The Profession of Science and its Powers

JOSEPH BEN-DAVID

TODAY scientific research as an occupation is a " profession ", like medicine, law and engineering. There are considerable differences among these occupations, but certain common features justify their inclusion in a single category. These features are: (1) a higher educational qualification as a prerequisite to entry into the occupation; (2) the privilege of monopoly in the performance of certain functions (such as treating patients, signing blueprints for construction projects); (3) a measure of control of admission into the occupation, as a means of maintaining its standards and status; and (4) the formal or informal authority of a professional body over the conduct of its members, a resistance against lay interference in the affairs of the profession and regulation of competition among members of the profession.[1]

While other occupations possess some of these features, they are considered as legitimate only among professions. Thus, for example, the regulation of competition among physicians is enforced by law; all kinds of rights in this respect are granted to local medical associations. The same actions, however, are considered illegitimate or actually criminal if performed by businessmen, and are viewed as economically pernicious extortion if they are enforced by trade unions.

The final component of this distinctive constellation of features of the professional occupations is: (5) a limitation on the contractual obligations of the professional towards his client or employer. The patient cannot order a certain kind of treatment from his doctor; university teachers enjoy academic freedom to teach the way they want and to some extent what they want.

These features are not equally present in all the professional occupations. The educational qualification, the privilege of monopoly and the discretionary freedom are probably present in all of them. Control of the right to practise and corporate self-regulation are also widespread. There are, however, large differences in the exercise of these functions among various professions within the same country, and within the same profession in different countries.

The possession of these features is itself a corporate privilege. In other occupations this kind of privilege was abolished in most European countries

[1] The most exhaustive and systematic description of the development of the professions and professionalism is still Carr-Saunders, A. M. and Wilson, P. A., *The Professions* (Oxford: Clarendon Press, 1933). This book deals only with Great Britain, but the present conception of the professions and of the professional ethos developed mainly in that country and the United States. See also Reader, W. J., *Professional Men: The Rise of the Professional Classes in Nineteenth-Century England* (New York: Basic Books, Inc., 1966), and Moore, Wilbert E., *The Professions: Roles and Rules* (New York: Russell Sage Foundation, 1970).

between the seventeenth and nineteenth centuries. This is not to say that all occupational privileges were effectively abolished or that there are equal occupational opportunities for everyone in the regime which succeeded that of corporate privilege. Monopolies and cartels have been established by industrialists and merchants of the most diverse kinds; bureaucracies and trade unions have restricted the free market for labour. But none of these is a corporate privilege in the medieval sense, since, at least in principle, control over the exercise of the privilege is vested in bodies which are not part of the occupational group. Monopolies are granted and supervised by governments, bureaucracies are controlled by non-bureaucratic bodies, such as entrepreneurs and parliaments, and trade unions exercise their privileges through bargaining and strikes. Only in the professions is the right to control the exercise of the privilege vested in the profession itself. The question of why this special privilege was granted to the professions and not to other occupations has been asked before, but the explanations given were valid only with regard to the classical learned professions of medicine and law. Here I propose to deal with the occupational activity of scientific research as a profession.[2]

Corporate Organisations: Academies

Autonomous corporate scientific bodies became centres of scientific activity in the seventeenth century, at a time when science was practised by unpaid amateurs and when this corporate autonomy had no economic importance. The original models were the Italian academies, but the most important were the Royal Society of London and the Paris Academy of Sciences. They became important when the development of the practice of science into a coherent, acknowledged intellectual activity engendered formally established institutions for communication and competent assessment of scientific works; hitherto, informal correspondence between individuals and the conventional publication of treatises had sufficed. Another factor in the emergence of the corporate institutions of scientists was the need to legitimate the new type of activity within the existing social order; otherwise scientific activity might have been regarded as subversive of traditional, particularly religious, institutions.[3]

Subversion of traditional beliefs is an inherent potentiality of science, as it is of any activity the aim of which is original discovery or expression. The very emergence of modern science could be interpreted as a denial of

[2] Because of the great conspicuousness of academic scientists, and the sharp distinction between academic and non-academic scientists in some countries (which is discussed in this paper), the profession of scientific research in general received little systematic attention. An outstanding exception is Shils, Edward, "The Profession of Science", *The Advancement of Science* (June, 1968), pp. 469–479.

[3] The present view on the social conditions of the rise of modern science in the seventeenth century is elaborated in Ben-David, Joseph, *The Scientist's Role in Society: A Comparative Study* (Englewood Cliffs, N.J.: Prentice-Hall, 1971), pp. 45–74.

the traditional view of the universe and thus it had far-reaching implications for religion. It was moreover a process the end of which could not be foreseen. The early " statesmen of science " saw that it would be necessary to protect scientific activity against attacks by the custodians of the traditional views of the cosmos and that corporate bodies enjoying the auspices of governmental authority were therefore in order.

The patrons and protectors of science believed that the dangers could be contained; they were confident that the scientific method was a means of distinguishing truth from error in a way which would not be destructive. Unlike the verbal arguments of speculative philosophers which culminated in unresolvable dissension and conflict, the rigorous logic of mathematical proofs and experimental tests led to results which sooner or later were bound to command universal assent. The granting of intellectual autonomy to science was, therefore, not considered as especially dangerous since the freedom of science to subvert tradition, it was believed, was not, if appropriately brought into the framework of institutions, inconsistent with the maintenance of social responsibility and order. In fact the scientific method was seen as the most effective way to establish such order, more powerful than any speculative philosophical tradition or theological doctrine. Therefore, science was granted the freedom of " cognitive subversion ", because the scientific method was seen as a self-regulating mechanism, which, through its internal discipline, was capable of delimiting the spread of the subversion which it brought in its train and preventing the abuse of intellectual freedom.

The scientific method was not a divine revelation; it was a creation of man. Only if used by competent persons in an appropriate manner could the scientific method decide between the true and false. In the hands of the incompetent, or the dishonest, the method was useless and even dangerous. If the administration of science lay in incompetent hands, mankind would be exposed to the dangers of " false prophecy " which subverts tradition for diabolical purposes or at least for ulterior motives. It was regarded as necessary, therefore, that some kind of social body be established for the competent assessment of scientific works, to define and maintain the boundary between valid scientific findings on the one side and error and non-science on the other. Not the scientific method alone, but its proper use by competent persons was regarded as a guarantee of an effective self-regulation.

