There is an important sense in which questions about modern psychology's social origins are fundamental to any consideration of the social context of specific psychological ideas during the present century. For this is a period during which the creation and dissemination of psychological ideas becomes decisively influenced by the existence of an increasingly powerful professional-academic group of "psychologists" who claim a monopoly on the production and reproduction of valid psychological knowledge. Psychological ideas, of course, had been in existence long before the present century; what is changed is the immediate social context in which they are developed and exchanged. The existence of the new, increasingly well-organized professional group provides the ground on which particular ideas, methods, and systems are projected as figures whose appearance unmistakably depends on the figure-ground relationship.

Before the present century psychological ideas were widely produced and exchanged among philosophers, medical men, political econo-
mists, historians, artists, and others. What is quite new in more recent history is the appearance of groups of specialists making increasingly successful claims to the monopoly of psychological truth. The members of these groups set themselves up and are gradually accepted as the arbiters of what does and what does not constitute valid psychological knowledge. To be taken seriously, psychological ideas must now pass through the prism formed by the normative and institutional framework of the community of recognized specialists.

It is this development, rather than the use of a new methodology, that defines the nature of the famous transition from psychology's long past to its short history. Techniques of experimentation and quantification are potential tools devoid of much historical significance in themselves; their real significance derives from the manner of their use and from who uses them for what purpose. Empirical techniques were applied to psychological problems quite extensively in the nineteenth century by professional philosophers, natural scientists, physicians, and amateurs. The crucial changes did not come until the application of these techniques was used to legitimize the claims to a monopoly of valid psychological knowledge by a self-conscious and organized community of specialists.

From this point on the relationship between psychological knowledge and its social context becomes complicated by the appearance of a new level constituted by the group of professionals, its interests, institutions, and culture. The reciprocal influence between psychological knowledge and interests and cognitive structures in the wider society becomes mediated by the community of specialists. It now becomes necessary to take into account the role played by members of this community in the wider society and the effects this has on the internal life of the community.

How then did this new community come into being? This is the question that requires an answer if we are to avoid taking the group's contemporary norms as given but instead recognize them as the problem to be explained.

But there is another reason for examining the origins of modern psychology in the context of a sociology of psychological knowledge. It so happens that this problem is one of the very few in the whole history of psychology that has, in fact, been analyzed from a sociological point of view (Ben-David & Collins, 1966). That analysis and the controversy following it (Ross, 1967) illustrate many of the difficulties a sociological approach to the history of science can encounter. Alternative sociological approaches can be applied to the question of the origins of modern psychology; the problem is to find the approach that is adequate to the problem.

One approach, well represented in the sociology of science litera-
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ture, is most aptly called the positivist approach. It has many character-
istics meriting examination. First of all, its conception of a scientific
dicipline is based on the notion of cumulative progress. This progress
distinguishes science from pre-science and is apt to be “measured” by
the number of research publications. No consideration is given to any-
thing like Kuhn’s “scientific revolutions” or to the significance of rival
schools of thought. Applied to the history of modern psychology, this type
of thinking leads to the traditional conclusion that modern psychology
started in Germany and subsequently shifted its center to the United
States. The figures on the relative numbers of publications seem to show
this clearly enough. What should be noted here is the tacit assumption
that the psychology which came to flourish in the United States around
1915 was essentially the same as that which appeared in Germany in the
1880’s. The possibility of a fundamental qualitative discontinuity cannot
even be considered in this approach, because if psychology is a science,
its development is by definition linear, cumulative, and continuous.

An important characteristic of positivist sociology of science is the
profoundly ahistorical nature of its explanatory categories. The norms
governing scientific activity have always been the same and have
always existed as ideas. What changes are the social roles of the bear-
ers of these ideas. It is when scientific ideas are held by individuals who
occupy the social role of professional scientist that they lead to a contin-
uous, cumulative research tradition (Ben-David, 1971).

The rise of new disciplines, like psychology, therefore, depends on
the invention of a new role, that of the professional practitioner of the
new science. Such new roles are frequently invented by individuals as
an opportunity for improving their career chances. Suppose an individ-
ual’s career is blocked in an established discipline; he or she may still
gain recognition by transferring some elements of that professional role
to a different disciplinary context with a different established role. This
is called “role hybridization.” In psychology the prime example of this
process is said to be Wundt, who used his physiological background to
establish a career as a philosopher, but a new kind of philosopher, one
who performed laboratory experiments on problems in psychology. Thus
a new role was born—that of the experimental psychologist—from the
“hybridization” of the previously established roles of physiologist and
philosopher. It merely remained for others to become socialized into this
role by Wundt personally, or by modeling his example, for psychology to
become established as a new scientific discipline.

