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The Rockefeller Foundation and the Origins of Behavior Genetics

Alan Gregg, director of the Rockefeller Foundation's Division of Medical Sciences, was sure that nature contributes more than nurture to differences in human behavior. Unfortunately, the public did not appear to share this conviction. Doctors and educators were particularly reluctant (or so Gregg thought) to admit the importance of genetics. In a 1945 letter to Clarence C. Little, then director of the Jackson Laboratory in Bar Harbor, Maine, Gregg expressed his opinion that:

> in medicine, after years of attention to the nature of the invading organism and various aspects of advanced disease, we are beginning to realize that susceptibility to infection and tendency to degenerative diseases and various abnormalities of form and function have much to do with genetics. Since education is so extraordinarily important for all parts of society and all fields of endeavor, a more accurate definition of the limitations of education through planned experience of the pupil would actually increase the effectiveness of education. There would not be such an incredible and insufferable amount of effort spent in present teaching methods wasted on material that cannot profit from such methods, and there might be a realization that man has as much to gain intellectually by wise matings as by $800,000 high schools.[1]

Gregg had been appointed director of the Medical Sciences (MS) Division during a major reorganization of Rockefeller philanthropies. In 1928 Raymond Fosdick, then chief counsel to John D. Rockefeller, effected a

consolidation of four Rockefeller boards, in which all programs related to "the advance of human knowledge" transferred to the Rockefeller Foundation (RF). The character and consequences of this merger have been described by others.[2] Suffice it here to say that it signaled a shift from traditional Rockefeller concerns with education and applied social service to support for scientific research. This development was reflected in the new title of the RF's Division of Medical Education, which in 1929 became the Division of Medical Sciences.

The reorganization was also accompanied by change in the kind of research supported at the foundation. In the past, funds had been awarded to the "best" individuals and institutions, with little regard to field. The new policy emphasized research that promised a substantial social return. In the view of RF trustees and officers, the times demanded planning, and planning, in turn, required a scientific understanding of behavior. However, a science of human behavior did not yet exist. Its development would thus become the central focus of the foundation. By 1933, the natural, social, and medical sciences divisions were officially joined in a project to develop "a new science of man," whose aim was the analysis and control of behavior.[3] A staff report of 1933 notes that all divisions should focus on two areas: the "conscious control of race and individual development with rather particular reference to mentality and temperament," and the "study and application of knowledge of social phenomena and social controls."[4]

This essay describes one strand in this broad program: work on the genetics of mental traits. RF-funded efforts in this area were instrumental in the development of the field that would come to be called "behavior genetics." Of course no single institution was responsible for the creation of this (or any) discipline. A number of wealthy amateur eugenists, small foundations, and the American Eugenics Society (AES) all supported research on the genetics of behavior in the 1930s and 1940s. Their efforts, however, were dwarfed by those of the Rockefeller Foundation.

In the 1930s and 1940s, the government provided little support for research, apart from that conducted at its own institutions. Thus the private foundations contributed most of the funds available for research by nongovernment scientists. Among them, the Rockefeller Foundation was paramount. In the 1930s it accounted for more than one-third of foundation giving to all fields and nearly three-quarters of funds expended in support of research in the natural sciences.[5]

The decision to promote work on the genetics of behavior reflected certain social commitments of RF trustees and officers. By the early 1930s, when the "science of man" program was established, the eugenics movement had already come under attack within the scientific community. Many of those who wished to distance themselves from the movement's scientific crudity and reactionary politics, however, shared its strong hereditarian

assumptions and at least long-term commitments to the breeding of a better race. Not only did these people associate eugenics with scientific naïveté and open propaganda, they also tended to define eugenics as a movement tainted by these failings. Up-to-date research, whatever its aims, thus could not be eugenics. And officers of the reorganized RF saw themselves on the cutting edge of science.

In his essay for this volume, Garland Allen looks at one aspect of the transformation of the eugenics movement. This essay describes another. Behavior genetics, even in the 1930s–1950s, was not merely the old movement under a new name. There was, however, a strong underlying continuity of beliefs and commitments. The field of behavior genetics emerged from the efforts of various institutions—most importantly the RF—to demonstrate the falsity of environmentalist assumptions. The character of this quest (and its uncertain success) will be illustrated with a case study of the first major American behavior genetics project: "Genetics and the Social Behavior of Mammals" at the Jackson Laboratory in Bar Harbor, Maine. The study, headed by John Paul Scott, began in 1945. By the time Rockefeller funding ended eleven years later, it had absorbed more funds than any other genetics project supported by either the MS or Natural Sciences (NS) Division. The "science of man" program, with its emphasis on behavior, began in 1933. Why did the RF wait until 1945 to support American research in behavior genetics? The answer to that question lies in the complicated history of earlier Rockefeller efforts in human biology.

Origins of The Rockefeller Program

It is sometimes said that the RF of the 1930s spurned applied human biology, which was viewed as a throwback to the older emphasis on the application of science to social service and reform and hopelessly entangled with eugenics.[6] That RF officers wished to have nothing to do with eugenics is especially stressed in Gerald Jonas's recent book on the foundation's role in the development of modern science. According to Jonas, Max Mason, who became president of the reorganized foundation in 1929, determined from the start not to support eugenics. His rejection of that program was "unequivocal."[7]

It is true that, even before the reorganization, attitudes of RF officers toward eugenics were mixed. Thus in the 1920s Edwin Embree, director of the old RF's Division of Studies, was thwarted in his efforts to develop a eugenically oriented program in human biology. In 1925 Embree told Raymond Fosdick, the Rockefeller family representative on the RF Board of Trustees and a member of the advisory committee of the AES, that he was:

> tremendously interested in the sciences of human biology, the possibilities of which we are beginning to explore. If it is possible to do anything in such matters as eugenics and a better understanding of mental processes, we shall be making contributions indeed. I realize that it is going to be much more difficult to take bad ideas out of people than it has been to extract hookworms; harder to give them good inheritance than good surgery. While a more complicated undertaking, it is also even more worth investment and speculation.[8]

Embree's plans were opposed, however, by Richard Pearce, the director of the Division of Medical Education, and Simon Flexner, head of the Rockefeller Institute, both of whom apparently believed that the prestige of academic biology and genetics was rising whereas that of eugenics and mental hygiene was in decline.[9] Pearce and Flexner succeeded in convincing Fosdick not to support a large-scale project in this area. Although he lacked trustee endorsement for his efforts, Embree did not give up. On an extended trip to Europe in 1926, ostensibly in connection with nursing education, he met with biologists to promote his program. Frustrated by resistance at the foundation, however, he soon resigned.[10]

With Mason's 1929 appointment as president, eugenics—at least of the mainstream kind—fell still further from favor. In his first year Mason rejected a proposal from the Eugenics Research Association for a project on human inheritance, race mixing, and differential birthrate.[11] His protégé, Warren Weaver, who was appointed director of the Natural Sciences Division in 1933, also adopted a critical stance. During the 1930s the RF declined both Frank Lillie's and C. C. Little's proposals for support of institutes for the study of biology and social problems. But these refusals do not constitute the whole story of the RF and eugenics. As we will see, proposals that were explicitly eugenic in intent *were* funded in the 1930s, although not in Weaver's division and not in the United States. Moreover, analysis of rejected proposals indicates that even Weaver's attitude cannot be characterized as a simple repudiation of eugenics.

