The Management of Science: The Experience of Warren Weaver and the Rockefeller Foundation Programme in Molecular Biology

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IN the United States prior to the Second World War, the large private foundations, such as the Carnegie or Rockefeller foundations, were the most significant patrons of academic science. Government and industry spent more than the foundations but they were not patrons of science. They themselves designed and did the research which they paid for. They affected academic science only indirectly, as employers of academically trained scientists. The foundations not only supported academic science directly, but were also a significant source of the ideas and policies which influenced the development of science.[1]

Even within the Rockefeller Foundation, however, the aims, modes and objects of patronage were not uniform. From 1913 to about 1921, the policy was to aid general teaching and practical applications of science, such as public health programmes. From 1922 to 1929, emphasis was put on scientific and professional education, especially medical; in the 1930s and 1940s, a new policy provided aid for individual scientists within a framework of problems selected by the main officers of the Foundation; and from 1950, it emphasised a practical aim in agricultural science—breeding " miracle grains " for the " green revolution ".[2] Dr. Warren Weaver played a very great role in the developments in the period of the 1930s and 1940s.

The natural sciences division of the Foundation was directed by Warren Weaver from 1932. Dr. Weaver's policy was directed to the development of " molecular biology "—he coined the term in 1938,[3] and he provided much of the support for the applications of new physical and chemical techniques to biology in the 1930s, such as the use of isotopes, ultracentrifuges, X-ray crystallography, etc. Weaver's policies deeply influenced several disciplines, notably biochemistry.

This study does not concern Weaver's programme itself, but rather the

[1] On foundations in general, see Hollis, Ernest V., *Philanthropic Foundations and Higher Education* (New York: Columbia University Press, 1938); Lindeman, Eduard C., *Wealth and Culture* (New York: Harcourt, Brace, 1938); Coben, Stanley, " Foundation Officials and Fellowships: Innovation in the Patronage of Science ", *Minerva*, XIV, 2 (Summer 1976), pp. 225–240.

[2] Fosdick, Raymond B., *The Story of the Rockefeller Foundation* (New York: Harper, 1952); and *Adventure in Giving: The Story of the General Education Board* (New York: Harper, 1962).

[3] Weaver, Warren, " Molecular Biology: the Origin of the Term ", *Science*, CLXX 3958 (6 November, 1970), pp. 581–582.

special role which Weaver created: that of "a manager of science". In applying the term "manager" to science, I mean to suggest a role having to do with formulation of policy and oversight on a national scale; it was an executive role which did not imply detailed supervision. It was inspired by an idealistic vision of a well-managed society, which was also to be found in national politics in the 1930s. Whereas foundations had previously avoided substantive decisions in their allocations to research, Weaver selected projects according to an elaborate plan and he was active in formulating specific schemes of research which flowed from that plan and in finding competent scientists to carry them out. He made it his business to oversee a large area within the chemical and biological sciences as a whole, to set a schedule of priorities, to strengthen weak points in the system, to discern promising opportunities, and to make provision for future growth. He exercised a managerial role of the sort pioneered about the same time in Great Britain by Walter Fletcher, who was then secretary of the Medical Research Council.[4] In some ways Weaver's mode of management of research anticipated the subsequent major governmental patrons of science, such as the National Science Foundation and the National Institutes of Health, the directors of which had in theory similar opportunities, although in practice the vaster scale of these organisations and the adoption of the practice of peer-review of proposals limited the discretionary power of the "manager".

Several factors help to account for Weaver's success. These included the ideals of the Foundation regarding science, and its own institutional responsibilities for furthering rational social policies; Weaver's own conception of a biology reformed by the methods of physical science; the administrative structure of the Foundation, which permitted internal specialisation which could accommodate Weaver's plan; and the administrative policies of Raymond Fosdick, who maintained a balance of power between officers and trustees.[5]

Foundation Ideals and Reorganisation: 1913–28

The leaders of the Foundation combined an enthusiastic and idealistic conception of science and research with scepticism about the academic institutions in which most research was carried out. The large foundations had arisen in the Progressive era, and their goals were consonant with the idea that human welfare was best promoted by the systematic and rational application of objective knowledge. The *raison d'être* of the Foundation was the provision of a model of disinterested behaviour in ". . . promoting

[4] Thomson, A. Landsborough, *Half a Century of Medical Research*, vol. I (London: H.M. Stationery Office, 1973).

[5] The Rockefeller Foundation Archives are located at the Rockefeller Archives Center, Hillcrest, Pocantico Hills, North Tarrytown, N.Y. 10591. Documents in this archive are identified by the following shorthand notation: RF. series number. box number, folder number.

procedures in the rationalization of life ".[6] As examples of objective knowledge, the sciences were highly regarded: the physical sciences for their rational perfection; the social sciences for their potential utility in bringing about a rational social order. Within the Foundation, " science " had a broad meaning: it was " organised knowledge " and not the science produced by narrowly specialised academic disciplines. For officers of the Foundation, " research " meant the systematic striving for knowledge about socially important matters, not the highly specialised research of academic scientists, who tended to be guided by the criteria of relevance to the development of their own disciplines. Thus Foundation officers made a sharp distinction between research and education, and were sceptical about the idea that the university was the proper institution for large-scale scientific research.

Rockefeller funds were used to support research, but they were so used within the Rockefeller Institute, which was an institution concerned with medicine as a whole and not with the development of distinctive academic disciplines. It was run by Simon Flexner in the German style; department directors had complete freedom to pursue basic problems in accordance with their own views as to what was important, and regardless of the constraints of disciplinary boundaries and the demands of teaching.[7] The task of the other Rockefeller philanthropic institutions—the General Education Board, the International Education Board, and the Foundation—was to support college teaching and the demonstration and application of knowledge to improve human welfare. In 1916 the Rockefeller Foundation declined to support academic research through McKeen Cattell's Committee of 100.[8] The leaders of the 1920s, such as Wycliffe Rose and Abraham Flexner, opposed grants to individual investigators for particular projects as no better than academic charity; the powerful foundations had, in their own view, a higher responsibility to all of society.

The Rockefeller Foundation was extremely sensitive regarding the legitimacy of its intervention. On the one hand, the wealth and power of the large private foundations could only be justified if they had a large and beneficent influence on national institutions. On the other hand, the foundations were legally only quasi-public institutions; they were not accountable to the electorate or to any political body; their right to influence national institutions was unclear and, before the First World War, highly controversial. Like other quasi-public institutions of the Progressive period, such as the National Research Council or the National Civic League, the Rockefeller Foundation was in a dilemma: it was intended to be the disinterested benefactor of society but it was also open to the criticism

[6] AF.900.22.168. Agenda for Meeting, 11 April, 1933, pp. 61–65.

[7] Corner, George, *The History of the Rockefeller Institute* (New York: Rockefeller Institute Press, 1964).

[8] RF.951.1.1. Correspondence, 1913–1916. Reingold, Nathan, ' The Case of the Disappearing Laboratory ", *American Quarterly* (in press).

of being only a powerful vested interest in disguise. As a result, Foundation officers were very sensitive to the criticism that they were using their great wealth to promote special groups or to tell individuals what to do. The trustees, who were conservative men of affairs, were opposed to " planning " in principle and wished to avoid giving aid to particular individuals in accordance with a general plan, since such aid might be construed as " dictating the course of scientific research ". Fear of public criticism reinforced their faith in the benefits of a policy of *laissez-faire.*[9]

This conflict between the need to exercise influence and the fear of dictatorial power was resolved in the 1920s by adopting certain policies regarding aid to colleges and universities. These involved giving only to institutions, not to individuals, and in the form of capital grants which were then allocated by the beneficiaries and not by the Foundation; it made gifts to the " best " institutions and on a regional or national scale. It was the policy of the Foundation to " make the peaks higher ", to help strong institutions to serve as regional models, and to allow normal competitive pressures to reform the national system by eliminating the weak and stimulating the more progressive.[10] Money was provided only to an institution as a whole—for buildings or endowment, not for specific pieces of research. While the Foundation thus avoided dictation to the universities, the science departments of universities were being enormously stimulated by these general capital grants.[11] Its grants to individuals took the form of fellowships, awarded on the basis of outstanding scientific achievement or promise by committees of the National Research Council.[12] In this way the Foundation managed to divest itself of all decisions regarding allocation of money to individuals. It thus avoided the appearance of partiality and could claim to be helping the system to do more efficiently what it was already doing without any dictation of policy.

Before about 1923, the Rockefeller philanthropic foundations concentrated on the support of general education and public health. From 1923 to 1929, however, Rose and others began to turn their interest to scientific and medical education. As a result, huge sums were expended on all areas of science: $45 million on the natural and medical sciences from 1913 to 1933, all but a tiny fraction on plant and endowment and most to a few major institutions.[13] Rose directed the General Education Board and the International Education Board. From 1923 to 1929 the latter granted $16 million for the natural sciences; 1·6 per cent. went directly for research, and 98 per cent. of the funds spent in the United States went

9 Lindeman, E. C., *op. cit.*

10 Fosdick, R. B., *op. cit.*, 1952. RF.800.22.167. Transcript of Conference, 29 October, 1930, pp. 102–106.

11 Coben, Stanley, " The Scientific Establishment and the Transmission of Quantum Mechanics to the United States, 1919–1932 ", *American Historical Review*, LXXVI, 2 (April 1971), pp. 442–466.

