

THE INDIVIDUAL SCIENTIST

Where we desire to be informed, 'tis good to contest with men above ourselves; but, to confirm and establish our opinions, 'tis best to argue with judgements below our own, that the frequent spoils and victories over their reasons may settle in ourselves an esteem and confirmed opinion of our own.

SIR THOMAS BROWNE

The concept of public knowledge faces us with the following paradox: the scientist regards himself as entirely free in his research; how can he simultaneously be a cog in a social machine for producing knowledge?

This is a more special case of the familiar problem of liberty in society at large. A free citizen of a republic similarly regards himself as able to make his own decisions about his life; yet in a well-ordered society he will in fact be performing a specialist function with the same reliable certainty as an ant or bee.

The answer is, of course, that by his upbringing he is conditioned to the norms of his society; he learns to work for reward, to covet money, to obey traffic signals, to be honest, loyal, faithful, etc., so that his freedom is circumscribed by invisible glass walls within his own personality, so that he does what society expects of him without having to be told.

Similarly, the professional research worker is conditioned to the norms of the scholarly community. This does not constrain his field of study (any more than ordinary bourgeois society need tell John Doe to be a stockbroker rather than an ironmonger) nor tell him what discoveries to make in that field, but the whole style of his investigations and their subsequent publication is strongly determined by those norms. It is not enough to be intellectually acquainted with the current consensus; he must learn how to behave like a scientist; indeed, he must learn to 'think scientifically'. He must internalize the scientific attitude so that he cannot even con-

ceive of, say, writing a scholarly paper in Zen, or recording the epoch of an eclipse by reference to the age of the reigning monarch, or pay a claque to abuse the reading of his opponent's papers.

Now it is sometimes held that the scientific attitude is so completely logical, rational and unequivocal that such self-discipline is unnecessary. One is told that the scientist succeeds by freeing his mind of cant, by being absolutely honest, by sitting down before the facts like a little child, by measuring all things, by having no preconceived notions, by observing impersonally and objectively, etc., etc. These are admirable virtues and no doubt every serious scholar, in every age, has tried to practise them. They are virtues that are strongly reinforced in some aspects and applications by a scientific career—although we all know of scientists of the highest repute whose political attitudes and moral principles are infantile, or even dishonest. But in any particular context these scientific virtues are neither self-evident nor easy to practise. Only hindsight justifies the charge of prejudice against those who did not immediately accept all of Galileo's arguments, or who were astonished and confused by Darwin's views on the Descent of Man. Honesty, lack of cant, objectivity, etc. are impossible conditions to achieve, and sometimes quite irrelevant to the business of making scientific discoveries.

What I have tried to show, in chapter 3, is that the criteria of proof in Science are public, and not private; that the allegiance of the scientist is towards the creation of a consensus. The rationale of the 'scientific attitude' is not that there is a set of angelic qualities of mind possessed by individual scientists that guarantees the validity of their every thought—as if they were, so to speak, well-tuned computing machines whose logical circuits precluded them from error—but that scientists learn to communicate with one another in such tones as to further the consensible end to which they are all striving, and eventually train themselves to construct their own internal dialogues in the same language. A private psychological censor takes over from the public policeman or parent, and conforms our behaviour to social norms. But he does not keep whispering into our ear, 'Be honest, be truthful, be objective',

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in a chorus of pious aspiration; he says, 'Have you checked for instrumental errors? Is that series convergent? Would anyone understand that sentence? What is the present status of that old bit of theory?' and so on.

In other words, a peculiar quality of the research scientist—and this he shares with professional academics and scholars in all the faculties—is that he has very high internal critical standards for arguments within the context of his discipline. He knows his consensus and he has to decide whether a proposed change is acceptable. When faced with the argument for such a change, whether from another person or from his own mind, he instinctively puts on his referee's mantle, and weighs it up as if he were an official representative of his intellectual community; he does not say, 'Can I believe this?' but rather, 'Would *they* be convinced by this evidence?' Far from being *impersonal* he tries to be *omnipersonal* in his judgement.

Where does 'creativity' come in? Well, of course, a good scientist has got to have it, for it has become the definition of being a good scientist! The better word is *imagination*—the ability to construct new patterns and combinations of ideas.

But scientific imagination is strongly constrained: scientific speculation is far from idle. It must act within a well-organized framework of ideas and facts, with rigid rules of argument and proof. Even the cosmologists do not spin their marvellous webs of space and time out of mere fantasy; they use the logic of the tensor calculus and astronomical observations to construct rational systems compelling by their elegance and simplicity. It is much more like extremely academic painting or poetry, in which the art is to say something new within the official stylistic conventions, than abstract expressionism or even blank verse. The typical scientific paper is akin to a sonnet, or a fugue, or a master's game of chess, in its respect for the regulations. Many scientists delight in just this feature of the game; it is more satisfactory, and the victory is more definite, to win under such prescribed conditions. Scientific imagination is a rare gift, but its achievements are not so much at the

mercy of fashion and prejudice as those of poetic and artistic genius.