Such a social mechanism was required not only for the protection of the lay public from quacks and intellectual counterfeiters, but also for the protection and just reward of scientists. Since the public, even if it wanted and valued science in general, was usually uninterested in, and incapable of appreciating, particular contributions to its advancement, scientists would have been deprived of the appreciation and stimulus emanating from the like-minded and qualified. Hence the desire for a special body the judgements of which were scientifically competent and at the same time accepted and honoured by the general public.[4]

These functions were performed by the academies, which, in order to perform them effectively, had to have some official standing. They also had to be completely autonomous; otherwise they could not truthfully represent the objective scientific view as established by the self-regulating methods of experiment and mathematical proof.

The Regulation of Subversion

The substantive frontiers of science have never been stable, and cannot be established *a priori*. The scientific method has been only a procedure for criticism and testing, not for discovery. Discovery could not be as formally codified as the canons of criticism and the criteria of testing. Hence the degree of risk allowed in the search for the scientific understanding of fields not previously explored in a scientific manner has been a major problem in the institutionalisation and organisation of science. Some societies have been ready to assume a considerable risk in laying themselves open to the unforeseeable outcome of the efforts of discovery. Others have been much less so.

The first course was taken in England. Or, to be more precise, the increased appreciation of science in seventeenth-century England was part and parcel of the process of change which, between 1640 and 1689, turned England from a traditional, religious society into a pluralistic, democratic one. Science served during this time as the symbol of modern, " advancing " knowledge, as contrasted with knowledge attested by the authority of tradition.[5]

Therefore, except for a brief period under the Restoration, the *institutional* demarcation of science from non-science was not a major issue.[6] Strenuous efforts had been made by scientifically more or less competent intellectuals to use the scientific approach as a model for the solution of political, economic, moral and technological problems. The fact that many

[4] The analysis of the importance of an institutional framework in the proper assessment of scientific works is mainly the achievement of Merton, Robert K., " Priorities in Scientific Discovery ", *American Sociological Review*, XXII (December, 1954), pp. 635–659, and in Zuckerman, Harriet, and Merton, Robert K., " Patterns of Evaluation in Science: Institutionalisation, Structure and Functions of the Referee System ", *Minerva*, IX, 1 (January, 1971), pp. 66–100.

[5] The role of science in the transformation of the traditional religious culture of Europe into a modern one is treated in Jones, R. F., *Ancients and Moderns: A Study of the Rise of the Scientific Movement in Seventeenth Century England* (St. Louis: Washington University Press, second edition, 1961), and Westfall, Richard S., *Science and Religion in Seventeenth Century England* (New Haven, Conn.: Yale University Press, 1958). For some important qualifications of Jones's views, see Debus, Allen G., *Science and Education in the Seventeenth Century* (London: MacDonald, and New York: Elsevier, Inc., 1970), pp. 1–64, and Rattansi, P.M., " The Social Interpretation of Science in the Seventeenth Century ", in Mathias, P. (ed.), *Science and Society, 1600–1900* (Cambridge University Press, 1972), pp. 1–32.

[6] This is not to say that demarcation itself was not a problem. It was and has always been. But there is a difference between demarcation taking place as an ongoing debate, and an officially established line of demarcation.

of these attempts were quasi-scientific was not perceived as presenting a grave danger either to the integrity of science or to the order of society, perhaps because religious and political homogeneity had already been destroyed and a quite wide range of diversity had come to be tolerated. Hence, the extension of the scientific approach to these socially sensitive concerns did not arouse much apprehension. There had already been so much conflict that these " scientific " inquiries were accepted because they held out the possibility of softening and diminishing conflict. The self-regulatory mechanisms of science did not seem very far removed from the self-regulating market, the self-regulating polity of checks and balances and the toleration of religious and political heterodoxy. Openness to criticism and innovation were characteristic of the latter and the same sympathies were extended to scientific arrangements.[7]

In France, on the other hand, science was less in harmony with the prevailing trends of religious and political thought. It could be protected only if it were insulated and this was accomplished by the establishment of an authoritative, governmentally sponsored academy charged with maintaining a strict boundary between science and non-science. Discoveries were regarded as legitimate. They could be freely published when they occurred in the proper domain of science but they were subject to censorship when they appeared to fall in the sphere of non-science.

The Royal Society of London, in contrast, took upon itself the function of representing science to the public and of rewarding scientific discovery; it was less concerned with the function of the institutional demarcation of science from non-science. The Royal Society was never granted the power to regulate the work of scientists, or to determine, in an officially binding way, who was a scientist and who was not, what was science or was not. Its authority rested purely on the excellence of the accomplishment of its members and the freely granted acknowledgement of scientists all over Europe that judgements rendered by fellows of the Royal Society were scientifically valid.

The Paris Academy of Sciences had a considerable degree of actual control—as distinct from influence—over scientific publications and the granting of letters-patent to inventors. Membership in it was not merely a public recognition of excellence, but also a source of income, power and legally guaranteed privileges.[8]

[7] For the emergence of the idea of the self-regulation of the economic system and its relationship to the new scientific method, see Letwin, William, *The Origins of Scientific Economies* (Garden City, N.Y.: Anchor Books, Doubleday & Co., Inc., 1965), pp. 187–192, 205–220. The systematic generalisation of this idea to political self-regulation (*laissez-faire*) occurred later, but for this too the basis was laid in the seventeenth century in the tendency to view moral laws as laws of nature.

[8] For the similarities in, and the differences between, the structure and the functions of the Royal Society in London and the Paris Academy of Sciences, see Brown, Harcourt, *Scientific Organizations in 17th Century France (1620–1680)* (Baltimore, Md.: Williams and Wilkins, 1934); Stimson, Dorothy, *Scientists and Amateurs* (London: Sigma Books, 1949); and Hahn, Roger, *The Anatomy of a Scientific Institution: The Paris Academy of Sciences, 1666–1803* (Berkeley, Los Angeles, London: University of California Press,

So the Academy of Sciences came to be perceived as a political body charged with the regulation of science, rather than the representative body of the self-regulating scientific community. This regulating function—unlike the representative one—could not be effectively performed by a small elite over a long period of time. With the growth and diversification of scientific endeavour, however, the need to subject every discovery—and even every invention—to the authoritative decision of a small body of scientists became increasingly cumbersome, stifling and inefficient. In consequence, the supremacy of the Paris Academy was shortlived, while the Royal Society has managed to maintain its standing for more than three centuries. The latter never claimed to be a body apart from and in control of the scientific community. It played an important role in the self-regulation of the scientific community, but it never claimed any sovereignty over the regulation. It had no coercive power to add strength to its regulation. The demarcation of science from non-science, as well as the evaluation of excellence, was left to some degree to the scientific community in general.