12 RF.915.22.168. Agenda for Meeting, 11 April, 1933, pp. 32ff. See Coben, S., *op. cit.*, 1971.

13 *Ibid.*, pp. 28–32.

to two institutions. The General Education Board invested $12·2 million on the natural sciences in the United States; 1·1 per cent. was ear-marked for research and 98 per cent. went to nine institutions. The medical education divisions of the Rockefeller Foundation and General Education Board between 1914 and 1932 put $28·1 million into medical schools; prior to 1929 only $60,000 went to research as such.[14] Comparable sums were spent on the social sciences by the Laura Spelman Rockefeller Memorial Fund.

Changing Policies

The very success of the policies of the 1920s created problems. American universities in this decade, stimulated in part by Rockefeller philanthropy, simply outgrew the resources of the Foundation. The demand for higher education expanded; academic science became a favoured object of the gifts of alumni; the growth of industrial research provided many posts for scientists trained in universities. The rising costs of scientific education and research, stimulated by the high standards of the foundations, outgrew income from endowments. All this meant that by the mid-1920s the Rockefeller Foundation, with its fixed endowment, was simply no longer able to have a dominant influence on the whole of academic science, at least not by capital grants. For the Foundation to remain a significant influence it had to concentrate its efforts on a narrower range of activities.

The entry of the Foundation into support of science had occurred through the initiatives of active officers like Rose, Beardsley Ruml, or Richard Pearce, rather than by a planned and coordinated effort. The five Rockefeller boards staked out and defended independent territories which often overlapped or left vital areas unrepresented; there was confusion and dismay among potential beneficiaries, and the administrative quagmire in the New York offices became more and more obvious. In 1928 a complete reorganisation of the boards was effected by Raymond Fosdick, John D. Rockefeller's chief counsel, an advocate of the League of Nations and a progressive reformer. A single Foundation was created, with five divisions: the natural sciences, medical sciences, social sciences, humanities, and medical education. These divisions took over the activities of the boards.[15]

Fosdick's plan for reorganisation included two main policies. The first was a policy of concentration of effort on science, and moreover on one aspect of science—the "advancement of knowledge". Although Fosdick shared with his fellow-trustees the broad conception of research as including the discovery and application of knowledge—especially in the social sciences—his strategy meant that the Foundation would henceforth concentrate on grants to research in universities. Second, Fosdick did not wish

[14] *Ibid.*, pp. 19–20, 24–25.

[15] Fosdick, R. B., *op. cit.*, 1952. A detailed study of the reorganisation of 1928 is in preparation.

to concentrate on a single area of science, such as physical science, but to deal with the whole range of learning. Hence the divisional structure, which corresponded to the divisions of knowledge in universities. Although the divisional structure implied acceptance of the academic disciplines, Fosdick did not envisage each division as contributing to the support of a particular discipline; the aim was to deal with man in all his aspects—natural, biological, medical, social, and cultural, Thus Fosdick avoided binding the Foundation to a highly defined, specific programme, or a passive role of granting money to university departments to spend at their own discretion. He kept the making of decisions within the Foundation by providing a coordinating plan.

Fosdick's strategy aroused the slumbering conflict among the various ideals of the Foundation. Concentration on " the advancement of knowledge " seemed to question the stated task of the Foundation of promoting " the welfare of mankind ". Fosdick's broad definition of " research " did not mollify critics, such as Rose and Abraham Flexner, who felt that the traditional ideals of aiding education and promoting the rational organisation of society had given way to charity for an academic elite.[16] Both Rose and Flexner, who bitterly opposed the new policy, retired in 1928. The trustees remained apprehensive about the new policy, and its social relevance remained a controversial issue into the 1930s. Fosdick's broad plan for the " sciences of man " left a good deal of room for the officers in charge of each division to exercise their initiative. Officers became in effect the makers of science policy. This entailed assembling a staff with sufficient technical competence to judge the merit of research proposals and to keep in contact with a large number of individual projects. In developing his programme, and in his role as manager of science, Warren Weaver had to be acutely sensitive to the proper limits of the Foundation as the patron and guide of scientific research.

Interim: 1929–32

The period from 1929 to 1932 was marked by uncertainty as to how to put the new policies into effect. The habits of the 1920s were continued, at least in the natural sciences. Of the \$11·9 million spent on the natural sciences, \$7·98 million (67 per cent.) was capital investment, and most of the \$1·63 million spent on research was in the form of " fluid " or discretionary grants to universities, which like the National Research Council fellowships (\$1·86 million) were allocated by the institutional recipient.[17] Individual research grants of \$368,000 went mainly to Europeans, thanks to Rose's network of connections and his deference towards European science. The fields which were favoured reflected prior interests: marine biology and oceanography—\$3·14 million or 26·5 per cent.—biology—

[16] See Flexner, Abraham, *Funds and Foundations* (New York: Harper, 1952), pp. 77–100.
[17] RF.915.22.168. Agenda for Meeting, 11 April, 1933, p. 31.

$1·25 million or 10·5 per cent., with the rest scattered over eight fields.[18] There were few clear precedents as to how programmes should be developed and administered. Should the Foundation support mainly training or research, institutions or departments? Should it continue the policy of making the peaks higher, or should it support worthy individuals wherever they were found? These issues were discussed in a two-day staff conference in October 1930, but the only clear resolution was to continue the National Research Council fellowships.[19]

The ability of the divisions to put together a programme depended a great deal on the officer in charge. Edmund Day, a veteran of Ruml's programme, rapidly developed a broad programme in the social sciences. Alan Gregg, a lieutenant of Pearce in the Medical Education Board, had a thorough familiarity with European medical science and began at once to construct a programme in brain and neurological research and psychiatry, which were to be the main themes of the medical sciences division.[20] The natural sciences were more problematic, and there was little in the Foundation's experience to guide the way. Rose's enthusiasm had been limited to the safer physical sciences. There had been in the mid-1920s a nascent programme in "human biology", organised by Edwin Embree in a new "division of studies".[21] Following the model of Foundation programmes in mental hygiene and public health, Embree focused on aspects of biological science which touched on social concerns: human genetics, racial biology, physical anthropology and race, brain research and experimental and marine biology. A large grant had gone to Raymond Pearl for work on mammalian genetics and racial hybrids; capital grants were made to the laboratories at Woods Hole and Pacific Grove.[22] For various reasons, however, Embree's programme did not survive the reorganisation. Beside the fact that it concentrated on the more controversial fringes of human biology, it represented a model which Fosdick had explicitly rejected: namely, a programme organised around a specific problem. Fosdick's aim was the broad development of biology along with the other natural sciences.

Although it was agreed that the remnants of Embree's programme be terminated, there was no agreement as to what was to take its place. There had been uncertainty in the last phases of the reorganisation over the natural sciences division. A suggestion for a separate division of biology was rejected. A division of agriculture and forestry survived up to the very last moment, and Gregg continued to think of a future division of biology with medicine and agriculture attached to it.[23] There was hesitation in

[18] *Ibid.*, pp. 36–37.
[19] R.F.900.21.160. Staff Conference, 2–3 October, 1930.
[20] RF.900.22.167. Transcript of Conference, 29 October, 1930, pp. 82–83.
[21] RF. Files on Division of Studies and Edwin Embree, *passim.*
[22] RF.915.22.168. Agenda for Meeting, 11 April, 1933.
[23] RF.900.17.125. Gunn to George Vincent, 17 April, 1928, Vincent to Gunn, 27 April, 1928. RF.900.19.139. "Report on Reorganization". 22 May, 1928. RF.915.3.19. "Agri-

appointing a full-time director for the natural sciences division. Max Mason acted as director from 1928 to 1929, before becoming president. Mason was a mathematical physicist, and his projects were in accordance with his background; the outcome was projects in the application of X-ray crystallography to chemistry, and perhaps in time, he thought, to biology.[24] For a few years Richard Pearce and William Carter, from the medical education division, served as acting directors of the natural sciences.[25] In September 1930, Mason persuaded Herman Spoehr to take the post. Spoehr was a professor of plant physiology, and his tentative proposals for a programme in the natural sciences in October 1930 pointed to the desirability of developing research in the basic sciences underlying agriculture and forestry, especially forestry, which was deemed a vital national resource in need of basic science.[26] He envisaged a range of activities from physics to biology, dealing with energy and photosynthesis, and centred on the study of enzymes, vitamins, and other accessory factors in cells.[27]

Spoehr resigned in August 1931, however, and in April 1932 Mason visited Harvard and Massachusetts Institute of Technology in search of a new director from the physical sciences. He consulted Karl Compton, A. A. Noyes, and Arthur Lamb, an organic chemist, who expressed some interest in the appointment himself, but was unwilling to leave research altogether.[28] Finally in the autumn of 1931 Mason chose his one-time colleague at the University of Wisconsin, Warren Weaver. Like his teacher, Weaver was a conservative classical physicist, firm in the belief that the new quantum mechanics was a flash in the pan.[29] By 1932, however, quantum physics was firmly established in American universities[30]; Weaver came to New York with the conviction that the long-range future of physical science lay in its application to biology. This was what Mason and the trustees wanted to hear. Overcoming his uncertainty as to whether a physicist would be able to develop and conduct a programme in biology, Weaver accepted the appointment.

