
The Origins of the Psychological Experiment as a Social Institution

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ABSTRACT: *The psychological experiment involves a set of institutionalized role patterns that have evolved historically. This evolution can be studied by analyzing published experimental reports. From the beginning, there were two models for the social structure of the psychological experiment, the Leipzig and the Paris model. The latter emerged in the context of work on experimental hypnosis and involved a rigid social differentiation between experimenter and subject. By contrast, the Leipzig model involved the interchangeability of experimenter and subject roles. American investigators adopted both these models but also introduced more impersonal and less intensive relations between experimenters and subjects. Some implications of the multimodal origins of experimental situations are discussed.*

Whatever else it may be, the psychological experiment is clearly a social institution that has flourished in certain societies and that implies patterns of social regulations that closely circumscribe the relationships of those who participate in it. The investigative situations in which knowledge about human psychology is gathered are highly institutionalized and involve a generally accepted distribution of role expectations among the participants, a clearly understood status differential, and an elaborate set of rules governing the permissible interactions among the role incumbents.

The sizable literature devoted to various aspects of the social psychology of the psychological experiment (e.g., Adair, 1973; Jung, 1982; Rosenthal & Rosnow, 1975; Silverman, 1977) has paid virtually no attention to the history of the social features of the psychological experiment. Apart from Schultz's (1970) recognition of the fact that there is a history to the use of human subjects in psychological research, there has been no systematic attempt to uncover this history.

Because the rules of scientific experimentation prescribe the preparation of a formal account of the proceedings for publication, the literature in psychological journals contains much material that throws light on historically changing social practices. Of course, published experimental articles mainly con-

tain information on the public and formal aspects of the experimental situation, but it is precisely these aspects that are of interest in tracing historical changes in institutionalized patterns.

In pursuing this kind of analysis the wisest course is to pay special attention to historical beginnings. It is in the early stages of the growth of a field that fundamental directions of development are laid down and that traditions are established that become implicit models for later generations. Historians and philosophers of science have long recognized this by paying an extraordinary amount of attention to those 17th century developments that mark the beginning of modern physical science, even though the scientific production of that period represents only a tiny fraction of what was accomplished later. In the case of experimental psychology, the last two decades of the 19th and the first two decades of the 20th century occupy a similar position.

The material that provides the basis for the present discussion is drawn from the first half of this period, a time when the practice of psychological experimentation was in the process of becoming institutionalized. It involves an analysis of the procedures reported in all empirical studies published in the major relevant journals for the period from 1879 to 1898. This includes two German-language journals (*Philosophische Studien* and *Zeitschrift für Psychologie und Physiologie der Sinnesorgane*), two French language journals (*Revue philosophique* and *Année psychologique*), and four English-language journals (*Mind*, *Pedagogical Seminary*, *American Journal of Psychology*, and *Psychological Review*).

Incipient Institutionalization

One striking feature that characterized the first half of this period is the absence of an agreed-upon uniform nomenclature for identifying the participants in a psychological experiment. Although in the case of Wundt and his first group of students we are dealing with a research community that was quite sharply defined in terms of goals, methods, and knowledge of each other's work, there are clearly no strong conventions about the identification of experimenter and subject roles. In their published reports, different members of the Leipzig group used different

terms to refer to experimenters and subjects in their experimental studies. The terms *experimenter* and *subject* (*Versuchsperson*) do occur in several of the experimental reports, but their usage is far from general, and they are also used interchangeably with other terms. Some of these other terms were to have a more extended life, but others hardly survived this early period.

Not only are there several alternative labels for the experimental subject, but there is also no uniformity of usage within the research community. This becomes particularly striking when two investigators are concerned with the same type of experimentally established phenomena but use different terms to refer to their experimental subjects. For example, in an early investigation of the "time sense," Kollert (1883) referred to his subjects as *reactors*, whereas Estel (1885), whose study followed directly from Kollert's, switched to the term *observers*. This lack of semantic uniformity is also reflected in the fact that some investigators used two or three terms interchangeably within the same experimental report to refer to their experimental subjects (e.g., Merkel, 1885).

