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Science and Its Malfunctions

Klaus Fischer

Universität Trier, Germany

fischer@uni-trier.de

Abstract: This article explores the nature, causes, and genesis of malfunctions of science. Topics explored are: deception; subordination to dominant spirit and paradigms of the times; impacts of dogmatism and social interests; ingroup-outgroup behavior and other effects of the social structure of science; inappropriate peer review and malfunctions of prevailing academic review system; misappreciation of innovative research; impact of networking and loyalties on scientific progress; impact of economic interests and liaisons; pathological interpretations of scienticity and validity of scientific results; defects of scientometric measures as indicators of achievement, as a manifestation of the impact of medial code on science; trustworthiness or lack thereof of Science Citation Index as measure of research and scientific achievement. The impact of social, economic, political, cultural, linguistic, medial, religious, and other codes on scientific process and behavior of scientists are explored.

1. THE PURPOSE AND FORM OF SCIENCE

(1) Before speaking of the malfunctions of science, its functions need to be identified. This alone could become a very lengthy task. For what follows it appears that a very general characterization is sufficient, without exhaustive clarification of detail. According to this minimal definition, science can be functionally characterized as that part of society, which aims at the acquisition of "certified" information, i.e., information tested with a positive result according to the currently accepted methodological rules, such as widens our knowledge of certain aspects of reality. Although the daily tasks of a business consultant, a lawyer, an artist or a priest are partly of the same kind, the extension of knowledge of a certain part of the world is the scientist's primary goal and *not only* an instrument for

Professor Dr. Klaus Fischer holds a faculty chair in the department of philosophy at the University of Trier, Germany. His research interests are in the fields of history and philosophy of science, sociology of science, sociology of knowledge, and philosophy of nature and in selected areas of modern philosophy such as Neurophilosophy and Neoconstructivism. More specific priorities include thematic foci on specific individuals and schools in science and scientific theory such as Popper, Kuhn, Feyerabend, and the Vienna Circle. In the area of history of science, he is interested in Galileo and Einstein. Other interests include history of Empiricism, history of university, innovation processes, possibilities of science, and history of science in the 20th century (nuclear physics, molecular genetics, cosmology, chaos theory, and artificial intelligence). The present article is an abridged English translation of an article by the author, entitled "Fehlfunktionen der Wissenschaft," recently published in German in the journal *Erwägen Wissen Ethik* (Vol. 18, Issue 1, 2007, pp. 3-16).

reaching another goal.

(2) In the empirical history of science, this goal takes on various forms. Depending on the basic attitudes of an epoch, it can metamorphose into the intention of reading God's thoughts, finding the signature of the world, uncovering the beauty of nature or demystifying it, spreading enlightenment, unlocking the cosmic secret, investigating incredible phenomena, satisfying curiosity, or fulfilling personal ambition.

(3) Even researchers for whom science is primarily a means to something else, for example to be famous or rich, to attain social recognition or authority, to gain power over others or over nature, to increase utility for themselves or society, to find peace of mind, to reach certitude of salvation, to pursue religious goals, to further political ideologies, or to save the world, can also make significant scientific accomplishments. The specific mixture of motives, though, leads to conflicts of purpose and to behavioral consequences in which the acquisition of verified information does not serve as highest maxim, but recedes before other ideals.

2. A PHENOMENOLOGY OF SCIENTIFIC MALFUNCTION

2.1. Deception

(4) One of the most marked signs of scientific malfunction would be the increase in the amount and varieties of scientific misbehavior. Recent surveys from the natural sciences in the United States point out that questionable research behavior is more prevalent than usually assumed. A third of all scientists make anonymous admission of questionable behavior and almost forty percent of young researchers report being the victim or the witness of unethical behavior on the part of others (Martinson 2005; Physik-Journal 2005; Kirby).

(5) Whereas science views the uncovering of great instances of deception as success in scientific control which strengthens the system, habituation to many small daily instances of human and methodical failure leads to a gradual erosion of scientific standards. The struggle against bad scientific ethics is not decided by public investigations of "cases" such as that of Friedhelm Herrmann and Marion Brach, Jan Hendrik Schön or Hwang Woo-Suk. It is instead carried out in the gray zone between what is methodically or ethically ideal and what is manifest deception (Fischer 2004a).

2.2. The Spirit of the Times

(6) The effects of the spirit of the times or of political correctness are a subtle source of scientific malfunction. Their negative effects are partly due to the misperceptions it causes, partly due to a false evaluation of scientific theories, problems, arguments, and data, partly due to a degradation or even defamation of those who think otherwise, possibly in good faith. If this game is played in a consistent and industrious manner it can do much more harm to research than direct deception.

(7) From today's point of view those psychologists, therapists and analysts were subject to the influence of the spirit of the times who (especially in the U.S.) thought they had uncovered millions of cases of misused children (Ofshe & Watters 1996), multiple personality (Hacking 1996), abduction by beings from outer space. Elaine Showalter (1997) has invented the term "hystoria" for this kind of intellectual confusion, which can take on an epidemic scale, basing the word on the term "hysteria," the name for a sickness which a hundred years ago the physicians of the time thought upper class women tended to have because of a disposition of their gender.

(8) We do not wish to become involved in current debates, but rather to exemplify the workings of the spirit of the times. Its effect consists in self-attribution of knowledge not really had, as well as in considering some theories, hypotheses, models and interpretations to be far more certain, or far more flawed, than they are. In some cases they take on such a degree of certainty in the minds of those subject to the spirit of the times that those who are of differing opinions are even considered indecent, dishonest, disbelievers or heretics. Sometimes it takes on the form of self-attribution of ignorance (Willgerodt 2004), when available data and arguments are not taken into account, but are considered inadmissible, unreliable, or even as ideological. Areas which are susceptible today are studies of risk, climatic change, intelligence, especially regarding gender and race, atomic energy, mind and brain, and free will.