In this fluid situation which prevailed from 1928 to 1931 the shape of divisional programmes depended a great deal on individual preference. Weaver's presence ensured that the terms of reference of the natural sciences division would not be forestry, agriculture, or medicine, but the physical sciences.

culture, 1927–1930". RF.915.3.22. Gregg, Alan, "A Division of Agricultural Science", 1 April, 1929. It was apparently David Edsall's advice which tipped the balance against an agriculture division: see RF.900.17.125, Edsall to Fosdick, 29 May, 1928. Vincent to Edsall, 14 June, 1928.

24 Rockefeller Foundation, *Annual Report, 1930*, pp. 189–191. Grants were made to Max von Laue, William Bragg and Linus Pauling.

25 RF.915.1.1. Norma Thompson to Mason, 31 March, 1930.

26 RF.900.22.167. Conference, 29 October, 1930, pp. 92–95.

27 *Ibid.*, p. 91.

28 RF.915.1.1. Max Mason Diary, 22 April and 10 June, 1931.

29 Weaver, Warren, *Scene of Change* (New York: Scribners, 1970), pp. 45, 56–60.

30 Coben, S., *op. cit.*, 1971.

"Psychobiology" and "Vital Processes": The theme of Weaver's programme from 1932 to the late 1940s was the "new biology", reformed and inspired by the application of techniques of molecular physics and chemistry, and the standards of experimental rigour of the physical sciences. Weaver's programme took various names: "vital processes", "psychobiology", "experimental biology", "molecular biology", but the idea behind it was constant.

There was nothing novel about Weaver's conception. The notion of the sciences progressing—in accordance with Comte's classification of the sciences—towards chemistry, physics, and ultimately mathematics was a commonplace. The 1920s and 1930s were a period of particular enthusiasm for the "reductionist" programme. Within many biological disciplines reformers were promoting the promise of physical science. In genetics, T. H. Morgan and his school were asserting that the next major advance would be the chemical understanding of the gene and gene-expression. In embryology, the early 1930s was the period of the greatest enthusiasm for the "organiser" theory and chemical embryology. In endocrinology, sensational chemical and biochemical discoveries regarding hormones were bringing the subject from the clinic into the chemical laboratory.[31] These programmes were widely circulated in the pronouncements of professional societies and in the popular press. Throughout the 1920s chemists had been increasingly eager to promote biology as a field rich in opportunities for research. In the late 1920s and early 1930s Max Delbrück, Niels Bohr, Pascual Jordan and other physicists prophesied a biological quantum revolution.[32] Weaver's expectations were rather more modest than "new laws" of living matter, but he shared with the physicist-biologists a view of biology as an underdeveloped subject, rich in potential but shackled by unscientific habits and traditions. Professional biologists knew well that the application of physical and chemical techniques had long been a recurrent ideal in biology; they also knew the difficulties of doing research in the light of this ideal.

Although Weaver's programme for a reformed biology was not a novelty for biologists, his "outsider's" perspective was critically important for his new role as a promoter of science. Because he was not trained in a biological discipline, he tended not to conceive biology in terms of the established disciplines, but rather in terms of large problems to be attacked from all available points of view. In formulating his programme, Weaver did not think in terms of promoting work within particular disciplines as a biologist might have done. He saw the disciplines as providing opportunities for a selective application of mathematical, physical, and chemical techniques to biological problems.

[31] See Allen, Garland, *Biology in the Twentieth Century* (New York: Wiley Interscience, 1975).

[32] Olby, Robert, *The Path to the Double Helix* (Seattle: University of Washington Press, 1974).

His approach was also congruent with Fosdick's and the trustees' distrust of work done exclusively within the boundaries of traditionally defined academic disciplines, and their conception of science as the application of knowledge to human problems without regard for disciplinary traditions and interests. For example, one trustee in 1930 asked Alan Gregg: "What impression do you get of the artificial division we have made of physics, chemistry, bacteriology and pathology? Are they in the way now or not? Isn't it about time we forgot those names and scrambled the whole thing to see if we cannot get some new terms? Haven't we interfered with our development?" Gregg assured him that he saw no advantage in holding strictly to conventional academic categories.[33] Weaver too tended to think in terms of problems rather than disciplines. In retrospect, the difference between Weaver and the trustees over basic and "relevant" research was less important than the fundamental agreement on the role of the Foundation in the "management of research".

Weaver's first detailed proposal for his division—prepared in the autumn of 1932—was a broad programme of support for the whole range of the natural sciences. Major sections included mathematics, physics and chemistry of vital processes; mathematics, physics and chemistry of the earth and atmosphere; genetic biology; and quantitative psychology. Minor sections included the structure of matter; physical and colloidal chemistry; and the theory of probability and statistics.[34]

The application of physical science to biology was to be the contribution of the natural sciences division to the large programme of a new science of man, especially the scientific study of mentality and temperament.[35] The programme was to be supported by the basic fields of physical sciences dealing with earth and atmosphere—man's physical environment; fundamental problems, which included the structure of inorganic matter, from atoms to galaxies; colloidal chemistry, which was then in vogue in biology and medical fields as an explanation of the special properties of living matter; and the mathematical techniques which underlay all sciences. In keeping with Weaver's and Mason's interests, physics was prominent in most areas. It was an ambitious plan—too ambitious for a year as lean as 1933.

Weaver's proposal emerged from Mason's office shorn of psychology and all the minor areas in physical science. Except for the investigation of vital processes, including genetics, only earth science remained, and Mason made it clear that he saw earth science as marginal to the interests of the Foundation.[36] No project would qualify for support simply because it was

[33] RF.900.22.167. Conference, 29 October, 1930, pp. 64–65. See also RF.915.1.2. Jerome Green to Raymond Fosdick, 29 March, 1937.

[34] RF.915.1.1. Weaver to Lauder Jones, 19 November, 1932. Weaver memo, 18 October, 1932.

[35] FR.915.21.160. Memorandum of Staff Conference, 14 March, 1933.

[36] RF.900.22.168. Agenda for Meeting, 11 April, 1933. Weaver's statement is on pp. 76–87 and includes proposed budgets.

intrinsically interesting; it would have to be directly related to the programme.[37] Only a few small grants were made in earth science before the trustees discontinued it in 1934.[38] In effect, the natural sciences programme was concentrated from the outset on " mathematics, physics, and chemistry of vital processes ".

In Weaver's original plan, " vital processes " were the bridge to Gregg's programme in psychiatry and neuroscience. An unintended result of Mason's elimination of all but " vital processes " was that Weaver's programme overlapped in virtually every area with Gregg's. Since Weaver had lost those fields in which he was most at home, he tended at first to rely rather heavily on Gregg's greater experience. As a result, the most striking feature of Weaver's programme from 1933 to about 1935 was its close relation to " psychobiology ".

In the report to the trustees in December 1933, for instance, the medical sciences and the natural sciences programmes were presented as a single unit, comprising psychobiology (psychiatry, neurophysiology); internal secretions (hormones and enzymes); nutrition (vitamins); radiation effects; sex biology; experimental and chemical embryology; genetics; general and cell physiology (nerve conduction, osmosis); and biophysics and biochemistry (spectroscopy, microchemical analysis, and basic studies). The contributory role of the natural sciences programmes was made explicit.[39] Gregg aimed to bring the methods and rigour of the biological sciences to psychiatry and the behavourial sciences; Weaver aimed to introduce the methods of physical science into biology.[40] The division of labour was not along disciplinary lines. Weaver was to take those sciences which required further development as basic sciences before being ready for medical or psychiatric application.[41] The Foundation at that time did not encourage a sharp distinction between pure and applied science, but between science which was applicable and that which was not yet applicable. All science was to be ultimately useful, but to be used it had first to be perfected as science. This conception of science made it possible for Weaver to develop the basic biological sciences without transgressing on the obligation of social utility.

" Psychobiology " provided administrative shelter for Weaver's fledgling and vulnerable programme. So long as Weaver and Gregg worked as one unit, both basic and clinical aspects were developed, and the functional division of labour allowed Weaver to support the basic biological sciences under the auspices of " psychobiology ". At the same time, this arrangement enabled Weaver to concentrate on basic disciplines such as bio-

[37] RF.915.21.160. Memo of Staff Conference, 14 March, 1933.