Echoes of the Leipzig pattern during this early period are to be found in scattered experimental psychological reports published in English. Thus G. Stanley Hall variously referred to the experimenter as the *operator* or the *attendant*, and to the subject as the *observer*, *percipient*, or *subject* (Hall & Donaldson, 1885; Hall & Jastrow, 1886; Hall & Motora, 1887). One early report from the Johns Hopkins laboratory used the awkward formulation *the individual under experiment* (Stevens, 1886), but not surprisingly, this did not catch on. Among these early experimenters Cattell was relatively consistent in his use of the term *subject*, but even he used it interchangeably with *observer* (Cattell, 1888; Cattell & Bryant, 1889) at times, and in his pioneering article on mental tests he introduced the unsuccessful term *experimentee* (Cattell, 1890).

The original terminological chaos soon yielded to a very limited number of accepted terms, as the practice of psychological experimentation established its own recognized models and institutionalized patterns. By the mid-1890s, half of the experimental reports in the *American Journal of Psychology* and *Psychological Review* used the term *subject*, and a quarter preferred *observer*. *Reagent* was a distant third, and all other terms have remained purely idiosyncratic. However, it is worth noting that the crystallization of routine practice did not involve the adoption of a *single* model of usage but of a

small number of alternative models. It took virtually another half century for one model to achieve overwhelming preponderance. As late as 1930 there was heated discussion about the rival claims of *observer* and *subject* (Bentley, 1929; Dashiell, 1930). The establishment of alternative terminologies was only a sign of the multimodal development of early experimental practice that needs to be examined.

Role Structure of the Wundtian Experiment

In the social system of the contemporary mainstream experiment the function of serving as a data source is confined to the subject role, and this function cannot be combined with theoretical conceptualization, task administration, or publishing. No such segregation existed in the early stages of psychological experimentation. Wundt's students frequently alternated with one another as stimulus administrators and sources of data within the same experiment. The case of Cattell and Berger has been documented in some detail (Sokal, 1980); another early instance is provided by Lorenz and Merkel (Lorenz, 1885). Moreover, the person under whose name the published account of the experiment appeared was not necessarily the one who had played the experimenter role. For instance, Mehner (1885) published an article on an experiment in which he had functioned solely as the subject although two other persons had functioned as experimenters at various times.

Nor was the role of functioning as a data source considered incompatible with the function of theoretical conceptualization. Wundt, who generally shouldered much of this latter function himself in the Leipzig experiments, also featured quite regularly as a subject or data source in these experiments, especially in the early days of his laboratory (e.g., Dietze, 1885; Estel, 1885; Friedrich, 1883; Kollert, 1883; Tischer, 1883; Trautsholdt, 1883). The experimental reports, however, were published under the names of his students. It is perfectly clear that the role of data source, or subject, was considered to be of higher or more important status than the role of experimenter. The role of experimental subject was quite compatible with Wundt's exalted status, but once his laboratory got under way, none of the experimental reports mention him as having actually functioned in the role of experimenter.

We might also note that the participants in these early psychological experiments were never strangers to one another. They interacted outside the laboratory as professor and student, as fellow students, and often as friends. They clearly saw themselves as engaged in a common enterprise in which all the participants were regarded as collaborators, including the person who happened to be functioning as the experimental subject at any particular time.

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In fact, when introducing the experimental subjects in their published reports, authors would sometimes do so not by saying that the observers (or subjects) were so and so, but by saying "my co-workers (*Mitarbeiter*) were," followed by the names (Lange, 1888; Titchener, 1893). This involved a complex set of reciprocal obligations and services, such as functioning as experimenter and subject for each other.