In 1931, Lillie wrote to Mason, resurrecting a proposal for an "Institute of Racial Biology" that he had first suggested in 1924. Indeed, he merely enclosed the original memorandum with his letter to Mason. According to Lillie:

> The future of human society depends on the preservation of individual health and its extension into the field of public health; but it depends no less on social health, that is the biological composition of the population. We are at a turning point in the history of human society—the age of dispersion and differentiation of races is past. The era of universal contact and amalgamation has come. Moreover, the populations press on their borders everywhere, and also, unfortunately, the best stock biologically is not everywhere the most rapidly breeding stock. The political and social problems involved are fundamentally problems of genetic biology.[12]

Weaver considered Lillie's suggestion compatible with RF aims and deliberated the question of funding for a decade. Since it would promote basic research, neither Weaver nor Lillie associated the proposed institute with eugenics. Indeed, Lillie wrote that "it should be kept free of all propaganda concerning eugenics, birth control, etc.; and in such connections aim merely to furnish the indispensable scientific foundations on which social prophylaxis of the future must depend."[13]

A number of factors contributed to the project's ultimate rejection. It violated foundation policies both against committing funds for long periods and endowing large institutions.[14] The institute would be devoted to a specific problem—an approach rejected by Raymond Fosdick.[15] In addition, 1931 was a very poor time for establishing enterprises that required a substantial infusion of funds. And Lillie's own research on biological maturation and sexual development, with aims similar to the proposed institute's, continued to receive support through the early 1940s.[16]

Six years later, Little advanced a somewhat similar proposal. In 1937 he met with the directors of the three RF divisions (Edmund Day of the Social Sciences Division, Weaver, and Gregg) to discuss the possibility of Rockefeller funding of an Institute of Social Biology and Medicine. The follow-up proposal reflects an array of loosely linked concerns: genetics in medicine, physiology of sex and contraception, human psychology, growth and development, "population problems," eugenics. The prose is dramatic (there is a "terrifyingly urgent . . . need to preserve the sex cells of civilization—the centers of creative initiative before our overfed and undernourished civilization becomes a great uninspired eunuch with no power to generate the units that build the future"), but neither the content nor the relationship of these concerns is further defined.[17] Thus failure of these two programs implies little about the RF stance toward the subject of eugenics as such.

The severest antieugenic remarks were voiced by Warren Weaver. For example, in a 1933 report he wrote that "work in human genetics should receive special consideration as rapidly as sound possibilities present themselves. The attack planned, however, is a basic and long-range one, and such a subject as eugenics, for example, would not be given support."[18] He also asked (rhetorically) "whether we can develop so sound and extensive a genetics that we can hope to breed, in the future, superior men?"[19] These comments are not necessarily contradictory. Weaver believed that "the human race needs, and needs desperately," a science of human genetics, which would ultimately be used to produce a better race.[20] His contribution to this end, however, would be funding of basic work in mammalian and microbial genetics.

Galton's own definition of "eugenics"—"the study of agencies under social control which may improve or impair the racial qualities of future generations"—would seem to describe Weaver's efforts. But Weaver himself obviously applied a narrower definition. This is not the place to consider how Weaver's activities are best characterized, for Weaver was not responsible for funding work in human genetics.[21] That field was the province of Alan Gregg, whose Medical Sciences Division was responsible for research with human subjects.

Alan Gregg, Psychobiology, and Human Genetics

Gregg first joined the foundation in 1919 as a young MD and three years later was offered a position as Richard Pearce's assistant in the Division of Medical Education.[22] By the mid-1920s, that division had already begun to shift resources to the study of biology and psychology in relation to medicine and public health—a policy reflected in large grants to two German Institutes: Emil Kraepelin's biologically oriented Institute of Psychiatry in Munich and Oskar Vogt's Institute for Brain Research in Berlin.[23]

However, the pace of change greatly accelerated after Gregg succeeded Pearce as director of the MS Division on the latter's death in 1930. During Gregg's tenure, the division drastically reduced its programs in medical education in order to support research in a new area variously titled "psychobiology," "mental hygiene," or, later, "psychiatry." Under these rubrics, Gregg supported a wide variety of biologically oriented approaches—endocrinological, neurophysiological, and genetic—to the understanding of behavior. Gregg's program in "psychobiology" (later called "psychiatry") absorbed about three-fourths of the funds expended by the MS division.[24]

In Gregg's view, the costs of our failure to understand the workings of the human mind were manifest in the "economic, moral, social, and spiritual losses occasioned by the feebleminded, the delinquents, the criminal insane, the emotionally unstable, the psychopathic personalities," as well as in the less extreme but far more common (and preventable) fears, phobias, and aberrant behavior of otherwise sane human beings.[25] In many countries, he argued, more beds were devoted to the care of mental cases than to all other diseases combined. He also considered the educational system to be enormously wasteful. (Indeed, Gregg's correspondence and internal memoranda actually focus much more on the failures of education than on medicine.)

The new field of psychobiology, designed to address these problems, encompassed various approaches to the understanding of mind. Work in human genetics constituted only one component in a multifaceted research program. But given Gregg's belief that differences in human cognitive abilities as well as susceptibility to mental illness were largely attributable to differences in genes, and his assumptions about the relevance of such differences for social policy, it was an important element.