Until the end of the eighteenth century the organisational needs of science were satisfied by corporate bodies—*i.e.*, the academies—of the scientific elite which were guided by the currents of opinion in the scientific community. The variations between the functions of these corporate bodies in different countries depended on the extent of general freedom of speech and dissent in religion, politics, etc. Where there was no such freedom, the demarcation of science from non-science was a matter of great practical importance. It lent to science an invidiously attractive status, and also protected its standards from being diluted by amateurish work. But with the growth in the numbers of scientists and of scientific works—partly as a result of the high status of science—the position of the academies became anomalous in the eyes of some scientists themselves. At that point, these powerful academies became a hindrance to the free growth of science. Their formal privileges became unjustifiable, anachronistic class privileges in the eyes of those scientists who did not possess them. But it was only the control of science by privileged central academies which was resisted. The existence of independent scientific societies continued to be regarded as a suitable framework for the self-regulation of the scientific community.

Charismatic Inspiration versus Institutionalisation

Scientific research gradually became a salaried occupation in the course of the nineteenth century. There then arose problems of providing careers, organising the work and allocating the resources and rewards of scientific work. Once scientists began to be paid for their work, amateurs had little chance to compete. The mechanisms allocating payment became in prac-

1971). The comparisons made between the two institutions by the critics of the Paris Academy during the French Revolution are of particular interest. See Hahn, Roger, *op. cit.*, pp. 181–182.

tice the mechanisms by which science was demarcated from non-science. This eliminated the ambiguity previously prevailing in the demarcation of scientific activity. The establishment of an institutionally defined boundary line between science and non-science was a threat to the charismatic character of science.

Scientific achievement at the highest level was viewed as the work of genius. Genius was the result of inspiration or possession by the spirit which drives its carrier, the one possessed, to reach into and discern the centre of existence. Like prophecy, great scientific discovery was perceived as being performed by extraordinary spirits, driven by a profound inner force. Priestly functions could be institutionalised, training for the priesthood could be institutionalised but prophecy could not be, either in its preparation or its performance. A similar conception prevailed concerning great scientific discovery, and only great discovery counted.

This conception was an obstacle to institutionalisation. When resources and rewards were pre-empted by scientists who made a career of scientific work, genius which lacked formal qualifications was handicapped. It was particularly difficult to establish a salary scale for the creative activity of genius. Who could decide how much to pay per month, or year, for work with unknown, and from the point of view of the employer, perhaps undesirable results?

These were some of the reasons why scientists, as well as their patrons and supporters, were extremely reluctant to see scientific research become a full-time occupation for which aspirants qualified through formal training and the acquisition of degrees, and in which they then engaged continuously for the rest of their working life.

The first stage in the development of scientific research into a salaried profession occurred in France between the 1780s and the early decades of the nineteenth century.[9] It consisted of the establishment of a relatively large number of higher educational institutions for the training of physi-

[9] The growth of opportunities for the employment of scientists in France is described in Crosland, Maurice, *The Society of Arceuil: A View of French Science at the Time of Napoleon I* (London: Heinemann, 1967), and in Crosland, Maurice (ed.), *Science in France in the Revolutionary Era Described by Thomas Bugge* (Cambridge, Mass. and London: The MIT Press, 1969). Professor Crosland considers this increase in opportunities for employment as the beginning of professional science (see his Letter to the Editor, *Minerva*, VIII, 3 (July, 1970), pp. 453–454) on the ground that these opportunities made possible a greater continuity of research than had been the case previously. But this was only a first step towards professionalisation. In fact, the social structure of the scientific career in France did not become professional until the second half of the nineteenth century. There was no place like Paris to learn science, and scientists could easily find appointments and income in Paris which could sustain them and enable them to do research incidentally. But there was no institutional arrangement for the training of scientists, nor were there any careers designed or provided for those who wished to concentrate on research continuously and exclusively. Paris was the world centre of science, attracting aspiring scientists from everywhere in Europe, as it had been a centre for art and literature. It was an important stage on the path towards science as a profession, but the actual emergence of professional science took place elsewhere. By the time professionalisation occurred, Paris had ceased to be the centre. See Ben-David, Joseph, "The Rise and Decline of France as a Scientific Centre", *Minerva*, VIII, 2 (April, 1970), pp. 160–179.

cians, engineers and secondary school teachers, and for the provision of advanced lectures for the general public. In the educational philosophy which prevailed in these institutions, the scientific subjects were accorded a prominent place. It was argued that a sound training in science was a necessary foundation of professional practice in engineering and medicine, as well as of a good education in general.

These institutions had to employ scientists as teachers and thus teaching became the main source of livelihood for the majority of scientists. But this did not lead to the professionalisation of research. The new educational institutions did not train their pupils to conduct research, and their teachers were not employed with the understanding that they themselves would do research. Scientists received salaries on the grounds of their scientific knowledge but they were not paid to do research. Entry into a scientific role was still not institutionalised; becoming a scientist was still a kind of charismatic process. The aspirant scientist studied where and what he thought fit and worked as an apprentice in someone's laboratory. There was no formal termination of the period of training, and no definite point of entry into a " scientific career ". The scientists were those who were at a certain point " recognised " as being scientists.

The same conception of what was involved in being a scientist prevailed everywhere in the West, including Germany. But in the organisation of higher educational institutions, there was an important difference between France and Germany. The German universities assumed the function of the " recognition " of the scientist; and they reserved their teaching positions for recognised scientists and scholars. While in France recognition had no rules and no definite site, but occurred in a spontaneous and unspecified manner,[10] in Germany recognition was an official certification by the university. It took place in accordance with certain rules. Furthermore, while recognised scientists in both countries could obtain their livelihood as teachers in higher and secondary education (and, in France, in other capacities as well), in Germany a university professor was also, by definition, a recognised scientist. This was not the case in France. In Germany, professors were appointed on the basis of their scientific qualifications and accomplishments. To do research was at least as much a part of their official duties as teaching. Thus there arose in Germany a full-time occupational role, that of the university professor, whose professional duties explicitly included research. That was what he was paid to do. It was not something which he did in his spare time alongside teaching, providing medical services or acting as custodian of a museum or botanical garden.[11]

[10] There was, of course, official recognition through prizes, and election to the Institut, but these rewards came much later than the informal recognition of the scientist by his peers and the instructed public.