Learning, imagination and critical sense—these are the three qualities which the scientific mind must possess in abundance. He must not be ignorant of the present and past consensus; he must not be blind to the possibility of changing it; he must not give credence to every passing whim or fancy. Of course, these qualities are not usually found in the same proportions, even in the very best scientists. Some men are very learned, able to bring to mind a relevant fact or notion on any subject, repositories of wisdom for their students and contemporaries. Although inhibited from imaginative steps by their attachment to existing knowledge, they are the safe moorings from which more lively craft venture forth. Some are very imaginative, having a hundred new ideas a day, of which ninety are patent nonsense, another nine are eventually found wrong and the remaining one is a winner.* By themselves, such scientists are incapable of picking out the serious words from their own jests, but they are a yeast which ferments more pedestrian minds into activity. Some scientists, alas for themselves, are saddled with such a strong critical sense that their imaginations are hobbled by the knowledge of all the weaknesses in their own arguments—but they are the anvils upon which other people's reasons are forged, and earn professional respect by the power and integrity of their minds.

Yet these qualities are not necessarily antagonistic or mutually exclusive. It requires imagination to see the flaw in conventional arguments, so as to criticise them adequately. Suspicion of facile new ideas may be the motivation for a deep search that eventually leads to a revolutionary discovery. Ignorance of the latest methods for attacking a problem may be an advantage in allowing one to look at it with a child-like eye; on the other hand, wide knowledge of many cognate fields may provide weapons for a new and 'imaginative' approach.

* Of one such man, now a very distinguished educationalist, it is said that he has slowed down to only ten ideas a day, but now gets ten per cent of them right!

But the psychological strategies that are called for in the solution of scientific problems are no more to be reduced to a formula than the strategies of life, love, war or business enterprise, and the qualities to which I have referred are not to be derived from, say, deeper inherent pre-existent factors of the mind such as I.Q., introversion, divergence, verbal facility, etc. It is true, in general, that good scientists are usually quite intelligent—in the top ten per cent of the population, say—but this only suggests that the ability to solve set-piece problems quickly is helpful in the longer process of asking and answering unsolved problems; it may indicate no more than that the right to receive an education in the current consensus of a high science is restricted to those who have shown this particular skill. The attempt to construct a psychological profile of the 'creative' scientific intellect, without reference to the training or other experience that the person has undergone in the scientific field, is ludicrous; one might as sensibly construct a profile of 'saintliness' without reference to the theology or ecclesiology of the day.

This I assert on the basis of one evident fact—that the modern scientific worker is made, not born. Just what draws young men into the learned professions and into scholarly pursuits is very hard to uncover. The factors are multifold—the prestige and fame of successful scientists, the desire to be 'useful' rather than materially successful, natural curiosity, precocious academic talent, a philosophical bent, family background, and so on. But we know, from personal enquiry and by elaborate statistics, that many scientists—even some of the best—came into the profession quite accidentally, without an early intention or a deep youthful interest in science. Although they may eventually feel that they have found their vocation, one often has the impression that many of them would be equally happy and successful as doctors or lawyers, churchmen or engineers. The world of research is manned largely by what is aptly called a Scientific Civil Service—officers of a professional bureaucracy rather than an elect of the intellect. Research is not a 'natural' activity of mankind and it took many centuries of civilization for the technique of it to be discovered;

but now almost anyone of good intelligence can be taught to do it passably, just as almost any healthy person can learn such 'unnatural' activities as skiing, or parachuting or driving a motor car.

In its heroic age, Science was created by men with the will to withdraw themselves from the commonplace notions and pursuits of their day. Many of them were actual social recluses, enabled by inherited wealth, an undemanding profession, monastic seclusion, an academic, clerical or political sinecure, to free themselves from the ordinary life around them so as to devote themselves to pure knowledge. Others, such as Gilbert, Franklin, Lavoisier, were extraordinarily capable in worldly affairs, and yet made for themselves the time to prosecute their researches. Except for a few national academies, there were no institutions offering a full-time career in science, and the general intellectual atmosphere was not particularly friendly to 'Natural Philosophy'.

Our picture of the inner scientific life is strongly coloured by the mythology of this period. We see the man of science as a lonely, dedicated personality, grappling with problems that he has set himself, sensitive to the work of others, but not primarily governed by their demands upon him. In Riesman's categorization,* he is inner-directed, and follows his own star. Whether this has anything to do with protestantism, or whether, as suggested by Feuer,† it was originally associated with liberal agnosticism, is a deep historical question. The fact remains that the scientist of the seventeenth and eighteenth centuries had to free himself from dogmatic clericalism before he could even begin work, and hence had to acquire a peculiarly strong independence of spirit.

Yet the virtues of curiosity, intellectual freedom, the questioning of all accepted doctrines, etc., which were so essential in that phase (and which are, of course, still essential to good science now), are not sufficient to make a man into a successful research worker. Those virtues are to be found in many cranky, eccentric persons, whose would-be contributions to Science are worthless

* *The Lonely Crowd* (New Haven: Yale University Press, 1950).

† *The Scientific Intellectual* (New York: Basic Books, 1963).

because they have not been subjected to the consensible discipline. In our histories of Science we celebrate the successes of that small band of warriors whom hind-sight informs us to have been on the right track; we do not bother with all the other little bands, wandering in the wilderness with strange philosophical and religious banners which would demand the same courageous virtues of their adherents.