Two features of this account are worthy of note at this point. In the
first place, the motor of historical change is sought at the level of in-
dividual motivation. It is in the course of an individual attempt at pro-
moting a personal career that a new role is invented or emulated. Sec-
ond, the individual motives that play such an important role are not
established by means of concrete biographical research but are imputed in terms of an implied abstract model of rational decision making in calculating career chances. For instance, Ben-David and Collins, whose account of the social origins of modern psychology is at issue here, suggest that at the time Wundt was looking for a full professorship, advancement in physiology was particularly difficult, most of the full chairs being filled by incumbents who held them for decades. Wundt is assumed to have made the decision to transfer to philosophy, where advancement was somewhat easier, and to have used his experimental physiological techniques as a source of special status within this area of second choice. On this account the role of the modern psychologist is essentially the invention of a single individual, Wilhelm Wundt, to whom most of the next generation of psychologists are linked by means of a complex "genealogical" tree based on "discipleship."

At this point we become aware of a fundamental difficulty that arises from attempting to combine a positivist approach to "evidence" with an individualistic theory of historical change. The latter attaches primary importance to individual intentions, but the former devalues direct "subjective" reports on these intentions which must therefore be inferred from "objective" data. On what criterion of relevance are such data to be selected from the potentially limitless array of historical information? Clearly the criterion of relevance is provided by the need to reconstruct the choice situation confronting the historical individual. At this point the sham objectivity of the procedure becomes apparent; the perspective of the sociologist is merely substituted for that of the historical figure in question. So in reconstructing Wundt's choice, Ben-David and Collins consider only the two fields of physiology and philosophy. But as a matter of fact, Wundt's appointment while he was looking for promotion to a full chair, was in "medical psychology and anthropology," and the most obvious area for him to have considered, purely from a career point of view, was psychiatry, an area in which rather more full chairs were created at German universities between 1873 and 1880 than in physiology and philosophy combined (von Ferber, 1956). In any case, the source from which Ben-David and Collins draw their data also shows that among those who, like Wundt, entered academic ranks in the 1850's and 1860's, 78% of those in physiology ended up as full professors, while only 31% of those in philosophy had that fortune (von Ferber, 1956, pp. 83-84), a fact which is difficult to reconcile with the assumption of greater career chances in philosophy.

Wundt himself gives a rather different account of his change from physiology to philosophy. After beginning his autobiography with an account of his youthful recollections of the revolution of 1848, he addresses those who might consider this a strange introduction to the life of an academic: "I could not help doing it this way, were I not to remain silent about an aspect of..."ividely than much e different life interes which, I suspect, th olved [Wundt, 192C od in his life, when place, Wundt was ir and, for several yea of the state of Bader realized he could ne (Schlotte, 1955–1956) work became incre concern, a change faculty of medicine

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about an aspect of my recollections that has remained in memory more vividly than much else. It was my fate that the habitual coexistence of different life interests over many years led to a change of occupation in which, I suspect, the political impressions of my youth were not uninvolved [Wundt, 1920, p. 15]." As a matter of fact, during the critical period in his life, when the redirection from physiology to philosophy took place, Wundt was involved heavily in workers' educational associations and, for several years, was a member of Landtag, the quasiparliament, of the state of Baden. From his own statements it appears that he finally realized he could not combine a political career with an academic one (Schlotte, 1955–1956). He chose the latter, but from then on his academic work became increasingly more directed toward issues of broad human concern, a change that could only be facilitated by a switch from the faculty of medicine to the faculty of philosophy.

In any case, Wundt is a singularly inappropriate figure to choose as the originator of the modern psychologist's professional identity. He was in fact a strong opponent of the separation of psychology from philosophy, holding that the most important problems in psychology were so closely connected with philosophical problems that a separation of the two would reduce the psychologist to the level of an artisan imprisoned by a covert and naive metaphysics (Wundt, 1913). When a professional society of German psychologists was eventually formed in 1904, Wundt took no part in it—it was not even able to meet in Leipzig during Wundt's lifetime. As for his supposed contribution in making systematic experimental work part of the psychologist's role definition, it should be remembered that, for Wundt, experimental psychology was but a small part of his life's work. In any case, the example of someone like Helmholtz shows that systematic experimental work on psychological problems has no necessary connection with the elaboration of the role of professional psychologist.

