AMERICAN SOCIOLOGICAL REVIEW

August 1966 Volume 31, No. 4

SOCIAL FACTORS IN THE ORIGINS OF A NEW SCIENCE: THE CASE OF PSYCHOLOGY *

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The uninterrupted growth of a scientific field depends upon the existence of a scientific community permanently devoting itself to the field. Therefore a new idea is not sufficient to start the take-off into sustained growth in a new field; a new role must be created as well. In scientific psychology, this occurred in the late nineteenth century in Germany. Using Germany as the positive case, and France, Britain and the United States as negative cases, it is shown that the new role resulted from academic career opportunities favoring the mobility of practitioners and students of physiology into other fields, and from the relatively low academic standing of speculative philosophy and its consequent receptivity to persons and ideas which promised to turn the study of the human mind into an experimental science.

THE PROBLEM

THE growth of scientific disciplines, as of many other phenomena, can be represented by an S-shaped curve.[1] First there is a long period, going back to prehistory, during which there are various ups and downs but no continuous growth; this is followed by a spurt of accelerated growth; eventually the development slows down and approaches a ceiling.[2] This typical pattern is obtained whether one uses as the index of growth the numbers of publications, discoveries, or people doing research in the subject; the pattern corresponds well with the intuitive picture one obtains from the histories of the different sciences.

The process, as presented in the accounts of scientific development, can be presented schematically as follows. Ideas beget ideas until the time is ripe for a new and coherent system of thought and research to arise. Thenceforth the system possesses a life of its own. It is identified as a new field of science, is eventually given a name of its own (such as chemistry or psychology), and grows rapidly into maturity. This still leaves open the question of beginnings. If the whole story consisted of ideas begetting ideas, then growth would have to start at an exponentially accelerating rate (to a point of saturation) right from the first relevant idea. Since this does not happen, it has to be assumed either that only a few ideas are capable of generating new ones—the rest simply being sterile—or that ideas are not self-generating, and, even if potentially fertile, have to be carried from person to person and implanted in some special way in order to give rise to new generation.

Common sense indicates that both state-

* This paper is partly based on an M.A. thesis by Randall Collins (University of California at Berkeley, 1965). The authors are indebted to Professors David Krech and Harold Wilensky for their comments and suggestions, and to the Comparative National Development Project of the Institute of International Studies of the University of California at Berkeley for financial support.

[1] Derek de Solla Price, *Little Science, Big Science*, New York: Columbia University Press, 1963, pp. 1–32; Gerald Holton, "Scientific Research and Scholarship: Notes Toward the Design of Proper Scales," *Daedalus*, 91 (Spring, 1962), pp. 362–99.

[2] Although this may be followed by escalation into further growth, it is unnecessary for the purpose of the present paper to consider this possibility.

ments are true. Not all original ideas are fertile, and some potentially fertile ideas are lost or left unused because they are not communicated effectively. Nevertheless histories of science have concentrated on the first type of explanation. If an idea has no historical consequences, the historian of ideas will take it for granted that something must have been at fault with the idea. Conversely, when an idea with a seemingly not-so-brilliant beginning proves capable of further growth, he will assume that it must have had hidden qualities which ensured its success. Obviously he will find no difficulty after the fact in demonstrating the correctness of his hunches.

In this paper, we shall pursue the other tack. Instead of trying to show what inherent qualities made one idea fertile and another infertile, we shall ask how it happened that at a certain point in time the transmission and diffusion of ideas relating to a given field became strikingly increased in effectiveness. Instead of contemplating the internal structure of intellectual mutations,[3] we shall concentrate on the environmental mechanisms which determine the selection of mutations. Specifically, we postulate that: (1) the ideas necessary for the creation of a new discipline are usually available over a relatively prolonged period of time and in several places;[4] (2) only a few of these potential beginnings lead to further growth; (3) such growth occurs where and when persons become interested in the new idea, not only as intellectual content but also as a potential means of establishing a new intellectual identity and particularly a new occupational role; and (4) the conditions under which such interest arises can be identified and used as the basis for eventually building a predictive theory.

THE CASE OF PSYCHOLOGY: THE TAKE-OFF INTO ACCELERATED GROWTH

The earliest beginnings of psychology reach back into prehistory. Explanations of human thought and behavior are inherent in every language; with the rise of philosophies, more abstract and systematic formulations came into being. Finally, in the nineteenth century, the methods of natural science were applied to the subject. Using publications in experimental and physiological psychology as an index of the growth of modern scientific psychology, we find that the acceleration started about 1870, and that the period of rapid growth was reached about 1890. (Table 1).[5]

The place where accelerated growth began can be ascertained from comparisons of the growth in different countries. The pattern is similar to that found in other nineteenth century sciences. The main development occurs in Germany, to be continued in the twentieth century in the United States, with a much more modest growth in Britain. For a while France also seems to develop strongly, but production there declines soon after the initial spurt around the turn of the century (Table 2.) Moreover, French development seems to have been isolated from the mainstream; it has been quoted in major textbooks less than its relative share in production of publications would indicate. (Table 3).

These are the data to be explained. Since the conditions under which something new is created are not necessarily the same as the conditions under which the innovation is effectively received somewhere else, we shall confine ourselves to the explanation of the take-off, and leave the analysis of the diffusion of the new field for another discussion.

PROCEDURE

Originally the subject matter of psychology was divided between speculative phi-

[3] This is not to say that such contemplation is necessarily useless. Its potential utility depends on finding identifiable characteristics which predict what is and what is not a "fertile" idea.

[4] This accords with the oft-noted phenomenon of multiple discoveries in science. Cf. Robert K. Merton, "Singletons and Multiples in Scientific Discovery: A Chapter in the Sociology of Science," *Proceedings of the American Philosophical Society,* 105 (1961), pp. 471–486.

[5] These publications do not represent the total number of reports of experimental and physiological researches in psychology, but rather review articles, books, and papers dealing with the theory and methodology of experimental and physiological psychology. Complete tables of research reports are not available for this period; however, this particular bibliography may be more useful for our purposes than they would have been. It represents a set of self-conscious summaries of scientific work in the field; therefore it indicates the rise of interest in scientific psychology better than would a collection of researches which may not at the time have been considered relevant to psychology.

TABLE 1. NUMBER OF PUBLICATIONS IN EXPERIMENTAL AND PHYSIOLOGICAL PSYCHOLOGY, BY NATIONALITY AND DECADE, 1797–1896

Decade	Nationality					
	German	French	British	American	Other	Total
1797–1806	1	1	..	..	..	2
1807–1816	2	1	..	..	..	3
1817–1826	1	..	3	..	..	4
1827–1836	4	3	2	..	..	9
1837–1846	11	4	2	..	1	18
1847–1856	15	2	6	1	..	24
1857–1866	16	8	7	..	3	34
1867–1876	38	11	15	1	4	69
1877–1886	57	22	17	9	12	117
1887–1896	84	50	13	78	21	246

Source: J. Mark Baldwin (ed.), *Dictionary of Philosophy and Psychology,* New York: Macmillan, 1905, vol. III, Part 2, pp. 950–64.

losophy and physiology. Towards 1880, specialized psychological publications came to constitute the bulk of the work in the field, and philosophical psychology was widely disparaged by the "new psychologists." [6] The acceleration of production was associated with a growing consciousness among these men of the existence of a distinct field of psychology, and of the need for distinguishing their work from traditional fields. It is usually assumed that the emergence of a new group devoting itself to a new specialty is an effect of intellectual growth. As knowledge in a field increases, no one is able to master all of it any more, and specialization is the necessary result. We shall try to show, however, that the new scientific identity may precede and indeed make possible the growth in scientific production. At least in the rise of the new psychology, social factors played an important role, independently of intellectual content.