[11] The steeply hierarchical character of academic science and its demarcation from non-academic professional science are described in Paulsen, Friedrich, *Die deutschen Universitäten und das Universitätsstudium* (Hildesheim: Georg Olm, 1966) (first published

All this did not yet mean that research had come to be considered a regular occupation. There were still safeguards explicitly designed to preserve the charismatic quality of science. Lectures, seminars, examinations and other prescribed tasks were not sufficient conditions of entry into academic positions, which were the only recognised and paid positions in research until the end of the nineteenth century. The would-be academic scientist still had to do his work on his own, and submit it for recognition only after its completion. The acceptance of the *Habilitationsschrift* and the conferral of the *venia legendi*—the right to teach in a university—were acts of recognition given for original (and, therefore, unpredictable) accomplishment; they were not the recognition of the successful completion of a prescribed course of training for professional activity. The right to lecture at the university was a right of the recognised scientist. The recipient of the *venia legendi* became a *Privatdozent*; he received no salary, only the fees of the students who attended his lectures. Although he was expected to do research, no provision was made for it and he received no payment for doing it.

The *Privatdozenten* were to constitute a stratum of unsalaried, freelance, albeit qualified scientists. Only a few of them could expect to be appointed as professors with regular salaries. And although professors were appointed on the basis of their research and were expected to do research as well as teach, *Privatdozenten* had to do their research privately. Just as they were free to decide how to teach and what to teach, so they could decide on what research to do and how to do it; they were not provided with laboratories in which to do it although they were provided with teaching and seminar rooms.

The universities were, therefore, conceived as teaching academies, and conferred upon their members the privileges of corporate freedom similar to those of the academies. The term "academic freedom" was coined for these universities to emphasise that these were not educational institutions in the ordinary sense, but centres of research and of teaching based on original inquiry.

For a variety of reasons these arrangements did not work as intended, and research became a regular occupation, in spite of the intentions of preventing such a development. Those who decided to vie for recognition through the submission of a *Habilitationsschrift* did so in the hope of

in 1902); Weber, Max, "Science as a Profession", in Gerth, H. H. and Wright Mills, C. (eds.), *From Max Weber: Essays in Sociology* (London: Kegan Paul, Trubner & Co. Ltd., 1947), pp. 129–156 (first published in 1919); Busch, Alexander, *Die Geschichte des Privatdozenten* (Stuttgart: F. Enke, 1959); Busch, Alexander, "The Vicissitudes of the *Privatdozent:* Breakdown and Adaptation in the Recruitment of the German University Teacher", *Minerva*, I, 3 (Spring, 1963), pp. 319–341; Zloczower, A., *Career Opportunities and the Growth of Scientific Discovery in 19th Century Germany* (Jerusalem: Hebrew University, Eliezer Kaplan School of Economics and Social Sciences, 1966); Ashby, Eric, "The Future of the Nineteenth Century Idea of a University", *Minerva*, VI, 1 (Autumn, 1967), pp. 3–17; Ben-David, Joseph, *op. cit.* (1971), pp. 108–138; and Turner, Stephen, "The Growth of Professorial Research in Prussia, 1818–1848: Causes and Context" (Princeton: Princeton University, n.d., mimeographed).

ultimately becoming university professors. And since there were numerous universities, it was not unreasonable for the *Privatdozent* to calculate his chances for appointment. These varied a great deal at different times and in different fields.

The possibility of a career in research, even if it was accessible only to a tiny fraction of the students, led to the provision of seminars and laboratory instruction where students were actually trained in research. In the laboratory sciences, where several students and assistants could work on experiments based on the ideas of a single person, there emerged by the end of the nineteenth century bureaucratically organised research institutes. As a result of this evolution the universities produced considerable numbers of more or less competent research workers capable of doing more or less original work, just as they produced physicians, lawyers, etc. In the course of time the *Habilitation* became increasingly a formal qualification for which one worked in a programmatic way. The title of *Privatdozent* had become a professional degree, like a second and higher doctorate, and ceased to be a testimonial of charismatic recognition. Scientific work, like other highly skilled work, became an occupation in which there was a wide range of talents and achievements. It ceased to be regarded as something which could be done only by charismatic geniuses. The assumption that scientific discovery was a charismatic action became openly self-contradictory. On the one side, it treated the research of the professional stratum as charismatic and therefore not subject to institutional organisation while, at the same time, students were being trained to do research. The anomaly was not aggravated because the charismatic quality of scientific activity was disappearing; on the contrary, it had never been so evident. Never were more great discoveries made by great scientific personalities. There were no dissenters about the importance of the recognition of the great discoveries and discoverers in science, or about the inevitability of very unequal distributions of scientific genius. The anomaly which many intelligent observers began to sense consisted in the fact that not all the great discoverers were professors, while scientific influence and financial resources for research were monopolised by the professors, *i.e.*, the ordinary or full professors.

Although the anomaly was widely acknowledged, the remedy of abolishing excessive academic privilege seemed to be worse than the illness. Without some distinction, such as existed between professors and other research workers, there could not be a clear-cut institutional demarcation between true innovative science and routine research. The abolition of such demarcation was considered as threatening in nineteenth- and even twentieth-century Germany, as under the *ancien régime* in France. In addition to the status-consciousness of a hierarchical society, there was also genuine concern about the need to preserve the arrangements required by the charismatic character of scientific discovery, which might be threatened by the abolition of the line of demarcation between profoundly original contributions to science and the more routine achievements of institutionally trained professionals who were no more than competent.

The problems which had appeared in late eighteenth-century France thus reappeared in late nineteenth-century Germany. The academic freedom of the universities, originally designed as a safeguard of the freedom of all qualified scientists and students and a condition of the maintenance of the non-bureaucratic, charismatic character of science, became a source of bureaucratic power and invidious distinction. For *Privatdozenten* and assistants who saw themselves as persons on the lower rung of the ladder of the academic career and saw the powers of the *Ordinarien* as obstacles to their upward movement, the salaries, the research institutes and the self-governing rights of the professors were constant reminders of their servitude and subordination. Even if they shared the view of scientific discovery as a product of charismatic inspiration, they could not but regard the existing distribution of facilities and rewards as handicaps to their own charismatic potentialities.