In the 1930s, however, few attractive opportunities existed in the United States to promote research on the genetics of mental traits. American psychiatrists had little interest in biology and virtually none in genetics. Geneticists, on the other hand, focused on corn and fruit flies and ignored humans. At least that is what RF officers believed. Thus in a 1936 report that includes an extensive assessment of the state of American genetics, Charles B. Davenport of Cold Spring Harbor is characterized as the "leading American worker in human genetics."[26] He is also the only American mentioned. In the 1930s, therefore, Gregg looked to Europe.

He considered the Scandinavian countries particularly suitable for work in human genetics given their homogeneous and stable populations and the existence of accurate and complete medical records. Beginning in 1930, a number of small grants in human genetics/eugenics were approved by the RF Paris Office to the Pathological Institute of Copenhagen, directed by Oluf Thomsen. His student, Tage Kemp, also received two RF fellowships (one enabling him to work with Davenport at Cold Spring Harbor) and a grant in 1934 in support of work on the genetics of psychopathology. In 1936 Gregg appropriated $90,000 toward establishment of an Institute of Human Genetics, directed by Kemp, at the University of Copenhagen. This institute was to engage both in research, especially on heritability of mental traits, and in genetic counseling explicitly informed by eugenic concerns.

Germany was thought to be particularly advanced in research on the genetics of mental traits. As a result, it received a large share of the funds expended by the MS Division. Between 1930 and 1935, Gregg contributed $125,000 for a nationwide anthropological survey of the German people directed by Eugen Fischer. The project was undertaken, according to an RF report, "to provide a means of finding a scientific basis for the study of the racial or biological composition of the German people and of the interaction of biological and social factors in determining the character of the present population."[27] Between 1932 and 1935, the RF also appropriated funds for twin research and for studies of the effects of poisons on the germ plasm at Fischer's Kaiser Wilhelm Institute (KWI) for Anthropology, Human Genetics, and Eugenics. Other genetic/eugenic studies, at the KWI for Brain Research and the German Psychiatric Institute (under Ernst Rudin, an author of the German sterilization law), also continued to receive RF funds even after Hitler's seizure of power. In 1939 the foundation finally ended support of all programs in Germany. Except for two grants to Viennese social scientists, the only projects still being funded were in Gregg's psychiatry program.[28]

Foundation funds in Britain were channeled through the Medical Research Council, which in the mid-1930s elaborated a new program in psychiatry and related subjects to be directed by the Mental Disorders Committee. During the 1930s the MS Division supported projects by D. K. Henderson and T. A. Munro at Edinburgh on consanguineous marriage and mental disorders, Janet Vaughan at Hammersmith on human heredity in relation to psychic disturbances and neurological diseases, Lionel Penrose for the Colchester Survey on mental deficiency, and R. A. Fisher for serological research.

Work in serological genetics was considered especially exciting. In the early 1930s it seemed likely that serological analysis could be used to identify heterozygote carriers of genes responsible for mental defect. If heritable antigens in blood were linked to such genes, they could serve as genetic "markers" for the traits of interest. The identification of such markers would solve a problem that had bedeviled eugenics since the 1910s: the fact that schemes for segregating or sterilizing the mentally deficient would only slowly reduce their numbers since most genes responsible for mental defects were hidden in apparently normal carriers. Policies that prevented only the affected from breeding would thus work very slowly.

In the 1910s and 1920s some eugenicists hoped to overcome this obstacle by making use of the fact that many "recessive" genes in fact have slight phenotypic effects, that is, the phenomenon of partial dominance. If a normal mentality were not completely dominant over a defective one, heterozygote carriers might yet be identifiable, for example through mental tests. In the late 1920s, however, the invention of serological genetics appeared to promise a far more straightforward approach to this problem than one relying on human symptoms. Some geneticists hoped that those recessive genes causing mental defects when homozygous would themselves have serological effects and thus be directly detectable.[29] Even in the absence of any serological effects specific to these genes, however, it was assumed they would be closely linked with others occurring on the same chromosomes. Given the rapidly increasing number of recognized serological reactions, the prospect of finding one or more of the genes linked to those responsible for a given defect seemed quite promising—provided funds were available for carrying out systematic research in pedigrees exhibiting anomalies. Thus Gregg supported a number of projects in serological genetics in Denmark and Germany, as well as Britain.[30]

Turning to America

During World War II, Gregg hoped that the Carnegie Institution would eventually become a major source of support for work in human genetics. After 1917 Carnegie took over funding of the Eugenics Record Office (ERO), then the leading eugenics institute in the United States. By the mid-1930s, however, Carnegie trustees had become disenchanted. Among other embarrassments, the ERO's superintendent, Harry Laughlin, insisted

on praising Nazi eugenic policies. In 1939 the ERO was closed.[11] Gregg thought that Frederick Osborn, a Carnegie trustee who was also the secretary of the American Eugenics Society, might persuade it to reenter the field. Osborn had been called to Washington during World War II, however, to direct the Army's Division of Information and Education. In 1944 it looked as though Osborn might stay in Washington for some time, making it unlikely that Carnegie would become a major force in human genetics. In Gregg's view, the RF needed to take up the slack.[12]

In 1945 he awarded the first in a series of substantial grants to the psychiatrist Franz Kallmann for research on the genetics of schizophrenia. As a half-Jew, Kallmann had been removed from his position at the KWI for Biology in Berlin in 1935. The following year he emigrated to the United States and found a position at the New York State Psychiatric Institute. His research there was supported by the Carnegie.

Kallmann's views on the "nature-nurture" question were extreme even for the 1930s. In 1935, while still in Germany, he proposed to extend the compulsory sterilization law of 1933 to include the heterozygous carriers of the gene for schizophrenia. Kallmann believed that these apparently healthy carriers would exhibit minor anomalies and thus be detectable. He therefore proposed testing all close relatives of schizophrenics. The testing program was to have been so massive, and would have involved the consequent sterilization of so many people, that it was considered impractical even by the Nazis.[13]

In the same year Gregg also made a large grant to the Bar Harbor project. Its goal—or rather Gregg's—was conclusively to demonstrate a high heritability of human intelligence. The subjects in this study were dogs.

In a contemporary perspective, the use of dogs to substantiate claims about human behavior may appear somewhat peculiar. As we will see, however, in 1945 there was no consensus that humans make the best subjects for human genetics, even the genetics of mental traits. And mental traits themselves were a central concern of work in medical genetics during the interwar and immediate postwar periods. There was, in fact, no real distinction between medical and what would later be called behavior genetics. The emphasis on mentality, in turn, reflected the field's eugenic origins.