It was the generation that succeeded Wundt which first conceived a distinct professional identity for the psychologist. The first effective steps in that direction were not taken until the closing years of the nineteenth century and the early years of the twentieth. Those who took those steps, parvenues that they were, had reasons of their own for seeking to shine in the reflected prestige of an established figure like Wundt. In doing so they laid the foundations for what, yet another generation later, was to become a full blown "origin myth" (Samelson, 1974). What is of interest in the present context is that the positivist sociologist of science, because of his basically ahistorical perspective, becomes a ready victim of the distorted historical reconstructions that later generations project on the past. The tendency is to treat historical evidence as if it were an array of objective facts, whereas it usually consists of reconstructions by highly interested parties. Moreover, the relationship
between successive generations is reduced to that of "founders" and "followers" or "masters" and "pupils," a device, which, incidentally, makes it possible to treat the whole development of modern psychology, after Wundt's supposed deed of creation, as a process of internal development within the discipline, without regard for the critical role of extradisciplinary factors.

The role of these factors may be illustrated by comparing the early development of psychology in Germany and in the United States. In Germany psychology was very slow to develop the institutional forms of an autonomous discipline. As late as 1910 there were a mere four academic positions in psychology in the entire German university system, and only one of these was a full professorship (von Ferber, 1956, pp. 83-84). Even at this time psychology was not an examination subject. Practically all those who supervised research and taught psychology held appointments in philosophy, and this was no mere formality because most of them continued to combine their psychological interests with active work in philosophy. This was, of course, in total contrast to the situation in the United States, where university departments of psychology were mushrooming and where there was a small army of professional psychologists whose ties to philosophy were nonexistent (Gamfield, 1973). The German professional society was founded twelve years after the American Psychological Association (APA). Psychology as an autonomous discipline is an American and not a German invention, a fact which, incidentally, was very apparent to Wundt, who criticized some of his colleagues for wanting to follow an American model that he considered inappropriate in the German context (Wundt, 1913).

There are very good reasons why the American development had to diverge from that on the other side of the Atlantic. For one thing, the academic and professional environments in which potential practitioners of the new discipline found themselves were quite different. In Germany, philosophers had immense prestige, a formidable tradition, and occupied powerful academic positions. In the United States, professional philosophers were few and counted for very little—in fact their professional society only split from the APA in 1901; psychology was the senior discipline! While the German medical schools had been centers of research commanding the respect of the world for several generations, the pitiable state of American medical schools was notorious. (The first effective step toward reform was the formation of the Johns Hopkins School in 1893, although even at the time of the Flexner report in 1910, the professional consolidation of medicine was far from complete.) Whereas American psychology laid claim to almost virgin territory, German psychology had, at every step, to take into account certain very well-entrenched interests.

This state of affairs was a reflection of the general lack of profes-
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sionalization in American higher education during most of the nine-

teenth century, as contrasted with the very high level of professional-

ation of German academics. In 1884, at Harvard University, only 19 out of 189 faculty members had Ph.D.’s; at Michigan, 6 out of 88 (Rudolph, 1962). Not until the 1890’s did that qualification begin to be generally ex-
pected at the most prestigious American institutions. After completing the Ph.D., the German academic would work for several years on a post-
doctoral dissertation, which was required in order to obtain the right to teach. After several more years of scholarly activity, he might if he was lucky expect a salaried university appointment. Both countries experi-

cenced very considerable university expansion after 1870. But whereas in Germany this expansion took place in a well-established structure, in the United State the creation of the appropriate structures coincided with the expansion itself. In the United States psychology got in on the ground floor of the modern university system; in Germany it was a Johnny-come-lately that had to take its assigned place in the established order. That is why, when one looks at the life work of early AmeERICAN psychologists like William James, Stanley Hall, J. M. Baldwin, and J. McKeen Cattell, their youthful travels in Germany appear to have been almost totally irrelevant. Even when they thought they were drawing on their European experience, which was not very often, the transplanted element necessarily took on a very different form because of the vast dif-

ference in context.

What is most significant from the point of view of the sociology of psychological knowledge is that the difference in the social context determined the nature of the new discipline in quite fundamental ways. To say that experimental work in the laboratory constituted the common ground for the new discipline in both countries is to give experimenta-
tional significance, which it certainly has had for many psycholo-

ists, but which is something to be explained rather than a basis for ex-

planation. Differences of conception about the nature of psychological experiment were profound, as were views about the role that was to be assigned to experiment within psychology as a whole. For some, the paradigmatic psychological experiment was based on the in-
trospection of a handful of highly trained and sophisticated observers; for others, it was based on the observation of the movements of animals in artificial environments. For some, experimental psychology was the whole of psychology; for others, it was but a small part. Positivist sociol-

ogy does not concern itself with such issues because it never questions the social context of the content of a discipline. Its “measures” of scientific activity are limited strictly to externals, such as the number of publications and the use of labels (for example, “experi-

mentation”). For the sociology of knowledge, on the other hand, such measures are meaningless in themselves; the social context of the actual
content of scientific ideas is the problem. This is perhaps the key difference between the two perspectives.