The Progress of Institutionalisation: Training for Research

Misgivings about the professionalisation of science were not confined to Germany. Nevertheless, certain features of research were professionalised in the United States, and to some extent in Great Britain as well. The Ph.D. course in the United States became a programme for training persons for scientific careers.[12] The qualification entitled a person to full membership in specialised professional associations. The possession of a Ph.D. carried with it a set of expectations in the employment market. The employer of a Ph.D. took it for granted that such a person would conduct research, and would have to be granted considerable autonomy in his work. It was also taken for granted that research, even if useful to the employer, could properly be evaluated only by other scientists, and that the research worker would be interested in their recognition and not only in the income received from his employer.

The universities did not lose their special importance in the " recognition " of who was a scientist. The procedures of selecting incumbents for professorial chairs at the leading universities have carried the connotation of reward for exceptional achievement. The freedom of the academic teacher in his teaching and research, bolstered by permanent tenure, was the model for research workers who were engaged in scientific activity outside academic institutions. But still the difference between academic research and other kinds of research, and between the full professor and

[12] For the development of the Ph.D. degree and some of the accompanying doubts, see Veysey, Laurence R., *The Emergence of the American University* (Chicago and London: University of Chicago Press, 1965), pp. 149–179, 313–314, 418–423. Professor Veysey thinks that the missionary zeal of the pioneers who established the Ph.D. programme was partially in contradiction to the professional character of the doctoral training. But pioneers of other professions have shown similar missionary zeal, and this seems to be a characteristic of the founders of professions generally.

those at the lower grades, ceased to be an unbridged disjunction. There was no charismatic status automatically attached to a salaried position, whatever its rank. Some professorships were usually filled by persons of exceptional gifts, but a professorship in the United States became little more than the best-remunerated stage of a normal career.

In England the situation has been essentially similar.[13] There the Ph.D. has even now not attained the importance which it acquired in the United States, but there, too, there emerged a conception of professional qualification in science, and of membership in a professional community. All scientific positions were open, in principle, to all qualified scientists (*i.e.*, those possessing an honours degree), and no salaried positions were institutionally demarcated for the monopoly of scientifically charismatic individuals.

This acceptance of professionalism has not led to the abolition of the demarcation between science and non-science. This was maintained by a system of scientific recognition and reward operating through such processes as the refereeing of publications, election to honorific bodies like the Royal Society, appointments to posts in the universities with the most eminent departments and informal professional opinion. In the absence of the institutional abyss which separated the German professor from his inferiors and which was intended to protect genuine science from spurious science, the institutions in Great Britain, such as the various scientific associations, academies, societies, councils and journals, which administered this system of allocation of appointments and honours on behalf of the scientific community and its various branches, assumed great importance. They performed the function of maintaining the conditions in which the charismatic element in scientific discovery could operate without obstruction at a time when research was becoming extensively institutionalised.

The " professional " character of scientific work was the result of the interaction between the processes and representative organisations of the scientific community and the conduct of research as a life-long, remunerated and graduated career. Leadership in each field was the outcome of scientific opinion. It was to a large extent concentrated at the leading universities; it set standards of training, qualification and achievement for the profession. The scientific community regulated scientific work independently of the lay users of science and the lay employers of scientists; it did not, however, do so exclusively, since users and employers also exerted some influence. Still, the influence of the scientific community was extremely powerful. Unlike that of the users and employers, which varies from place

13 About the circumstances and the motives of the introduction of training in science in England, see Cardwell, D. S. L., *The Organization of Science in England in the Nineteenth Century* (London: Heinemann, 1957). Although it does not deal directly with this subject, an impression of the professional career and of the relatively unified professional character of English science can be gained from Hutchinson, Eric, " Scientists as an Inferior Class: The Early Years of the DSIR ", *Minerva*, VIII, 3 (July, 1970), pp. 396–411.

to place and from time to time, the influence of the scientific community is consistent and persistent. Furthermore, users and employers exert their influence on the trained scientist, while the scientific community forms the scientist through his training and through continued pressure of its standards and expectations. These are determined by academic scientists who, enjoying " academic freedom ", are subject only to the self-regulation of the scientific community.

Changes in the Loci of Self-Government

This arrangement, which gave a larger place in the allocation of resources to the mechanisms of the market than had been characteristic of science before it became so pertinent to technology, was not adequate to the charismatic element in science, *i.e.*, to the need for original research. To the academic visitor from Europe, the American university of the first decade of this century was a bureaucratic teaching institution, with no safe provision for research.[14] But this opinion took no account of the likelihood that those market conditions would be increasingly influenced in favour of science by the spread of scientific professionalism. Initially, scientific research in the United States received little support and that came mainly from private sources, that is, from the leading private universities which were eager to promote research and from individual philanthropists and private foundations which helped them to do so. A consequence of this private support for research was a gradual improvement of the standards, first, of the academic profession, and then of those professions which had an increasingly scientific basis. This rise in standards furthered the demand for original research, which in turn lent more power to professionalism; the reciprocal influences continued to the benefit of the quality of scientific work.

As a result, the professional autonomy of science in the United States has grown constantly. In the beginning its main results were the constant strengthening of academic freedom, in particular, and of professional autonomy in general. University professors obtained more or less complete freedom to decide what and how to teach and investigate in their respective fields, and less than complete, but still considerable, freedom to determine the time spent on research. Scientific associations, including medical, technological, etc., associations with large proportions of their members engaged in professional practice, followed suit by raising standards of training, increasing the emphasis on research in the course of training and instituting sabbatical arrangements, refresher courses, etc., for keeping their members abreast of ongoing research. All this has had a considerable effect on the allocation of resources for research. As a result of increased demand, financial resources increased, although the actual allocation of funds took place through the market.

[14] For an account of the American university as given by a very penetrating European observer, see Weber, Max, *op. cit.*, pp. 129–133.

This structure considerably mitigated the invidiousness of the distinction between scientists of different ranks. The demarcation between original and routine research became less definite and more realistic than the sharply defined separation. It also demonstrated the rigidity and injustice of the identification of scientific charisma with the incumbents of certain positions.