The scientific community has always asked more of its members than adherence to some bizarre philosophical doctrine, or mere intellectual curiosity and exuberance. Through its organs of communication it brings together new discoveries and theories, and gets them criticized. In the early days, such criticism often broke into personal controversy; but eventually social and psychological mechanisms evolved by which the delicate balance between tradition and innovation, between individual pride and group achievement, could be preserved. To be in full communion within this parish, one must be ready to withstand the wounding criticism of others and to deal firmly, but without malice, with their stupidities. One must be ready to yield to an honest argument and yet not throw away a genuine point. However much public comment may have been forestalled and discounted by one's own self-critical care, there is always a risk in presenting new scientific work; every new paper is a hostage to fortune, open to the arrows of scorn or the cruel winds of neglect. There must have been a phase, perhaps in the eighteenth century, when the general scientific community was consolidated by the expulsion of these personalities who could not live within this psychological frame.*

But this process of the recruitment of scholars by voluntary self-selection from the laity was superseded in the nineteenth century. The German universities, which seem to have been the first large-scale, self-consciously professional research organizations to

* Was the reform of the Royal Society in the mid-nineteenth century, when amateur and dilettante gentlemen interested in Science ceased to be elected as Fellows, a belated recognition of this change? Professionalism here means common adherence to the norms of the consensus group, rather than pay for doing research.

offer a career of Science and Scholarship to anyone with the talents for it, depended upon a system of deliberate apprenticeship.

This system, being in fact a State Civil Service, was very formal and rigidly hierarchical. The junior posts were lowly and ill-paid. Preferment was at the mercy of the judgement of the seniors upon the products of long years of study. Recognition often came very late, and only after the publication of a thorough and complete piece of research, equivalent to the dissertation required for a higher doctorate at a British university—the sort of work one might now expect of a candidate for a Senior Lectureship, or Readership, in Britain or an Associate Professorship in the United States. Many failed these rigorous tests, and withdrew to a less demanding profession.

Here we see the scientific community moulding would-be entrants into conformity with its norms. During his long apprenticeship, the German academic internalized the conventions and criteria of the scientific life. From bitter experience he learnt not to get caught in personal controversy, nor to speculate on the basis of inadequate evidence. Under the harsh eye of his own professor he acquired the habit of checking and re-checking his observations, of writing accurately and impersonally, and of being the foremost critic of his own work. The carrot of a juicy Chair drew forth his utmost of energy and imagination—yet always within the constraints of the discipline of his seniors.

In its day, the German academic system was the admiration of the world, and extraordinarily successful as a medium in which scientific research flourished. I do not say that a large proportion of its members were not, by nature, highly gifted and dedicated men; but also by their nurture and employment they were encouraged to contribute much more purposefully to the scientific consensus than the more dilettante English and French.* With Alexander von Humboldt as their Moltke, they constituted almost an officer corps trained to collaborate and compete in the battle against ignorance.

* See J. Ben-David, 'Scientific Productivity and Academic Organization in Nineteenth Century Medicine': reported in *The Sociology of Science* (eds. Barber and Hirsch) pp. 305-28.

There was a price to pay. The individual scientist was no longer working solely for his own amusement, nor for the abstract advancement of learning, nor for posthumous glory; he was also seeking personal promotion. As we are now fully aware, this is the catalyst for the release of vast floods of tedious, prolix minutiae, impressive for quantity if seldom for quality; 'publish or perish' is not a new cry. And the tone of academic life was often repressive, spiteful and pompous, the ultimate rewards, in status and salary, being large enough, but too slow to come.*

The mechanisms of character formation which drove the German academic system were paternalistic—often patriarchal. The power of the *Professor Ordinarius* over his students and assistants was almost that of a Roman father over his family. Upon his recommendation depended all hope of promotion. The only escape from his despotism, however benevolent, was by being elected to a Chair, to become a despot oneself.

Here was the social situation within which the Freudian theory was evolved, and to which it would seem to apply with peculiar force. The student seeks to supplant his intellectual parent—but cannot fail both to admire and imitate him. The 'scientific attitude' was thus passed from generation to generation of scholars, as a very firm and self-conscious mental posture.

It has always been easy, specially for gentlemanly British dons, to sneer at the industrious pettifogging of German scholarship at its worst. But this is only the unimaginative extreme of its major virtue of critical rigour. The German system emphasized the social character of *Wissenschaft* (which included Historical and Classical studies as well as the Natural Sciences) by making sure that every new discovery or theory was thoroughly examined and exhaustively tested before admission into the consensus. The virtues that it most strongly encouraged were those of painstaking care, loving attention to detail, precision of language and of argu-

* For evidence on this, let me recommend Ernest Jones' *Life of Sigmund Freud* (London: Hogarth Press, 1953–7) and a moving essay by Max Weber, 'Science as a Vocation', reprinted in *From Max Weber* (eds. Gerth and Mills, New York 1958) pp. 129–56)—two genuises who suffered the pains of the system to the full.

ment. No side turning was to be left unexplored, no gaps were to be tolerated in the logic. The great era of German Science (which died in 1930 and has not been revived) was not only an age of experimental, observational and textual discoveries; it was the age when the foundations of Pure Mathematics were drilled deeper, to reach the firmer bedrock of formal logic; it was an age of the treatise and *Handbuch*, and of scientific and technological education.

It is the argument of this essay that such attention to observational accuracy, logical rigour and encyclopaedic detail is quite as essential to Science as imagination and inspiration. Without these 'Germanic' virtues, Science would disintegrate into schools and sects, prophets and their coteries of disciples.