Another very important difference arises from the nature of the motivation considered relevant in determining the history of scientific disciplines. As previously mentioned, in Ben-David and Collins', account of the origin of modern psychology, the status striving or career aspirations of key individuals provides the motor for the emergence of a new professional identity. On this view, the social factors determining the relative status of various fields are accepted as given; the perspective is an individual and not a societal one. From the point of view of society, however, the development of new scientific specialties and sub-specialties involves a shift in the division of labor. The production of certain kinds of knowledge becomes the prerogative of a group with a particular professional identity. The history of the establishment of the new discipline or sub-discipline is essentially the history of the establishment of this prerogative. But the success with which such a prerogative is claimed depends on the effectiveness with which the new group manages to legitimize its activities. To be effective, such efforts at legitimation have to take into account the norms and interests of the established power groups controlling the distribution of those material resources on which the production of knowledge depends. In the modern context this means those who control university appointments, as well as those who control institutional settings (of an educational, clinical, industrial, or administrative character) which are relevant for the practice of the profession. Such groups, of course, have legitimation problems of their own, so their norms and interests tend to reflect the basic political and economic realities of the society in which they flourish.

If we compare the situations facing aspiring psychologists in Germany and in the United States at the turn of the century, it becomes obvious that there were crucial differences. In Germany, the potential subject matter of a new discipline was claimed to a large extent by the existing academic and professional establishment. On the one hand, this meant considerable scope for working on psychological problems without a breaking from existing disciplinary affiliations, so the need for a separate disciplinary identity was not very strong. On the other hand, when psychologists did make claims to a separate existence, they met strong resistance from established interests. To face this resistance successfully, the psychologists had to meet the establishment on its own ground. In practice this usually meant persuading the philosophical establishment that psychology was philosophically acceptable and respectable. German psychology never really succeeded in doing this, even in the Weimar period, but in trying it maintained forms of philosophical sophistication that were totally foreign to the American psychology of the time. As late as 1929 the German Psychological Society published a protest against posts in psychology statement tries to justsionship to philosoph and philosophy has in phenomenology, epistemology, and also stresses that methodology, and for dominated by the con In the United States before a very rants, research fund hands of either busi represented their into dependent discipline, social forces. The in American psychology of genuine social pow control the actions of ot control and in tangent of the contemplation professional educatic human fallout from dealt with; man had industial system; proc alternative sources become acceptable i technical competence American psychosce that was totally if the claim that experilaws governing all hur fore, be considered if efforts to control peq ures in the decisive fe press themselves in t representave staten successful text, The E Psychology supplie upon which sociok other sciences dea based... The faci
Perhaps the key difference in the nature of the modern history of scientific and Collingwood's account of career aspirations may be seen in the emergence of a new group of actors determining the social order: the perspective of the individual or the group, the establishment of such a perspective, the new group's efforts at legitimization of the established material resources on the modern context, as well as those who were in the clinical, industrial, or community practice of the problems of their own. Thus psychological problems, so the need for action. On the other hand, this resistance was based on its own philosophical equality and acceptance, and succeeded in doing this. Obtained forms of philosophy to the American Psychological Society.

Modern Psychology published a protest against the tendency to reduce the number of university posts in psychology in favor of philosophy! Characteristically, this statement tries to justify the existence of psychology in terms of its relationship to philosophy: "The reciprocal influence between psychology and philosophy has become steadily stronger, especially in relation to phenomenology, epistemology, and the theory of values (Buhler, 1930)." It also stresses that modern psychology is not limited to experimental methods. What emerged in Germany was a psychology whose problems, methodology, and forms of conceptualization remained very directly dominated by the concerns of philosophy playing the role of Big Brother.

In the United States, however, psychologists had to justify themselves before a very different tribunal. Control of university appointments, research funds, and professional opportunities was vested in the hands of either businessmen and their appointees, or politicians who represented their interests. If psychology was to emerge as a viable independent discipline, it would have to be in a form acceptable to these social forces. The inclinations of those on whose decisions the fate of American psychology depended were clear. They were men in positions of genuine social power who were anxious to use their positions to control the actions of others. They were interested in techniques of social control and in tangible performance. Their image of man was hardly that of the contemplative philosopher: A huge system of secondary and professional education had to be built practically from scratch; the human fallout from wide-scale migration and urbanization had to be dealt with; man had to be made to adapt to a rapidly rationalized industrial system; products had to be sold. In view of the weakness of alternative sources of professional expertise, psychologists might become acceptable if they could reasonably promise to develop the technical competence needed to deal appropriately with these problems.