Although we may analyze the psychological experiment as a miniature social system, it is far from being hermetically sealed off from broader institutional and cultural contexts. The social structure of a psychological research situation cannot be created in a cultural and institutional vacuum but must make use of whatever material happens to be available at a particular time and place. The Wundtian program of systematic psychological research constituted a genuine innovation, but its social features clearly carried the mark of the institutional context in which it arose. That context was, of course, the late 19th century German university with its elitism and its emphasis on active involvement in research for both faculty and students. Thus, its basic organizational units were not teaching departments, as in America, but research institutes, of which Wundt's laboratory was an example, even though it did not gain formal recognition for a few years. Admittance to these institutes and research seminars was restricted, but once students were accepted they became involved in a collaborative enterprise; although the direction was determined by the professor, there was also a basic demand that students involve themselves actively in the material under investigation. Where that material was defined in terms of Wundtian concepts of psychic causality, the structure of the investigative situation followed as a matter of course (Danziger, 1980). The collaborative interchange of roles and the high status of the observer or subject role were a natural consequence of extending an existing social pattern of academic research to a subject matter that the investigators carried around in their heads instead of finding it spread out before them.

Experimental Hypnosis as the Alternative

At exactly the same time that Wundt's Leipzig laboratory was getting under way, a group of French investigators embarked on the systematic use of experimental hypnosis as a tool of psychological research. First on the scene was Richet (1879, 1880), soon followed by Beaunis (1885, 1886), Binet and Féré (Binet, 1886; Binet & Féré, 1885) and the Belgian, Delboeuf (1886a, 1886b). In their studies, various psychological functions were investigated under conditions of experimentally induced hypnosis. Contrary to the practice in Leipzig, there was no interchange of experimental roles among the partic-

ipants in these French studies. There was a clear and permanent distinction between experimenters and individuals experimented on. Experimenters remained experimenters, and hypnotic subjects remained hypnotic subjects. Moreover, there was a glaring status difference between these male scientists and their lay subjects, who were generally female. The distribution of social functions approximated the pattern in the typical modern experiment, with the function of providing the data source being strictly segregated in an experimental subject role.

The link made between role-playing in hypnosis and role-playing in the psychological experiment, which ushered in the period of contemporary concern with the social psychology of the psychological experiment (Orne, 1962), turns out to be more than just an analogy. From the point of view of its social psychology it is the hypnosis experiment rather than the Wundtian experiment that constitutes the historical prototype of the modern psychology experiment. By 1890, Binet had turned from experiments on hypnotized subjects to experiments on infants (Binet, 1890). This was possible without altering the essential social structure of the experimental situation. For the Wundtian experiment, on the other hand, this kind of extension of scope was not possible, and in due course it paid for this limitation by becoming virtually extinct.

As we have seen, the hypnosis experiment of the 1880s involved a rigid and well-defined role and status structure from the beginning. What was the origin of this instant social structure? Very likely, it was the medical context in which these experiments were carried out. The subjects in these experiments were essentially a clinical population of hysterics and "somnambulists," and the experimenters were for the most part strongly identified with the medical profession. The experiments themselves arose directly out of ongoing medical research into the nature of hysteria and hypnosis. Before experimental sessions began, the experimenter and the subject were already linked in a doctor-patient relationship, and the essential features of this relationship were simply continued into the experimental situation. The whole situation was defined in medical terms. A crucial feature of this definition was the understanding that the psychological states and phenomena under study were something that the subject or patient underwent or suffered. This contrasted quite sharply with the Wundtian experiment in which most of the phenomena studied were understood as the products of the individual's activity.

The much less fluid social structure of the hypnosis experiment is reflected in a high degree of linguistic uniformity in referring to the participants in the experimental situation. It is in this context that we find the first consistent usage of the term

subject in experimental psychology. These medically oriented experimenters quite spontaneously referred to a case they experimented on as a *subject* (*sujet*), because that term had long been in use to designate a living being who was the object of medical care or naturalistic observation. This usage goes back at least to Buffon in the 18th century. Before that a *subject* was a corpse used for purposes of anatomical dissection, and by the early 19th century people spoke of patients as being good or bad subjects for surgery (*Grand Larousse*, 1973; Littré, 1968). When hypnosis came to be seen as an essentially medical matter, which was certainly the case in Paris in the 1880s, there was nothing more natural than to extend an already established linguistic usage to yet another object of medical scrutiny. However, within the medical context we immediately get the formulation *healthy subject* (*sujet sains*) (Féré, 1885, and in the titles of studies by Bremaud and by Bottey cited in Dessoir, 1888) when it is a matter of comparing the performance of normal and abnormal individuals. From this it is a very short step to the generalized use of the term *subject* to refer to any individual under psychological investigation. This step was quickly taken by Binet and others.