The first step is to determine the persons who consciously identified themselves as practitioners of a new science investigating mental phenomena by means of empirical methods such as experimentation, systematical observation, and measurement, (irrespective of whether they called themselves "psychologists" or "experimental philosophers"). Operationally, there are three conditions for the existence of such a new scientific identity: (1) the person must do empirical work in the subject matter of psychology; (2) he must not have some

[6] Richard Müller-Freienfels, *Die Hauptrichtung der gegenwärtigen Psychologie,* Leipzig: Quelle & Meyer, 1929, pp. 3–6.

TABLE 2. ANNUAL AVERAGE NUMBER OF PUBLICATIONS IN PSYCHOLOGY, BY LANGUAGE, 1896–1955

Years	German	English			French	Other	Total
		Total	American	British			
1896–1900	764	745	....	...	709	270	2494
1901–1905	1119	747	....	...	660	210	2781
1906–1910	1508	941	....	...	478	158	3185
1911–1915	1356	1090	....	...	376	160	2982
1916–1920	386	1639	....	...	159	191	2395
1921–1925	1163	1850	....	...	326	315	3653
1926–1930	1761	2654	....	...	428	913	5951
1931–1935	1362	3371	....	...	472	975	6376
1936–1940	1160	....	3238	328	299	747	6330
1941–1945	216	....	3411	296	72	299	4465
1946–1950	203	....	4257	346	246	560	5662
1951–1955	459	....	5955	557	502	572	8385

Source: Samuel W. Fernberger, "Number of Psychological Publications in Different Languages," *American Journal of Psychology,* 30 (1917), 141–50; 39 (1926), 578–80; 49 (1936), 680–84; 59 (1946), 284–90; 69 (1956), 304–09.

TABLE 3. PER CENT DISTRIBUTION OF REFERENCES IN PSYCHOLOGY TEXTS BY LANGUAGE

Text	Language				
	Total	English	German	French	Other
Ladd, *Elements of Physiological Psychology,* 1887.	100.0 (420)	21.1	70.0	7.4	0.5
Ladd & Woodworth, 2nd edition, 1911.	100.0 (581)	45.6	47.0	5.2	2.2
Woodworth, *Experimental Psychology,* 1938.	100.0 (1735)	70.9	24.5	3.1	1.5
Woodworth & Schlosberg, 2nd edition, 1954.	100.0 (2359)	86.1	10.9	2.5	0.5

other clearly established scientific identity, such as physiologist; (3) he must be a part of an on-going group of scientific psychologists, rather than an isolated individual.

Taking these points in order: (1) The first group to be excluded are speculative philosophers such as Descartes, Locke, Hartley, Herbart, and even Lotze as well as various "social philosophers." However much they may have theorized about the use of empirical methods, they are not classified as scientific psychologists if they did not actually use such methods. (2) Also excluded are those natural scientists, principally physiologists, whose experiments can be retrospectively included in psychology, but whose identification was clearly with the natural sciences. Psychiatrists are also excluded: at the time in question, they belonged to a medical discipline which was quite independent of philosophy, and thus of psychology. Moreover, their theories were rather self-consciously based on the views of nineteenth-century medical science.[7]

(3) Finally, we must make an operational distinction among three categories of persons: *forerunners, founders,* and *followers.* The first two are distinguished by whether or not they had students who became psychologists. An example of a forerunner would be the scientific dilettante—such as Francis Galton. These men did not consider themselves psychologists, nor were they so identified by their contemporaries. Generally they remained isolated from any specific discipline until historians of the science—which was created by other forces—offered them a posthumous home.

Those who were not themselves the students of psychologists, but who trained their own disciplines as psychologists, are the *founders* of the new discipline of psychology. Their disciples are the *followers.* The latter two classes can be considered psychologists proper. What we have referred to as "discipleship"—the fact of having studied under a man, or having worked under him as a laboratory assistant—is, we believe, an adequate measure of the existence of a consciously self-perpetuating identity, a "movement" or discipline. The use of purely objective criteria in establishing such lines of descent has the disadvantage that we may misjudge the extent of actual influence and identification, but the overall picture should be accurate.

The names to be classified are taken from five histories of psychology, including ones written in each of the countries to be examined.[8] For Germany and the United States, all names between 1800 and 1910 were taken. Beyond the latter date, the numbers of psychologists in these countries become so great that the histories are necessarily selective; moveover, scientific psychology was well into its second and third generations in these countries by this point. For Britain and France, all names between 1800 and

[7] Gregory Zilboorg, *A History of Medical Psychology,* New York: Norton, 1941, pp. 400, 411–12, 434–35, 441. Breuer and Freud were developing a psychological psychiatry at the end of the nineteenth century, but there was no contact (except of the most negative kind) between Freudianism and German academic psychology for many decades thereafter.

[8] Germany: Müller-Freienfels, *op. cit.;* France: Fernand-Lucian Mueller, *Historie de la Psychologie,* Paris: Payot, 1960; Britain: John C. Flugel, *A Hundred Years of Psychology,* 2nd edition, London: Duckworth, 1951; United States: Edwin G. Boring, *A History of Experimental Psychology,* 2nd ed., New York: Appleton-Century-Crofts, 1950; Robert I. Watson, *The Great Psychologists,* Philadelphia: Lippincott, 1963. Russia has not been treated in this analysis. The number of its contributions to psychological literature until recent years has been very small; its great innovators, Sechenov, Pavlov and Bekhterev, were all physiologists and would therefore have been excluded from the population of psychologists. They provide good examples of persons whose work could be integrated into scientific psychology only because subsequent developments elsewhere created such a discipline.

1940 were taken, since the numbers of names involved were much smaller than for either Germany or the United States. Scientific psychology became established in Britain or France considerably later than in the other two countries.[9]

RESULTS

Figures 1–4 show the population of scientific psychologists for each country in the form of genealogical charts.[10] A great many names of physiologists and philosophers had to be excluded from the histories of German psychology, among them many of the most eminent men in those fields in the nineteenth century. In Germany our population includes 32 names, five of which have no predecessors on the chart (Figure 1). Two names do not appear in the figure. Gustav Fechner has all of the characteristics of an innovator save one: he gave rise to no personal school of followers, although, as will be seen, he influenced some of the founders. On balance, he was probably more of a forerunner than a founder, as one cannot say that his innovation of psychophysics would have been developed into a discipline of experimental psychology if an institutionally-based movement had not been founded subsequently.[11] Karl Groos appears rather late to be an indigenous developer, having habilitated in 1889, nine years after Ebbinghaus, who was the last of other self-starters. In any case, he cannot be considered a founder, as he gave rise to no following. This brings us down to five men who can be regarded as the founders of scientific psychology in Germany: Wilhelm Wundt, Franz Brentano, G. E. Müller, Carl Stumpf, and Hermann Ebbinghaus.