Medicine and Mentality

At least in the Anglo-American world, eugenicists had always emphasized the importance of mental, rather then physical, characteristics. Most eugenicists were convinced of the heritability of virtually all aspects of intellect, personality, and character. Few would demur from the view expressed by the author of an early genetics text that "musical, literary, or artistic ability,

for example, mathematical aptitude and inventive genius, as well as cheerful disposition or a strong moral sense are probably all gifts that come in the germ plasm."[14]

Of all these traits, intelligence was generally the most highly valued, and its purported decline the cause of greatest concern. In the United States the primary source of degeneration was thought to be new immigrants from southern and eastern Europe. American eugenic literature of the 1910s and 1920s abounds in comparisons of reproductive rates of Harvard students with those of immigrant groups.[15] Central to the debate over immigration restriction (to the extent that it concerned biology) was the problem of "feeblemindedness." Of course eugenicists were concerned with other traits as well, such as criminality and shiftlessness. But they emphasized mental defect, thinking it the root cause of most other social problems. Some eugenicists believed there were special genes for criminality. Most, however, assumed people became criminals because they were feebleminded. In contrast, questions of health and disease received short shrift in eugenics literature.

Emphasis on mentality carried over to postWorld War II efforts to develop a science of medical, or more broadly, human genetics. In the decade following the end of the Second World War, this field had relatively less to do with studies of clinical disease than of intelligence and temperament. That is not surprising when we realize that most of the pioneers in human genetics—and their patrons—were active eugenicists. Five early presidents of the American Society of Human Genetics (which was founded in 1948) served on the Board of Directors of the American Eugenics Society (AES). Indeed, the AES itself played an important role in promoting and subsidizing both research and publications in the field. Its focus, however, was on intelligence and personality, not disease. The society's director, Frederick Osborn, declared in 1948 he "would not emphasize physical health as a direct objective of the eugenics effort." In Osborn's view, physical factors would take care of themselves. If those incapable of "sound thinking" could be induced not to breed, "that would take care of their physical characteristics at the same time. . . . Our practical program of eugenics needs then to be concerned only with mental qualities."[16]

A 1954 editorial in the society's journal likewise asserted that negative eugenics, aimed at reducing disease, is of far less importance than a positive program to raise the level of the population in respect to mental traits.

> The great possibilities for improvement lie in changing the distribution of births among normal people, so as to increase the proportion of children at the higher levels of normal capacity, while reducing the proportion of those at the lower levels of normality. This would raise the average level of the whole and greatly increase the possibilities for a better human society and for individual and social happiness. This is the

> great field for eugenic advance, and here lies the opportunity of the Eugenics Society.[37]

It should be noted that Osborn headed not only the AES but also the Rockefeller-funded Population Council, which supported most American twin research in the 1940s and 1950s.

Thus in the perspective both of the scientists and of those who funded their work, medical genetics had a very broad meaning. The genetics of mental deficiency, normal intelligence, and schizophrenia received at least as much attention as the genetics of clinical disease. This concern with intellectual and emotional variation was justified on two grounds. On the one hand, differences in susceptibility to disease were thought to have a large emotional component, which was itself highly heritable. On the other, work in human genetics was expected to inform genetic counseling. For counseling to be effective, it was essential to have a genetic picture of the individual as a whole. Most counselors believed it eugenically unwise to advise against reproduction if the individual possessing the defective gene was above average in character and intelligence. It was the "total genetic potential" that mattered and not just the obvious abnormality.[38] Thus in the immediate postwar as in the interwar period, medical genetics was, in large degree, behavior genetics. A study of intellectual and emotional variation was therefore perfectly congruent with medical genetics as it was conceived in the 1940s.

But intelligence and temperament in dogs? In late 1945, after the project was already under way, Gregg talked with R. A. Fisher in London. According to Gregg's diary, Fisher "said that one of the most valuable things that could be done in genetics would be work with dogs in studying temperament and nervous disposition."[39] The assumption that one could (and should) generalize from the behavior of other organisms to humans would have appeared plausible to many of Gregg's contemporaries.

In the 1940s some of the chief contributors to the field of "human genetics" worked with nonhuman organisms. Thus Curt Stern, author of the influential textbook *Human Genetics*, worked with *Drosophila*, and so did Hermann J. Muller, first president of the American Society of Human Genetics. Hans Nachtsheim, the most prominent German in this field during the immediate postwar period, worked with rabbits. The indirect approach to human genetics was strenuously defended by Alfred H. Sturtevant in his 1954 Presidential Address to the Pacific Division of the AAAS. According to Sturtevant, "man is one of the most unsatisfactory of all organisms for genetic study." He argued that:

> there are enough unambiguous examples known to make it clear that the same principles are at work in man as in all other higher animals and plants—and even without such evidence, enough is known about the cytology of human tissues to give us confidence that no peculiar kind of inheritance is to be expected in man. In fact, much of the argument concerning the practical aspects of the genetics of man is best based on experimental evidence from other organisms rather than on what is known directly from study of human populations. . . . [Human research] is especially unsatisfactory with respect to the most important of all human differences—namely, mental ones.[40]

There was, moreover, nothing new about using dogs in studies of human biology. Indeed, the RF had already funded an earlier genetics project with dogs: Charles Stockard's experiments testing whether breed differences in dog anatomy were attributable to hereditary disorders in ductless glands. Nor was the Bar Harbor project the first to utilize dogs to study human mental traits. The most famous example—well known to foundation officers—was Ivan Pavlov, who in 1929 began to relate his work on conditioned reflexes and experimental neuroses in dogs to human mental disease.[41] For about six years during the 1920s, the American psychologist W. Horsley Gantt worked with Pavlov at his Institute of Experimental Medicine. Like Pavlov, Gantt wished to use dogs to say something about human psychology. Unlike Pavlov, he was much concerned with uncovering the genetic basis for differences in the dogs' temperaments. One of the visitors to Pavlov's laboratory was Alan Gregg. Gregg was greatly impressed with Gantt's work and recommended him for a staff position at the Johns Hopkins Medical School, where Gantt moved in 1929.[42] From 1931 to the mid-1940s, the MS Division provided most of the funds for his research.[43]

Dogs seemed particularly appropriate for Gregg's postwar project. As noted earlier, he was convinced that a rational social policy depended on clear and compelling proof of the falsehood of contemporary environmentalist assumptions. Educators, doctors, and the general public—all had to be convinced. The problem, as Gregg came to see it, was that the heritability of behavioral traits had been demonstrated in organisms—such as fruit flies and rats—to which few persons could relate emotionally. The solution lay in making the point with an animal that exerted a strong emotional appeal. From this perspective, dogs were ideal. Gregg was thus led to approach the geneticist Little, a former Harvard classmate and son of a dog fancier, whose cancer research with mice was already supported by the foundation.