But the enforcement of high critical standards by distant, anonymous authorities and institutions, such as those of editors, referees and review writers, is psychologically impracticable. The public peace is not preserved by the abstract power of the impersonal judge and vigilant policemen; it depends in detail upon the conscience of the individual citizen, moulded in childhood by the direct and personal influence of stern but loving parents. High critical standards must become part of the intellectual conscience of the scholar; he must acquire the psychological strength to withstand the temptation of shoddy thinking and tawdry brilliance. The paternalistic upbringing of the German academic of the old school gave him this moral stiffening in its most puritan mood.

He also acquired from his intellectual parent/master a vision of the Philosophy of Nature to which his work was to contribute. For the born scientist, this is the inspiration of his life, transforming it from a career into a vocation.* In the German system this ideal was transferred from one generation of scholars to the next, from the mature professor to his immature students, by precept and by example. Their professionalism was thus always imbued with a vocational spirit, which gave it significance and inner meaning.

* For this vision at its most humble and pure, let me recommend some of the writings of Faraday, whose career is also of great interest as arising out of an old-fashioned apprenticeship as laboratory assistant to Davy, whom he eventually succeeded at the Royal Institution.

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Modern Science and the modern scientist were invented in nineteenth-century Germany; nowadays, of course, every nation on earth is striving to cultivate these exotic crops. It is interesting to observe the variations of technique that have entered as the 'Ph.D. system' has spread from one culture to another.

In Britain, for example, the aristocratic tradition of Oxford and Cambridge gave the young don a position of financial independence quite early in life. Formal education ceased after the first degree, and it was a point of honour for the supervisor of a research student not to interfere with the self-improvement and maturation of the young scholar. He was expected to choose his own problem, set about research on his own and quickly become a scientist in his own right. In other words, there still persisted the ethos of the phase in which the scientist was a self-appointed and dedicated amateur rather than a trained professional; until recently it was not thought too badly of an academic if he did no research at all—for who could blame him for failing to be called to such a vocation?

In the British environment the born scientist could, therefore, develop easily, without being conditioned or constrained by jealous seniors or by a strong intellectual orthodoxy. Critical standards were maintained by competitive scholarship of Fellowship examinations in the student phase, by considerable competition for good university posts and in the top ranks by the reward of the honorific title of Fellow of the Royal Society. But the middle ranks of British Science lack the sheer professionalism of the German scholarly corps. They feel impelled to imitate the casual, lackadaisical style of their abler contemporaries, without the brilliance to carry it off. They do not appreciate the value of thorough, painstaking, if pedestrian investigations, the observation of details, the collection of facts, the compilation of data, as bases for the work of more inspired scholars. Despite the old-boy networks that web British academics socially and institutionally, they do not collaborate freely, and there is little feeling of Science itself being a collective enterprise in which one should play an allotted role.

Other countries, such as Japan, copied the German system faithfully, but lacked an existing tradition of high Science to provide

the necessary leaders. Several generations of industrious but uninspired professors succeeded one another, producing vast quantities of tedious and irrelevant papers, before a few brilliant men, supported by their international reputation, could rise to the top and set higher standards of education and critical evaluation.

By contrast, in Germany itself the Nazi abomination murdered and exiled the leading scholars, repudiated logic and liberality and destroyed the soul of their academic system. When the young men came back from the war, they sought to rebuild it. But the famous Chairs were empty. No amount of solemn lecturing, reading of books, experimenting and writing could replace their wisdom and experience. There was no old man dozing in the front seat of the seminar room, waiting with an innocent-seeming question to prick a bubble of conceit, or with a word of encouragement to set fire to a modestly concealed imagination. For the past twenty years, German Pure Science has been bulky but flabby; it has lacked the inner tension between imagination and criticism, between the speculative and the factual, which was its previous glory. I know of no better demonstration of the importance of the social element in scholarship. The persons and institutions of high Science cannot be created out of Baconian principles and a supply of apparatus; they are not like factories. A mature scientist takes decades of training, and is the heir of subtle intellectual traditions. Academic institutions are governed largely by unrecorded principles, handed on from father to son, from master to pupils, in the intimacies of the seminar room, the study and the laboratory.

The introduction of the German system into the United States may be dated from the foundation of Johns Hopkins University—‘a Göttingen in Baltimore’—in 1876, although a number of American scholars had previously studied at German Universities. But something happened, in the more liberal and permissive social climate of the New World, to change the psychological character of the scientists that were created.

For example, graduate education in the United States has become very deliberate and organized, with a formidable load of

courses to be attended and examinations to be passed before research is allowed to proceed. To some extent this somewhat rigid and rigorous system grew up to remedy the deficiencies of undergraduate education; but it has become an institution in its own right, quite different from the haphazard attendance at lectures and seminars expected of the young German scholar.

An American graduate school gives a professional polish to the language and techniques of research. By taking formal training up to the very edge of the unknown, by teaching the most up-to-date methods and the latest discoveries, the young scientist is acquainted with the current consensus almost before it has been achieved.