In Britain, the biologists C. Lloyd Morgan and George Romanes were excluded, as well as the statistician Karl Pearson. Francis Galton, who instigated psychological testing in Britain but whose scientific interests extended from geographical exploration to chemistry, photography, and statistics, and who left no school of psychologists to carry on is also omitted. This leaves 9 names in British psychology, virtually all of whom go back to the German innovators, Wundt and Müller (Figure 2). The exceptions are G. H. Thomson who is not shown in the figure, who took his degree at Strassburg (a German university at the time) in 1906; and W. H. R. Rivers, who studied with Ewald Hering, a physiologist closely identified with the "new psychology" in Germany. But by the 1890's, one could hardly study in Germany without becoming aware of the new developments, and Rivers cannot be called an originator of experimental methods in the field of psychology.

In France, the names of numerous psychiatrists and some physiologists and biologists were excluded, leaving 10 names (Figure 3). Two men comprising the Swiss school can be traced back to Wundt; one—Victor Henri—worked with Müller, although he had previously worked with Alfred Binet.

[9] Information about biographies and careers has been drawn from the five histories of psychology cited above (especially Boring) and from: Mollie D. Boring and Edwin G. Boring, "Masters and Pupils among American Psychologists," *American Journal of Psychology,* 61 (1948) 527–34; Carl Murchison (ed.), *A History of Psychology in Autobiography,* Vols. I–IV, Worcester, Massachusetts: Clark University Press, 1930–1952; Carl Murchison (ed.), *Psychological Register,* Vols. II and III, Worcester, Massachusetts: Clark University Press, 1929–1933; *Minerva: Jahrbuch der Gelehrten Welt,* Leipzig: 1892–. "Germany" is taken to include Austria and the German-speaking universities of Switzerland and Central Europe; "France" includes French-speaking Switzerland and Belgium.

[10] Clearly, those charts do not represent the total population of such psychologists for this period, and men may appear to have no psychological followers only because they are not listed in the texts from which the names are drawn. Nevertheless, we feel justified in using this form of measurement of the rise of a discipline, because the visibility of the men who form such a movement is an important factor in its existence.

[11] Fechner was a retired physicist who devoted many years to writing pantheistic, anti-materialistic philosophical works. His writings met with little success, due to the reaction against Idealism that had developed by the mid-nineteenth century. In 1850, he took up the physiologist E. H. Weber's experiments on touch and muscle sense, in an attempt to establish mathematical laws of perception. This research, however, was an integral part of Fechner's pantheistic system; the laws of psychophysics were intended to give a demonstrable proof to his belief that mind and matter were aspects of the same thing, and he went on to propose an explanation of the entire physical world as composed of souls related to each other by material bodies. Cf. Robert I. Watson, *The Great Psychologists,* Philadelphia: Lippincott, 1963, p. 215, and E. G. Boring, "Fechner: Inadvertent Founder of Psychophysics," in E. G. Boring, *History, Psychology, and Science: Selected Papers,* New York: Wiley, 1963, pp. 126–131.

The self-starters appear to be Théodule-Armand Ribot, Henri Beaunis, Pierre Janet. Ribot cannot be considered a major innovator, as he made his reputation by publicizing German psychology, and was given the first chair of Experimental Psychology in France in 1889 as a result; he remained by and large a speculative philosopher. Beaunis was a physiologist who set up the first psychological laboratory in France in the same year; again, it is difficult to assign Beaunis a role as an independent innovator since a rash of laboratory-foundings had already been going on in Germany and the United States for a decade. Janet was an M.D. who succeeded to Ribot's chair in 1902 at the College de France; he was primarily a psychiatrist, however, and maintained a private practice throughout his career.

In France, then, there appear to be a number of figures without direct antecedants among the German psychologists. Some of them were obviously influenced by the Germans, others had ideas of their own. Had ideas been enough, the French school might have become an effective rival to the German school. But the French development from the German in that there was no continuity in France. Ribot and Beaunis each had but one important follower and Janet had two. This relative lack of descent resulted from a lack of interest in creating new roles for the new ideas. As will be shown later, those working in the new field were content to remain philosophers, psychiatrists, or broad-gauged scientific intellectuals, often interested in finding a scientific solution to some practical problem, like