2.3 Dogmatism and Social Interests

(9) Organized interests and social stratification in scientific systems (hierarchies of status among persons, laboratories, nations) interfere in various ways with the development of research. The *formal* structures of science usually contribute to stability. They tend to open up to new developments only slowly and under pressure from external competition. One particular aspect of social structuring is that which is related to the binding effect of a paradigm. Other than in the case of the institutionalized structures of science, this is rather of an *informal* nature. Like organized interests and hierarchies of status in laboratories, institutes and other organizational units, strong commitments to paradigms tend to hinder innovation, because they impede the free play of arguments, ideas and perspectives, the emergence of alternatives, and the recognition of unusual phenomena. Although bound to the rhetoric of epistemic progress, the critical attitude and the unbiased examination of novelty, research governed by a paradigm rewards only *internal* criticism, small progress within the given framework of thought, the formulation and examination of hypotheses which do not disturb this framework, but rather protect, improve, expand, embellish it. The refusal of all attempts to support radically new ways of thought and points of view, let alone to develop them, preserves the disciplinary matrix (cf. Kuhn) and the informal social structure attached to it. At the same time it also impedes the career of those scientists, usually young, who are connected to what is new (which always involves an informal social structure) and leads to an unjust distribution of recognition for achievements. Normal science rewards the guardians of the ruling consensus and punishes those who threaten to violate it. As a rule those succeed best who appear competent while adhering to current conventions of thought. Only in the best of all scientific worlds, in which many different paradigms compete with one another, would the negative effects of these innovation-impeding reward structures be sufficiently mollified.

(10) This enmity toward innovation manifests itself at many levels. Many examples show that phenomena which institutionalized science at first and in some cases for a long time had vehemently rejected originally, because they were not compatible with the disciplinary matrix of current research and hence had appeared unbelievable, were later able to prove themselves to be genuine. Examples are: the Greek Antikythera mechanism, meteors, solar wind, ball lightning, continental drift, reverse transcriptase, infectious proteins, life at temperatures above boiling temperature and life under extreme pressure, large impact craters on the earth, non-classical inheritance (epigenetic memory), oscillating chemical reactions (Belousov-Zhabotinsky Reaction), giant snowballs hitting the earth from outer space, or strange ionospheric

phenomena accompanying storms (sprites, “elves,” blue jets).

(11) Complementary to those “unbelievable” phenomena are those which science had viewed as certain for years, decades, even centuries, until finally, sometimes in a very short time, they literally dissolved as theoretical frameworks changed: Schiaparelli’s “Martian canals,” van Maanen’s “unwinding” galaxies, phlogiston, the symptoms of hysteria, neurasthenia and vegetative dystonia, the planet “vulcan,” “polywater.” An even more impressive witness of the connection between systems of theory (paradigms), dogmatic attitude (Gernert 1999), and social interests are given by those once dominating theories, paradigms, and world views whose active opponents suffered from lack of recognition of their research achievements or the granting of research opportunities by the system of institutionalized science. Although these theories are now themselves rotting in the junkyards of history, the damage which their faithful adherents caused by the oppression of concurrent ideas and of their representatives can not be made up for.

(12) There are obviously mechanisms which protect conforming (“good”) information and fend off dissonant (“dangerous”) novelties. Well-documented instances in the history of the sciences show two things: 1) the almost limitless facility with which scientists believe whatever fits their world view and possibly gives it more support (Broad & Wade 1984, 136ff.; for the humanities cf. the so-called Sokal-affair: Sokal & Bricmont 1999); 2) the complementary constellation where a practically impenetrable wall of scepticism is erected whenever anything (e.g., “out-of-place artefacts” in archaeology) is reported which might undermine the currently accepted scientific world view, its standard models, and its conventional ways of seeing things. Especially the latter is a strong deterrent to the development of science. It sometimes has tragic effects on individual scientists who refuse to fit into the mainstream and hence it represents a serious malfunction of science for several reasons.

2.4. Ingroup-Outgroup Behavior and Other Effects of the Social Structure of Science

(13) It is well known that many ideas and theories are not attributed to their first discoverers, because they were originally rejected, then forgotten, and later had to be thought out again by others. In these cases it is not the pioneers, but rather the second discovers or imitators who are rewarded and cited for their achievement. An example is Chandrasekhar and his theory of the evolution of stars (Wali 1991, Chap. 6). The young Indian astrophysicist did not publish his theory and turned to a new research field, after he encountered strong resistance from established scientists. More than twenty years later the theory was developed again by others. This case stands for many. It contradicts the spirit of an enterprise, in the internal logic of which only the acquisition of certified information (in the sense explained above) is decisive, not the age, status, nationality, gender, cultural background or attractiveness of those who originate information and technical achievements.

(14) There is another important aspect of such processes, which is of no small significance in connection with the politically propagated goal of rewarding scientists according to their achievements. According to the criteria of assessment of achievement, which does not evaluate retrospectively, but measures contemporary perceptions, the first discoverers wasted their time on “worthless” ideas, instead of occupying themselves with things which were of in-

terest to their mostly higher-ranking and more well-known colleagues. With respect to the opportunity costs of the "wasted time," they and their home institution would have been punished by reduced funding for their farsightedness and their creativity, which bore fruit not immediately, but only after twenty years. Despite its rhetoric of innovation, research politics, intoxicated by the promises of a growing evaluation business, at the beginning of the third millennium furthers such hidden innovation-inhibiting effects by relying too much on the consensus of the scientific community—which as a rule is the consensus of those who are in key organizational positions and hence tied to the political interests of their field.

(15) This innovation-impeding malfunction of science is the expression of the multidimensionality of scientific activity, which is not only a cognitive process, but also a social, political, economic, medial, cultural, etc., one (Fischer 2005 and 2006). This multidimensionality has the effect that scientific activities and products are also subject to social, medial, and political evaluation (to only name these three dimensions), in which the prestige, the publicity, the network of loyalties, the fame of a researcher, institute or university, and its past successes and failures, play a decisive role (Collins 1975 and 1981). It is a matter of trust, of focusing attention, of winning allies, of warding off the danger of potential opponents. Differences of position in the social and political systems of science effect different perceptions of problems and hence different reactions to conflicts and contradictions on the part of those involved (Bloor 1978; Caneva 1981). Which attempts at resolving a problem prevail at a given time, which alternatives one is tolerant, skeptical or hostile toward, will depend largely, although not exclusively, on the distribution of power within the scientific social system.

(16) This coupling of social, political and cognitive structure in science modifies the process of knowledge acquisition in a way which is basically foreign to it. Such interference of systems becomes pathological when the research code is dominated by the code of economics, politics, society, culture, media, religion, etc. (see below).

(17) One of the *social* bases for the development and success of new ideas consists in the subgroup of those lower-ranked researchers who do not expect to raise their status by allying with the mainstream, and by conforming to its rules and thinking. They are eager to latch onto and attempt to support that which is new and, from a social and cultural point of view, is seen as dissidence, even independently of its inherent qualities (see also Douglas & Wildavsky 1982). This is to be understood in a neutral sense. Novelty means merely that the ideas which are prevalent in this social milieu dissent from those which are presently in the middle of the attention of a field and are as a rule associated with higher-ranking researchers. It does not mean that these ideas are more of an aid to the goal of science than those which are current within the institutionalized centers of their field. In such marginal social areas of the same institutions not only do ideas flourish which further research and science but also strange creatures which are innovative and unusual, but do not carry any other advantages.