Thus, in the earliest years of experimental psychology there simultaneously emerged two very different models of the psychological experiment as a social situation. These can be called the Leipzig model and the Paris model. The Leipzig model involved a high degree of fluidity in the allocation of social functions in the experimental situation, reflected in the lack of a uniform terminology to refer to experimenter and subject roles as such. Persons playing these roles at any particular time were more likely to be defined in terms of their relationship to the physical apparatus than in terms of their relationship to each other. At the same time, the subject function had a higher status than the experimenter function, though these functions could be assumed by any participant in the experimental situation. In the Paris model, by contrast, experimenter and subject roles were rigidly segregated, with the experimenter clearly being in charge and the subject serving as an object of study who underwent certain manipulative interventions on the part of the experimenter. With this went a more uniform terminology that unambiguously identified the subject as such. The two models were typically employed to investigate different aspects of psychological functioning: the Leipzig model to study aspects of normal cognition and the Paris model to study abnormal functioning.

The occasional English-language studies of this early period were not the product of a research community as well defined as the Leipzig and Paris groups but were rather in the nature of relatively

isolated individual contributions. Accordingly, we find that in this period British and American contributors to the experimental literature tended to follow French and German models. This is reflected in their use of terminology. The first uses of the term *subject* occurred in the context of experiments involving the hypnotic state (Gurney, 1884; Hall, 1883). It is interesting to note that at this time G. Stanley Hall used the term *subject* in the context of his work in the area of experimental hypnosis but switched to the terms *percipient* (Hall & Donaldson, 1885) and *observer* (Hall & Jastrow, 1886) when reporting on his experimental work with normal individuals.

Cattell appears to have been the first to use the English term *subject* in describing a psychological experiment involving a normal adult human data source (Cattell, 1886). However, he was by no means sure of his ground, for in an 1889 paper coauthored by him we find the formulation *an observer or subject* (Cattell & Bryant, 1889), with *subject* in inverted commas. Putting the term *subject* in inverted commas, when used in this context, was quite common in the English literature of this time (Gurney, 1884, 1887; Gurney, Myers, & Podmore, 1886, pp. 330-331; Jacobs, 1887), indicating that this usage was of recent origin, possibly influenced by French models. The English term *subject* had acquired similar medical connotations as its French equivalent. In the 18th century it was used to refer to a corpse employed for anatomical dissection, and by the middle of the 19th century it could also mean "a person who presents himself for or undergoes medical or surgical treatment" (*Oxford English Dictionary*, 1933, Vol. 10). Hence its use in the context of hypnosis. It was already used occasionally by James Braid (e.g., Braid, 1960, p. 209).

American Innovations

By the turn of the century there were clear signs that American psychology would develop a style of research that differed in certain fundamental ways from either of the European models. The most striking feature of this style was the introduction of a new object of psychological investigation, that of a *population* of individuals.

In the forms in which it was first established, the psychological experiment involved strictly one-to-one relationships among the participants. However, there was another possibility. The recently expanded and bureaucratized educational systems of the economically advanced countries had produced large captive groups of young people who were not only a source of new psychological problems but who could also be seen as a potential source of psychological data. Although there were a few isolated and sporadic European moves in this direction, the sys-

tematic pursuit of this new style of psychological investigation was initially an American phenomenon.