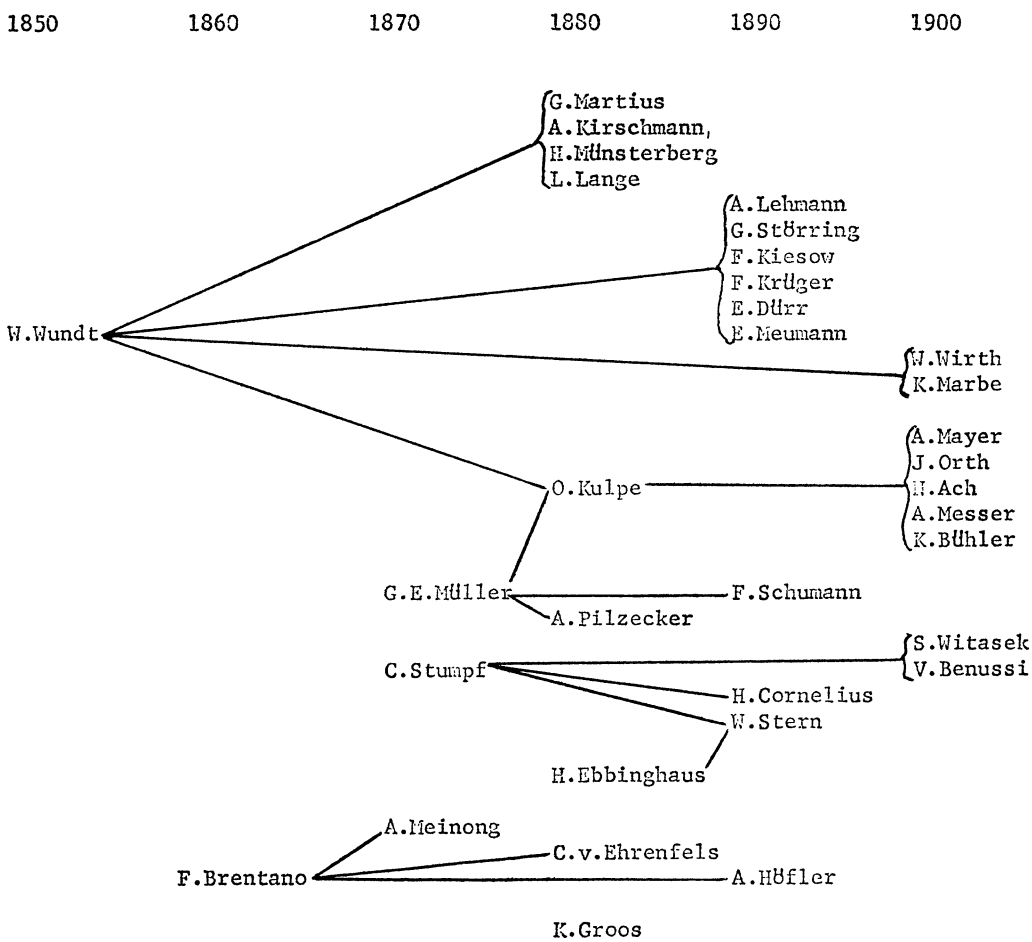

FIGURE 1. FOUNDERS AND FOLLOWERS AMONG GERMAN EXPERIMENTAL PSYCHOLOGISTS, BY DECADE OF HABILITATION, 1850–1909.

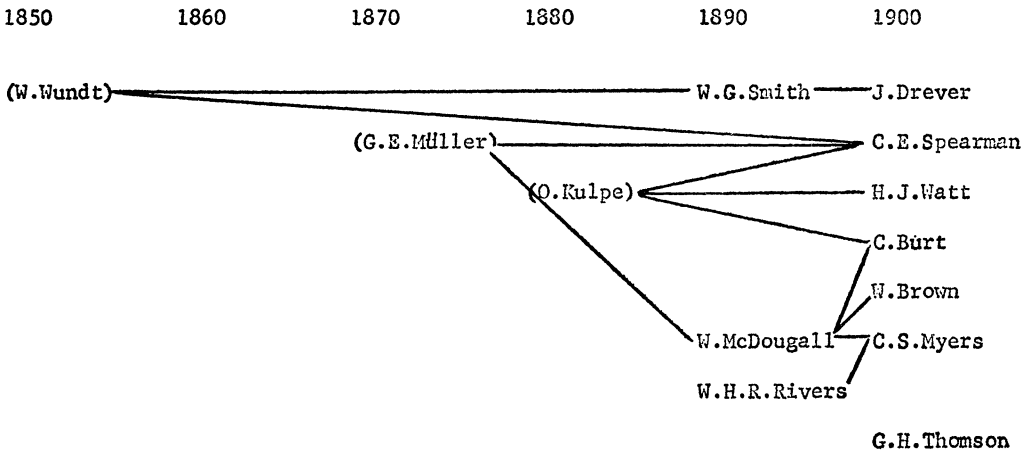

FIGURE 2. FOUNDERS AND FOLLOWERS AMONG BRITISH EXPERIMENTAL PSYCHOLOGISTS, BY DECADE OF HIGHEST DEGREE, 1850–1909.

Binet. They did not attempt, therefore, to create a coherent and systematic "paradigm," and to transmit it to the next generation.[12]

[12] Cf. Thomas S. Kuhn, *The Structure of Scientific Revolutions,* Chicago: University of Chicago Press, 1963, for a discussion of how sciences are able to make cumulative advances because they are integrated around a particular "paradigm" or model of scientific reality, with its implied methodology and research directions. Of course, it can be argued that psychology even today still lacks overwhelming consensus around a central, reality-defining theory of the sort that Kuhn means by a "paradigm," and that the term should be used only in such fields as physics which do have such a theory. We have used the term here more broadly, to refer to the necessity of a new discipline to have at least minimal consensus on the boundaries of the subject matter upon which its practitioners will focus their attention, and on an acceptable range of research methods.

Finally, in the United States virtually all excluded figures were speculative philosophers, among them George T. Ladd and John Dewey. Very few American physiologists or other natural scientists appeared in the histories. The remaining 37 figures, presented in Figure 4, were overwhelmingly influenced by the German innovators, particularly Wundt. Only one name lacks an antecedant: William James, who began as a physiologist and set up a small demonstration laboratory at Harvard in 1875 which he later claimed was the first psychological laboratory in the world. He became Professor of Philosophy at Harvard in 1885, and had his title changed to Professor of Psychology only in 1889. James is the closest America comes to an indigenous development in psychology, but his work was largely an exposi-

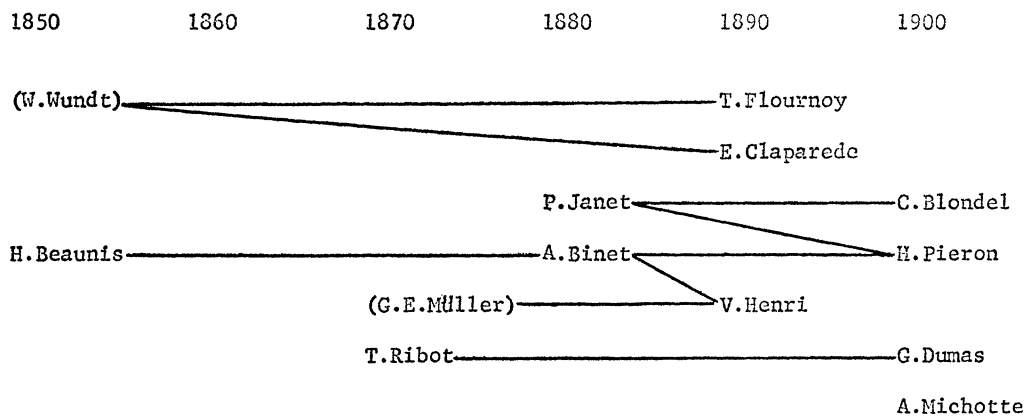

FIGURE 3. FOUNDERS AND FOLLOWERS AMONG FRENCH EXPERIMENTAL PSYCHOLOGISTS, BY DECADE OF HIGHEST DEGREE, 1850–1909.