(18) How is it possible that certain phases or areas of research can be dominated by cohesive social networks and the code of "society" which are based on solidarity but not on methodologically certified knowledge? The reason is that "networking" appears to bring many advantages to the individual researcher. His inclusion in scientific networks satisfies elementary needs for contact with colleagues, opportunities to cooperate, gaining access to restricted in-

formation, and receiving support in the fight for resources. It is a matter of give and take in which the community offers protection and services to the individual in return for his solidarity with the community.

(19) This barter appears more advantageous to the individual than it is, a part of its costs being hidden. Among these costs (or latent functions) of group-building are that the social pre-structuring of contacts effectively reduces the number of information channels which the member of a group generally makes use of. Because of our limited capacity to receive and process information the intrusion of dissonant information into the internal group will be effectively impeded thereby. This is a process essential for maintaining the disciplinary matrix and its social structure, depending in its effectiveness on how coherent and dense the network is.

(20) Since a part of the distorted or hidden information would be able to modify the cognitive matrix of a paradigmatic network much faster than it actually does, the filtering and altering of such information, although inherent to sciences bound to such networks, must be seen as a serious malfunction of science. It lies in the nature of socially organized science. Its negative consequences would be negligible only in the best of all scientific worlds, in which a great number of different paradigms, both on the side of supply and of demand (institutions, journals, funders, evaluators, etc.) would compete with each other and make the borders between the corresponding networks more permeable.

(21) The higher density of internal as opposed to external communication has another function besides the limitation of the quantity of "uncontrolled" and hence possibly dissonant information. It shows the group member how others think and exercises, by subtle reinforcements and sanctions, an orienting influence on his own thought, in particular on the disciplinary matrix which he accepts and on his scientific world view.

(22) A researcher in this position can scarcely be described as a lonely and critical seeker of truth, but rather as a narrow-minded, prejudiced, ideologically inclined member of a "herd in the sphere of thought" (Nietzsche), who believes in a world view which in essential respects has been provided by others and who can only rarely permit himself the luxury of doubt, among other things, because he feels such doubt only rarely, given the way information is selected, and because he knows that open criticism might cost him the solidarity of the network. As long as it can successfully guide the process of research (such as progress is seen by the network) the disciplinary matrix provides for the cohesion of the network just as social consensus provides for the perpetuation of the matrix. This filter-and-reinforce function of the social network in which the individual researcher is embedded is central to an understanding of the dynamics of science. It is also central to an understanding of the causes of intellectual intolerance in any science characterized by firmly knit social networks. Networks are the social bearers of cognitive structures, with the latter providing for a level of information-processing which is optimal under the given circumstances (Fischer 1987, 1988), and in turn solidifying the networks.

2.5. Inappropriate Peer-Review

(23) Further symptoms of malfunctioning in science can be found in the system, almost universal today, of peer review (cf. Fröhlich 2002; Fischer 2004b). Among the field experiments, which have been devoted to determining the reasons for the problems of this evaluation system, that of Peters and Ceci from 1982 is the most clear

and the most disturbing (Peters & Ceci 1982). The sobering result is that when secondary indications of quality are absent, the psychological referees seem to become blind to the inherent cognitive qualities of a piece of work, and the experimentally determined rate of reacceptance of articles which had already been accepted 18 to 32 months before, by the same journals, sinks to that quota which might be statistically expected. John Ziman's reaction shows the consternation and the helplessness of many commentators regarding this result (Ziman 1982, p. 245).

(24) One possible attitude toward such empirical studies is a flight to sarcasm. An example thereof is the "author's formula" which one of the investigators of the peer-review system, J. Scott Armstrong, also coeditor of the *Journal of Forecasting*, has presented on the basis of his experiences and the available empirical evidence. Authors who want to improve their chances and accelerate the acceptance of their manuscript should "(1) *not* pick an important problem, (2) *not* challenge existing beliefs, (3) *not* obtain surprising results, (4) *not* use simple methods, (5) *not* provide full disclosure, and (6) *not* write clearly" (Armstrong 1992, p. 197).

(25) Is this pessimistic view justified? What could a crucial experiment, which might answer this question, look like? How it treats its elite is decisive for the development of a science. According to a more narrow definition which is based on the goal of research, those belong to the elite of science who contributed decisively, in some way or another, to the acquisition of new knowledge. The real difficulty lies less in the determination of achievements far in the past than in those of the present or the recent past. Is today's system of evaluating scientific achievement capable of dealing with this task? According to the experiences of a considerable number of high-level researchers we must say: very often not!

(26) From the biographies and autobiographies of scholars, researchers and inventors we know of many examples in which the peer-review system has led to sorry misestimations of achievements of the highest-level. In a substantial number of cases referees of scientific journals have refused central contributions of later nobel prize winners or others who have become world-renowned, because they did not recognize their value. What trust does a system deserve which cannot even recognize quality of the highest kind in a reliable manner?

(27) In other cases work was in fact published in spite of difficulties, but not given recognition for a long time. Decades can pass until high-level work is recognized as such and correspondingly honored in the form of citations. For the political program of implementing a market-oriented instrument of guidance into science which would reward scientists according to their achievements, this amounts to a disaster. It is just the most innovative contributions which are most often misrecognized, and as a rule the young talents are those who bear the consequences and turn away from research in disappointment.

(28) To summarize, the following conclusions can be drawn from the available empirical investigations. One can say of the system of referral and evaluation that:

- It does not have clear standards of scientific quality or it does not apply them consistently. In the past, dozens of pioneers of future science and technology have been rejected, defamed, ridiculed, which sufficiently proves its inability to objectively judge the achievement of independent and creative researchers with unconventional ideas (many examples in Fischer 2002; Horrobin 1990; Sommer 2001; Hook 2002; Thiel 1944).

Empirical studies seem to indicate that the main problem might consist less in the criteria themselves than in their practical application (cf. Neidhardt 1988, Tab. A12, p. 148). From a historical perspective even many criteria are also questionable.

- It rewards conformity or existing alliances with certain theoretical and methodical orientations, while insisting that this conformity should not be complete, but should show individual nuances—a proof of originality. The main function of this tactical subordination consists in demonstrating the connectibility of the scientists' research and to convince referees that the author or applicant is not an intellectual adventurer, but understands himself as a part of the community effort of science. From a sociological point of view, it is a matter of social control in the system of science, which takes on the form of self-control in anticipation of the consequences of violating a norm.