C. C. Little and the Founding of the Jackson Laboratory

In 1929 Little resigned under pressure from the presidency of the University of Michigan. He had espoused birth control, tried to reform the university without the support of the faculty, and obtained a divorce. None of these

were popular activities. Reporting on the reasons for his departure, the school newspaper noted, "As a biologist, Dr. Little has been a strong advocate of race betterment programs and the science of eugenics and his courageous statements of his views on controversial subjects, such as scientific consideration of the question of birth control, have led to criticism in some quarters."[44]

Fortunately, Little had wealthy friends in the Detroit business and industrial establishments. Before his stint at Michigan, he had been president of the University of Maine. The university ran a summer course on Mt. Desert Island, where Edsel Ford, Roscoe B. Jackson (president of the Hudson Motor Company), and George Dorr (one of the largest landowners on the island) were summer residents. Jackson, his brother-in-law Richard Webber (who owned the J. L. Hudson Department stores in Michigan), and Ford offered to finance a private institute for cancer research on land provided by Dorr. Although Jackson soon left for Europe where he contracted typhoid fever and died without a will, the project was continued by his widow and the others. With the Depression, these private funds were substantially reduced, and Little turned to the RF for support.

During its first decade the Jackson Laboratory was devoted exclusively to cancer research; the RF provided funds for building, research, and maintenance of a mammalian stock center. However, Little's eugenic concerns remained strong and would soon intersect with an interest of Gregg's.

In late 1941 Gregg received a letter from Little proposing that the laboratory breed a uniform strain of dog to be used in cancer-related experiments. Gregg's response was favorable, but he noted that a breeding program might also serve purposes that were, in all likelihood, incompatible with the project suggested by Little. Were it possible to test intelligence in dogs, Gregg reasoned, it should also be possible to breed a pet that was both amiable and extremely smart. In his view, anatomy had been stressed to a ridiculous degree. People actually appreciated intelligence and good disposition far more than anatomical features in dogs. Gregg was sure there would be a substantial market for a dog both friendly and bright. He also believed there would be two valuable by-products of a project to breed such a pet: (1) their owners would be led to see the relevance of genetics to intellectual performance and (2) they would come to feel indebted to experimental biology for having produced such a pet rather than be critical of the cruelty of animal experimentation.[45]

In 1941 Gregg was merely speculating on the possibility of testing and breeding for intelligence in dogs. But the idea continued to simmer, and two years later he wrote Edwin B. Wilson of the Harvard School of Public Health asking whether he thought that methods used with rats to measure and breed for intelligence would also work with dogs. Wilson liked the idea and suggested talking it over with Little, whom he had seen acting as a judge in the Boston Dog Show. Gregg was delighted with Wilson's reply. He was particularly pleased Wilson had grasped his point about the value of demonstrating heritability of intelligence in dogs. In Gregg's view, most people would never be impressed with the demonstration that there are more intelligent and less intelligent rats. They would simply "dispense with that phenomenon in much the same way as we do with trained fleas."[46]

Soon after, Gregg wrote to Little. He noted that although rat work was useful, it was inaccessible to ordinary people. These people would, however, recognize and appreciate an intelligent dog. Current breeders refused to produce such a pet, fearing that their "morphologically perfect animals" would no longer be in demand. Gregg asked whether it would not be worthwhile to spend fifteen or twenty years breeding an extremely smart but small dog, "just to show that genetically intelligence is capturable and reproducible." In this letter, the social agenda is bluntly described. "My point of departure," he wrote:

> is a conviction that one of the constant afflictions of educators is their ignorance of the hereditary equipment of their pupils. Educators think that environment is everything, but it is not. Consequently, a great deal of their effort is wasted or worse. . . . I'd like to see the talents of some geneticists devoted to the task of showing in a clear and readily accessible form that there are some aspects of intelligence which are . . . transmitted hereditarily. . . .if as a result of some such effort a highly intelligent, scientifically tested animal were to be available for any considerable number of Americans and if such an animal was conspicuously intelligent and satisfactory as a pet, I believe the inference would be almost inescapable that in human beings also intelligence is affected by heredity and that the limitations of education in certain instances are clearly coming from genetic rather than pedagogic sources.[47]

Little's reply was enthusiastic. In his view, the dog would make an ideal experimental subject. Fanciers had already established striking and important differences among breeds. Moreover, dogs are much easier to breed, and much more prolific, than are any of the primates. He affirmed the social value of the proposed project as well. "If we are not convinced of the importance of individual variation and if we do not understand how it arises and how to utilize it," he wrote, "we shall never be able to create a democracy that will have in its own make-up the characteristics necessary to criticize it and to shape its destiny as it evolves."[48] Robert Yerkes also endorsed using dogs, noting, among other factors, that they make a strong emotional appeal to humans without arousing religious prejudice or superstitious bias.[49]

Gregg was, in any case, now prepared to move ahead. He informally promised Little $50,000 a year for ten years, plus another $50,000 to set up

a laboratory. (The RF ultimately awarded $632,000 to the project, over a total of eleven years.) He also asked him to recommend a scientific director.

John Paul Scott

Little had been a student of William E. Castle's at Harvard. So had Sewall Wright at about the same time, and the men were well acquainted. Two of Wright's former Ph.D. students at Chicago—Elizabeth Shull Russell and William L. Russell—were already members of the small staff of the Jackson Laboratory. It was thus natural for Little to turn to Wright for advice concerning an appropriate director. Wright recommended John Paul Scott, a thirty-five-year-old former student of his at Chicago, with a strong interest in the genetics of behavior. Indeed, Scott was at the time the only American with a Ph.D. in genetics who was interested in the behavior of mammals. At the invitation of the Russells, he already had worked two summers at the laboratory on a study of differences in fighting behavior among males in different inbred strains of mice.[50]