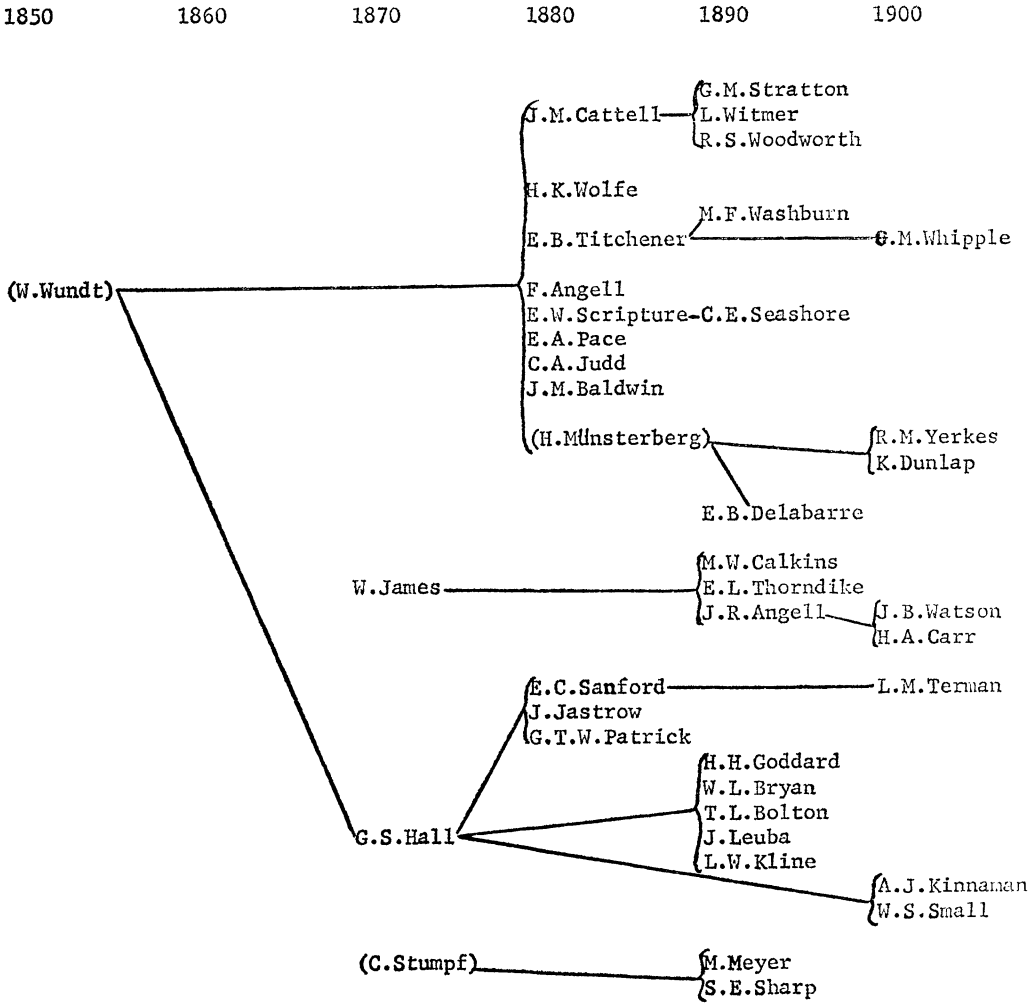

FIGURE 4. FOUNDERS AND FOLLOWERS AMONG AMERICAN EXPERIMENTAL PSYCHOLOGISTS, BY DECADE OF HIGHEST DEGREE, 1850–1909.

tion of European ideas and discoveries (he had visited Germany in 1869, while preparing to teach physiology); he himself became increasingly interested in philosophy during the time that experimental psychology was developing in America. (All of James' major philosophical works date from 1897, when his title was changed back to professor of Philosophy.) The first generation of experimentalists were almost entirely students of Wundt, including G. Stanley Hall, who did not take his degree with Wundt. Hall in 1881 set up the first functioning psychological laboratory in the U. S. at Johns Hopkins after returning from a visit with the German psychologists, and the lines to succeeding generations can be clearly traced. Without any important contributions from American philosophers or natural scientists, experimental psychology suddenly sprang up in the United States, transplanted from Germany.

Germany, then, is where the crucial conditions for the innovation of scientific psychology are to be sought. Ideas which could have given rise to a cumulative tradition could be found outside of Germany. In fact, towards the end of the nineteenth century, France nearly rivalled Germany as a center for such ideas. But as shown in Table 2, French production declined rapidly after a momentary peak around 1900, while German, American, and, to a much lesser extent, British work continued to grow. Fig-

ures 1–4 indicate that only in Germany had there developed an autonomous network for the regular transmission and reception of the new ideas. Subsequently the U. S. and later Britain linked up with this network, and the U. S. eventually became its center. France only partially linked up with it and it did not develop a network of its own. In the absence of such a network, innovations remained isolated events; only the existence of networks could make them into a cumulative process.[13]

We shall not here follow the entire story of the creation of communication networks and their diffusion from country to country, but shall confine ourselves to the original establishment of the German network. For this purpose, all the other countries will be treated as negative instances, with Germany as the sole positive case. The question to be answered is: Why did an effective network for the communication of these new ideas develop only in Germany?

ROLE-HYBRIDIZATION

The answer is that the conditions for the establishment of a new professional role variety, committed to the new field, existed only in Germany. Ideas which are not cultivated by people whose regular jobs are to cultivate them are like souls hovering in a mythological limbo before entering a body. They can light upon the dreams or the imagination of one person here and another one there, of someone who lives today or of someone else who will be born in a thousand years. If, however, ideas become the end-products of scientific roles, they can be likened to genes which are transmitted from generation to generation through a reliable and natural process; under normal conditions, they will not only survive but increase.

There are several ways in which new scientific role varieties arise. The present instance is a case of role-hybridization: the individual moving from one role to another, such as from one profession or academic field to another, may be placed at least momentarily in a position of role conflict.[14] This conflict can be resolved by giving up the attitudes and behaviors appropriate to the old role and adopting those of the new role; in this case, identification with the old reference group must be withdrawn. However, the individual may be unwilling to give up his identification with his old reference group, as it may carry higher status (intellectual as well as perhaps social) than his new group. In this case, he may attempt to resolve the conflict by innovating, i.e. fitting the methods and techniques of the old role to the materials of the new one, with the deliberate purpose of creating a new role.

Examples of scientific roles created by this process are psychoanalysis, which was created by a man who moved from the prestigious profession of scientific research to the relatively lower-status occupation of German medical practice; Freud attempted to maintain his status by trying to raise medical practice into a form of scientific research, and as a result created psychoanalysis. Similarly, Pasteur gave rise to bacteriology by maintaining his theoretical perspectives after

[13] A further indication of the weakness of the French system is the relatively greater mortality of French psychological journals. Between 1850 and 1950, 70 per cent of the psychological journals begun in France had ceased, as compared to 50 per cent for the United States, 51 per cent for Germany (before 1934, excluding the many stoppages during the Nazi era), and 21 per cent for Britain. Cf. Robert S. Daniel and Chauncey M. Louttit, *Professional Problems in Psychology*, New York: Prentice-Hall, 1953, pp. 25, 358–74.

[14] Joseph Ben-David, "Roles and Innovations in Medicine," *American Journal of Sociology,* 65 (1960), pp. 557–68. John T. Gullahorn and Jeanne E. Gullahorn, "Role Conflict and its Resolution," *Sociological Quarterly,* 4 (1963), pp. 32–48, have distinguished between two kinds of role conflict: "status-produced role conflict," in which the occupant of a single status position is subjected to conflicting expectations by the different persons with whom he deals, and "contingent role conflict," in which the conflicts arise from the simultaneous occupancy of two statuses. Most of the discussions in the literature have dealt with the first variety, e.g. Robert K. Merton, "The Role-Set: Problems in Sociological Theory," *British Journal of Sociology,* 8 (1957), pp. 106–120; and Neal Gross, Ward S. Mason, and Alexander W. MacEachern, *Explorations in Role Analysis,* New York: Wiley & Sons, 1958. We are distinguishing a third kind of role conflict, resulting from mobility rather than from the "static" situations indicated above. See Peter M. Blau, "Social Mobility and Interpersonal Relations," *American Sociological Review,* 21 (1956), pp. 290–95. For a discussion of why scientists would tend to identify with a traditional discipline rather than with an emerging specialty of lower prestige, see Warren O. Hagstrom, *The Scientific Community,* New York: Basic Books, 1965, pp. 53, 209.

moving into research on wine fermentation, and elaborated his discovery into a new specialty.