The demarcation of the sphere of " true ", *i.e.*, fundamental and original science from that of doubtful, routine or applied scientific work took various forms in various countries once science became a salaried occupation. The mode of demarcation was embodied in the organisation of scientific work and the structure of the scientific career. The main alternatives were two: one, an official institutionalised demarcation between the two types of science by setting apart certain positions, such as the professorship, and certain rights, such as academic freedom, for the first type (" true science "), thus creating an institutionalised distinction between classes of scientists. The other alternative was to consider all professional (*i.e.*, formally qualified) scientists as possessing the standing of citizens in the scientific community, leaving the separation of " true " from other science to the institutions of assessment in the scientific community and treating academic freedom as a variant of professional autonomy. Both these arrangements rested on the assumption that the scientific community was capable of and in need of effective self-regulation, and that some kind of corporate autonomy was required as a framework for this self-regulation. They differed from each other in that in the former arrangement the universities were the corporate bodies, dominating the machinery of self-government of the scientific community, and in the latter case scientific societies and associations exercised a larger share of the power, in partnership with the universities. Both systems distinguished original research and fundamental discovery from other types of scientific work and rewarded them accordingly, but in the system in which the universities had an almost monopolistic voice, the distinction was much more radical and the hierarchical ranking much steeper. In the system in which the power was shared between universities and the scientific and professional societies, the hierarchy was not disjunctive and the strata were not mutually exclusive.

The self-regulation of the wider professional scientific community has important limits. With the exception of organised medicine, which has had a monopoly of a vital service in the United States since the 1920s, so that the medical community could virtually determine the raising of its standards and, thereby, the scale of financial support for medical research, other fields in the United States and Western Europe still depended much more directly on the " general public " and the market mechanisms for the financial support of their endeavours to raise their intellectual standards through increased research.

Changing Powers of the Self-Government of Science

This situation changed after the Second World War as a result of the more ample availability of governmental funds for research. Of course, the magnitude of these funds is ultimately determined by government. But in this decision, and even more in the decision of how to spend these funds, governments have relied on the representatives of the scientific community. Thus the scientific community assumed a new function. Previously it had allocated scientific recognition and thereby established the leaders of the different fields of science. Now it also took upon itself responsibilities for the direct allocation of funds for research, and claimed the right to an important voice in the determination of the total sums spent on research. The question is to what extent the institutional arrangements and traditions of the scientific community have been capable of discharging these new functions satisfactorily.[15]

The justification for charging the scientific community with these functions of allocation has been that there are many problems of the allocation of funds with which only scientists can deal. Only they can determine what is and what is not a problem worthy of investigation, and only they can assess the results.

This argument is particularly relevant to the allocation of funds within a given field. But considerable differences exist between fields. Allocation by the scientific community itself has been most effective in experimental science, where it was originally conceived. There, the complete freedom of the individual investigator has usually been consistent with the tradition of responsibility to the criteria by which achievement is evaluated; there is much consensus about these criteria which are fairly unambiguous. Arbitrariness and eccentricity are thus controllable in this sphere. Experimental scientists are professionally the most rigorous of all the members of the intellectual community. There is, furthermore, in comparison with other intellectual endeavours, much consensus among experimental scientists concerning the goals of research. They agree more about what are the worthwhile and fruitful questions to investigate, and their work is usually much more closely articulated with that of their colleagues than in other fields. This consensus about important and fruitful problems is to a large extent the result of the constitutive constraints of experimental work.

The possibilities of such work are limited by available instruments and processes. Powerful new instruments such as the particle accelerator and

[15] The changes which have occurred in the role and influence of the scientists since the Second World War have been described and analysed in Gilpin, Robert and Wright, Christopher (eds.), *Scientists and National Policy Making* (New York and London: Columbia University Press, 1964); Price, Don K., *The Scientific Estate* (Cambridge, Mass.: The Belknap Press of the Harvard University Press, 1965); Greenberg, Daniel S., *The Politics of American Science* (Harmondsworth, Middlesex: Penguin Books, 1969); and Orlans, Harold (ed.), *Science Policy and the University* (Washington, D.C.: The Brookings Institution, 1968).

the electronic microscope, and processes such as chromatography, offered new opportunities which could be exploited for work on a given range of problems. Every new invention of this kind will, therefore, attract many able scientists to work on the problems which these techniques render open to fruitful study. Experimental work is, furthermore, a laborious and expensive process where every experimenter is limited to a few problems. Without reliance on the work of others, no single worker in the field can get very far. This encourages cooperation and division of labour.

Hence, in these fields there will be no better mechanisms for the allocation of resources than the processes whereby the scientific community allocates recognition and prestige. It would be ineffective and wasteful to rely on the judgement of any body other than the scientific community.[16]

Internal Limits to the Powers of Self-Government

Even in experimental fields, however, the working of the scientific community will be perfect only as long as there are worthwhile discoveries to evaluate, assimilate and develop. However, there is no procedure or institutional arrangement to ensure that there will always be new discoveries, since, although there are methods for testing discoveries, there is none for making them. Scientific communities can arrive at a theoretical *impasse* which makes advance impossible. Or, at any rate, the state of a scientific field may be such that only modest advances can be made, and no amount of investment can accelerate progress. In such situations the sense of being part of an advancing front is lost, and is replaced by behaviour on a basis of individual trial and error. This cannot provide the basis of consensual judgements about priority in research. Hence, self-regulation will become less effective even within an experimental field.

In non-experimental fields, the effectiveness of allocation by the mechanisms of self-government is more doubtful. Even in mathematics, where the criteria of validity and excellence are firmly established, there is the problem of deciding what is important. Even where this produces valid and theoretically important results, there is always a danger of a

[16] The view of the "scientific community" as a body which evolves its own policies emerged in the 1940s and the 1950s: see Polanyi, Michael, *The Logic of Liberty* (London: Routledge and Kegan Paul, 1951), pp. 53–57, and Shils, Edward, "Scientific Community: Thoughts After Hamburg", *Bulletin of the Atomic Scientists*, X (May, 1954), pp. 151–155 (reprinted in Shils, Edward, *The Intellectuals and the Powers. Selected Papers*, Vol. I (Chicago and London: University of Chicago Press, 1972), pp. 204–212). The sociology of this community has been explored by Holton, Gerald, "Scientific Research and Scholarship", *Daedalus*, XCI (Spring, 1962), pp. 362–399; Kuhn, Thomas S., *The Structure of Scientific Revolutions* (University of Chicago Press, 1962); de Solla Price, Derek J., *Little Science, Big Science* (New York: Columbia University Press, 1963); Hagstrom, Warren H., *The Scientific Community* (New York: Basic Books, Inc., 1965); Storer, Norman, *The Social System of Science* (New York: Holt, Rinehart and Winston, 1966); and Crane, Diana, *Invisible Colleges: Diffusion of Knowledge in Scientific Communities* (Chicago and London: University of Chicago Press, 1972).

field of research falling apart into a large number of disconnected investigations. Not subject to the limitations of natural events and experimental tools which impose a commonly accepted range of worthwhile inquiries among empirical scientists, the community of mathematicians cannot ensure the coherence of its activities. The self-regulation of the scientific community can break down, not just as a result of the exhaustion of ideas, but as a result of the absence of criteria for the comparison of achievements.