[38] RF.915.1.1. Weaver to Lauder Jones, 16 February, 1934. Weaver to Fosdick, 14 November, 1934. RF.900.22.166. " Report of the Committee of Appraisal ", December, 1934, p. 61.

[39] RF.915.1.7. " The Medical and Natural Sciences ", 13 December, 1933.

[40] RF.915.1.1. " Report of the Committee of Appraisal ". . . . pp. 70–75.

[41] RF.915.1.7. " The Medical and Natural Sciences ". . . . pp. 5–6.

chemistry, when the medical sciences and natural sciences programmes later diverged. The early division of labour between Weaver and Gregg established the natural sciences as the one division which could legitimately foster basic research in the natural sciences. It seems unlikely that an attempt to foster the physical sciences as such could have succeeded in 1933 and 1934 any better than the proposal to develop the earth sciences. The association with " psychobiology " was probably critical for Weaver's later freedom to support those sides of biochemistry, genetics, and cell physiology which were to be identified as " molecular biology ".

Pressures for " relevance " were particularly intense in the depth of the depression of the 1930s. The euphoria about physical science as the source of industrial productivity and social progress, which had accompanied the boom, had collapsed. Physical science was blamed for causing the industrial recession and there was talk of a moratorium on scientific research to enable society to cope with a disaster brought about by too rapid technical progress.[42] One concrete result of the crisis in faith was a marked shift of goodwill towards the biological and social sciences. C. D. Broad's coupling of " physics and death " and " psychology and life " were an extreme form of a common theme.[43] Some physical scientists turned to biology and psychology for problems which might raise their declining prestige and ensure the survival of research programmes. Foundation directors, pressed by criticism of capitalist institutions and demands for constructive leadership, likewise looked with greater favour on " socially relevant " programmes.

Weaver was sensitive to these currents of feeling, and their implications for his own plans:

> There is . . . a lesson to be learned from our present situation: . . . our understanding and control of inanimate forces has outrun our understanding and control of animate forces. This, in turn, points to the desirability of an increased emphasis, within science, on biology and psychology, and on the special developments in mathematics, physics, and chemistry which are . . . fundamental to biology and psychology.[44]

For Fosdick, Mason, and Weaver, attacks on reason reinforced their faith in the need for science to dissipate superstition and irrationality, to bring the individual into " . . . a more intelligent, a more accurately adjusted and a happier relationship with our modern scientific civilization ".[45] For Weaver, the application of physical science to biology would help to counteract proposals such as the " moratorium on science ". He presented " psychobiology " to the trustees with fervour:

[42] See Pursell, Carroll, " The Savage Struck by Lightning: the Idea of a Research Moratorium, 1927–1937 ", *Lex et Scientia*, X (1974), pp. 146–158.

[43] Cited by Weaver, RF.915.1.6. " The Science of Man ", 29 November, 1933.

[44] RF.915.1.6. Weaver, W., " The Benefits from Science ", 27 January, 1933, pp. 9–10. pp. 9–10.

[45] *Ibid.*, p. 5. See also Mason's remarks in RF.900.22.168. Agenda for Meeting, 11 April, 1933, pp. 61–65.

There is a strong and growing belief, held by many thoughtful scientists—even by many of the ablest specialists in the physical sciences—that the past fifty or one hundred years have seen the supremacy of physics and chemistry, but that hope for the future of mankind depends in a basic way upon the development during the next fifty years of a new biology and a new psychology. As one views the present state of the world, with its terrific tension, its paradoxical confusion of abundance, and its almost uncontrollable mechanical expertness, one is tempted to charge the physical sciences with having helped to produce a situation that man has neither the wits to manage nor the nerves to endure. One should be critical in distinguishing between basic pure science and the inventive and technological activity that is often incorrectly referred to as science: and yet the fact must be faced that no one hopes or expects that technological advances will not continue.

The challenge of this situation is obvious. Can man gain an intelligent control of his own power? Can we develop so sound and extensive a genetics that we can hope to breed, in the future, superior men? Can we obtain enough knowledge of physiology and psychobiology of sex so that man can bring this pervasive, highly important, and dangerous aspect of life under rational control? Can we unravel the tangled problem of the endocrine glands, and develop, before it is too late, a therapy for the whole hideous range of mental and physical disorders which result from glandular disturbances? Can we solve the mysteries of the various vitamins . . . ? Can we release psychology from its present confusion and ineffectiveness and shape it into a tool which every man can use every day? Can man acquire enough knowledge of his own vital processes so that we can hope to rationalize human behavior? Can we, in short, create a new science of Man?

This point of view has recently been realized by various scientists, philosophers and statesmen; many of the techniques are at hand; but direction, stimulation, support and leadership are for the most part lacking. The foundation has a unique chance to correlate and direct existing forces and to stimulate the creation of new forces for a coherent and strategic attack. The proposed program recognizes here one of the most inspiring opportunities with which science has ever been faced.[46]

The prophecies of a " science holiday " mood made Weaver think that it was urgent to apply the natural sciences to " psychobiology ". The close association between Weaver and Gregg was temporary. By 1935 Weaver was breaking away from " psychobiology " and developing a programme for support of the biological and allied physical sciences. This trend was given implicit approval by an official appraisal of the Foundation's policy late in 1934.

Appraisal: 1934

Although the trustees approved the divisional programme in December 1933 without apparent controversy, they were anxious in general about the future of the Foundation, and at the same meeting appointed a committee of appraisal under the chairmanship of Raymond Fosdick, to consider

[46] RF.915.1.7. Weaver, Warren, " Progress Report, the NS ", 14 February, 1934, pp. 1–3. A similar argument in the 1960s was used by Dr. Alvin Weinberg in favour of giving molecular biology priority over high energv physics. See Weinberg, Alvin, *Reflections on Big Science* (Cambridge, Mass.: MIT Press, 1967).

whether new " sailing directions " were called for.[47] The economic forecast was bleak. Yield on the Foundation's securities had dropped from 6·59 per cent. of book value in 1929 to 4·21 per cent. in 1933, and their reduced market value following the crash precluded selling capital.[48] Even more worrying, the costs of termination of old programmes from the 1920s were reaching a peak in 1934–35, just as demands for new programmes were rising.[49] The trustees were in an anxious and critical mood, unlikely to respond enthusiastically to large new schemes. One trustee said that he was " definitely agnostic " about the wisdom of the policy of investing in new knowledge for the future, when the very survival of present civilisation was in the balance. The wave of juvenile crime, unemployment, " bread lines ", and " guerilla warfare " against all established institutions convinced him that the Foundation should pour its resources into immediate measures to stave off collapse[50]:

From my point of view, the Rockefeller Foundation's work has become too largely an investment in remote futures with an attendant policy of ignoring the present to such an extent that civilization may never reach the future. I am quite convinced personally that a large proportion of all the money that we have available for appropriation might be turned into a study of the crime situation.[51]

However, there is no evidence that Fosdick ever considered major changes in policy, such as discontinuing support of research or eliminating entire divisions. For Fosdick the appraisal was an occasion to reassert the consensus of 1928; to reduce the trustees' mistrust, he wanted an occasion to lay all doubts and differences openly on the table and to improve communication between officers and trustees. The general result of Fosdick's report was to reinforce existing policy: this was to concentrate on major programmes, and to give officers more power to support and guide scientific research. The stability of Foundation programmes owed a great deal to Fosdick's energetic and effective management.

The themes of Fosdick's report in December 1934 were economy and concentration. There was no disagreement about the need for economy but there was contention about concentration on special programmes. The policy of concentration implied approval of existing programmes, and it reinforced the officers' powers to design and manage the course of scientific research. The prospect of an administrative group intervening in the internal affairs of academic science, while disregarding larger social

[47] RF.900.22.166. " Report of the Committee of Appraisal ". . . . The other members, James Angell and Walter Stewart, were little involved. The Report was conceived and written by Fosdick.

[48] *Ibid.*, p. 30.

[49] RF.915.2.16. Weaver, Warren, " Report on NS for Committee of Review ", November 1938. So anxious were the trustees over the dead hand of old obligations that they appointed two committees to estimate termination costs of old programmes. RF.915.1.1. Weaver to Lauder Jones, 19 January, 1933.

[50] RF.900.21.160. Ernest M. Hopkins to Fosdick, 16 November, 1934. Hopkins to Mason, 16 November, 1934.

[51] *Ibid.*; Hopkins to Fosdick, 22 November, 1934.

problems, made the trustees uneasy. Fosdick simply enjoined the officers to be tactful in exercising this power:

We do not have to be cynical to admit that if a foundation announces an interest in anthropology or astronomy or physio-chemical reactions, there will be plenty of institutions that will develop a zeal for the prosecution of these studies. The responsibility which this inescapable fact throws upon a foundation is enormous. The possession of funds carries with it power to establish trends and styles of intellectual endeavour. With the best will in the world the trustees of a foundation may select unwisely. . . . To guard against these evils requires critical judgement, common sense, wide understanding and eternal vigilance; and frankly, in this matter of promoting research your committee is inclined to believe that the Foundation has followed its enthusiasms too far.