American psychologists responded to this opportunity with a promise that was totally innovative. This promise involved nothing less than the claim that experimental psychology would supply the fundamental laws governing all human activity, irrespective of context. It must, therefore, be considered the "master science" of human affairs, guiding all efforts to control people. On appropriate occasions, most of the key figures in the decisive formative period of American psychology would express themselves in these terms. By way of illustration, consider some representative statements from Thorndike's (1907) very popular and successful text, The Elements of Psychology:

Psychology supplies or should supply the fundamental principles upon which sociology, history, anthropology, linguistics and the other sciences dealing with human thought and action should be based... The facts and laws of psychology... should provide the
general basis for the interpretation and explanation of the great events studied by history, the complex activities of civilized society, the motives that control the actions of labor and capital. . . . Theoretically, history, sociology, economics, linguistics and the other "humanities" or sciences of human affairs are all varieties of Psychology.

Such statements imply that economic, social, and historical problems are basically psychological problems or should be treated as such. From this perspective was created, for the first time, the image of a general science of behavior, the laws of which were to be as abstract and ahistorical as the laws of physics. Shortly afterward the goal of the new science came to be enunciated in a slogan still found in its introductory textbooks: "The prediction and control of behavior." This goal is totally at variance with the goals that Wundt had in mind for psychology; his goals were concerned neither with prediction nor with control nor with behavior. Nor did Wundt's German successors ever develop such goals for their discipline. Their chances of gaining the respect of the academic establishment would have been even slimmer if they had. The gross difference in the institutional positions of German and of American psychology, therefore, had its counterpart in the complete divergence of aims. This is not surprising, because the goals of a discipline define its position relative to other disciplines. In developing its goals a discipline defines its role in terms of the overall division of labor among disciplines.

The goals of a discipline are fundamental to its self-definition. They constitute a statement of its intellectual interest, the ultimate criterion of relevance in terms of which methods, concepts, and evidence are judged. What holds together the practitioners of a field is their common intellectual interest and the fact that they all basically have the same aims. This aim defines both the domain within which the practitioners are to work and the ways in which they propose to act on this domain. Needless to say, the domain within which they work is not something simply given from the outside, but is itself an intellectual construction of the practitioners of the discipline. The development of an intellectual interest involves not only the formulation of a program of work but also the conceptual distinction of the material on which this work is to be performed. Thus, the category of "behavior," as used by modern psychology, is an intellectual construction identifying a certain domain as potential material for legitimate psychological work. Its domain is obviously quite different from those defined, for example, by such terms as "immediate experience" or "social action."

Intellectual interests clearly involve what have been called "metaphysical paradigms" (Masterman, 1970) in some recent discussions. But for the sociology of knowledge it is important to recognize the element of interest in these meta claims on the basis of which frequently disputes a group can successfully be convinced that the
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interest in these metaphysical presuppositions. Individuals make social claims on the basis of such presuppositions. Disputes on this level are frequently disputes about areas of recognized competency. Before a group can successfully claim special rights over any area, others must be convinced that the area exists and that its nature demands the kinds of special competence (techniques, conceptual models, relevant experience) that the group in question commands. The self-definition of groups of specialists and the definition of the basic nature of their field of action are mutually complementary. To mention an obvious example, if certain quite fundamental preconceptions about the nature of the human organism did not command wide acceptance, the medical profession could not maintain its social position vis-a-vis potential competitors.

Intellectual interests, therefore, constitute the meeting point of social interests and cognitive constructions. The meeting occurs in individuals whose social interests necessarily require such constructions because their particular social identity depends on them. A social identity defines a social relationship—it must be recognized by significant others to exist. Establishing or maintaining a social identity therefore involves the acceptance of the identity claim by those others. The basis on which such acceptance is secured is provided by the compatibility of intellectual interests. Established groups with some social power recognize only those with compatible intellectual interests. Intellectual interests, therefore, have a legitimating function; and differences in intellectual interests arise not only because of differences among those who develop them, but also because of differences among those to whom they are directed as a means of legitimation. Society provides a network of such interests (a system of dependencies) that can be traced to the most important loci of social power. Under some historical circumstances, as in the American universities at the turn of the century, the relationship of professionals to the ultimate sources of power is relatively unmediated; in other cases, as in Germany, the relationship involves a multiplicity of entrenched mediating groups. It is, however, those groups most immediately concerned with the decisions affecting the life or death of a new discipline that are the target of attempts at legitimation and that therefore determine the general nature of the appeal.