Mobility of scholars from one field to another will occur when the chances of success (i.e., getting recognition, gaining a full chair at a relatively early age, making an outstanding contribution) in one discipline are poor, often as a result of overcrowding in a field in which the number of positions is stable. In such cases, many scholars will be likely to move into any related fields in which the conditions of competition are better. In some cases, this will mean that they move into a field with a standing relatively lower than their original field.[15] This creates the conditions for role conflict. Of course, not everyone placed in such a position will choose to or be able to innovate a new role, nor is it possible to predict exactly which individuals will do so. It is possible, however, to say that the chances of such a major innovation occurring in a discipline into which there is mobility from a higher-status discipline are considerably greater than in a discipline into which there is no such mobility, or which stands higher in status than the discipline from which mobility takes place. For example, if physiology has higher standing in an academic system than philosophy, but competitive conditions are better in the latter than in the former, one might expect a role-hybridization in which physiological methods will be applied to the material of philosophy (at their most adjacent point, psychology) in order to differentiate the innovator from the more traditional practitioners of the less respected discipline. This would not be expected if philosophy's status were equal or higher, or if the competitive conditions in philosophy were equal or worse than those in physiology.

Moreover, since a major academic innovation has a chance of success only if it can attract a sizable following, it is usually not enough (except perhaps in cases of striking utility, such as bacteriology), that an individual innovator be placed in a situation of role conflict. The conditions have to be general so as to ensure a widespread response to the innovation. The motivation of the man who merely joins such a movement is quite similar to that of the man who begins it—moving into a discipline of lower standing than his old one, he is likely to welcome the opportunity to raise his status through adopting the innovation. Even more importantly, the existence of such relationships between disciplines may have a vicarious effect upon individuals within the system who do not personally move from the high-status discipline to the low-status discipline. For example, the younger men in the low-status field may attempt to upgrade themselves by borrowing the methods of a high-status field. The simplest way to upgrade themselves would be to move to the other field, but they are restrained from doing this by the differences in competitive conditions. If they do not make the innovation themselves, they may be very receptive to an innovation by a migrating scientist. Even young scholars who have not yet chosen a field, knowing the relative prestige and conditions of competition in the several fields, will be attracted to the new hybridized role.

It is important to distinguish role-hybridization from what might be termed "idea-hybridization," the combination of ideas taken from different fields into a new intellectual synthesis. The latter does not attempt to bring about a new academic or professional role, nor does it generally give rise to a coherent and sustained movement with a permanent tradition.

Antecedents of modern psychology as far back as Descartes had discussed psychological functioning in a physiological perspective, but without giving rise to any movement to extend these ideas as other sciences were doing with their respective materials. Similar connections were made by the British associationists, from John Locke and David Hartley up to Alexander Bain, James Ward, and James Sully at the end of the 19th century, but without giving any indication that a continuous scientific tradition

[15] For the scholar or scientist, this is not simply a matter of social status or prestige, but rather of the effectiveness or ability of the field to make progress as judged by its own intellectual standards. Cf. Hagstrom, *op. cit.*, pp. 9–104, for a theoretical exposition of science as a form of social organization in which competition for recognition by the colleague group is a prime mechanism of control; see also pp. 208–220 for a general discussion of disciplinary differentiation.

would ever result from these theories. In Germany, Herbart and Lotze certainly fall into this category, along with Fechner, who introduced experimental methods into philosophical psychology in the 1850's with his psychophysics, but who did not thereby create any movement to reform the role of the psychologist-philosopher. Galton in England, and in France, such men as Ribot, Beaunis, and Binet must be considered more "idea-hybrids" than "role-hybrids"; rather than creating a new role, they merely added another facet to the established role of the multi-purpose intellectual such as had existed in these countries since the seventeenth century. Finally, William James in the United States would fall into the category of an "idea-hybrid," particularly since he finally decided on the traditional role of philosopher rather than the new role of scientific psychologist.

THE POSITIVE CASE

In the German universities of the 19th century, physiology was a highly productive, expanding science. One of its greatest periods of productivity took place between 1850 and 1870, when most of the chairs of physiology were first split off from anatomy. Fifteen chairs were created between 1850 and 1864. After that date, the field rapidly reached a limit of approximately one chair per university in a system comprising 19 universities before 1870 and 20 after 1870.[16] Table 4 shows that physiology, with approximately half as many chairs as philosophy, added only two full chairs from 1873–1910, whereas philosophy, already the largest field in the universities, added eight. The number of Extraordinary Professors and Privatdozents in physiology grew much more rapidly during this period than in philosophy. But these were poorly paid and largely honorific positions; their number indicates something of the competitive pressures in these fields for the truly desirable positions, the full professorships. Advancement was particularly difficult in physiology, since most of its full chairs, having been created at about the same time, were filled with men of about the same age who held them for decades.[17] Table 5 shows that in the 1850's, the chances of becoming a full professor were better for those habilitating in the medical sciences than in the philosophical disciplines. In the next decade, however, the situation was reversed and the relative competitive situation within the medical sciences steadily wors-

[16] Awraham Zloczower, *Career Opportunities and the Growth of Scientific Discovery in Nineteenth Century Germany with Special Reference to Physiology,* unpublished M.A. thesis, Department of Sociology, Hebrew University, 1960.

[17] *Ibid.*

TABLE 4. NUMBER OF ACADEMIC POSITIONS IN PHILOSOPHY AND PHYSIOLOGY IN THE GERMAN UNIVERSITY SYSTEM, 1864–1938

Field and Academic Position	1864	1873	1880	1890	1900	1910	1920	1931	1938
Philosophy									
Ordinary Professor	36	40	43	44	42	48	56	56	36
Extraordinary Professor	21	16	12	14	14	23	30	51	34
Dozents	23	21	18	19	25	43	45	32	21
Total	81	79	75	81	85	117	140	163	117
Physiology									
Ordinary Professor	15	19	20	20	20	21	24	27	21
Extraordinary Professor	3	3	4	66	9	12	15	24	18
Dozents	9	1	2	7	20	27	22	23	15
Total	27	23	27	33	49	61	66	80	67

Note: In the German university system, the rank of Ordinary Professor is equivalent to Full Professor and Extraordinary Professor to Associate Professor. Dozents are private lecturers.