We are by no means suggesting that research be omitted from the Foundation's activities. . . . But in our opinion we should avoid research for the sake of research without regard to its relevance. Moreover, there should be no exclusive interest in research as an end and aim. Indeed we would strongly advocate a shift of emphasis in favor not only of the dissemination of knowledge, but on the practical application of knowledge in fields where human need is great and opportunity is real. As a means of advancing knowledge, application can be as effective an instrument as research.[52]

Fosdick's intent was to make clear the limits both to the trustees' demands for " relevance " and the officers' liberty to pursue their special programmes.

The natural sciences came under little criticism for lack of relevance. For the social sciences, Fosdick's proposal for concentration meant more research which could be applied to social problems. For the medical sciences, it meant more psychiatry. Weaver correctly interpreted the appraisal as approval for increasing concentration on " vital processes " and basic biology.[53]

There were contrary views among the experts who had been invited by the committee of appraisal to render opinions about Weaver's programme for the study of ' vital processes ".[54] Henry Dakin, who operated a private laboratory in physiological chemistry, argued strongly against any interference with individual scientific genius: ". . . to sum up: Less plan, less emphasis on the future coördination of scientific knowledge and its human implications, and more scientific opportunism."[55] Dakin shared with George Ellery Hale and others a conception of science as high culture, as a calling which could not be planned or managed, and saw with dismay the rise of science in governmental institutions and industrial research laboratories.[56] William Howell, emeritus professor of physiology at Johns

52 RF.900.22.166. " Report of the Committee on Appraisal ". . . . pp. 44–45. The statement that the Foundation would not promote research as an end in itself was deleted before the report passed. RF.915.1.1. Weaver to W. E. Tisdale, 27 December, 1934.

53 RF.915.1.1. Weaver to Tisdale, 27 December, 1934.

54 RF.915.4.41. Mason's nominees for this committee were all academic biologists (Mason to Fosdick, 28 March, 1934). However, Fosdick chose Simon Flexner as chairman, perhaps because he knew Flexner was not sympathetic to the Foundation's programmes.

55 *Ibid.*, H. D. Dakin to Simon Flexner, 16 November, 1934.

56 *Ibid.*, p. 2. " . . . the problems worked upon are set from the outside . . . instead of arising out of the interests of the workers themselves. One outcome has been the

Hopkins University, shared Dakin's fear that the promotion of favoured programmes by the Foundation would damage the "idealism and independence" of science, but did not think the Foundation should rely on scientific fashion in selecting projects as it had in the past. He favoured the establishment of certain long-range goals and the endowment of a few strong academic institutions to provide leadership in carrying them out.[57]

The most vigorous opponent of planning, and of Weaver's programme in particular, was the chairman of the sub-committee, Simon Flexner, who argued that such a plan would inevitably prohibit some areas of research and tempt scientists into others out of a desire for grants, rather than out of intrinsic interest.[58] (In this regard he was preaching what he practised as director of the Rockefeller Institute which was a bastion of scientific individualism.) Flexner opposed the proposal to concentrate on "vital processes" on the grounds that physical and chemical methods were general tools, not a "new programme"; he also questioned Weaver's competence to administer a biological programme:

I am . . . not sure that the officers, captivated by their own notions, may not have imposed their ideas on individual laboratories. . . . The power of the foundation is so great that I doubt whether an entirely neutral attitude on either side can be maintained. There is also something anomalous in mathematicians and physicists dominating in a wide way research in biology and medicine. . . . A disturbing element is that the chief men in charge are so completely "sold" to the program, and as I gather from a long talk last spring with President Mason, the "program" is looked upon as a significant innovation, which it can scarcely be said to be.[59]

Asked if, as a trustee, he would have supported Weaver's programme, Flexner replied: "I should have been able, I think, to point to examples in which the project given seemed to me to be framed not so much on its feasibility as because it fitted into the foundation scheme. I should not have disapproved of parts, and not have approved of the program as a whole".[60]

For Fosdick, however, fundamental policies were not in question:

Frankly, I got very little from their reports except, perhaps, their general feeling that if properly limited the Natural Science program was good. Their irrelevancies seem to center about two points: (1) the old row between university and institute research . . . (2) Planned research versus general research, *i.e.*, laissez faire . . .[61]

For Fosdick these issues were settled in 1928 when the Foundation decided to develop planned, concentrated programmes of research in universities.

development of a sort of competitive struggle for tangible results which gives to scientific research something of the character of a business proposition". On G. E. Hale's similar ideas, see Reingold, N., *op. cit.*

57 *Ibid.*, W. Howell to S. Flexner, 10 November, 1934, and addendum.

58 *Ibid.*, S. Flexner to Fosdick, 19 November, 1934, pp. 2–6.

59 *Ibid.*, S. Flexner to Fosdick, 20 November, 1934.

60 *Ibid.* See also Fosdick to S. Flexner, 19 November, 1934.

61 RF.915.4.41. R. Fosdick to W. Stewart, 25 November, 1934.

The question was not whether Weaver's programme would continue, but how fast and how far.

Flexner's specific criticism of the "psychobiology" programme corresponded to doubts in Fosdick's mind. In March 1934, he had invited Weaver to his home for a private discussion in which he warned him against being too zealous in presenting his programme to the trustees. He worried that Weaver was too much of an advocate of his plan, and that he was supporting "bizarre" or "esoteric" projects.[62] Fosdick's and Flexner's misgivings about the "psychobiology" programme were confirmed by David Edsall, dean of the Harvard Medical School and an active trustee, whom Fodick asked for an informal opinion following the inconclusive report by the sub-committee. Edsall was sympathetic to Fosdick's policies, having been chairman of a committee in 1928 which had approved a shift from medical education to research.[63] He had been a constant supporter of Weaver's programme, and had himself advised the new president of Harvard, James B. Conant, that the application of physical science to biology and medicine would in the next generation produce as many important advances as had occurred in the past generation in the more rigorous sciences such as biochemistry.[64] Nonetheless, Edsall advised caution and scepticism about "psychobiology". He too had been struck by what seemed to him overly optimistic claims by Mason and Weaver, and after consulting informed colleagues, he was still sceptical:

> There is apparently not any dependable evidence as yet . . . to arouse confidence that large efforts would be rapidly so productive as to justify great expenditures. I believe that . . . there will be a slow, painstaking accumulation of knowledge that in the course of a few decades is likely to be of profound importance, but I question very much whether there would be any prompt solution to any very important problems.[65]

Edsall favoured support for promising schemes, even if they were expensive or risky; but he discouraged rushing into a comprehensive, highly planned programme in the hope that money alone would produce large results.

Edsall apparently had in mind parts of the "psychobiology" programme which dealt with endocrinology and sex biology. The isolation of sex hormones in the early 1930s had engendered enthusiasm for hormone therapy. The pharmaceutical industry rushed to develop them commercially and there was a wave of publicity and claims of miraculous cures.[66] By the mid-1930s, a reaction had already begun to set in against excessive claims for "psychobiology". Fosdick's appraisal of Weaver's programme was as cautious as Edsall's: "The strategy would be to feel out the area,

62 RF.915.4.38. Weaver to Fosdick, 22 March, 1934.

63 RF.900.22.166. "Report of Committee of Appraisal". . . . p. 22.

64 RF.915.4.41. David Edsall to Fosdick, 23 November, 1934.

65 RF.900.22.166. "Report of Committee of Appraisal". . . . p. 32.

66 Diana Hall, personal communications.

to proceed cautiously, to be misled by no preconceived hopes, and to maintain a detached and healthy kind of skepticism in relation both to to the program as a whole and to its constituent parts." [67]

Fosdick's admonition to Weaver was in no way a refusal to accept his ideas about the management of science. Furthermore, every measure of economy approved by the trustees in fact increased the officers' control in promulgating and guiding their programmes. They were directed by the trustees to draw up schedules for the speediest abandonment of general programmes "consistent with Foundation obligations and dignity". The programmes to be abandoned were generally older ones which were not in the fields of concentration.[68] The fellowship programme was reduced in areas not related to special programmes; the National Research Council fellowships in mathematics, physical science, and medicine were trimmed. Fluid research grants to be spent as the universities desired were abolished, as were all grants given to scientifically backward institutions. The officers were directed to make maximal use of grants-in-aid for individual research schemes related to special programmes.[69] The trustees urged Weaver to change the name of his programme from "vital processes" to "experimental biology". They eliminated the earth sciences.[70] The prohibition of increased budgets and of large projects left intact the policy of concentration on special programmes. This gave Weaver and other divisional officers a larger, not a more constricted, role in the planning and management of their programmes. Fosdick chose not Flexner's policy of less management, but Edsall's policy of more stringent and judicious selection of projects.