This has its values—but also its dangers. The American scholar, however clever or stupid he may be at heart, is not ignorant of his subject; he does not go pottering on with antiquated methods and ideas, scorning powerful new tools, saying (as I have heard it in the Cavendish) that because Rutherford could get along without quantum mechanics, so would he. But it also lends itself dreadfully to the sway of fashion. A new idea, not fully worked out or universally accepted as public knowledge, is often taught a little too dogmatically by enthusiastic champions to not very critical students. The cult of progress, of the latest thing, is strong in America, and the natural swing of opinion, interest and undiscriminating acceptance is reinforced by the too rapid incorporation of half-baked and speculative notions into the canon of the graduate school curriculum. If, in the end, the only true education is self-education, there may be much unlearning to do in later years. An orthodoxy enshrined in books is less compelling than the psychological tyranny of the lecture course, whose inner contradictions may be shuffled over and hidden by the inevitable (and glorious) ambiguities of the spoken word.

On the other hand, thesis work for the Ph.D., although taken quite seriously, is not perhaps so tough and critical as in the old German system, nor even as in Britain. This is a dangerous topic on which to generalize, as the standard is so variable from subject to subject and from school to school. It is noteworthy that the practice of having an external examiner for the dissertation—i.e.,

an expert from another university to report on its scientific merit—which is universal and compulsory in Britain, seems to be unknown in the United States. The standard of a Ph.D. is therefore the standard of the University that awards it, not some general standard recognized by the whole scientific community. It is not surprising that many American Ph.D. theses do not really attain the level of being publishable as scientific papers in reputable journals and that their expository style is often so poor.

It seems to me that many American Ph.D.s, in spite of the rigour of their formal training, do not acquire a very critical attitude towards their own and other people's work. Although he may seem to be using the very latest and most powerful methods, and has learned to avoid obvious mistakes of technique, algebraic errors, etc., the student is not conditioned to watch out for the logical hiatus, the falsely excluded middle, the verbal ambiguity, the divergent mathematical formula, the alternative explanation. Turgidity and verbosity stuffed with technical jargon may mask the problem to be solved, and the nature of the proposed solution may be muffled under a blanket of half-meaningful, solemn and portentous sentences.

I do not mean at all to play the chauvinistic game of sneering at such feeble stuff. Scientific research is an intensely demanding art, which comes easily to very few men, and the best American science is of incomparable virtuosity. But the American style of graduate school has come to replace the German *Institut* as the model system for the training of young scientists, the world over, and we must be aware of its defects as well as its virtues.

The major weakness is the attempt to teach this delicate and subtle activity by mass methods. It is all too easy to enlarge the classroom, add a few more benches and lecture to twice as many graduate students as one can know by name. The allotment of grades by tests and written exercises may appear to be an adequate substitute for argument, discussion and personal assessment. A clear lecture course on the latest developments may seem as valuable as the labour of disabusing each student, in private, of his misconceptions and misunderstandings of the literature.

But scholarship, for all its social aspects, is an intensely individual activity. The young scholar *must* learn to work on his own. The graduate school often has an atmosphere of the broiler house, of forced feeding. The course work is too rich a diet, and the knowledge it contains has been jelled too soon. The method of examination tends to make the student credulous, and distrustful of his own powers of comprehension of all these deep mysteries; he does not learn early enough that Homer can nod. He does not acquire that deep suspicion, counter-suggestibility and independence of mind that are so essential in scientific work. It is not so bad, perhaps, that he should be 'other directed'—which all social beings must be—but that his ideas of what will please his contemporaries and professors are success in the tests and exercises, ability to play the fashionable game, facility with the jargon, to be followed somehow by a 'breakthrough' that will bring fame. He does not see himself as engaged in a larger struggle with ignorance and error, as a member of a great movement, as a contributor to man's understanding of nature. The graduate school, by its mechanization of learning, has thrown its philosophy out of the window.

Of course, the research training of the thesis does something to redress this. But where this is done as part of a larger team, as is now very common in certain fields, the scientific spirit may not be very apparent. It is very important for the student to carry out a nearly independent investigation of his own, starting from the idea that a problem might exist, through its formulation and solution, to the final publication of the results. To be a member of a team directed by a distant and very busy leader, building just one technical link in a complicated experiment, is an inadequate apprenticeship to the art; it is as if the pupils of Rubens were to be accounted artists after five years of painting-in the buttons on his larger compositions. The fact that the apparatus is ultimately designed to catch a few omega-minus particles, or some photons from a Magellanic cloud, does not make the job of wiring its logical circuits more 'scientific' and less 'technological'. High technical standards may be achieved by the student, without a grasp of the deeper intellectual issues.

The strength of the old German system was the way in which the spirit of enquiry was passed on as an oral tradition. The enormous expansion of graduate studies and scientific research the world over since the Second World War has been too rapid for this spirit to percolate from the few centres of excellence to all the institutions now engaged in the training of scientists. The prejudice against the creation of personal 'schools' around distinguished scholars does not favour the handing on of high critical standards where these exist. It sometimes seems of Ph.D.s of mediocre universities that they have no concept of Science as a collective enterprise, and no criteria by which to judge their own efforts except those of fashion and novelty.

Does this matter? Will not Science grow just the same, as brick is piled on brick? It is true that further experience will mature them. The custom of spending several years as a postdoctoral fellow in a leading research school gives many a young scientist the opportunity to come into contact with higher standards, both of imagination and of criticism.

Science may err, but it is, after all, self-correcting. The appeal to experiment and logic is not vain. Even though we cannot positively assert that there are 'objective' criteria to be satisfied eventually by our theories, all our experience over the past 300 years makes this a reasonable belief upon which to base our studies. Scientists may make mistakes but truth must surely triumph. The pretentious, the feeble, the baseless speculation, the trivial irrelevance—all these will eventually be forgotten and buried in the library stackrooms. The process of critical evaluation and the principle of the consensus are powerful enough to deal, as they have dealt in the past, with all such follies.