At the age of fifteen, Scott read Albert Wiggam's popular eugenic tract, *The Fruit of the Family Tree*. In an autobiographical essay, Scott characterized the book as "old-fashioned eugenics, based on the simple theory that all the world's ills were due to bad heredity, and if we would only apply our knowledge of animal breeding to humans, Utopia would follow." He also wrote, "I did not really swallow this, naive as I was, but it did occur to me that if heredity had all that an important effect on behavior, someone ought to study it scientifically. And so began my interest in behavior genetics."[51] It is thus not surprising that, while working on his Ph.D. at Chicago, Scott should have been influenced by the ecologist/animal behaviorist W. C. Allee. He even did a bit of behavioral genetics in Allee's laboratory with *Drosophila* stocks borrowed from Wright. That work caught the attention of Theodosius Dobzhansky, who suggested a follow-up study using a more sophisticated technique. Like Dobzhansky, Scott was interested in the genetics of other organisms primarily for what they could teach about humans. Unlike Dobzhansky, Scott was convinced that work of human relevance would have to be done with mammals.[52]

After obtaining his degree in 1935, Scott accepted a position at Wabash College in Indiana. There he became convinced the social sciences were unscientific because they ignored biology. More than that, the solution to major social problems lay in the application of biological concepts to social phenomena. Scott reports that as the idea took hold, he "began to feel a little like the apostle Paul on the road to Damascus."[53] He determined to begin work on a new interdisciplinary science, debating whether to call it biosociology or sociobiology (finally settling on the latter). He understood,

however, that he would not be taken seriously by social scientists unless he could "speak their language."[54] Thus Scott and his wife moved to Boston for a year, where he studied in various libraries and began to write a book on social organization in humans and other animals. In 1939 he returned to Wabash, where he did both field and laboratory research on animal behavior and continued to write his book (without finding a publisher) on the relevance of animal behavior to human affairs.

That was the situation in 1945 when he was invited by Alan Gregg to direct a study of the genetics of behavior in dogs. The Jackson Laboratory was a weak and struggling institution. There was no tenure for the staff or firm support. But Scott was enthusiastic about research with dogs, both because of the enormous variability among and within breeds and because dogs are "timid or confident, peaceful or aggressive, and may be born with undershot jaws, club feet, or hemophilia;" that is, they vary in just the same respects as do humans.[55] Allee advised him to accept, and Little promised him a free hand to set up the research project and choose his associates. Scott agreed to go and invented a title for himself: chairman of the Division of Behavior Studies.[56]

The Project

In the summer of 1946, the RF funded a conference on "Genetics and Social Behavior" at the Jackson Laboratory. Two RF officers, Alan Gregg and Robert Morrison, were in attendance. So were scientists from a variety of fields; indeed, they constituted a veritable "who's who" of research on behavior. Social psychology was represented by Gardner and Lois Murphy, comparative psychology by Robert M. Yerkes (who also chaired the conference), Theodore Schneirla, O. H. Mowrer, and C. R. Carpenter, physiology by Frank Beach and Benson Ginsburg (another Wright student, who had been an assistant of Allee's at Chicago and would later work on the project as a summer investigator), and genetics by Clyde Keeler, C. S. Hall, H. H. Strandskov, and John L. Fuller. Fuller joined the project full-time the following year; he would later coauthor the first text in the field of behavior genetics.[57] Science journalism was represented by Walter Kaempffert of the *New York Times* and Gobind Lal of the Hearst papers, among others.[58] The scientific conferees were asked for advice on how best to proceed with the project; the journalists, how best to publicize the results. The latter also constituted a "Committee on Social Interpretation," whose job was to ensure that the public not only heard about the study but understood its point.

For thirteen years, Scott and Fuller collected data on five breeds of dogs: African basenjis, beagles, American cocker spaniels, Shetland sheep dogs, and wire-haired fox terriers. (Purebreds with very different behaviors

but similar size were chosen, in order to use the same apparatus for all.) In the first phase of research, the five breeds were raised in the same environment, and their similarities and differences measured. At the same time, to study the development of behavior, daily observations of the puppies were recorded from their birth to sixteen weeks of age. In the second phase, the basenjis and cocker spaniels were cross-bred, using a classical Mendelian design.[59] In all, some 300 puppies were rated in thirty major tests (each of which included multiple measurements); factor analysis and analysis of variance were applied to at least 8,000 separate pieces of data. The results of these analyses were unexpected, both to the investigators and the RF.

In the first place, correlations among different behavioral tests were low. Breed differences existed, but the same breed would do very well in one test and badly in another; none was distinctly better in problem solving. Thus no breed could be said to be smarter than another. An individual from any breed was able to perform well in a situation where it could be motivated and for which it had the physical capabilities. Within breeds, the capacities of individual animals were highly variable, but most animals could perform required tasks by mobilizing different capacities.[60] The experimenters thus concluded that "nothing like the general-intelligence factor sometimes postulated for humans" exists for dogs.[61]

Nor did they find evidence of general temperamental factors. Breeds fearful in one situation were confident in another. Breed differences were also strongly influenced by habit and training. For example, raising puppies of two breeds together reduced their differences. In general, behavior of young animals was highly variable within individual animals and strikingly similar among them. Thus it appeared genetic differences do not appear early in development, to be modified by later experience, "but are themselves developed under the influence of environmental factors and may appear in full flower only later in life."[62]

Responses to problem-solving tests were greatly affected by emotional and motivational factors. It was extremely difficult to separate these reactions from a tendency to perform well in a particular situation. On the basis of their heritability estimates for various traits in dogs, Scott concluded that human estimates, especially for intelligence, "appear to be far too high." Indeed, he even speculated that "human differences in 'intelligence' reflect only differences in motivation rather than cognitive capacities."[63] What could the RF do with such results?

In a 1944 memo of his interview with Little, Gregg wrote of their agreement on the project's objective: the clear demonstration "to large numbers of persons of the fact that intelligence and other valuable qualities are not created by education as much as brought out by it and that the effectiveness of educational measures is definitely qualified by the inherent potentialities of the recipients thereof."[64] In correspondence, internal memoranda,

motions to appropriate funds, and even the published RF annual reports, the primary objective is always defined as a demonstration of the limits of mass education.[65]

Scott and Fuller did not provide such a demonstration. Even worse, they called constant attention to what they saw as the social implications of the study's results. In 1956, at the project's conclusion, these were summarized by the investigators as follows: "The behavior traits do not appear to be preorganized by heredity. Rather a dog inherits a number of abilities which can be organized in different ways to meet different situations. . . . This means, in terms of human behavior, that the best sort of social environment is one which permits a large degree of individual freedom of behavior. Most individuals can reach desired goals if they are allowed sufficient freedom in the way in which they reach these goals."[66] Given these conclusions, it will perhaps come as no surprise to learn that the "Committee on Social Interpretation" was never mobilized.