From Psychobiology to Molecular Biology: 1934–38

The most important changes in Weaver's programme between 1934 and the Second World War represented a shift from "psychobiology" towards "molecular biology"; a decline in the invocation of utility, a reduction in the support of those aspects of endocrinology, sex research, and nutrition associated with clinical application; and an increasing reference to the physical sciences, particularly organic chemistry. What did not change was the officers' freedom to select a project to be undertaken according to the criterion of how well it would fit into the programme. Weaver found the most appropriate projects in the application of new physical methods in biochemistry, selected areas of cell physiology, and genetics, *i.e.*, "molecular biology".[71]

To evaluate and select research projects which fitted his programme in "experimental biology" and to identify the most able investigators required

[67] RF.900.22.166. "Report of Committee of Appraisal" . . . pp. 58–59.
[68] *Ibid.*, pp. 36–37.
[69] *Ibid.*, pp. 78–79, 89, 90–92, 46–47.
[70] RF.900.22.166. "Report of Committee of Appraisal" . . . p. 61.
[71] RF.915.1.1. Weaver to W. E. Tisdale, 27 December, 1934. Tisdale to Weaver, 16 January, 1935. Weaver to Tisdale, 8 February, 1935.

an administrative staff and many personal connections in eight different disciplines. Weaver had to know whose opinions might be relied upon, how to evaluate applicants. He tended at first to rely on bodies of external advisors, *e.g.* the National Research Council committees and fellowship boards,[72] but as he became more confident and better informed, he took a more active role, seeking out applicants and selecting projects himself. A fellowship programme in " vital processes " was developed.[73] The National Research Council committees on sex research and radiation effects continued to administer these two areas, but they became ever smaller parts of the programme. When Weaver considered creating six more Council committees in endocrinology, nutrition, genetics, etc., he intended that they should have only advisory functions to carry out surveys and find promising individuals and projects for Foundation support.[74]

A great deal of energy was expended in the first few years in debating the advantages and disadvantages of various forms of patronage, and the modes of deciding on their recipients. It was thought that the awards of fellowships to aid recruitment and training would be best administered by committees of experts. The grant-in-aid or small grants to individuals —$100 to $1,000—for specific and limited pieces of research were conceived as supplements for former Foundation fellows in their first appointments. These were difficult to administer. The grants to institutions, on which Weaver had first hoped to rely, were to be administered by university or departmental " research committees " and were to strengthen " university science " as a whole.[75] Like the fellowship programme, these had the advantage of not requiring highly qualified administrative staffs, but they did not leave room for direct control by the officers of the Foundation. Weaver's original intention was to use all these forms of support in combination. But the financial crisis of 1933–34 compelled the suspension of institutional grants and scattered grants-in-aid, and Weaver was forced to rely on " project-grants " of medium size.

The " project-grants " were made to individuals or groups of individuals, often in different disciplines; they amounted to about $6,700 per year and were usually given for three years. They were given for planned or programmatic projects usually involving the application of some physical technique to biological problems and the collaboration of physical scientists and biologists. The " project-grant " was conceived as a provisional compromise between the institutional grant and the grant-in-aid.[76] But it

[72] RF.915.1.1. " Progress Report ", 27 January, 1933. RF.915.22.168. Agenda for Meeting, April 1933, pp. 83–87.

[73] RF.915.1.6. " Progress Report ", 27 January, 1933. RF.900.23.171. " Director's Report", 11 December, 1934, pp. 13–14. See also Weaver to F. B. Hanson, 20 April, 1933.

[74] RF.915. " Progress Report ", 14 February, 1934, pp. 3–4.

[75] RF.915.1.6. Weaver, Warren, " Science and Foundation Program ", 26 January, 1933, pp. 13–14. For example, Weaver pressed for a capital grant to the institute of organic chemistry at Göttingen on the grounds that its weakness hindered Foundation programmes in other institutes of that university.

[76] RF.915.22.168. Agenda for Meeting, 11 April, 1933, pp. 83–86.

quickly became the favoured instrument for Weaver's programme in " experimental biology ". It was an ideal form; it was large enough not to cause complaints among the trustees of dispersal of resources and small enough not to appear extravagant. It enabled scientists in different disciplines to collaborate and thus disarmed criticisms which charged that progress would not come from proceeding along disciplinary lines. It also gave Weaver control over policy. Since each grant was made for a specific project, with an eye on its long-term potentiality, it could be carefully selected and could often be discreetly influenced by Weaver. Typical " project-grants " were those to Harold Urey and a group of Columbia University biologists and biochemists in 1934 for biological investigations with heavy water [77]; the grant to Niels Bohr, George von Hevesy, and August Krogh, for use of radioactive isotopes in physiology; and to The Svedberg, to develop the ultracentrifuge for biochemical work. By the end of 1933 five such grants had been made by the natural sciences and three by the medical sciences divisions.[78] By the end of 1934 Weaver had selected 39 projects in the various fields of his programme: 16 in biochemistry and biophysics, 10 in physiology and embryology, six in genetics and seven in endocrinology and nutrition.[79] For Weaver, the advantages of " project-grants " were great: they were selective and helped " the best " scientists; they were efficient, since the universities paid the overheads for research facilities; they enabled the Foundation to foster the " natural and genuine common interests " between biologists and chemists.[80] They were an ideal device for advancing his programme for a " new biology ".

The shift from " psychobiology " to " molecular biology " was made easier by structural changes within the Foundation. The division of labour between the natural sciences and medical sciences was officially recognised by Fosdick in 1937, and the transfer of endocrinology and sex biology to Gregg permitted Weaver to deal with the physical sciences. Improvements in economic conditions also permitted expansion in that direction and successes in fields such as biochemistry provided Weaver with the justification for supporting an obviously promising field of research.

There was at first no formal policy covering the jurisdictions of Weaver and Gregg, owing to the considerable overlapping of interests. Borderline projects, such as Einar Lundsgaard's work on the biochemistry of

[77] RF. Files on " Columbia, Heavy Water " and " Columbia Biological Chemistry ". Kohler, R. E., " Rudolph Schoenheimer, Isotopic Tracers and Biochemistry in the 1930's ", *Historical Studies in the Physical Sciences* (in press). See also RF.915.1.8. " Progress Report ", 16 May, 1936, p. 49.

[78] RF.915.1.7. " Medical and Natural Sciences ", 13 December, 1933, p. 8. The budgets were $185,000 and $154,900 for the natural science and medical projects, and but for the moratorium on grants over one year, appropriations would have exceeded $1 million.

[79] RF.900.23.171. " Director's Report ", 11 December, 1934, pp. 17–19. Of these 39, 31 were in the United States, and six of the eight European projects were in biochemistry.

[80] *Ibid.*, pp. 19–20.

muscular contraction or Henry H. Dale's work on acetylcholine and nerve transmission, were allocated by Weaver and Gregg.[81] Gregg took those projects in their eight fields which were related to psychobiology and Weaver took the more basic studies. Both saw their interests in terms of large problems rather than disciplines [82] and this calmed the trustees' apprehensions of the domination of pre-established disciplinary patterns. The good relations between Gregg and Weaver also helped President Mason to overcome the trustees' misgivings about artificial boundaries within the Foundation.[83] Such cooperation between divisions was greatly welcomed by Fosdick and the trustees in the early 1930s as a sign that the divisional structure did not impede the study of large problems.

In 1937 Fosdick decided to separate the medical sciences and natural sciences by subject division in order to resolve certain difficulties which had arisen in the relationship between the officers and the trustees. Before the reorganisation of 1928, the trustees made decisions about institutions and policies on social problems or public health; experience in law or business qualified them to do so. When the Foundation became primarily concerned with scientific research, the trustees became responsible for appropriating large sums of money for purposes in which they had no expertise. Moreover, they were provided with little explanation in advance and had no way of judging whether what the officers put before them were the best proposals.[84] The trustees' suspicion of disciplinary narrowness in academic science was aggravated by lack of regular communication with the officers; criticisms of administrative inefficiency and duplication were used to resist divisional programmes. The overlapping interests of the natural sciences and medical sciences divisions were especially vulnerable to such criticism.

In 1936 Mason retired as president, and Fosdick succeeded him. The succession by a scientist of a trustee as president was intended to bridge the gap between the officers and trustees. Fosdick encouraged more open discussion of troubling issues between officers and trustees, and Weaver took every opportunity to explain his plans and procedures. One of Fosdick's first moves as president was to rationalise administrative structure so that " psychobiology " was entirely removed from the natural sciences division. Weaver was relieved of endocrinology, nutrition and sex biology, rather to his relief, for they had lost their fashionable appeal.[85] Moreover the separation left him free to follow his own line of interest. It per-

[81] These allocations were made on an *ad hoc* basis. RF.915.1.2. Weaver to Tisdale, 27 March, 1934. D. P. O'Brien to Gregg, 29 October, 1934. Gregg to O'Brien, 19 July, 1935. Weaver diary, 10 December, 1935.

[82] RF.915.1.1. Weaver to Lauder Jones, 26 January, 1933.