Source: Christian von Ferber, *Die Entwicklung des Lehrkorpers der deutschen Universitaten und Hochschulen,* 1864–1954, vol. III in Helmut Plessner (ed.), *Untersuchungen zur Lage der deutschen Hochschullehrer,* Gottingen: Van den Hoeck, 1953–56, pp. 204, 207.

TABLE 5. HIGHEST RANK REACHED BY SCHOLARS IN THE GERMAN UNIVERSITY SYSTEM WHO HABILITATED IN THE MEDICAL FACULTY AND PHILOSOPHICAL FACULTY (NATURAL SCIENCES EXCLUDED), 1850–1909

Year and Faculty	Rank				Per cent remaining dozents
	Ordinary Professors	Extra-ordinary Professors	Privat-dozents	Total	
1850–59					
Medicine	57	19	15	91	16.5
Philosophy	53	13	15	83	18.1
1860–69					
Medicine	72	44	37	153	24.2
Philosophy	68	24	22	114	19.3
1870–79					
Medicine	94	74	53	221	24.0
Philosophy	138	24	26	188	13.8
1880–89					
Medicine	89	59	64	212	30.2
Philosophy	118	25	36	179	20.1
1890–99					
Medicine	131	57	138	326	42.3
Philosophy	162	33	66	261	25.3
1900–09					
Medicine	184	48	249	481	51.8
Philosophy	142	25	75	242	31.0

Source: von Ferber, *op. cit.*, p. 81.

ened through the rest of the century. Clearly, from about 1860 on, philosophy offered much more favorable competitive conditions than did physiology. The first condition for the occurrence of role-hybridization was thus present.

The second condition was provided by the trend of the prestige conflict that raged between philosophy and the natural sciences throughout the nineteenth century in Germany. Before 1830, the great systems of Idealism claimed for philosophy the position of a super-science, deriving by speculation all that might be painstakingly discovered by empirical methods. But these pretensions were shattered by the rapidly expanding natural sciences, led first by the chemists, then by the physiologists. Paulsen notes the contempt in which speculative philosophy came to be held after the rise of the sciences in the 1830's, a contempt which was receding only at the end of the century.[18] Hermann von Helmholtz, the physicist and physiologist, was the leading propagandist for the scientific attack on philosophical speculation; in his student days in 1845 in Berlin, he banded together with a group of young scientists (including Emil Du Bois-Reymond, Ernst Brucke, and Carl Ludwig), who swore to uphold the principle: "No other forces than common physical chemical ones are active in the organism." [19] By the 1860's, the scientists were near to extinguishing the academic reputation of philosophy and its "super-science" pretensions.[20]

Wundt began his career as a physiologist in 1857, at the height of the competition for the new chairs being created in physiology. He remained a Dozent for 17 years, however, and after being passed over for the chair of physiology at Heidelberg in 1871, made the transition to philosophy.[21] This transition was made in 1874 with the chair at the University of Zurich, which served as something of a "waiting-room" for appointments to one of the great universities in Germany proper. On the strength of his *Physiological Psychology* in that year, he won a first-class chair of philosophy at Leipzig in 1875.

Before Wundt began to take philosophy

[18] Friedrich Paulsen, *The German Universities and University Study*, New York: Longmans Green, 1906.

[19] Edwin G. Boring, *op. cit.*, p. 708.

[20] G. Stanley Hall, *Founders of Modern Psychology*, New York: Appleton, 1912, p. 138.

[21] Edwin G. Boring, *op. cit.*, p. 319.

as a second reference group, he was doing the same kind of things that Helmholtz, Hering, Frans Donders, and many other physiologists were doing—experimenting on the functions of the sense-organs and the nervous system, and occasionally pointing out that their work made speculative philosophy a superfluous anachronism. Wundt had once been an assistant to Helmholtz, the leader of the anti-philosophical movement; Wundt's move into philosophy must have been an acute identity crisis for him, which could be resolved only by innovating a new philosophical method.[22] Using Fechner's empirical methods of studying perception, Wundt proposed to build metaphysics on a solid basis, thus making philosophy a science.[23] To preserve his scientific status, he was forced not only to carry out a revolution in philosophy by replacing logical speculation with empirical research, but also to widely advertise the fact that he was in a different kind of enterprise than the traditional philosophers.

Brentano, Stumpf, Müller, and Ebbinghaus were all philosophers who became interested in using empirical methods in their field. Apparently, they were aware of the onslaught physiology was making into the territory of philosophy; rather than accept its deteriorating position, they in effect "went over to the enemy." It is known that Stumpf met Fechner and E. H. Weber in his days as a Dozent; [24] Müller also corresponded with Fechner; [25] and Ebbinhaus apparently decided to re-enter the academic world after accidentally encountering a copy of Fechner's *Elements*.[26] Brentano, although he makes reference to Helmholtz, Fechner, and Wundt in his first major work, *Psychology from an Empirical Standpoint* (1874), was considerably less influenced by them than were the others. He also remained the least experimental of this group of founders. Wundt is undoubtedly the central figure. He had the largest following and he articulated the ideology of the "philosophical revolution" most clearly. The others, originally philosophers, put the position less strongly and had smaller personal followings. Yet they were role-hybrids to some extent, as clearly appears when one compares them with Fechner. The latter had the decisive idea, but was content to write about it and submit it to what Derek de Solla Price calls "the general archives of science." The philosophers, however, influenced by the example of Wundt, used it for the creation of a new role variety.

[22] Helmholtz may well have seen it as a kind of treason; there are reports that it was Helmholtz's antagonism to his former assistant that blocked the appointment of the latter to Berlin in 1894. Cf. *Ibid.*, p. 389.

[23] Hall, *op. cit.*, pp. 323–326.

[24] Edwin G. Boring, *op. cit.*, p. 363.

[25] *Ibid.*, p. 374.

[26] *Ibid.*, p. 387.

THE NEGATIVE CASES

In France, there was no innovation of using experimental methods in philosophy. There was heavy competition in the French academic system for positions in all the natural sciences; the physiologists were fairly hard-pressed, having fewer than one chair per university even at the turn of the century (Table 6). The number of available positions in philosophy was a little better. However, the relative situation was nothing like in Germany, where physiology had been filling up for several decades, whereas in France it was still expanding into all of the universities for the first time.

TABLE 6. NUMBER OF ACADEMIC POSITIONS IN PHILOSOPHY AND PHYSIOLOGY IN THE FRENCH UNIVERSITY SYSTEM, 1892–1923

	Philosophy		Physiology		
Year	Full chairs	Total	Full chairs	Total	Number of Universities[a]
1892	17	27	10	17	15
1900	20	28	12	20	15
1910	22	30	14	27	15
1923	22	*	17	*	16

[a] Includes College de France.

* Figures on positions below the level of full professor are not available for 1923.

Source: *Minerva, Jahrbuch der Gelehrten Welt,* 2 (1892), 10 (1900), 20 (1910), 27 (1923).