There are, moreover, centres of such acknowledged excellence, in all the major advanced nations, that the gradual diffusion of genuine scholarly standards is continually occurring. The loss to German Science in the 1930s was a tremendous gain to Britain and America. Even if the exiled scholars had in fact done no more research, they were able to convey to their new pupils and colleagues the great tradition to which I have continually referred.

The restless journeys of scientists, their conferences, sabbatical leaves, summer schools, brain drains, etc. serve to unify the scientific world, and give glimpses of the best to the most humble toilers in the vineyard. The criminality of cultural barriers such as the Iron Curtain is that they prevent these contacts and thereby hinder the achievement of the consensus of public knowledge to which we are all devoted.

The manifest internationalism of Science is not a bourgeois or communist conspiracy; it is not mere sentimentality about the Brotherhood of Man; it is inherent in the very nature of Science itself, which must always seek to encompass the largest public for the knowledge it aspires to. Having given our splendid discovery to the world, we find it intolerable to think that there might be someone, in Milwaukee or Sverdlovsk, in Pernambuco or Chittagong, who ought to know about it and is prevented by purely political obstacles from reading about it or giving it adequate criticism. This is something that non-scientists do not understand—that anyone who works in the same scientific field, who can use the same technical language, who has faced the same problems, is a colleague and comrade. It is partly a matter of having to struggle with a common enemy—Nature—but I would ground it upon the consensus principle itself. Internationalism is a primary principle of Science, demanded by the inmost law of its being.

To appreciate this, one has only to visit a scientific laboratory in a politically or geographically isolated country. It is not that the library lacks the proper books, or that the journals arrive a little late; it is the absence of contact with the current *informal* consensus, of conversation with genuine colleagues or visitors with wild new ideas, of a reliable assessment of the quality of one's work. Scientific work is only meaningful in the social context of the scientific community.

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In the above pages I have gone to some lengths in criticizing some contemporary features of the scientific life. Perhaps, in a philosophical essay, one should be cooler and less engaged. But philosophical principles are supposed to be guides to action. One

of the virtues of the consensus definition of Science is that it helps us to see what we are training scientists *for*. By linking the intellectual, the individual and the communal aspects of research and scholarship, it provides us with something of a moral or ethical philosophy, as well as the rational metaphysic discussed in previous chapters. It is very important to be able to give better grounds for criticizing the products of some particular academic system or graduate school than purely technical defects in their education—but this is only one example of the sort of value judgement or ethical analysis that we now have at our command.

Moreover, we must be continually concerned about the quality of the training of young scientists. Civilized societies are nowadays committed to spending something like one per cent of their resources on research, which could mean that something like one in a thousand of the population must become a professional scientist. Most of these men and women, however admirable, sincere and conscientious they may be, are in Science as a career rather than a vocation. Unlike the gentlemanly dilettante of the past, they cannot withdraw from it if the call does not come. Like the clergy, they may often be trapped between simple material desires and the demands of the religious life. It is not merely a matter of making their scientific work more skilful; we must also make their lives more meaningful, and preserve the scholarly system itself from the effects of disillusioned careerism, spiteful jealousy, hypocritical time-serving, corruption, unprincipled ambition and other evils that may arise. The whole argument of this book is to emphasize the social character of the scientific life. The health of the scientific community depends upon the details of the social transactions of its members and their acceptance of the principles upon which the conventions of the community are based.

Some of those conventions are easily learnt. The general principle of giving recognition to priority is deeply respected in the scientific world for obvious reasons. Ideas and discoveries are the only products of our labour; an idea that is already public, or learnt from someone else, has cost us no labour, and is therefore valueless:

insistence on priority is necessary to prevent plagiarism and fraud; it is the signature on the title deeds of our achievements. It is precisely the public nature of scientific knowledge, its freedom, its communism, its lack of copyright and patents and other restraints upon its use, that makes this so important. The alchemist kept the secret of transmutation, to make a private hoard of gold; the scientist, in a sense, publishes the secret in return for a million pennies of recognition from those who use his technique.

As Merton* has pointed out, there is no real mystery about the bitter conflicts over priority that sometimes arise; the recognition of originality is at stake, and ordinary personal pride can inflame a fancied wrong into a sordid dispute. But why should *originality*, in the sense of independent discovery, be the only claim to recognition in Science? What is the basis of the convention that a scientist is to be judged upon his published work in all matters involving jobs, prizes, promotion, honorific titles and other rewards?

The answer is simply that recognition is given for—being a good scientist. One's *job* is to produce original published work, and hence to contribute to Public Knowledge. Concede that the consensus principle is at the heart of science and the problem scarcely exists. Unpublished work cannot be assessed, and does not satisfy the condition of being public. Unoriginal work is otiose, and speaks for no skill beyond that of an amanuensis.† It is true that there are, as I shall argue in the next chapter, other ways of contributing to Public Knowledge than by primary papers. For example, the writing of reviews and textbooks is scarcely taken into account in the assessment of scientific reputations. But this relative lack of emphasis on subsidiary aspects of scientific activity does not conflict with the general principle.