[83] *Ibid.*, Mason to Strode, 9 May, 1934.

[84] RF.915.1.2. Weaver to Tisdale, 26 May, 1936. RF.915.1.8. Weaver, Warren, " Program and Administration ", 10 October, 1937, p. 18.

[85] RF.915.1.2. Fosdick to Greene, 25 March, 1937. RF.915.1.8. Weaver, Warren, " Progress Report ", 16 May, 1938, pp. 32, 47. RF.915.1.12. " Report of the Committee of Review ", November, 1938, p. 29.

mitted him to press the expansion of his programme into basic sciences such as organic chemistry, which had previously been defined as too distant from experimental biology. Since it was unofficial policy to maintain approximate parity between divisions, Weaver claimed that the new arrangement with medical sciences ought not to result in a contraction of natural sciences: "I have hoped that when our program was narrowed on the one flank, by excluding endocrinology and sex research, it would be widened on the remainder." [86] In December 1937, one week after the trustees' approval of the separation, Weaver pressed Fosdick to accept organic and physical chemistry as part of his programme.[87] He asked Fosdick for a new staff-member in biochemistry,[88] and later proposed a series of surveys. His order of preferred fields reveals the new shape of his programme:

If we were to try out this procedure in one field, my choice definitely would be biochemistry: and this field is so large and so important to us that I would be inclined to suggest two rather heavily overlapping surveys,—one by a biochemist and one by an organic chemist. . . . If the survey in biochemistry proved useful, I would suggest genetics as a second choice and general and cellular physiology as a third choice. A somewhat briefer survey would suffice for the field of embryology and developmental mechanics; and we might eventually wish to have a general study made of biophysics.[89]

He thus re-entered areas in the physical sciences which he had been unable to cultivate in the straitened early years of the economic depression.

The assignment of "psychobiology" to Gregg clarified the internal division of labour within the Foundation between applied and basic programmes. The officers agreed that each division should develop a different conception of "pure" and "applied" research. The social sciences were less abstract and hence would be closer to concrete social and economic problems. The abstract natural sciences rightly focused on strengthening the sciences as such. The medical sciences required a judicious combination of basic research and application.[90] This specialisation of functions within the Foundation made it possible for Weaver's division to pursue goals which were not immediately concerned with "the welfare of mankind". As long as Weaver's programmes were seen as contributing to the more practical programmes of other divisions, Weaver was able to concentrate on "molecular biology" without stirring up divisional rivalries or diverging from the more general ideals of the Foundation.

Weaver's shift towards organic and biochemistry was also facilitated by improving economic conditions. As old programmes were ended and the

86 RF.915.1.2. Weaver to Fosdick, 29 November, 1937.

87 RF.915.1.8. Weaver, "Program and Administration", 1 October, 1937, pp. 24ff.

88 RF.915.1.2. Weaver to Fosdick, 20 December, 1937.

89 RF.915.3.26. Weaver to Fosdick, 4 October, 1938, p. 2.

90 RF.900.23.172. "Summary of Conference", 10–11 October, 1938, pp. 3–4. RF.915.2.12. Weaver memorandum, 27 November, 1940; Gregg memorandum, 12 November, 1940; on the divergence of the administrative policies of the NS and MS divisions.

national economy revived, the trustees again began to worry about "scatteration" and to look to larger undertakings.[91] In November 1936, Weaver wrote Tisdale that the stringent policies of 1934–36 were ending and urged him to look for opportunities to make larger institutional grants, such as the grant to the departments of organic and physical chemistry at Oxford University which was then pending before the trustees.[92] Tisdale worried that the trustees' dislike of small projects endangered the grant-in-aid programme, but Weaver saw more clearly that the natural sciences programme would only benefit from an increase in large "project-grants".[93] The pressure of hard times had forced Weaver to eliminate all but the essentials of his programme in vital processes, pruning away what probably would have been diversionary interests in earth science, colloid chemistry, etc. Thanks to this pressure a coherent programme formed around a single theme, and when growth resumed, it was these areas and areas functionally related to them which grew most rapidly. Economic exigencies aided Weaver's ability to plan and pursue his own policy for science.

Weaver's "Investment" Policies

The opportunity for fruitful work in a science, as it is conceived by a foundation, is not a property of a science alone, but rather a function of the correspondence between the aims of the foundation and scientific potentiality. In his role as a manager of science Weaver had to consider such criteria as appropriateness to the resources and policies of the Foundation and its desire for visible results, as well as the scientific importance of the project judged by scientific standards. The ideal area for investment from the Foundation's point of view was one with a certain degree of past success, but with patently great potentialities—not so needy as to require large investment before any results would become evident but not so well provided for that more support would have diminishing returns.

Weaver gained in confidence and experience in dealing smoothly with the trustees, and the trustees became more confident of Weaver's judgment. Since the application of new physical techniques to old problems in biology or biochemistry was almost certain to yield new and often striking results, it was an ideal plan for an expanding programme. When there was a second review of the policies of the Foundation in 1938, Weaver guided the committee of experts smoothly. Despite their uneasiness with the term "experimental biology", they enthusiastically approved of his programme, policies, and procedures. The trustees too were delighted.[94]

Weaver's reports showed the discriminating way he applied "invest-

[91] RF.900.23.172. "Minutes of Trustees Meeting", 30 November, 1937, p. 4.
[92] RF.915.1.2. Weaver to Tisdale, 19 November, 1936.
[93] *Ibid.*, Tisdale to Weaver, 18 December, 1936, p. 3.
[94] RF.915.2.9, 10 and 12. RF.915.3.26 and 27 contain reports and correspondence concerning the review of 1938.

ment" criteria to his programmes.[95] Experimental embryology he saw as a small but healthy field, which needed no special stimulation or long-term development funds. He thought that there would be opportunity for important advances in biochemical and X-ray analysis of developing embryos, but that the field would not reach this stage for some time to come. Hence he was content to allow this field to remain small.[96] Nutrition was a vast and rapidly expanding field of great public importance but, since it was also well supported by governmental agencies and the pharmaceutical industry, there was little need for him to come to its support. For this reason Weaver confined his interest in the field of nutrition to the biochemistry of vitamins.[97] Biophysics, in contrast with these, was an undeveloped discipline without a core of coherent problems and methods, recognised academic standing, or university departments and outstanding investigators.[98] "Biophysics . . . is still for the most part an orphan subject. Able young physicists, however genuine their interest, hesitate to devote themselves to a profession which is insufficiently recognized to offer a reasonable chance for a permanent job." [99] To build biophysics as a field would require the endowment of chairs, construction of laboratories, and systematic recruitment and training, a task which the Foundation was unwilling to assume in 1938.

Biochemistry, cell physiology, and genetics more and more clearly offered the best prospect of important and immediate results. They were well-established disciplines with well-defined problems and techniques, notable records of success, good facilities and sources of recruits, and most important, undeveloped areas within them of great promise, especially through the application of new physical-chemical techniques. In cell physiology, there were Robert Chambers' studies of cell microstructure with his micromanipulator,[100] and the spectroscopical studies of cell oxidation and reduction-systems conducted by Otto Warburg and others. In genetics, the chemistry and physiology of mutation, gene structure, and gene expression provided immediately exploitable opportunities.[101] All the early work of George Beadle and Boris Ephrussi, and Alfred Kühn's work on eye colour "hormones" were supported by the natural sciences division. So was Beadle's and Tatum's development of biochemical genetics in the early 1940s. Weaver saw mammalian genetics as containing a long-range

95 RF.915.1.8. Weaver, Warren, "Progress Report", 16 May, 1936, pp. 42–44. Research supported under this heading included photosynthesis, effects of ultraviolet rays on vitamins, X-rays on tissues and Gurwitch's alleged "mitogenic radiation" from dividing cells.

96 *Ibid.*, pp. 14–16.

97 *Ibid.*, pp. 32–41.

98 *Ibid.*, p. 8. He also noted that many biologists disliked the term "biophysics".

99 RF.915.1.3. Weaver to Fosdick, 17 October, 1938. This memorandum was addressed to a new foundation concerning opportunities for patronage of experimental biology. Weaver recommended biophysics to those with faith and patience.

100 RF.915.1.8. Weaver, Warren, "Progress Report", 16 May, 1936, pp. 17–18.

101 Rockefeller Foundation, *Annual Report 1935*, pp. 150–151, stresses the importance of "physiological genetics". See also RF files on Alfred Kühn, Boris Ephrussi, George Beadle, and M. R. Erwin.

opportunity, which would require a considerable investment in institutions, equipment and personnel.[102]

Biochemistry seemed to Weaver to be the most promising field for the application of physical techniques; most of his projects in "molecular biology" were in this field, as were most of the projects involving the cooperation of biologists and physical scientists.[103] So was Theodor Svedberg's work on proteins using the ultracentrifuge; a number of the first ultracentrifuges in the United States were built with the funds provided by the Rockefeller Foundation. The application of isotopes to biochemistry by Hans Clarke and Rudolf Schoenheimer and other groups was particularly successful, and William Astbury's work on the X-ray crystallography of macromolecules found special favour with Weaver. Weaver also emphasised the critical importance of chemistry:

> . . . developments within the United States of the whole divisional program are being and will continue to be critically limited by the weakness in this country of those fields of chemistry which should contribute most directly to biological studies. This remark applies mildly to physical chemical studies of high molecular weight compounds and to surface chemistry; but this remark applies with full force to the organic structural chemistry of natural substances. This field has been notably developed in Europe . . . the leadership in American organic chemistry can be counted on less than the fingers of one hand.[104]

Organic chemists such as Robert Robinson were critical figures in Weaver's programme for the development of "molecular biology".