In principle there is no solution to these problems. The self-regulating mechanism of science cannot ensure the continued production of new ideas, nor, in the absence of such criteria of relevance which exist only in the empirical sciences, can it establish a consensus about goals which are worthy of exploration. It is true that so far there has always been a way out. Mathematics has from time to time been revitalised by turning to " applied " problems (that is, to the solution of theoretical problems arising out of empirical science). Moreover, the exhaustion of theory has never occurred in all the fields of science at one and the same time. The blocking of the advance of science in one field has not prevented it from advancing at the same time in other fields, and from eventually removing the obstacles by outflanking them.

This, however, does not mean that self-regulation is adequate to resolve all the possible difficulties of the scientific community. First of all, there is no assurance that what occurred in the past will also happen in the future. Underlying the progress of science has been the belief in the inexhaustibility of nature and in the unending amplitude of the stock of interesting problems. If the assumption of inexhaustible possibilities is true (which, of course, no one can know), the belief in the value of the unending search, or the determination to pursue it, might become attenuated. It is not known what may cause them to come to an end. Their persistence appears to depend on the continuing occurrence of conspicuous discoveries which prove the continued charismatic power of the scientific enterprise; it depends too on a belief that the discoveries of science are useful and meaningful to the non-scientist as well as to the scientist. Both conditions might be endangered by exclusive reliance on self-regulation by the professional scientific communities. The possibility that such a community might encourage the continuation of routine research beyond the exhaustion of ideas and/or talent could shake the belief in the charismatic powers of science. The tendency of the scientific community to overreach itself is inherent in the tendency to force professional standards higher and higher. The unbridled extension of the professional autonomy of scientists, without regard to the social uses of the results, might well lead to a surfeit of scientific information of doubtful importance and a resultant loss of sense of relevance.

Another limitation on the power of the self-regulation of the professional scientific community is to be seen in its difficulties in making rational decisions about the shift of resources from one field to another. The outflanking of the obstacles which made possible the continued growth of science was the result of spontaneous shifts of interests among scientists. When scientific research was inexpensive and was done more or less single-handedly, the shifts could occur by trial and error and by the selection of the more successful trials by alert young scientists and later by alert academic administrators. These solvent responses to the pioneers' way out of the *impasse* in the centre of the scientific community occur at its periphery. Young scientists who have not yet " arrived ", students and administrators are all at the periphery of the self-governing community of science.

Nowadays research usually requires large funds, and the administration of research funds is either in the hands of, or is greatly influenced by, the representatives of the various scientific communities. The shifting of funds is difficult to accomplish when it comes to transfers from well-established to less well-established fields which are not represented in the honorific and decision-making bodies of science. Certainly the assessments made within the different sectors of the scientific community are not what is needed, since effective communication and valid assessment exist only within given fields. There is nothing comparable over a wide range of different specialised fields. A biologist is a poor judge of achievements in physics and *vice versa.* It is, therefore, meaningless to say that the allocation of funds for different fields should take place according to the relative importance attached to them by the scientific community, because there is no body of knowledge and opinion within the wider scientific community for making comparisons between fields. And the more professionalised the different scientific communities are, the more difficult it is to arrive at decisions entailing comparisons of separate fields of research.[17]

Most scientific work is supported because of the expectation of social benefits, such as improved health, higher productivity, etc. Research related to health, agriculture and manufacturing industry is much more heavily supported than research which has no such apparent relationship to social welfare, economic progress or military effectiveness.

The rationale of support for this type of research is not unambiguously established because thus far it has been impossible to measure whether the returns to the investor from applied research have been as great as some alternative uses of his capital. Nevertheless the support continues, because

[17] The problems of establishing criteria and mechanisms for the allocation of funds for research have been dealt with in Price, Don K., *op. cit.*; The National Academy of Sciences, *Basic Research and National Goals: A Report to the Committee on Science and Astronautics, U.S. House of Representatives* (Washington, D.C.: US Government Printing Office, 1965); Weinberg, Alvin M., *Reflections on Big Science* (Cambridge, Mass. and London: The MIT Press, 1967); Shils, Edward, *op. cit.* (1968); and Johnson, Harry G., " Some Economic Aspects of Science ", *Minerva*, X, 1 (January, 1972), pp. 10–18. See also Shils, Edward (ed.), *Criteria for Scientific Development: Public Policy and National Goals* (Cambridge, Mass. and London: The MIT Press, 1968), especially the essays by Alvin M. Weinberg, Stephen Toulmin and Simon Rottenberg.

the plausibility of the belief that on a global scale and in the long run mankind has derived material and not only cognitive or cultural benefits from research. Because of the indeterminateness of the relationship between investment in research and the return from the investment, attempts to fix the magnitude of expenditures on research, on the bases of recommendations by the scientific community, to the effect that all qualified research workers or all promising research projects should be supported, can only discredit the belief in the usefulness of science. In the most fortunate outcome, such recommendations culminate in potentially useful discoveries. But this is not enough. A potentially useful discovery, to become actually useful, must meet such requirements as practical exploitability and a high position in the prevailing scale of social priorities. Neither of these can be established by the self-regulating mechanisms of the scientific community.

Successes and Failures of Self-Regulation

I can now undertake to answer the question I asked at the beginning of this paper—the question why scientists have been accorded the privilege of corporate autonomy. The original purpose of this autonomy was not to confer economic privilege, since in the seventeenth and eighteenth centuries scientific activity was not a paid occupation. Nor was it an attempt to gain corporate powers for scientists to make legally binding decisions in their field of intellectual interests, since scientists believed, with considerable justification, that the validity of scientific finding could be established without recourse to any other than intellectual authority. The scientific community saw itself as a self-regulating group which could combine intellectual freedom with responsibility, discipline and consensus.