- It favors certain paradigms, themes and types of argumentation. A comparison of articles submitted to the *Zeitschrift für Soziologie* between 1972 and 1980 with those published by this journal shows that the chance of publication depends very strongly on the theoretical orientation of an article. The average rate of diffusion of articles from the circle of functionalism, for example, was eleven times higher than that of articles from that of critical theory and five times higher than that of articles from the area of symbolic interactionism (cf. Sahner 1982, p. 88, Table 4). Sahner conjectures that journals with a system of referees are more impenetrable for innovations than those where editors are granted the decision about manuscripts. Altogether this publication system disadvantages above all those theoretical orientations that are only insufficiently represented by the preferences of editors and referees. Individual preferences are thus translated into quality judgements.

- It respects, or even submits to, the social status, the political power and the fame of applicants, institutions, journals and countries. This phenomenon has been called the "Matthew Effect" by Robert K. Merton. It can be found at all levels of the scientific system. Although it does not always have a negative effect on research, it must be viewed as a serious malfunction of the scientific system, because it rests on effects which, being mainly social, political and medial, are foreign to the system. In extreme cases it can lead to misuse of office and of position (Martin 1992; Sommer 2001).

- It avoids risk and disadvantages projects which are innovative, explorative, speculative, interdisciplinary as against those which, because they can be located in a clear methodical and theoretical framework, will bring about small, but certain scientific results.

2.6 The Misappreciation of Innovative Research

(29) Empirical results concerning the function of the peer review system reveal that it encounters particular difficulties with novelty. This becomes especially visible when novelty consists not only in the application or improvement of a known set of instruments, but when novelty departs from the framework and does in fact venture into terra incognita (Fischer 2004b). Although moving into new territory is one of the deepest sources of motivation for research, the system of evaluation tends in general to impede novelty, as soon as it has gone be-

yond a certain format. This is not its intention, but it is a consequence of the effort to minimize the risk of misjudgment. Consequently the referees for the evaluation of applications for resources (outside funding, publication venues, etc.) will prefer small, but certain gains to the uncertain chance of a jackpot. This means that well-known names and institutions, as well as projects with a certain conventional dress already have a bonus at the beginning of the race. A sign of the conservatism of the system of evaluation is that the average referee prefers articles with positive and conventional results to those with controversial ones (Sahner 1979; Mahoney 1976), putting manuscripts at a disadvantage in which a hypothesis which is currently of high regard is falsified (Armstrong 1997, p. 71; Martin 1997, Chap. 5).

(30) Among the researchers, well-known today, whose innovative ideas once experienced vehement rejection or disinterest on the part of the "scientific community" are Alfred Wegener, Alan Turing, Konrad Zuse, Konstantin Ziolkowsky, Hermann Oberth, Peyton Rous, Mitchell Feigenbaum, Frank Rosenblatt, Barbara McClintock, Stanley Prusiner, Andrei Linde, Günter Blobel, Noam Chomsky, Karl Popper (further examples in Fischer 2002; Horrobin 1990; Sommer 2001, Nissani 1995). It affects first of all younger, unknown, non-established scientists who have distanced themselves too far from actual consensus, such as Polly Matzinger (Hanisch 1997), Lynn Margulis (Margulis 1999), Candace B. Pert (Pert 1999) or Bonnie Bassler (Breuer 2003), also such as have violated the borders of their discipline. Occasionally it also hits established scientists, even nobel prize recipients when they depart too far from the consensus of the majority (e.g., Brian Josephson), or if they too openly admit that they themselves do not know what will come out of their investigations.

(31) This openness regarding results is the natural characteristic of innovative research, but it is unsettling to those always eager to have everything "under control" and be accountable to others, for example for granting outside funding. This structure of research funding does not adequately correspond to the manner in which successful science functions, which must always count on what is unforeseen and be able to react to it flexibly. It always has chaotic (non-calculable) properties, because it cannot predict its own future, in particular the many intervening accidents, the dynamics of interactions arising in certain situations, the new factors emerging in the course of the game, the surprising non-linearities evolving from chaos; as Günter Blobel says: If you can predict what you're going to do for five years, its probably going to be bad" (Goodman 1995).

(32) That does not mean that research and science are not plannable per se. Although it may be that a great part of science conducted today—and perhaps an even greater part of technology—rests on planning, a strategically central aspect of science and technology seems to be inaccessible to control: the scouting of new paths in unknown territory. Fleming could not plan to discover penicillin, Galilei to see the moons of Jupiter, Columbus to find America, Kamerlingh-Onnes to discover superconduction, Rutherford to hit upon a supermassive nucleus in the atom, Penzias and Wilson to notice cosmic background radiation, Bednorz and Müller to hit upon high-temperature superconduction, Perlmutter and Schmidt to discover the accelerated expansion of the universe.

(33) It can be argued that many discoveries do indeed have plannable elements and are based on theoretical expectations, which have in part been confirmed. There are discoveries and developments the conditions for which are so well-defined that they

could have been called *intentional* if there had been the necessary scientific far-sightedness. In these may be counted flying machines, automobiles, electronmicroscopes, scanning tunnelling and atomic force microscopy, transistors, integrated circuits, computers, atom bombs, fusion reactors, genetically altered creatures, nanomachines, as well as electromagnetic waves and the structure of DNA. On close inspection, each case exhibits, all planning notwithstanding, aspects of the unforeseeable. As an example one may take the development of flying machines or the endless history of fusion reactors. Cases like these show that chance, "serendipity," subjective drives and hopes, but also adjunct innovations in neighbouring fields (Sommer 2001; Roberts 1989), played a greater role than the planners of science would like to concede. The mania which can be observed today for wanting to have all aspects of research under control and to restrict its freedom in order to avoid, because of considerations of cost, "unnecessary repetitive research"—although the latter is an important tribunal for testing and improvement—is responsible for many of the most unsettling malfunctions of science: the growth of monocultures, the withering of variety and hence of the presupposition for the selection of the optimal variant, and the absence of fundamental innovations.

2.7 Misbehavior toward Young Scientists

(34) In well-documented cases researches have misused positional and institutional authority to credit themselves with achievements which were really attained by coworkers of lower status. Ombudspersons for good scientific practice can confirm that sensibility for misbehavior toward young scientists is still very underdeveloped, at least in the German system. Many supervisors, department heads or other superiors still seem to regard it as their right to use, pass on, or include in publications results by doctoral and postdoctoral students and by subordinates, even without their consent (Tröger 2005). In some cases this usurpation of property rights goes so far that results of persons from the groups mentioned have been passed on by superiors or supervisors even against the will of their originators or without indication of origin. Power of position, status, threat of repression are used, in order to keep those affected from attempting to maintain their rights by appealing to university committees, ombudspersons, or if necessary to courts.