A primitive form of the new style was promoted with considerable early success by G. Stanley Hall. The research reported in his journal *Pedagogical Seminary* (founded only 4 years after the *American Journal of Psychology*) differs from other psychological research of the time in several interesting ways, not the least of which is its predominant focus on psychological characteristics of populations rather than individuals. Studies are commonly based on data from hundreds of subjects, and investigations involving thousands of children are by no means rare (e.g., Barnes, 1895; Kratz, 1896; Schallenger, 1894).

Obviously, this kind of research involved a very different set of social relationships than those that characterized the academic laboratories of the time. The contrast with the Leipzig model was total, with the Paris model, somewhat less so. As in the latter, the asymmetry between investigators and the human objects of investigation was clearly established and fixed. Also, in both cases the human data source was categorized as something less than a normally responsible and enlightened adult. However, the novel features of Hall's research situations are so profound that one cannot possibly regard them simply as a replication of the Paris model. There is a fundamental shift of interest from the *analysis* of psychological processes, necessarily manifested in specific individuals, to the *distribution* of psychological characteristics in populations. Age and sex differences, as well as differences among various educationally or clinically defined populations, are the bread and butter of this research. This means that individual subjects become totally anonymous, and their specific contributions to the research enterprise remain unidentified and unreported. The research contact between investigator and subject becomes much less intense and extends over much shorter periods of time. Psychological tasks are administered in group sessions, and instead of an extended sequence of interactions between investigator and subjects, there is generally a one-shot episode of instruction followed by response. Paper and pencil instruments are the favored medium for this exchange.

Although the research sponsored by Hall could find much common ground with emerging work on mental tests, it could be called *experimental* only within the loosest definition of that term. Nevertheless, some of the American experimental literature of the time bears the marks of a similar style (e.g., Baldwin, Shaw, & Warren, 1895; Griffing, 1895; Jastrow, 1894; Kirkpatrick, 1894). Here the experimental tasks were administered to children and college students in group sessions, and the results were presented simply as group averages without

any analysis of the response patterns of individual subjects. This was a striking departure from the then-established practice of attributing all experimental results to specific individuals. What emerged was an impersonal style of research in which experimental subjects played an anonymous role, experimenter-subject contacts were relatively brief, and the experimenter was interested in the aggregate data to be obtained from many subjects.

What would be an appropriate label for this model of experimentation, which clearly differed in important ways from the other two models? The early investigators themselves sometimes referred to these as *statistical* studies. But one hesitates to follow them because within the Leipzig model the statistics of error were employed quite widely. It was more a matter of the function of statistics being different in the two cases. The third model could be referred to as "Galtonian," but in the present context that might be misleading because the application of Galtonian statistics to studies of this type effectively belongs to a later period. Some early studies sponsored by Franz Boas at Clark (Bolton, 1891; Bryan, 1892) were exceptional in their statistical sophistication. Most of these early "statistical" studies were statistical only in the crude sense that they were trying to obtain averages and percentages. The application of Galtonian, or more accurately Pearsonian, techniques to studies of this type did not really develop until after the turn of the century, with Boas's expupil, Thorndike, playing a key role.

Both conceptually and historically it makes for greater clarity to distinguish between the forms of statistical technology and the research practices that provide the context for the application of this technology. This is not to deny the development of intimate ties between the two aspects, as indeed happened in the 20th century history of the Galtonian paradigm. But in terms of the emergence of certain patterns of social practice in psychological research, we have to consider the research community most closely identified with this historical process. Pinpointing such a community in early American psychology is a little more doubtful than in the European cases that have been discussed. Many American psychologists engaged in a mixture of practices at that time. However, one major center clearly overshadowed all others in its systematic employment of new research practices, not sanctioned by the accepted European models. This was Hall's little empire at Clark, and so it does not seem inappropriate to refer to this third pattern of research practice as the Clark model.