Corporate privileges were required to safeguard science from political and religious interference, and some corporate organisation was needed to represent science to lay society and to serve as a framework for the communication, assessment and rewarding of scientific achievement. Corporate privileges could of course be abused for selfish purposes. This happened in societies where corporate privilege created an institutional boundary separating the scientific elite from the rank-and-file scientists.

Present-day scientific professionalism has eliminated many of the invidious aspects of institutional distinction between the elite and the rank and file of science, and probably enhanced the effectiveness of the processes of communication and assessment in the scientific community as a whole. This professionalised system of science has furnished a suitable set of institutions for scientific self-regulation. The evidence for this is that its incorporation into itself of a considerable measure of equality and democracy did not prevent, but rather encouraged, the exercise of the charismatic powers of scientific creativity and the vesting of leadership of the scientific community in the hands of those possessing these charismatic powers.

Scientific professionalism has also been an effective means of securing funds for research. But with the rise of governmental support of science on a large scale, scientific professionalism has perhaps become too successful for its continued health. The success of the claim of the profession for autonomy in the distribution of the funds for research and for influence in decisions regarding the total amount has placed in the hands of the profession powers such as it never possessed before. Instead of depending on the appreciation and goodwill of university presidents, students, philanthropic industrialists and other potential beneficiaries of science, the power has passed to the representatives of the scientific community. They now possess great power in the allocation of very large sums for research to universities, in influencing the assignment of research contracts to industrial enterprises and in offering stipends and employment to graduate students and other scientists.

By and large, this power has probably been exercised wisely. In many phases of the allocation of funds (especially in the distribution of funds within intellectually thriving fields), the self-regulating activities of the scientific community have resulted in effective guidance. In dealing, however, with stagnating fields, or with the task of shifting resources from one field to another, this guidance has proved much less effective. Had the scientific community possessed fewer powers of decision over its own affairs, it might perhaps have fared better when the slowdown of investment in science occurred.

It is clear that the self-regulating arrangements of the scientific community cannot offer guidance for decisions regarding the total outlay of funds for science. Even if there were perfect public knowledge about the inherent potentialities of each scientific field for the discovery of new and useful knowledge under the conditions prevailing in a society at a given time, so that there were no risk of waste in any investment, this would still not be enough. The final decision could only be taken on the basis of a judgement of the value of new knowledge, and of the different applications of scientific knowledge as compared to other alternative social ends. Such a decision far exceeds the jurisdiction and competence of the self-regulatory mechanisms of the scientific community. The attempt of the scientific community to monopolise social decisions about science might, therefore, in the end be as self-defeating as the attempts of the priesthoods of great religions to control the course of religious sensibility and religious beliefs.

The professional ethos of science has generated an aspiration to control all the conditions thought necessary for the continued growth of science; latterly it has been compelled to yield to a more differentiated attitude which distinguishes between various sets of such conditions. Complete, or almost complete, professional autonomy has now been restricted to the allocation of funds for different projects within particular fields of basic research, and to the ways of spending the research funds. In other decisions the representatives of the scientific community can only act as expert consultants, as spokesmen for science as a value and as a legitimate professional interest.

Thus, in the determination of the total expenditures on basic research by governmental or other agencies, the function which only scientists can perform is the estimation of the upper limits of the funds which can be expended on research without undue risk of waste (in view of the state of the art and the availability of scientific manpower), and the lowest limit needed to maintain a given scientific capacity. Beyond this scientists can only plead and contend for maximal expenditure between these two points.

The scientific community should not try to act as the allocator of funds between different fields. As has been shown by Dr. Alvin Weinberg,[18] purely scientific considerations do not offer all the criteria needed for a rational choice between alternative fields. Since, moreover, active scientists are committed to specific fields, they are unlikely to possess a detached view of the whole field of science and the objectivity needed for such a choice. Historical precedents indicate that university presidents, other academic administrators and the professional aspirations of students have played a very important role in this respect. Control of the allocation of funds between fields by central consultative bodies representing the community of scientists engaged in research might well deprive university administrators and, to some extent also students, of influence in scientific choice.[19]

Finally, the scientific community has to beware of the tendency to lay down directions for mission-oriented science. Of course, the propagation of the view that science can be of service in the solution of practical problems has always been an important part of the rhetoric of scientists. In a general way, the argument is true and reasonable. It is also legitimate for scientists to suggest possible applications of scientific discoveries.

The role which the scientific community can play in the application of science is quite different from its role in basic science.[20] In the latter, the scientific community has all the competence (and *only* the scientific community has the competence) to assess the results of research. Furthermore, within a given field (especially in advancing fields), the leading members of any particular scientific community are most likely to be in a better position than anyone else to make informed estimates about the scientific potentialities of persons and projects.

When, however, it comes to the application of science, the scientific community is not more capable than others of judging the practical results of the research properly, or guessing the practical potentialities of initiators and projects. It is occasionally less capable of doing so. A Watt, an

18 Weinberg, Alvin M., *op. cit.*, pp. 65–84.

19 For the role of academic administrators and students, especially in competitive academic systems where there is variety and choice, see Turner, Stephen, *op. cit.*, Zloczower, A., *op. cit.* and Ben-David, Joseph, *American Higher Education: Directions Old and New* (New York: McGraw-Hill, 1971), pp. 25–47, 87–109.

20 I deliberately avoid using the term "applied research" because its definition is ambiguous.

Edison or even a Siemens probably would not have passed the scrutiny of a representative scientific body. And even a Pasteur, who might personally have passed the scrutiny, would probably not have succeeded in getting his projects accepted.

Of course, there are also contrary examples, such as the manufacture of the atomic bomb, computers, electronics and some fields of chemistry, where scientists were the ones who foresaw some of the practical results. But even in these fields scientific imagination and knowledge were not enough. Only in combination with technological inventiveness, organisational talent, economic enterprise and financial competence did the results attain practical value.

In view of all this it may be concluded that scientists and the scientific community can only participate in the still inadequately understood processes of using scientific research for the solution of practical problems. Claims for the exclusiveness of their expertise, for an exclusively professional control over the allocation of funds and the execution of projects with such ends in view might bring short-run benefits for science. They are unlikely to serve the long-range objective of making research an increasingly more useful tool for man.