(35) Another type of misbehavior, which is especially harmful to the careers of young scientists, is the use of formal authority and positional power in fighting against other paradigms, research approaches or scientific positions. Since new research approaches are often supported by young scientists, a political structure which allows or tolerates such things leads to lack of innovation or even to animosity toward such in the countries, disciplines, organizations or departments involved.

3. CAUSES AND GENESIS OF MALFUNCTIONS

(36) Studies of science have not yet made human deficiencies as causes of specific malfunctions of science a subject of systematic study. This reservedness could be unjustified. It may be that the disinterested search for information which is certified in the sense explained above—such as has no other motive than the investigation of the structure of the world—is expecting too much of human beings from a moral point of view. That may be an ideal which surpasses the measures of an entity guided by considerations of utility which stand in the service of interests, emotions, and desires:

in the interest of social recognition, bettering of economic position, control of the personal environment, warding off of perceived dangers, and resistance to dissonant information. To an entity addicted to fame, sex, money, power, the search for security and sensory stimulation, striving towards truth about the fabric of the world may seem an unprofitable tenet. Biologically based insufficiencies might also play a role in the failure of man to come to grasps with the classical ideal of the scientist's role. Perhaps the causal texture of the world is too complex for our brains—at least at the present stage of evolution. The British empiricists, as well as Descartes, have seen it this way, although they were of the opinion that our faculty of knowledge is still enough for practical life and to find our way to the markets. Research in its originally intended form as the search for that which holds the world together in its innermost core might turn out to be vain from an evolutionary point of view. The success of this enterprise played no role in the selection of our genes so far, our genetic equipment not being optimized with regard to this goal.

3.1 The New Network Ideology: Loyalty or Scientific Progress?

(37) Other factors appear, however, to be more significant for the explanation of present-day malfunctions of the institutionalized systems of science and research. If we consider results from recent sociology of science, we find indications of the sources of misjudgment, distortion, discrimination, and favoritism, which we have already referred to in the paragraph on the phenomenology of malfunctions. The most important factor seems to be the particular embedding of a researcher with given paradigmatic and social commitments in the institutional framework of science. This location creates certain orientations of interest. In one case it can be connected to higher formal authority and to the power of distributing research resources and employment, granting or denying access to publication opportunities, directing organizations, initiating decisions in the system of peer review and establishing interpretations which are binding. In other cases this may all be lacking. The position of a researcher in scientific networks, in editorial boards, scientific associations, allocation commissions, laboratories, or government boards sets up positive or negative mechanisms which can lead to unfair evaluations. Unfair evaluations can also hit those who carry undesirable personal characteristics, the wrong nationality, ethnic group membership, political ideology, gender, or an "unfavorable prehistory" in any desired sense.

(38) One of the conditions for these causes of malfunctions to work in an unmitigated way is the very restricted competition between institutes, laboratories, financers, journals, publishers. In addition to the processes of concentration which already exist, politicians encourage monocultures by openly pursuing the goal of avoiding "double research" in order to save money. This goal is to be reached only in the short term, for without comparative research there is no possibility of examination, by socially impartial and critical judges, of the strengths and weaknesses of the what has been attained. In a system in which oligopolistic or monopolistic structures prevail there cannot develop those variety of ideas, theories, perspectives and arguments which would be necessary for a fruitful culture of scientific discussion and hence for a selection of the best variety of solution to a problem. Even worse, it is only in monopolistic and oligopolistic markets that many of the deformations mentioned above (deception, dogmatism, obsequiousness to the spirit of the times, inappropriate judgement of colleagues, rejection of novelty, op-

pression and exploitation of young scientists) can exert themselves in full force. In markets over which the demanders of research have no control, the suppliers can, if buyers act unfairly, look elsewhere and seek out another institute or laboratory, another boss, another sponsor, another journal, another publisher, etc. In the present-day research landscape these suppliers must, however, realize that variety is only apparent: in many branches of research there are only very few laboratories, sponsors, and journals which really come into question, and they are all bound up in one and the same network of referees and deciders, who believe themselves to be "guardians of standards," but from a sociological point of view are guarding the purity of their world view. Anyone who has spoiled his relationship with a part of the network will not get any hearing elsewhere in it. He is "excommunicated" and can at best pass a marginal existence on the rim of the system as a notorious outsider, eccentric, or fool.

(39) The political tendency, which has recently become manifest both at national levels and in the European Union, of supporting primarily research projects carried by "networks" leads, whether intended or not, to the replacement of knowledge as a motive by social loyalties, the immediate goal of which is to form and foster social ties in the service of economic advantage. Such loyalties can, especially when the networks involved have common "skeletons in the closet" after fighting together, be cemented as "social capital" and take on mafia-like traits. To put it lightly, one could call this *publicly organized colonization of science by society*. Its highest goal is no longer the attainment of knowledge which has been certified in the present sense, but rather solidaristic action toward the greatest possible economic advantage. It should not be a surprise that "networking" has become one of the most important conditions for advancing a career and obtaining outside funding. If the networks are tightly knitted enough and if they know how to work secretly, "metering of achievement" provides no correction to science based on loyalty and orientation toward profit. The potential controllers are either themselves part of the network or they hesitate to criticize it out of rational fear of the network's power to influence opinion. The existence of a competing network improves the situation only at first glance. The judgement of the evaluators is also determined by non-scientific motives in this case. What they do in a particular case depends on the constellation of powers. If there is hope of dealing a clear and permanent defeat to a competitive network, they will not shy from issuing intrinsically, although not formally, defamatory reports. If the competition is equally strong, they will prefer to proceed according to the principle "live and let live," since it must be feared that retaliation in kind will be given. The principle that criticism should be as objective, impartial, comprehensive and open as possible has lost its institutional backing in a science which is permeated by networks demanding cohesive loyalty. It turns into an atavistic reminiscence of a "heroic era" in which science was pursued more "for its own sake." Only a great variety of competitors with open boundaries and weak internal controls could save it.