Weaver's intensifying focus on a few academic disciplines did not blur the outlines of his main goal: the application of the techniques of physical science to biology. He was not simply a passive patron but a promoter of science along particular lines. His aim was to pursue a goal which was not confined to any one discipline, not to develop disciplines as such. His own measure of his success was the extent to which his programme had changed the course of biology. In 1936 he wrote:

> A considerable part of the support given to date admittedly has added to the quantity of research in the chosen fields but without changing, in any significant way, the nature or quality of such research; but there are underway certain researches, certain general developments, certain reorientations of interest which would not have occurred if this program had not been followed. The major success of the program rests, although as yet potentially rather than actually, in these deeper influences.[105]

Weaver identified the work of Pauling, Hogness, Wrinch, Robert Robinson, Astbury, Runnstrom, and Bohr, Krogh and Hevesy, as the best evidence of this deeper influence.[106]

[102] RF.915.1.8. Weaver, Warren, "Progress Report", 16 May, 1936, pp. 19–25.
[103] *Ibid.*, pp. 8–11.
[104] *Ibid.*, pp. 10–11. For similar statements, see RF.915.1.7. "Progress Report NS", 14 February, 1934, pp. 10ff. RF.915.2.16. "Report of the Committee of Review", November, 1938, p. 12.
[105] RF.915.1.8. Weaver, Warren, "Progress Report", 16 May, 1936, p. 52.
[106] *Ibid.*, pp. 52–53.

Because Weaver's conception of the scope and direction of academic disciplines was much broader than those of the scientists whom he supported, his policy resulted in real changes of direction within certain disciplines, biochemistry in particular. The recipients of " project-grants " in " biochemistry-biophysics " included only a few who were biochemists in the conventional academic sense—Vincent du Vigneaud, Hans Clarke, and Hans Krebs. The older leaders of American biochemistry were conspicuously absent; physical and organic chemists, physiologists, physicists, mathematicians and biologists made up the majority. Weaver thought that the most promising work in " biochemistry " was being done by outsiders to the discipline and he acted accordingly. As a result a new generation of biochemists emerged in the late 1930s, many from the Rockefeller Foundation programme; they knew how to use isotopes and the ultracentrifuge, and they combined a knowledge of organic chemistry with a sensitivity to physiological processes.

The term " molecular biology ", which was first used by Weaver and Fosdick in 1938, suggests this subtle combination of a base in a discipline and a trans-disciplinary goal.[107] This combination of traditional disciplines with a broader programme, supported by the general policies of the Foundation and imaginative and skilful management of scientific research, had profound effects on the structure and research programme of several disciplines.[108]

Conclusion

In assessing Warren Weaver's and the Foundation's part in the determination of science policy, one must distinguish between the actual choice of fields, and the establishment of a role of patron and manager of science. The former seems to have been a matter of historical contingency, resulting from Weaver's preference and his assessment of current scientific interests. The alternatives were open in the years between 1929 and 1932, and the opportunity for individual influence correspondingly great. The creation of the managerial role has more complex determinants in the structure, ideals and policies of the Foundation as an institution. The ideals of the Foundation were influenced by the managerial ideals of the Progressive period. More than either industry or government in the 1920s, the large foundations espoused the idea that science was a cultural " resource " to be managed in the interests of the country as a whole. The trustees' misgivings regarding the support of academic science for important social tasks should not obscure their fundamental premise that science was central to culture, and that the Rockefeller Foundation had

107 Rockefeller Foundation, *Annual Report 1938*, pp. 39–40, 203–217. RF.915.1.3. Weaver to Trevor Arnett, 27 December, 1938.

108 A detailed analysis of the effects of Weaver's programme on the discipline of biochemistry is in preparation.

a special responsibility for its advancement. Fosdick in particular believed in the values of order and rationality, and in reorganising the Foundation and in maintaining his policies in the 1930s Fosdick made it possible for Weaver to develop an active role in the formulation of a policy for these ends. His policies were controversial and vulnerable; directives from the trustees which fluctuated with economic and political conditions would have made it difficult to frame and execute a coherent long-term plan and to hold the trust of scientists. Fosdick's constant support was crucial for the success of Weaver's role as a manager of science. The organisation of the Foundation, representing all branches of science, and its divisional structure, following academic lines rather than those set by substantive problems, were also important. A very limited or practical aim would have left no opportunity for managerial activities by the officers which required a clear scheme regarding the desired direction of science, the exercise of judgement in selecting projects, and the exercise of influence over the recipients of the Foundation's funds. Institutional arrangements in themselves only provide opportunity. Weaver was able to give reality to his potential influence because his aims were supported by Fosdick and the other officers and because they fitted into the ideals of the Foundation.

The idea of the manager, possessing skills and expert knowledge and an interest only in the well-being of his institution, dates at least from the time when the corporation became the dominant American institution.[109] The managerial ideal was applied to science itself, and Weaver was not the first or only one to do so. In his study of American agricultural research stations, Professor Charles Rosenberg has suggested that the domestication of academic science in a new institution was made possible by the emergence of a new social role, the "scientist-entrepreneur".[110] With sympathetic understanding of both the academic scientist and the practical needs of the institution in which they worked, the scientist-entrepreneurs created a new audience for science by bringing together groups with quite different conceptions of science and its social use. In the rise of the industrial research laboratories, there had been, for instance, scientist-entrepreneurs such as Willis Whitney of the General Electric Company and Kenneth Mees of the Kodak Company. In governmental bureaux, scientist-entrepreneurs like Harvey Wiley and Gifford Pinchot were pioneers of a managerial role for scientists in governmental regulation. Weaver too may be seen as one sort of scientist-entrepreneur, creating not a new institutional setting for academic scientists but rather

[109] See Wiebe, Robert H., *The Search for Order 1877–1920* (New York: Hill and Wang, 1967). The classic statement of the managerial idea is Lippmann, Walter, *Drift and Mastery* (New York: Mitchell Kennerley, 1914).

[110] Rosenberg, Charles E., " Science, Technology and Economic Growth: the Case of the Agricultural Experiment Station Scientist, 1875–1914 ", in Daniels, George H. (ed.), *Nineteenth Century American Science* (Evanston: Northwestern University Press, 1972), pp. 181–209.

a working relationship between academic scientists and their new patrons, the private foundations. Weaver was unique in that he was not the director of research within an institution where research was actually carried on, but rather the overseer of a far-flung system of academic scientists whose research was supported by the Foundation.

Weaver's role as overseer and manager of this system may be compared with other attempts at the promotion of a national system of scientific organisation. George Ellery Hale's conception of the National Research Council as a mediating organisation, locating opportunities for research and allocating governmental funds to individual scientists, was a conception of the management of science not unlike that of the Rockefeller Foundation.[111] As Elihu Root once put it, the idea was to apply the scientific method to science itself. But Hale's plan foundered through his own unwillingness to accept a measure of accountability with patronage, and on the federal government's preference for its own scientific bureaux in which control was more detailed and continuous. The Science Advisory Board was likewise a mode of representing the various parts of academic science to a potential federal patron. It failed to be truly representative and was left politically vulnerable: it was unable to formulate an accepted set of rules for the rights and responsibilities of patron and scientist.[112]

Perhaps because Weaver was neither an official representative of professional science nor of government, he was more successful in striking a balance between the partly overlapping, partly conflicting needs of patron and recipient of patronage. The structure and ideals of the large foundations were perhaps more suited to the exercise of a supple, imaginative and firm managerial initiative. As a result of its historical links with and understanding of universities, and its quasi-public status, the Foundation was in a unique position to mediate between the scientific and the public interests. The very factors within the Rockefeller Foundation which made it possible for the role of the manager of science to become established may also be cited to explain why that role remained peculiar to the large foundations, and why the massive support of science from the public purse has tended either to be directed primarily to practical ends, or to be unilluminated by a keen and discriminating vision.

111 Kevles, Daniel J., " George Ellery Hale, the First World War, and the Advancement of Science in America ", *Isis*, LIX, 199 (Winter 1968), pp. 427–437.

112 Auerbach, Lewis E., " Scientists in the New Deal: A Pre-war Episode in the Relations between Science and Government in the United States ", *Minerva*, III, 4 (Summer 1965), pp. 457–482.