of constituting a more flexible and dynamic mechanism of evaluating scientific results than that of clumsy and unconcerned monopolists catering for their own interests far from the progress of knowledge proper, and eaten away by institutional bureaucracy which run the show in today's science.

5. It is necessary to overcome the tendency of standardizing the treatment of scientific results, for they should – by definition – go out of the bounds of standard. The most important in research is not how much it complies with the established canon and accepted criteria. Sure, that does matter, too, since just through the adopted and conventional, the mutual understanding ("common language") between the reviewer and the author gets secured. However much more important is what precisely the research adds to established

knowledge the latter does not encompass (new elements, changes in the structure, non-ordinary applications) – at least in an explicit form.

No formal parameters (the quantity of publications, the number of references to them, the category of edition where they are placed, etc.) may serve a ground on which the evaluation of scientific findings or the researcher who has obtained them is made. The only measure is the ideas advanced by him. How much original they are and what vistas open before science and practice? Are they substantiated, and in what way? The main function of science as a social institute and of any individual researcher is the production of new knowledge. When the work does not contain it nothing is to be evaluated. Once the measure of its newness is established, one may set out to a broader evaluation. But they can only supplement the primary rating, not supersede it.

6. In the "big science", increasingly often and to a greater extent, the center of gravity shifts from the censorship and immediate suppression of ideas out of favor to the prohibiting of their emergence. When some directions of search are not supported they scarcely beget conceptions one afterwards has to fight against. The first a dominating group does when meeting with a disliked novelty is not criticizing or debunking, but hushing it up. That is why a secured opportunity to present an idea and freely discuss it, and as much unalterable right to publicly answer to criticism are of most important prerequisites of curbing the resistance to the new.

A key role at making and polishing of conceptual novelties pertains to the atmosphere of benevolent doubt and sound skepticism. There is no progress of knowledge where prophesying of the "pillars" is taken for an absolute truth, while insights of the "humbles" are dismissed out of hand. Any idea put forward for consideration by the scientific community should be subject to substantial and grounded criticism. The unvarnished, concerned intercourse – with captious but just appraisals – is a prerequisite of viability of emerging knowledge. In the scientific culture, a beginner and a Nobel Prize winner should have equal right to express their ideas and, what is even more decisive, be equally vulnerable to arrows of criticism. Actually, that is a manifestation of not only patronage to the young but also respect to the venerable, because criticizing their ideas means they are taken for living, with still real cognitive, not authoritarian weight in science. For, the ideas out of critical discussion are dead and cannot be a source of inspiration for others.

Sure, these conclusions are of general nature and need to be elaborated on. But they outline the framework of interrelated measures which allows of transforming the current practice of evaluating scientific plans, findings, and personnel and directing them at the dynamic renewal of science. In other words, these measures indicate the ways science policy should be rearranged in to keep and, all the more, to increase the pace of the progress of knowledge. Elsewise – if the current state of affairs is preserved or its organizational reformation is confined to skin-deep or imitative mending – the break on the renewal of science will start working in full, and with each new generation, the investments in research and development will return increasingly less accretion of knowledge. But for knowledge-intensive and high-tech civilization, that will be the worst, if not to say ruinous, scenario.

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