3.2 "Dangerous Liaison": The Economic Connection

(40) There is also another way in which contemporary scientific policy unintentionally contributes to the furtherance of malfunctions such as proneness to deceit, dogmatism, inappropriate peer evaluation, subservience to the spirit of the times, and rejection of novelty. Under budget strains it calls upon science to seek funding from private sponsors and from industry. In the same breath it undertakes to curtail the

freedom of science to "simply research at will" and insists on social "relevance." Which research is relevant from a political viewpoint is indeed difficult to determine. According to one particular political world view or another, this or that appears more important, the main risk appearing to lie here or there. Since there is no clear criterion for non-scientific relevance, this reorientation makes science into a plaything of conflicting non-scientific agents. The internal motivations of science which under more favorable conditions see to it that research always presses forward ("plus ultra"—Francis Bacon) atrophy and makes space for pure utilitarian research which does not increase knowledge but only applies and exploits it until the reservoir of fundamental knowledge is drained empty.

(41) From the point of view of system theory, the politically intended penetration of research by social, economic and political goals and standards represents a pathological interpenetration of the subsystems of society (cf. Fischer 2005 and 2006). This process is pathological because science should be called upon or even forced to make values and codes foreign to its system into its primary ones. For example, only such research as "pays off" is socially "relevant" or politically correct is to be financed by society. It becomes clear how questionable this demand is if one takes a step back from daily affairs and looks at other times and contexts. In the former Eastern Block, in national socialist Germany, in Christian countries before the Enlightenment (and even up to modern times), in Islamic countries today, we find analogous forms of pathological interpenetration, where in these cases the goals, standards and values of politics or religion were what penetrated science and restricted its freedom.

(42) One malfunction of science which is becoming more and more important derives from economization of research which politics is striving to realize. Why is this intrusion dangerous to science? Let us consider the implications for science of the code specific to the economic system. Just as in politics, truth is only a secondary code in business and is always subordinate to the primary goal, getting the best return within the horizon of given planning. What strategies does business use to get there? The following means have proven themselves: not to mention or to play down own research results when they are at odds with expectations; to make research by independent institutes appear doubtful through obfuscation in the media and by court action; to defame and marginalize critical researchers (Martin 1997); to use non-disclosure clauses which prevent researchers in one's own laboratories or in subsidiary institutions from publishing negative results which could harm the marketing of products! That the collateral harm of these strategies might concern the health and life of many buyers or users of the products pumped into the market is of interest to their producers or distributors only to the extent that liability claims or image problems follow which could reduce the profit margins hoped for.

(43) What helps the strategy of the enterprises is the fact that the scientific results used are often ambivalent and open to interpretation—hence share zones of interpenetration with the realm of culture—and that science itself is methodically complicated and full of snares. By the choice of control substances with strategic advantages, by adjusting the dosage of test and control substances, by selection of experimental subjects from suitable subgroups, by the choice of suitable statistical procedures and finally by an art of interpretation which is rhetorically armored, many substances can be proven to be effective and having few side effects, as long as, in the phrase of sarcastic critics, they have effects "not much worse than a drink of water dis-

tilled three times" (Paulus 2004; Montori 2004). The surprising fact that the scientific results of such test series depend "decisively on the kind of sponsor" (Lutterotti 2003) has a plausible explanation therein. Astonishing correlations like these make cooperation between business and science a balancing act for both sides. For business, they bear the danger of letting capital sink in the sand—for science, of suspending its inherent rules. This balancing act can be especially delicate when researchers themselves become businessmen. In this case they must, at least in the area where they act as businessmen, let themselves be led by its code. This is not a question of dishonest intentions or unethical behavior. They *must* adopt this code, for otherwise they will not remain entrepreneurs for long. Yet as soon as they enter the laboratory, the rules of research apply again. How long can a scientist do such acrobatics before starting to make "compromises"?

(44) Providing services and accepting tasks for other systems is not at all per se damaging for science. Knowledge is good for many things. It may be the development of de-escalation strategies for police, bunker-busting "mini-nukes," the conception of sales strategies for detergents, bio-tomatoes, big macs and tampons. It may be the organization of election campaigns for parties of every persuasion or conception of a laser show for a municipal cultural event. The same is true of law, business, politics or culture. They also provide instruments and skills which can be useful or essential to other subsystems. One of the most important functions of systemic interpenetration consists in the possibility of letting specialists take over necessary, but difficult tasks of the macrosystem.

(45) From the internal perspective of a system, for example of business, interpenetration means that what counts are not only capital and effective production, but also reliable information, power structures, legal rules, cultural resources, religious values and social connections. They are necessary components of the system. But in a well-functioning system they are only means (secondary codes), which are always subordinate to the primary goal of business, making profit, cultivating good returns, or increasing capital. It is not a question of motivation or of more or less "good will" in those involved. An entrepreneur is just as little free to strive for profit or not as a politician is free to strive for means of influence (hence power) or not, or as a researcher is free to strive for valid knowledge or not. The entrepreneur who has other primary goals soon leaves the game because of lack of capital. The politician who has other primary goals than the preservation or augmentation of power will soon lose it and exit the political game. The scientist and researcher who has other *primary* goals than the development of valid knowledge leaves the field of research and enters into the functional logic of another system.

3.3 Pathological Interpenetrations

(46) For science, interpenetrations with other subsystems means that research also has secondary codes even under "normal conditions," hence that it is marked by power structures, social networks, competitive fights for prestige and attention, and struggle for rights of interpretation. The economic aspect of science is for example visible in the fact that certain elements of competition have exceptional importance *within the bounds of the system of science.* Scientists as well as laboratories, disciplines, sciences and universities compete for property rights, publication success, funding, qualified personnel, etc. The political dimension of science is found in the hierarchies which structure its institutions and communication systems. The social element in the system of science becomes visi-

ble when loyalties and group membership become important to the process of science. The medial dimension becomes evident when scientist strive to place their publications in "high-impact" journals, when they try to win as many citations as possible, or when they quote some names or publications much more frequently than others, because certain works of certain authors have aroused especial attention in the past. The juristic dimension of research is founded in the fact that the activity of research is bound by the constitutional law of the macro-system as well as by its more specific laws, insofar as they are of significance for it. The cultural dimension of science consists in its results being not merely interpretation-free information devoid of purpose, but rather yielding means for the most various attempts at giving a meaningful perspective on all areas of reality and life. That science has a religious dimension today is not recognizable at first sight. An analysis of the most recent controversies in the field of cosmology, artificial intelligence, neurobiology, psychology (free will) or genetics makes it clear, however, that it is not exclusively a matter of clarifying matters of fact, but also of problems of transcendent meaning.

Systems can yield achievements of great value for other systems only as long as they follow their own steerage and not foreign rules. When the latter occurs, they degenerate. Law, business, or science suffer a process of decay when they stand under the guardianship of politics and ideology; religion does so when it is understood mainly as a field of ethics or of social concern; politics does, if it only obeys economic imperatives; and science does, if it is above all seen as a cultural event or as subordinate to and an appendix of institutionalized religion, etc. The invasion of science by foreign standards and goals effects a disturbance of its internal mechanism of rules, which in an extreme case can lead to collapse of the system. That which presents itself today as an "offer of aid" to replace lack of state support is in effect the attempt at a "hostile acquisition" of science by economics, politics, society, media, and religion.