In terms of this analysis the emergence of new scholarly or scientific communities is characterized by the emergence of new intellectual interests. New scientific groups differ from established ones because they want to do different things. It is, of course, quite possible for specific theories or techniques in a field to change without any change in the nature of the field, as long as the final aims of the practitioners of that field remain the same. It is only a change in these aims that involves the creation of a new discipline or subdiscipline, that is to say, the emer-
gence of a new group of practitioners defined by a new intellectual interest. Techniques derive their significance from the interest in the service of which they are applied. It is quite misleading to discuss a technique like experimentation in abstraction from the aims it is meant to serve. Thus, the experimental technique used by Wundt to analyze mental processes is a profoundly different technique, both in principle and in concrete practice, from the experimental technique used by a behaviorist to predict and control behavior.

The concept of intellectual interest makes it possible to overcome the absolute separation of "social factors" and "intellectual content," which is characteristic of the positivist sociology of science (Ben-David & Collins, 1967). For such interests are simultaneously an active factor in the social process of the division of scientific work and a formulation of intellectual presuppositions. Failure to recognize the critical role played by the category of intellectual interest is likely to lead to sterile debates about the relative importance of "internal" and "external" factors in the development of scientific disciplines. The basic intellectual interest of a discipline faces both outward and inward: outward, in that it serves to legitimize the activities of its practitioners vis-a-vis significant target groups; inward, in that it establishes the norms by which the work of practitioners is judged. Once the institutional foundations of a discipline have solidified, these norms tend to take on a quasi-autonomous validity of their own, and the link to the external legitimating function may become less explicit.

One of the most succinct and influential expressions of the outwardly directed aspect of modern psychology's intellectual interest is found in J. B. Watson's (1913) famous paper, "Psychology as the Behaviorist Views It": "If psychology would follow the plan I suggest the educator, the physician, the jurist and the businessman could utilize our data in a practical way, as soon as we are able, experimentally, to obtain them." Watson's argument was irresistible—two years later he was elected president of the American Psychological Association. The reason his message found such immediate and massive resonance was that most American psychologists already accepted the premise that it was the business of their discipline to produce data to be utilized "in a practical way" by educators, businessmen, and so on, and to produce them quickly. Given this premise, Watson's prescription, stripped of a few polemical exaggerations, was obviously on the right lines.

What Watson had done was to place the final rhetorical seal on the establishment of psychology as an administrative science, a technology to be wielded by society's managers to direct the actions of those in their charge into desired channels. Such a psychology is a very different kind of discipline from one that sees itself as a handmaiden of philosophy. It must define both its material and its methods differently.
For the practical purposes of social control and administration, it is the
ovt actions of people that matter; their subjective experience is of in-
trest only insofar as it is absolutely necessary to take it into account
in order to manipulate effectively their external activity. This represents a
reversal of the position characteristic of the psychology that flourished
in Germany, where external activity was of interest only insofar as it sh-
ed light on subjective experience. The crucial distinction here is not
whether psychology is to be the study of overt activity or of subjective ex-
perience, but which of the two should be the focus of the psychologist’s
primary interest, reducing the other to the status of a means to an end.

If the question of the relative status of external activity and subjective
experience involves the self-definition of psychology as an adminis-
trative rather than as a humanistic discipline, the relation of psychology
to the social sciences is expressed by the role assigned to social psychol-
y. One of the most striking aspects of the relationship between early
American psychology and Wundt’s kind of psychology is the total obli-
ation that Wundt’s massive social psychology suffered, even among those
American psychologists who had been his students. (Judd, the exception,
remained unheard and turned to interests outside the officially defined
domain of the discipline, as did G. H. Mead, the only other American
scholar of note who paid serious attention to Wundt’s social psychology.)
At the very beginning of his program Wundt projected two kinds of psy-
chology: physiological and social, the former employing experimental
methods, the latter, nonexperimental methods (Wundt, 1922). The rela-
tionship between the two is asymmetrical because, while one cannot
predict the results of social interaction from a knowledge of individual
psychology, one can draw conclusions about individual psychology from
a study of social products (for example, myths provide data on the activ-
ity of fantasy, and social norms and customs (Sitten) provide insights
into motives). In discussing these relationships Wundt contrasts two con-
ceptions of psychological laws, the “metaphysical,” which considers
them to be independent of time and place (by analogy with classical
mechanics), and his own conception of psychological laws as essentially
developmental, which means that the laws of social psychology become
essentially historical (Wundt, 1887). His contributions to this social psy-
chology were monumental, not only in the ten-volume Völkerpsychologie,
but also in the earlier Ethics, as well as in various papers which contain
a great deal of social-psychological material. Nor were these idiosyn-
cratic interests. By 1900 over 200 items per annum were being listed
under “social psychology” in the annual bibliography of the psycholog-
ical literature published by the Zeitschrift für Psychologie.