Conclusion

In this essay the RF appears as a kind of helpless giant. Notwithstanding Gregg's clear social agenda and near total control of the purse strings, he was unable to obtain results useful for his purposes. Thus even before the development of federal funding and the advent of peer review, scientists seem to have achieved considerable autonomy. This conclusion needs to be qualified, however. Had the results of the project been more to Gregg's liking, a committee existed to ensure that its message was brought to the public. As it turned out, the study was effectively buried.

The trend of behavior genetics generally has been much more compatible with Gregg's than with Scott's assumptions. The direction of the field may be explained, at least in part, by the social agenda that informed decisions of the RF and other financial patrons in the field's early years. Most original investigators were funded by some combination of the RF, the AES, or smaller, right-wing foundations such as the McGregor and Pioneer Funds (as well as by private patrons, usually wealthy amateur eugenicists). The officers of these organizations were often linked by close personal relationships as well as by overlapping institutional memberships. Thus Frederick Osborn, director of the American Eugenics Society, was also a trustee of the Carnegie Corporation and president of the Population Council, which was founded in 1952 and fully funded by the Rockefeller family. There were of course differences in outlook and emphasis among these sponsors. But all could agree with the authors of a Population Council report that "social science research of the post-war period has been disproportionately devoted to studies of the effects of differences in the environment with no regard to

differences in the genetic material on which the environment act."[67] And they all sought to remedy this "imbalance."

The funding process itself, as we have seen in the case of the Jackson Laboratory, was highly personalized. When mistakes were made, they were thus difficult to reverse. However, patrons were usually able to identify scientists whose views were congruent with their own. The strong hereditarian thrust of contemporary behavior genetics should thus be no surprise.

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Notes

Citations to documents from the records of the Rockefeller Foundation, Rockefeller Archive Center, North Tarrytown, New York, provide Record Group, Series, Box, and Folder numbers.

1. Alan Gregg to C. C. Little, 16 March, 1945, RF 1.2, 200A, 134, 1190.

2. Robert E. Kohler, "A Policy for the Advancement of Science: The Rockefeller Foundation, 1924/1929," *Minerva*, 1978, *16:* 480–513. See also Raymond B. Fosdick, *The Story of the Rockefeller Foundation* (New York: Harper and Brothers, 1952), pp. 135–144.

3. Warren Weaver, "The Science of Man," 29 November, 1933, RF 3, 915, 1, 6. The phrase was Weaver's. The reorientation of the social sciences had begun even earlier. In 1922 Beardsley Ruml became director of the Laura Spelman Rockefeller Memorial (later merged with the RF). Under his direction, the memorial moved away from its practice of appropriating money to welfare organizations and toward development of a knowledge of human behavior "which in the hands of competent technicians may be expected to result in substantial social control." Ruml is quoted in Franz Samelson, "Organizing the Kingdom of Behavior," *Journal of the History of the Behavioral Sciences*, 1985, *21:* 33–47, on p. 39. The Institute of Human Relations at Yale was one product of this new commitment.

4. Staff Report, 14 March, 1933, RF 3, 904, 4, 25.

5. Roger L. Geiger, *To Advance Knowledge: The Growth of American Research Universities, 1900–1940* (New York: Oxford University Press, 1986), p. 166.

6. For example, see Kohler, "A Policy for the Advancement of Science," p. 496. Lily Kay presents an alternative view in *The Molecular Vision of Life: Cal Tech, the Rockefeller Foundation, and the Rise of the New Biology* (New York: Oxford University Press, 1991).

7. Gerald Jonas, *The Circuit Riders: Rockefeller Money and the Rise of Modern Science* (New York: W. W. Norton, 1989), p. 170.

8. Edwin Embree to Raymond Fosdick, 26 August, 1925, RF 3, 915, 4, 22.

9. Kohler, "A Policy for the Advancement of Science," p. 500.

10. Ibid., p. 501

11. Jonas, *The Circuit Riders*, p. 168.

12. Frank R. Lillie to Wickliffe Rose, 17 June, 1924, enclosed in letter of Lillie to Max Mason, 5 June, 1931, RF 1.1, 216D, 8, 104.

13. Lillie to Mason, 5 June, 1931, RF 1.1, 216D, 8, 104.

14. Barbara A. Kimmelman, "An Effort in Reductionist Sociobiology: The Rockefeller Foundation and Physiological Genetics, 1930–1942" (unpublished manuscript, 1981).

15. Robert E. Kohler, "The Management of Science: The Experience of Warren Weaver and the Rockefeller Foundation Programme in Molecular Biology," *Minerva*, 1976, *14:* 279–306, on p. 285.

16. Lillie (who died in 1947) was awarded $180,000 for the period 1938–1941. "Resolution" of 6 April, 1938, RF 1.1, 216D, 8, 103.

17. C. C. Little, nine-page enclosure in letter of 2 February, 1937 to Alan Gregg, on p. 5, RF 1.1, 200D, 143, 1774.

18. Warren Weaver, "Natural Sciences—Policy: Past Program and Proposed Future Program," extract from agenda for special meeting of trustees, 11 April, 1933, on pp. 79–80, RF 3, 915, 1, 6.

19. Warren Weaver, "Progress Report: The Natural Sciences," 1934, RF 3, 915, 1, 7.

20. Warren Weaver, "Progress Report: The NS Program in Experimental Biology," 16 May, 1936, p. 4, RF 3, 915, 1, 8.

21. Weaver's role in the development of molecular biology has been a subject of much debate. See Kohler, "The Management of Science," and the challenge by Pnina Abir-Am in "The Discourse of Physical Power and Biological Knowledge in the 1930s: A Reappraisal of the Rockefeller Foundation's 'Policy' in Molecular Biology," *Social Studies of Science*, 1982, *12:* 341–382. Unfortunately, as Theodore Brown has noted, recent scholarship on the RF has largely ignored Gregg, even though his program in psychobiology was a major focus of the foundation. See Theodore M. Brown, "Alan Gregg and the Rockefeller Foundation's Support of Franz Alexander's Psychosomatic Research," *Bulletin of the History of Medicine*, 1987, *61:* 155–182.

22. A more detailed discussion of Gregg's background and career can be found in Wilder Penfield's breathless biography, *The Difficult Art of Giving: The Epic of Alan Gregg* (Boston: Little, Brown, 1967) and Brown, "Alan Gregg."