* R. K. Merton 'Priorities in Scientific Discovery: A chapter in the Sociology of Science' reprinted in *The Sociology of Science* (eds. Barber and Hirsch) pp. 447-85.

† I cannot fathom a remark by Hagstrom (*The Scientific Community*, p. 12) asking why scientists should not simply amuse themselves by solving problems that have already been solved. The analogy with mountain climbing is false: it is impossible to solve a problem, in the fullest sense, whose solution is known. Every student of mathematics knows the difference between a real problem and an 'exercise'.

On the other hand, as emphasized by Storer,* merely successful 'role-performance' in the scientific community—by teaching, sitting on committees, being 'useful' administratively or politically—does not gain much recognition. What this proves, I think, is that the scientific community is not nearly so troubled about its own well-being as some of the sociologists of Science seem to think. It is not a tribe, or an industrial corporation, trying to maintain its own stability and continuance as a social entity; it is a voluntary association of individuals dedicated to a transcendental aim—the advancement of knowledge. It is precisely the continued preservation of the principle of rewarding only 'original contributions' that indicates the vitality of that aim. The sort of anthropological analysis of the social system of Science attempted by Hagstrom and Storer, in terms of an exchange of 'contributions' for 'recognition' makes no sense without the explicit acknowledgement of the perfectly clear ideology and metaphysic that scientists, consciously or by unconscious tradition, are in fact obeying.

Some of the other norms or conventions of the scientific community, as set out in the writings of Merton, Barber, Storer and Hagstrom, may also be understood without great difficulty as consequences of the consensus ideal.

Thus, the norms of *Universalism*, *Organized Scepticism* and *Communality* scarcely need further elucidation. But consider, for example, *Humility* and *Disinterestedness*; why should scientists be expected to take neither pride nor personal profit from their achievement? The answer, as I see it, is that a contribution to Public Knowledge has to be persuasive. If it is put forward in a bombastic 'puffing' style, or if there is a suggestion that the author is being paid in cash for his opinions, it loses an immediate claim to credibility. As I have already remarked, the abstract, impersonal style of conventional scientific communications is an attempt by the author to make his work already seem part of the consensus. Lack of humility, or an apparent ulterior motive, would be signs that the work could not, as we say, 'speak for itself', and that we

* *The Social System of Science*, p. 26.

should watch particularly for evidence of weakness or even fraud in the argument. An author betraying these symptoms is treated not as morally delinquent but as having failed to internalize the critical intellectual standards of his Invisible College, and hence as suspect in his scientific judgement.* Because so much Science has to be taken on trust, one must be particularly scrupulous in one's writings—whatever sort of a scoundrel one may be in daily life!

But moral principles, norms and social conventions only become interesting when they begin to contradict one another. The objections to secrecy, in the light of the consensus principle, need not be rehearsed; we all understand the way in which this conflicts with the norms of national security, corporate profits, etc. Not to publish what ought to belong to the consensus is a crime against Science as such, and can only be justified by the demands of a social system with other ends.

In practice, however, subtler problems arise in the course of ordinary scientific life. One of the decisions that must be taken by every research worker is when to stop his investigations and write up and publish his discoveries. Some men are perfectionists; they never feel quite confident that they have tied up all loose ends and entirely proved their theories. Others (much more common) rush into print with the notebooks of the previous day's experiment, assuming, hopefully, that everybody will want to know about their astonishing discoveries.

There can be no firm convention governing this sort of decision, but the general principles involved are easily stated. In the first place, research is useless unless it is eventually published. There can never be an end to all the implications of a particular discovery, nor can every objection to a theory be entirely countered. One should, therefore, publish the work when it has reached such maturity as to be reasonably self-contained and self-consistent, and not open to definite objections. Let other people have a go at it then.

* A distinguished geophysicist once remarked in jest that nobody believed Wegener because he obviously had a bee in his bonnet about Continental Drift, whereas Jeffreys, a shy and retiring man, obviously had nothing to gain or lose by it!

On the other hand, premature publication is one of the curses of modern Science. Many scientists are so obsessed with the fear of being 'scooped' or are so anxious to notch up a good score of publications that they issue a long succession of scrappy communications instead of waiting until the work is complete and clear and can be written up as a whole. From the consensus standpoint, this is wasteful and tiresome. Each successive partial communication is demanding to be criticized and accepted but does not carry its own full justification with it. It is an abuse of privilege to claim the ear of the profession for what is, as yet, 'interesting', thought-provoking, speculative, but by no means fully established. At the moment that he is doing a particular piece of research a scientist is, so to speak, the champion of mankind against Nature on that field, and it is his duty to carry each round through to some sort of decision. It is an impertinence to keep shouting one's success after each thrust and parry, or to suppose that other people really care. The consensus itself does not progress by infinitesimal steps. To carry conviction, to overcome mental inertia and prejudice, one does much better to wait until one has built up a strong case for a substantial advance—a camel to swallow rather than a gnat to strain at.

Consider again the problem of deciding what research to do. If one knew what questions *could* be answered, one would not need to ask them. Many scientists, in fact, merely follow the line of least resistance, continuing to pursue the same sort of research, in the same field of science, throughout their lives. When this culminates in the elucidation of the structure of haemoglobin it is labelled heroic persistence and rewarded with a Nobel Prize; very often it is not unreasonably dubbed pedestrian and plodding.