American psychology took a different path. Conceiving itself to be the
“master science” (the foundation of the other social sciences, which
were in effect merely aspects of individual psychology), it could afford to
ignore cultural and historical levels of reality. It could not allow the existence of a social world that obeyed laws of its own because that might influence the very nature of individual psychology. Instead, it aimed at developing completely abstract laws of individual behavior of the type Wundt referred to as "metaphysical." By stripping human action of its social content, it defined its material as "behavior." While social and cultural reality became subsumed under the category of "stimulus," when social psychology finally made an appearance, it did so as a mere extension of individual psychology to situations in which "social stimuli" operated.

The denial of cultural and historical levels of reality entailed a reversal of the way in which the relation between the individual and his sociocultural environment was conceptualized. Wundt essentially defined that relation in terms of the construction of cultural products by interacting individuals; the individual was seen as active in relation to the culture. American social psychology, on the other hand, became a study of responses to social stimuli, of "social influence processes" conceived, until quite recently, in a strictly unidirectional manner. The individual ended up either at the receiving end of these social influences or as the manipulator. In both instances the relationship is an external one. Cultural products either confront the individual in the form of environmental pressures or they happen to be available as techniques for the control of others. In either case they are "found" by the individual, and the question of the constructive relationship between their qualities and the qualities of individuals never even arises. The kind of social psychology developed by Wundt as a necessary complement to the limited scope of experimental psychology was concerned therefore with a completely different set of problems than those which defined the domain of what was called social psychology by later generations.

Intellectual interests not only define the problems of a discipline, they also determine the prescribed manner of solving them. For modern psychology, these prescriptions have generally revolved around questions about the role and the nature of experimentation. Wundt considered it impossible to employ the experimental methodology of natural science in the investigation of "higher" psychological processes (where this had been attempted, it had merely resulted in pseudo-experiments that did not meet the criteria for scientific experiment [Wundt, 1907]). In Germany the claims for the extended applicability of the experimental method were carried by young psychologists who were also active in trying to promote psychology as an independent discipline. As we have seen, their efforts were not markedly successful, and during the 1920's they began talking in terms of "experiment-like" methods and even dropped the word "experimental" from the title of their association.

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“mandarins” (Ringer, 1969) to be placated, veneration for the experimental method knew no limits and quickly developed into a veritable mystique. Psychology increasingly legitimated its claims to a superior position among the human sciences by an appeal to its use of experimental methods, notwithstanding the fact that many psychologists either did not use such methods at all or used what can at best be described as a very weak facsimile of them. There are grounds for supposing that the ritual prestige of experimentation derived less from the status of pure science than from the tremendous appeal a technological paradigm had for the significant groups psychologists were trying to impress. On the one hand, experimentation was not a hallmark of some of the most admired sciences (for example, astronomy and the evolutionary biology of the time); on the other hand, the particular aspect of the complex set of techniques labeled “experimental” that was singled out as crucial by the psychologists was the manipulative aspect. No matter how careful the observations, how unreliable the data, how unsystematic the controls, how theoretically irrelevant the problem, as long as some intervention had taken place, not merely a necessary, but in fact the sufficient, condition for “scientific” experimentation was considered to have been met. At best this involved a failure to distinguish between experimentation for scientific and for other purposes, thus making it possible for a purely technological approach to knowledge to sail under the flag of science.

Differences in the practice of psychological experimentation play a crucial role in determining the place of psychology as a discipline. Experiments involving human subjects are social situations, the structure of which places very definite constraints on the type of knowledge it is possible to obtain within their context. The sociology of psychological knowledge cannot ignore that this knowledge is obtained in social settings, the features of which are drastically shaped by the intellectual interests of the psychologist. Apart from their primary influence on the norms of the discipline, it is chiefly through their effect on the structuring of investigative contexts that intellectual interests determine the kind of knowledge that will be available to practitioners of the discipline.

There are striking differences in the social context of investigation established by psychology as a philosophical discipline and psychology as a technology of behavior manipulation. The classical psychological experiment, which has become almost extinct, was based on the principle of the interchangeability of experimenter and subject. Both had to be highly trained psychological observers, and, as Wundt pointed out, the psychological training of the subject was, if anything, more important than the psychological training of the experimenter (Wundt, 1922, pp. 12-13). This style of experimentation remained characteristic of German psychology even after a different conception of the psychological experi-
ment began to receive some recognition just before World War I. This second conception of psychological experimentation had appeared among American psychologists before the turn of the century and quickly came to dominate the practice of all but a handful of them. It was based on a profound differentiation of the roles of experimenter and subject. The former was supposed to have a monopoly on training and enlightenment, while the latter was to be untrained and naive. The social situation of the experiment was characterized by a fundamental asymmetry that decisively influenced the kind of knowledge that could emerge from it.