23. Brown, "Alan Gregg," pp. 160–163.

24. Ibid., p. 156.

25. "President's Review," RF Annual Report, 1936, pp. 22–23.

26. Weaver, "The NS Program in Experimental Biology," pp. 23–25.

27. The report is quoted in Paul Weindling, "The Rockefeller Foundation and German Biomedical Sciences, 1920–40: From Educational Philanthropy to International Science Policy," in Nicolaas A. Rupke, ed., *Science, Politics and the Public Good: Essays in Honour of Margaret Gowing* (London: Macmillan, 1988), pp. 119–140, on p. 131.

28. Weindling, "The Rockefeller Foundation and German Biomedical Sciences," p. 133. On RF programs in Germany, see also Kristie Macrakis, "The Rockefeller Foundation and German Physics under National Socialism," *Minerva*, 1989, *27*: 33–57.

29. See memo of Allen Mawrer, principal of University College, 26 October, 1934, on "New Scheme of Research in Serological Genetics," RF 1.1, 401, 16, 220.

30. Weaver also supported serological research by A. H. Sturtevant at Cal Tech and Robert Irwin at the University of Wisconsin.

31. Garland E. Allen, "The Eugenics Record Office at Cold Spring Harbor, 1910–1940: An Essay in Institutional History," *Osiris*, 1986, *2*: 225–264.

32. Excerpt from Alan Gregg's diary, 22 June, 1944, RF 1.2, 200A, 133, 1189.

Benno Müller-Hill, *Murderous Science*, trans. George Fraser (New York: Oxford University Press, 1988), pp. 11, 28–29.

34. Herbert Eugene Walker, *Genetics: An Introduction to the Study of Heredity* (New York: Macmillan, 1913), p. 232.

35. For example, "From one thousand Roumanians today in Boston, at the present rate of breeding, will come a hundred thousand two hundred years hence to govern the fifty descendants of Harvard's sons" (William E. Castle, et al. *Heredity and Eugenics* [Chicago: University of Chicago Press, 1912], p. 309).

36. Frederick Osborn, "Heredity and Practical Eugenics Today," *Eugenical News*, 1948, *33*: 1–6, on pp. 5–6.

37. "Editorial" (by Osborn), *Eugenics Quarterly*, 1954, *1*: 2.

38. C. Nash Herndon, "Heredity Counseling," *Eugenics Quarterly*, 1955, *1*: 64–66, on p. 66.

39. Alan Gregg, diary entry, 17 November, 1945, RF 1, 401A, 16, 223.

40. Alfred H. Sturtevant, "Social Implications of the Genetics of Man," *Science*, 1954, *120*: 405–407, on p. 405.

41. Amy Sue Bix, "Pavlovian Science Comes to America: Experimental Research of W. Horsley Gantt at the Johns Hopkins University" (unpublished manuscript, 1988), p. 14.

42. Ibid., p. 23.

43. Ibid., p. 49.

44. Quoted in Jean Holstein, *The First Fifty Years at the Jackson Laboratory* (Bar Harbor: Jackson Laboratory, 1979), p. 11.

45. Alan Gregg to C. C. Little, 12 November, 1941, RF 1.1, 200D, 143, 1775.

46. Alan Gregg to E. B. Wilson, 30 December, 1943, RF 1.2, 200A, 133, 1189.

47. Alan Gregg to C. C. Little, 3 January, 1944, RF 1.1, 200, 133, 1189.

48. C. C. Little to Alan Gregg, 6 January, 1944, RF 1.2, 200A, 133, 1189.

49. Robert M. Yerkes to Alan Gregg, 31 March, 1944, RF 1.2, 200A, 133, 1189.

50. John Paul Scott, "Investigative Behavior: Toward a Science of Sociality," in D. A. Dewsbury, ed., *Leaders in the Study of Animal Behavior: Autobiographical Perspectives* (Lewisburg, Pa.: Bucknell University Press, 1985), pp. 388–429, on p. 410. Also personal interview with Scott, Bar Harbor, Maine, 27 August, 1988.

51. Scott, "Investigative Behavior," pp. 396–397.

52. Ibid., pp. 401–402.

53. Ibid., p. 404.

54. Ibid.

55. John Paul Scott and John L. Fuller, *Genetics and the Social Behavior of the Dog* (Chicago: University of Chicago Press, 1965), p. 4.

56. Scott, "Investigative Behavior," pp. 396–397.

57. John L. Fuller and William Thompson, *Behavior Genetics* (New York: Wiley, 1960).

58. A list of the forty participants is found in the "Minutes of the Conference on Genetics and Social Behavior," Roscoe B. Jackson Memorial Laboratory, Bar Harbor, Maine, 10–13 September, 1946. After the conference, Robert Morrison of the RF wrote to Scott suggesting that he try to develop "a strain of obviously schizophrenic dogs" in order to "provide a strong experimental argument for the hereditary nature of the human disease" (4 November, 1946, RF 1.2, 200A, 134, 1190).

59. Scott and Fuller made reciprocal crosses and then backcrossed the F1 males to their purebred mothers. They also crossed the same F1 males to their sisters to produce an F2. For further details, see discussions in Scott and Fuller, *Genetics and Social Behavior*, passim, and summary in Scott, "Investigative Behavior," p. 415.

60. Scott and Fuller, *Genetics and Social Behavior*, p. 367.

61. Scott, "Investigative Behavior," p. 416. See also Scott and Fuller, *Genetics and Social Behavior*, p. 388.

62. Scott and Fuller, *Genetics and Social Behavior*, p. 16.

63. Scott, "Investigative Behavior," p. 416.

64. Gregg memo of interview with Little, 17 April, 1944, RF 1.2, 200A, 133, 1189.

65. The first motion to appropriate money, on 19 January 1945, also reads: "Much advance must take place in mammalian genetics if we are to approach the study of human heredity wisely. Psychology and psychiatry ignore it and consequently exaggerate the role of experience and environment in the explanation of behavior. Educational policies reflect the same tendencies to disregard the importance of innate capacities or weaknesses." (RF 1.2, 200A, 133, 1189.) The five succeeding motions all include similar comments, as do the RF annual reports.

66. John P. Scott and John L. Fuller, "Heredity and the Social Behavior of Mammals," *The Roscoe B. Jackson Memorial Laboratory, 27th Annual Report, 1955–1956*, on p. 23.

67. "Development of Plans for Twin Study," Population Council, IV, 3B4.2, 39, 563, Rockefeller Archive Center.