But at any given moment there are always a number of scientists looking for quite new problems, and nearly new fields of research—whether because old trails have apparently petered out or, because newly graduated, they need to set themselves up in a subject. It is natural that they should seek entry into the field where the returns seem most promising—and that many should take the same decision at the same time. As we have already remarked, there is a

'clumping' effect, tending to favour a small number of 'fashionable' subjects rather than spreading the effort over the field as a whole.

Hagstrom* has discussed the phenomenon of fashion in Science at some length, and has elucidated many of the social factors by which prestige is allotted to various fields. Yet we must be careful that the clumping effect should not be confused with such purely imitative social behaviour as fashion in clothes. The simultaneous decision of many scientists that a new field is 'promising' may be fully justified; the defect, from the point of view of the orderly advance of knowledge, is that there is no means of preventing too many of them from crowding into it all at once. In the language of Control Engineering, there is an 'overshoot', and the stabilizing counterforce—the bitter competition that develops in the over-subscribed field—is slow to act.

The fact is that the current consensus carries with it not only knowledge of problems that *have* been solved, but also strong hints and opinions as to what problems now *could* be solved. A major discovery, of theory or of technique, implies the possibility of rapid progress in the solution of many old difficulties. The individual scientist, seeking not meretricious prestige or rapid promotion, but the furtherance of his subject, rightly judges that his powers may be best expended in this work of exploitation, rather than in some backwater that seems to lead nowhere. Or he may believe that by mastering the new technique he will be able to make progress with his own old research. It must always be remembered that scientific research is a very difficult art, in which ninety-nine per cent of frustration and perspiration is not always balanced by one per cent of inspiration and elation. It requires peculiar self-confidence and will-power not to follow a line that promises relatively easy returns.

This is not to deny that the judgement of the promise of a field is often shallow and faulty. Some people somehow think that by retracing the steps of a famous discovery, or repeating it with slight variations, they may make comparable contributions. Others suppose that anything done by a big name is likely to be worth imitating. Others, again, have never learnt that all research is in-

* *The Scientific Community*, p. 177.

teresting—and all research is also dull—regardless of the field, and imagine that they will especially enjoy the atmosphere of excitement and glamour that they have heard of (mostly in newspaper articles!) in a new field.

What I would emphasize is that this sort of reasoning, for all its defects, is not mere rationalization of the desire to be in a field of high status. Such feelings are present; it may be that they are becoming more and more dominant in the world of Science; but the ethical imperatives of the consensus principle are also active, and should not be prematurely explained away.

An active research scientist is continually facing similar problems. Should he collaborate with a senior man? How far should a controversy be carried? How much direct guidance should he give to a research student? Would an 'acknowledgement' of help received be adequate, or should he invite his assistant to be joint author of a paper? Should he take time off from experimentation to write a review article? Has he sufficient ideas to justify the expenditure of large sums of somebody else's money on equipment? Should he turn aside from 'fundamental' research to exploit a practical application?

In everyday life we learn to solve such conflicts by appeal to experience and to principle. In childhood we profit particularly from the instruction of our parents, so that in many cases we only know that it is 'right' to do as we should, be honest, truthful, considerate, etc. without detailed reflection on the principles at stake. The ethical principles of the scientific life are also learnt from our seniors—our professors, research directors and colleagues.

The defect of an oral tradition is that it can change imperceptibly from generation to generation, and cannot easily be transplanted from one culture to another. *Quis custodiet ipsos custodes?*: the apprenticeship system fails if the masters themselves do not understand their craft. To prevent a drift into decadence, or corruption, it is essential to anchor conventional behaviour upon articulate rational principles.

I do not believe that such decadence or corruption is rife in

modern Science. There are, as I have suggested, many unconscious self-correcting forces preserving the integrity of scholars against the spiritual temptations in becoming a 'Fifth Estate' of the realm. Yet the history of the Church offers many examples of the extraordinary way in which explicit principles of virtue may be corrupted and totally perverted. It does no harm to give a tug now and then at the cables, to see whether our moorings are holding.

It is disquieting, therefore, that the general problems raised in this chapter are given so little serious discussion. The books and articles on the 'Science of Science' seem to concentrate mainly on who ought to get how much out of which pork barrel, or whether a scientist can be happy as an administrator (is the answer, 'Yes, a very bad scientist in a very good administration', or is it 'a very good scientist in a very bad administration'?), or how strong your neuroses must be to give you a bright idea and such like eminently practical topics.

A start has been made in the writings of the new school of sociologists of Science, to which I have referred a number of times in this chapter, especially Hagstrom's excellent book, which is entirely authentic in its depiction of the opinions and behaviour of top scientists in the United States, and suggests most interesting sociological interpretation of this behaviour. But it takes certain norms of the scientific life as arbitrarily prescribed, and shows the individuals as apparently governed only by a wary and calculated self-interest within the framework of these norms—rather like children enjoying themselves as best they can within the inexplicable rules laid down by tiresome adults.

There is fine talk nowadays of teaching Science to art students, and showing them 'what makes scientists tick'. My real fear is that we do not teach what Science is to *science* students—that we have largely lost the feeling for a Philosophy of Nature and do not understand the rationale of the procedures by which that Philosophy may be established and enlarged. The principle of the intellectual consensus provides an explanation of many of these procedures and a justification for critical standards and behavioural norms that might otherwise seem arbitrary and outmoded.