It was a kind of knowledge that would be useful to those who were in positions to control and manipulate the behavior of others in educational, industrial, administrative or similar contexts. The fundamental division between controllers and controlled was built into the very fabric of the knowledge-generating source of this psychology. Data on human responses in situations where autonomy had been surrendered would be applicable to other, nonexperimental situations where autonomy and insight were similarly lacking, although probably not by the voluntary agreement of those affected. What this type of experimental situation was unlikely to yield was knowledge that might increase the level of self-insight or aid the emancipation from external control of human individuals. And even if the usable knowledge the new psychology was able to deliver lagged far behind its extravagant promises, what mattered most to its sponsors was that its research enterprise was designed manifestly to yield the kind of information that might be useful precisely in those situations where a group of people had the power to control the conditions under which other would have to act.

In the tales that are often told in the pages of introductory textbooks the profound methodological differences dividing psychologists in the early years of this century are traditionally represented as differences about the use of “introspection.” This is a bit of mystification that has its place in the professional socialization of the student, but that should not mislead the serious scholar. Wundt argued against the introspectionism represented by the Würzburg group and Titchener as strongly and incisively as he had argued against the now-forgotten introspectionism of the third quarter of the nineteenth century. But the behaviorists could not adopt him as an ally because the real issue involved different conceptions about the nature of the psychological experiment, and on that fundamental level they were poles apart. For the writers of textbooks this issue is, however, a profoundly threatening one. Their purpose is usually to represent psychology as a natural science and to justify this image by reference to the use of the experimental method. The crucial but implicit assumption is that there can be only one methodology of scientific experimentation. If it were to be revealed that modern psychology has been constituting valid experimental

While textbook and half-truths that more critical perspectives come the object of scrutiny, sociology fails. I always the same and qualitative terms, it disciplines construct for continuity in the development of the past while within the discourse of the form of dating its cause experimentalogy treats as historic causes of later generations. Noted in the highly interesting in the sense of discrediting his reason.

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While textbook writers can be expected to perpetuate the myths and half-truths that legitimate the claims made by their discipline, a more critical perspective is appropriate if the discipline itself is to become the object of scientific scrutiny. But this is precisely where positivist sociology fails. Because it presupposes that the nature of science is always the same and sees historical change in qualitative rather than quantitative terms, it becomes a ready victim of the "origin myths" disciplines construct for themselves. Such myths exaggerate the element of continuity in the development of the discipline and misrepresent those aspects of the past which call into question the prevailing intellectual interest within the discipline. In the case of modern psychology this takes the form of dating its origin to the founding of Wundt's laboratory, because experimentation has the status of a fetish. What positivist sociology treats as historical "data" are in fact the historical reconstructions of later generations. The role assigned to Wundt, for example, was created in the highly interested internal historiography of the discipline and misrepresents his real historical significance.

For the sociology of knowledge, on the other hand, history can never be an unproblematic source of "data." It always has to be reconstructed (Weimer, 1974), and the first step to avoid being mislead by existing reconstructions by interested parties is to consult primary sources. For the positivist sociology of science, such categories as "science," "knowledge," and "experimentation" are immutable; for the sociology of knowledge, they are problematic and their qualitatively distinct forms are to be explained.

The uncritical approach of the positivist sociology of science also characterizes its way of dealing with the role played by social interests in the historical development of scientific knowledge. The interests of scientists are always individual interests, status strivings, career aspirations, the motives involved in "discipleship," and so on. This at best can explain how some individuals manage to take advantage of an existing status structure; it is not an approach that provides any means of questioning the nature and origin of this structure. Moreover, there is a complete split between the motivation of individuals and the forces perpetuating social structures. For the individual scientist, knowledge-generating activity becomes a means to the achievement of essentially private ends.

On the other hand, the sociology of knowledge recognizes the personal interests as reflections of groups interests, which themselves arise from social conflicts. The interests of individuals in pursuit of knowledge are tied up with the interests of groups, whether they are fully aware of
this or not. Moreover, as the interests of any group exist only in a context of other interests, individuals act within a web of such relationships extending from their immediate professional environments to the whole society. It is therefore appropriate for the sociology of knowledge to trace the intellectual interests of individuals to those wider social interests that characterize whole societies at various stages of their history.

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