Besides, in France a central intellectual elite existed whose status was dependent on a diffuse evaluation of excellence rather than on regular university appointments and specialized attainment.[27] The lines of demarca-

[27] Joseph Ben-David and Awraham Zloczower, "Universities and Academic Systems in Modern So-

tion between disciplines were too amorphous to mean anything for a man like Binet, who could afford to dabble in law, entomology, psychiatry, experimental psychology, and educational testing. He could expect that some kind of facilities would be created for his patricular needs, and that his achievements would be recognized without the need for justifying them in the terms of a specific academic discipline.

Existing positions allowed a broad range of possible activities for their holders; Lucien Levy-Bruhl, the anthropologist, for example, held a chair of philosophy; Emile Durkheim, the sociologist, held a chair of education, and the few chairs of experimental psychology were likely to be turned over to men who were primarily psychiatrists such as Pierre Janet or Charles Blondel. The College de France, the most prestigious institution in France, rewarded unique individual accomplishments, but did not provide much opportunity for those following an established career, nor did it allow the training of "disciples," since its positions were for research rather than teaching. Ribot, by proselytizing German psychology, could have a new chair in Experimental Psychology established for himself at the College de France, but this personal recognition probably prevented him from developing a school of followers. The purely individual basis of recognition is indicated by the fact that Henri Pieron could have a new chair created for himself at the College de France (in the Physiology of Sensation) because the Professor of Archeology died without a suitably eminent successor.[28]

Unlike in the German system, disciplines were not differentiated sharply enough to create serious role conflicts among men with ideas. The elite comprised a single reference group of relatively non-specialized intellectuals and "philosophers" in the old eighteenth century tradition, and prestige adhered to the individual, not to the discipline. The French system, in short, was suited to picking up intellectual innovations by specific individuals, but was not at all suited for giving rise to movements attempting to create a new discipline.

The same conditions which prevented the development of a reference group conflict in France existed to an even greater extent in Britain. The relative number of chairs in philosophy and physiology was similar to that in France (Table 7). Both were about one per university, with chairs in philosophy in a slight lead over those in physiology, but with the latter expanding. The necessity of gaining an academic position was even less important than in France. In the latter country, one eventually had to obtain some kind of official position. In England, even this was unnecessary.

TABLE 7. NUMBER OF ACADEMIC POSITIONS IN PHILOSOPHY AND PHYSIOLOGY IN THE BRITISH UNIVERSITY SYSTEM, 1892–1923

Year	Philosophy		Physiology		Number of Universities
	Full chairs	Total	Full chairs	Total	
1892	13	15	9	20	10
1900	16	20	12	21	11
1910	19	38	14	29	16
1923	22	*	16	*	16

* Figures on positions below the level of full professor are not available for 1923.

Source: *Minerva, Jahrbuch der Gelehrten Welt,* 2 (1892), 10 (1900), 20 (1910), 27 (1923).

Before 1832, there were only two universities in all England and four in Scotland, and they were little more than an upper-class intellectual backwater. Four provincial universities were founded throughout the remainder of the century and another half dozen in the first decade of the twentieth century. Under the threat of being left behind by these technologically-minded, "lower-class" universities, Oxford and Cambridge began to take in the new sciences, and in the process, to recover intellectual as well as merely social pre-eminence.[29]

This process was still going on in the late nineteenth century; both philosophy and physiology were still centered to a considerable extent outside of the British universities.[30] From the point of view of the physi-

cieties," *European Journal of Sociology,* 3 (1962), pp. 45–85.

[28] Henri Pieron, "Autobiography," in Carl Murchison (ed.), *A History of Psychology in Autobiography,* vol. IV, Worcester, Mass.: Clark University Press, 1952.

[29] Walter H. B. Armytage, *Civic Universities,* London: Ernest Benn, 1955, pp. 178, 206.

[30] Both Herbert Spencer and J. S. Mill, for ex-

ologist fighting for entrance into the conservative strongholds, the academic philosophy taught there must have seemed a somewhat outdated and unduly privileged field. But the mobility factor was missing; it was still possible to attain the highest prestige in philosophy or in physiology outside of the universities. This non-university tradition provided a safety-valve which let off the pressure which might have led to the innovation of a new psychology.

In the United States as well, an indigenous innovation of experimental psychology failed to appear; however, a large and successful movement of followers of the German psychology did spring up in the 1880's, a full decade or two before such movements (on a smaller scale) appeared in France and Britain. Before this period, there had been a very large number of small colleges in the country.[31] In these colleges, psychology was a branch of philosophy of the eighteenth-century Scottish variety, with heavily religious overtones. It was taught by the college presidents, 90 per cent of whom were clergymen.[32] Philosophy occupied the same dominant position as in Germany in the early part of the century, but in other respects the colleges resembled the philosophical faculties (the lower, "undergraduate" section) of the German universities before von Humboldt's reforms in 1810. Learning was by rote, salaries were low, and there were no facilities for research. Teaching positions were merely sinecures for unsuccessful clergymen.[33] Under these conditions, there could be no movements to innovate new disciplines: there were no positions worth competing for, the institutions were too small for specialization, and research was not a function of the academic community at all. A vigorous movement in experimental psychology, clearly derivative of the German movements, grew up only after the foundings of the first graduate schools beginning in 1876.

ample, held no academic positions. Physiological research was largely carried on by medical practitioners in the independent hospitals. Cf. Abraham Flexner, *Medical Education: A Comparative Study,* New York: Macmillan, 1925.

[31] There were 182 colleges in 1861, averaging six faculty members each. Cf. Richard Hofstadter and Wolfgang Metzger, *The Development of Academic Freedom in The United States,* New York: Columbia University Press, 1955, pp. 211, 233.

[32] That is, the "faculty psychology" of Thomas Reid, Dugald Stewart and Thomas Brown; for the role of the president, see Hofstadter and Metzger, *op. cit.,* p. 297.

[33] Bernard Berelson, *Graduate Education in the United States,* New York: McGraw-Hill, 1960, p. 14.

SUMMARY

The innovation of experimental psychology was brought about by the mechanism of role-hybridization. Excluding the independently originated practical traditions in Britain and France which only later became attached to the movement in experimental psychology, this innovation took place only in Germany. Three factors were required: (a) an academic rather than an amateur role for both philosophers and physiologists; (b) a better competitive situation in philosophy than in physiology encouraging the mobility of men and methods into philosophy; (c) an academic standing of philosophy below that of physiology, requiring the physiologist to maintain his scientific standing by applying his empirical methods to the materials of philosophy.

Germany had all three factors. France had a measure of the first. All the persons involved eventually acquired full-time scientific appointments, but their careers had often started outside the academic framework, and their official positions were little standardized. The second factor was present to an insignificant degree, and the latter not at all, as prestige was attached to the individual and the formal honors he received rather than to the discipline. Britain was similar to France concerning the last two factors and the first was present to an even more limited exent than in France, since the amateur pattern still prevailed widely among philosophers and physiologists. The United States before 1880 lacked even the rudiments of an academic system in which these factors could operate.

This explains why the take-off occurred in Germany. The reason France never linked up with the mainstream of the development while the United States, and eventually Britain, did, remains to be investigated.