It seems, then, that psychology entered the 20th century with three different models for structuring the social interactions that are a necessary part of its research enterprise. For a few years these models

coexisted relatively peacefully, and occasionally two of them would even appear side by side in the same research report. But this was not a stable situation. Social relations in the laboratory were not hermetically sealed off from social relations outside. The demands of a wider social practice brought changes to investigative practices and favored some at the expense of others. New forms of older models and composite models appeared. Sometimes these were associated with monopolistic claims regarding their ability to provide the only guaranteed framework for the generation of true and worthwhile psychological knowledge. However, detailed consideration of these later developments lies outside the scope of this article.

Implications and Questions

The distinctions among the patterns of investigative social practice discussed here show that from its earliest beginnings experimental psychology involved a structuring of social interaction and that alternative ways of accomplishing this were always available. An analysis of these original cases suggests certain general implications.

One of these implications concerns the embeddedness of social psychological aspects of experimentation in a historically limited normative framework. For instance, it is not plausible to assume that generalizations about the social psychology of the subject role in any one of these models would hold for the other two. Yet, virtually all of the existing empirical work in this area tends to take a certain social structure of experimentation for granted. Although this has some practical justification, the principle of the historicity of social psychological generalizations (Gergen & Gergen, 1984) needs to be applied also to the social psychology of the psychological experiment.

Discussions about the pros and cons of experimentation in psychology represent another area that might well benefit from a more historically informed perspective. Such discussions are apt to revolve around something that is identified as *the* psychological experiment (e.g., Gadlin & Ingle, 1975; Kruglanski, 1976). But the definite article can be misleading in this context. From its beginnings experimental psychology used more than a single model of the experiment, and the differences among these models may be of more profound significance than their similarities. It is true that at certain times and in certain locations a particular model of experimentation achieved an overwhelming predominance, but this does not establish the existence of the psychological experiment as an ahistorical entity. Instead of equating a particular form of experimentation with experimentation as such, we should be asking questions about the scope and limits of

different social patterns of experimentation (Hendrick, 1977).

Another implication concerns the social contextualization of psychology. We know that the historical development of psychology cannot be divorced from characteristics of the culture and from structural features of the wider society within which psychology exists (e.g., Buss, 1979). Usually these influences are thought to operate on the level of conceptual preferences and theoretical biases. But if psychological ideas are not produced in a social vacuum, the same holds true of the psychological experiment as a miniature social system. The social interactions that are necessary for psychological experimentation were not designed from scratch on the basis of purely rational considerations but simply grew out of patterns of interaction that were already familiar to the participants. Medical and educational institutions provided the original forms of these patterns, just as medical and educational theories provided the sources of many psychological concepts. Other social institutions probably became relevant at a later stage. The point is that methodology is no more free of the influence of social contextual factors than is the formation of theoretical concepts.

This leads to a third implication, which concerns the need to relate differences in theoretical position to differences in the social practice of investigation. We get a one-sided, idealized picture of the development of psychology if we see it only in terms of changes in theoretical orientation. Where theoretical differences have been profound, they have generally been linked to different investigative practices (for an example from the history of psychology, see Böhme, 1977). This is true of the cases examined here, but it is equally true of 20th century cases like psychoanalysis, Gestalt psychology, and behaviorism. Once we have recognized this we have to resist the temptation of engaging in fruitless speculation about the chicken and egg problem of the priority of theoretical orientation and the social practices involved in investigation. What is important is the recognition that psychological theorizing is not an activity totally divorced from the social relationships that psychologists establish with those who are the source of their data.

The social situations that characterize psychological experiments are explicitly designed to function as knowledge-generating situations. Interactions that lead to the production of data that count as psychological knowledge are part of what have been called "social proof structures" (White, 1977). In the examples discussed here, different patterns of interaction were associated with the production of different kinds of knowledge. Insofar as this can be generalized, it leads to a relativization of questions about experimentation in psychology. As long as such questions

are framed in terms of experimentation in general and knowledge in the abstract, they are likely to remain sterile. A more promising approach would involve raising questions about relationships between the social structure of knowledge-generating situations and the nature of their products. In addressing such questions it would be as well not to ignore the potentially rich source of evidence buried in the published record of psychologists' past practices.

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