(47) The deformation of the symbolic code of science by economic, political, cultural, mass medial and social factors can therefore lead to serious functional disturbances of the system—in the extreme case to justification of its colonization by other systems. Summarizing, we may recall the following observable phenomena, which deeply contradict the functional norms of science:

- strategies of secrecy in genetic and military research, with a view to securing profit and power (dominance of the codes of politics, of the military and of economics);
- concealment of important information in experimental reports in order to prevent verification of the results by others and in order to secure one's own advantage (dominance of the code of the economy);
- unfair treatment of a scientist lower in the hierarchy by someone of higher rank (dominance of the code of politics);
- ritual admiration of the "decision-makers" of the system of research by those of lower rank, who hope to receive a benefit from this gesture of submission (dominance of the code of politics);
- "in-group out-group" behavior: exclusion or rejection of outsiders by means of undue criticism, mild "solidarical" critic of group members (dominance of the code of society);
- decisions of the "peer review system" on the basis of the reputation or recognition value of a person, institution or a paradigm —and not on the basis of the intrinsic quality of the project which is to be evaluated, of that of the essay, the

report or the book (dominance of the code of politics, of society or of the media);

- formation of communities of citation (dominance of the code of society);
- building of networks mainly in order to exploit sources of funding (dominance of the codes of society and of economics);
- confusion of productivity (emission of "papers" per time unit) with contribution to the process of "discovery of truth," or scientific achievement in science (dominance of the code of the mass media);
- confusion of the number of citations (of an author or of a publication) with the contribution of this person or publication to the process of "discovering truth" in science (dominance of the code of the mass media);
- cultural reevaluation, to be observed in many historical and contemporary examples, which sees science as *one* system of interpretation of reality among many possible other ones (dominance of the code of culture);
- confusion of the consensus of those involved, which is a result of decision-making, with an indication of "truth" or scientific quality (dominance of the code of politics or of society).

3.4 Scientometric Measures as Indicators of Achievement—a Medial Code for Science?

(48) Each of these forms of pathological interpenetration would deserve extensive treatment. We close by limiting attention to one point which in future will be more and more important in many countries: the evaluation of achievement by scientometric standards. Measures of publication and citation are already used for this purpose in some sciences (for example medicine in Germany) now. We want to ask whether this use is legitimate (Frank 2005, Chap. 3), or whether it documents an overflow of medial standards and hence a case of pathological interpenetration. Nowhere else does the sentence "To be is to be perceived" prevail as it does in the media. Should science adopt the medial code as a means of governance?

3.4.1 How Trustworthy is the Science Citation Index?

(49) If one wants to use citation statistics as a basis for evaluations, one must at least suppose that the database is essentially reliable and stable and that citations can be viewed as measures of achievement. Are these suppositions justified? Even the most comprehensive file of citations, the Science Citation Index, is subject to distortions both random and systematic. An internal audit showed that up to 10% of the citations were erroneous and not attributable. According to an independent empirical investigation of biochemistry in the Netherlands again roughly 10% of the expected data in the SCI were missing (or could not be found because of programming errors). These 10% were, moreover, not distributed evenly. For some research groups half of the data were missing. In the case of another it was just the most cited article which was missing, an article which was named as frequently as all the other articles of the research group together (Moed et al. 1985, p. 139f). It is evident that the mistake is to be noticed only if an undistorted sample can be used for comparison. This is a rare happening, since it presupposes that the work of the Institute for Scientific Information (ISI) has been replicated. The ISI is a private monopoly which dictates its prices, is not subject to quality control by competitors and evaluators, and must not let anyone know what is going on. The use of its data is at one's own risk.

(50) An incalculable distortion of the database already results from the actual process of citation as articles are produced. An empirical study of papers on the history of genetics has shown that on the average only about 30% of those sources which one would ideally need to cover the information presented were actually named. The variance lay between 0% and 64% (MacRoberts & MacRoberts 1986, p. 166). Secondly, one can empirically observe a tendency to cite by example. In the humanities and social sciences one has recourse to "classics" as a rule, even though these are out-of-date methodically and theoretically in comparison to more recent, non-cited articles which are in fact drawn from (Shadish et al. 1995, p. 488). In the natural sciences the most recent research articles in a sequence on which one is dependent are generally those which are cited. These citing habits contrast with the obliteration phenomenon: that certain authors and their work are not named or directly cited any more, since everyone knows anyway whom one is referring to. If MacRoberts's result can be generalized, then it is the following: the data basis for measuring achievement by enumeration of citations contains only a small part of what would be necessary for a complete evaluation of sources. In the case of a random sample that would perhaps be enough. But is the sample random?

(51) The kinds of offences which can be hidden in the references of a scientific treatise are familiar to most scientists from their own work: important publications and publications clearly used are often not cited. Some are simply forgotten, others are supposed to be well-known. Some people are negligent, others do not indicate some sources or do so only in a secretive manner. Yet others cite authors whose books they have never read. Certain texts are cited because "everyone knows them," but an insider knows that such sources can often only be known by hearsay.

(52) As long as such offenses remain in the domain of randomly distributed personal idiosyncrasies, they do not distort the results of the scientometricians in a systematic way, but rather only increase the "noise level" (which does indeed make it difficult to read a signal). However, what the sociology of science knows about social processes in the scientific system, about the exclusion of outsiders, about the non-perception of research in certain languages, countries, institutions, and about the spread of books and journals, does not support the inference that there is a random distribution. Citing is a social action. There are citations of politeness and ingratiation, which are due to social obligations, career hopes, etiquette, submission rituals, contact search or scientific self-localization. In the times of SCI, social networks are also recognizable by a specific politics of citation. Citation cartels are formed, the members of which regularly cite one another, in order to raise the citation rates therein with respect to the outside. In a time in which the SCI is used to evaluate rank this is not unreasonable behavior. But the opposite effect is also observable. Many sources are consciously ignored because one does not know or like the authors or because their home institutions have a lower prestige, are considered to be competitors, or lie in the wrong country.

(53) Citation numbers can in principle correlate with many variables. The dependencies which can dominate at any time depend on contingent circumstances, for example:

- the language an article is written in;
- the prestige (impact factor) of the journal in which an article appears;
- the circulation (actual availability) of the journal;
- the citation rules of the journals;

- the extent and structure of the research area;
- its dynamics of development ("hot" or "cold" field);
- the habits ("politics") of citation now current;
- the "followability," or "connectibility," of the articles concerned;
- the prestige of author and coauthors;
- the previous presence of the authors in the public arena of research;
- the scientific genealogy of the authors;
- the worldview (ideological position) of the authors;
- the disciplinary matrix which the authors are tied to;
- the reputation of the institution in which the authors work;
- the position of an author in the social and political system of science.

(54) Citation rates are also subject to short-term variations. The "estimated lifetime citation rates" of articles in an area of specialization can vary from year to year so that a direct comparison of citations of articles which have appeared in different years is no longer possible even in the same area of speciality (let alone in the same discipline). Different tendencies can be ascertained as well when a comparison is made among different disciplines regarding the probability of an article being cited (Moed et al. 1985, p. 155). Work from different areas of specialization, disciplines, times, etc.—the list of possible factors is open—are hence incomparable regarding the analysis of citations. Standardizations are also hardly possible because of the multitude of factors and their variability, because the number of cases in the statistical categories successively formed in search for exactness keeps decreasing, until only very few authors are left. Since many authors work in different specialities, it is practically impossible to compare them.

3.4.2. Citations as Indicators of Research Achievement?

(55) According to Robert K. Merton's classical approach, the citation of an author can be compared to the payment of a debt. Whoever cites uses the intellectual property (ideas, results, etc.) of another and must, according to the rules of scientific ethics, indicate the source or the inventor (Merton 1979). When applied to a discipline as a unit, the totality of citations of a researcher is an index of the use of the latter's published work.

(56) This view is based on certain assumptions. It assumes that one is aware of an intellectual debt. It assumes that it is recognized as necessary, judicious or opportune to repay the debt. Citations cannot, unless they are literal quotations, be made the subject of litigation. There are so many possibilities for combining ideas and coming to conclusions that a direct genealogy is hard to prove. This points not only to the central function of perception in this process, but also at the significance of the "ecology" in the science concerned. In order to be cited, someone must appear on the "radar screen" of the citer and be deemed a worthy object of attention. Even in ideal cases, citations reflect not the actual, but rather the perceived value of research for those who use its information for their own activity. In real science it seems more plausible to assume that citations mainly measure the utility which the members of a scientific community expect from the *act of citation* itself. That a publication is not useful to others at present or that a researcher does not see any use in citing it and does not believe it necessary to pay off an intellectual debt does not say the least regarding its inherent quality.

(57) There are many reasons for not perceiving or citing a publication. One of these

is language. Certain languages are systematically disadvantaged, others totally ignored. According to an empirical study by Montada et al. (1995), the publications of German psychologists in English-language journals are cited, *by German psychologists*, much more often than similar publications of the same authors in German-language journals. If there is such a thing as falsification in science, then *this* is a direct falsification of the assertion that rates of citation could be a standard of scientific quality. Identical contributions, which differ only by language and by journal of publication are by consequence cited about ten times more often. Does the quality of an article really increase by 1000% when it is translated into English and published in the "Journal of Abnormal and Social Psychology" rather than in the "Zeitschrift für Sozialpsychologie"?

(58) That an article is not cited frequently does not mean that it did not really have any influence and that it has not been used. But even if it has been cited, this does not mean that this is recognizable in a scientometric evaluation. It is possible that many of the articles which do cite it are not contained in the database of the SCI. Especially in the humanities a large part of primary literature still consists of monographs. The restriction of all three parts of the index to "high-impact, refereed journals" (mostly English-language) entails a further narrowing of the base of citations. As a justification of this restriction it is often pleaded that the other journals are of no importance, since the really significant papers appear in the aforementioned group of journals. This argument is wrong. On the one hand, it is quite unclear which conditions a journal must fulfil in order to be considered "refereed." Two mutually independent lists of 784 peer-reviewed journals from clinical medicine had only 54% in common (Eldrege 1997), that is to say that almost half of the journals were only in one list or the other. On the other hand, evaluations of the ISI show that of the most cited works of a discipline or of a time period many have been published in journals with a relatively low impact factor (e.g., Garfield 1988, p. 58), whereas even in high-impact journals most articles reach only low levels of citation rate. Since citations follow the Lotka distribution, the impact-values of journals can go upwards quickly because of a few highly cited articles, but also fall again, if such articles are less frequent in the following years. The great majority of articles in these journals thus profit without cause, in an evaluation based on impact values, from the presence of a relatively few highly cited works just as they suffer without cause from the absence of such works.

It is furthermore to be noted that the impact factors of journals can be different in the special areas which they serve. Medical journals whose articles on AIDS research have only a moderate level of impact may for example considerably surpass or lag behind these values in other specialities such as immunology and Alzheimer dementia research. Such differences are not taken into account by the calculation of the impact factor, but are rather smoothed out.

The impact factor can be manipulated to a large degree. A journal with a comparatively high number of editorial articles (such as obituaries, short notices, marginal glosses, reviews, commentaries, letters, corrections) can leave these out in the calculation of the number of published and citable articles of the last two years, although contributions in these categories are indeed cited. Since the impact factor is defined as the number of citations of a journal in the year in question divided by the number of "genuine articles" in the two previous years, this kind of polishing artificially drives up the impact.

(59) If high citation rates were indicators of scientific accomplishment, there should be a high correlation between the citations of certain articles in the time period following their publication and the judgements of experts years later concerning their significance for the development of their field. Yet the correlation is much poorer than expected! From fifteen publications retrospectively evaluated by historians of geology as the most important for the development of geology between 1935 and 1949, none appears in the list of the most cited geological articles between 1945 and 1954 (Brush 1991, p. 395 and 396).

(60) The question is how scientometric measures can yield valid results concerning the cognitive quality of publications or the scientific achievements of authors—if these measures cannot even serve as a reliable index of use, if the distribution of citations mirrors the structure of power and reward in a discipline more than its cognitive structure and if one understands by achievement the contribution to the expansion or improvement of our knowledge and not the consensus which has been created at a given time. It is easy to predict that the malfunctions of science which have been analyzed above will have an unimpeded impact on measures of citation. That can also be seen positively. Scientometric methods are a good means for investigating the social and political structure of special areas and disciplines, the autopoiesis of a science, to speak metaphorically (Fischer 1993). The result is an image of the self-perception of a discipline, which not only contains cognitive achievements presently perceived as such, but also the institutional mesh, the structure of authority and power, social commitments, economic ties, the spirit of the times, ongoing interpretations—all that in a mixture subject to variation, which can be separated into its components only with great difficulty.

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