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MAKING SOCIAL PSYCHOLOGY EXPERIMENTAL: A CONCEPTUAL HISTORY, 1920–1970

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The historical emergence of a field devoted to the experimental investigation of effects identified as "social" required a radical break with traditional conceptions of the social. Psychological experimentation was limited to the investigation of effects that were proximal, local, short-term, and decomposable. A viable accommodation to these constraints occurred in the closely related programs of Moede's experimental crowd psychology and Floyd Allport's experimental social psychology. Later, Kurt Lewin attempted to provide a different conceptual foundation for the field by drawing on certain precepts of Gestalt psychology and the philosophy of scientific experimentation developed by Ernst Cassirer. These ideas were poorly understood and were soon replaced by a methodological regime in which a new generation of statistical procedures and experimental design shaped implicit conceptions of the social in social psychological experiments through such procedures as randomization and the additive combination of variables. © 2000 John Wiley & Sons, Inc.

INTRODUCTION

The historical origins of experimental social psychology have been controversial. In the discipline's first quasi-official history G. W. Allport (1954) pinpointed a study by Norman Triplett (1898) as the first social psychological experiment, a claim that was repeated by most text book writers over several decades. Some time later, however, Hilary Haines and Graham Vaughan (1979) disqualified this account as a typical "origin myth" which supported an illusion of historical continuity and cumulative progress for experimental social psychology. They showed that Triplett's study was not intended to be social psychological at the time it was undertaken, whereas other studies around the same time, notably studies on suggestion, had a more explicit social psychological focus. Yet Allport ignored the latter. The reason, according to Haines and Vaughan, was that Triplett's study could be retrospectively interpreted as having been about social facilitation, thus establishing a continuity with a later research tradition that was undeniably part of experimental social psychology.

While Haines and Vaughan's critique of the Triplett myth was very effective, they also raised some more fundamental questions that still await an answer. In their review of candidates for the status of early social psychological experiments these authors noted the difficulty of deciding whether a particular experiment could be considered social psychological or not. Of course, this difficulty existed only while there was no recognized field self-identified as social psychology. Once the field became institutionalized—equipped with its own textbooks and journals, offering specific courses of instruction, a research network linked by cross-citation, etc.—it was relatively easy to identify social psychological studies. But before this was the case, one had retrospectively to apply later, explicit, conceptions of social psychology to earlier studies that were undertaken when such conceptions existed at best

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implicitly. In this respect, the choice of early suggestion experiments as constituting the beginnings of experimental social psychology seems to require no less an interpretive leap than the choice of Triplett's study. Although much later it was possible to see parallels between some of Alfred Binet's (1900) experiments on suggestion and experiments on conformity conducted half a century later, there is no sense in which Binet's work established a paradigm of experimentation adopted by a succession of experimenters following each other in some kind of historical continuity.

It is necessary to make a distinction between "experimental social psychology" and "experiments of social psychological relevance." The latter would be experiments addressing issues conceptualized in terms that are now recognizably social psychological in nature. Experiments on suggestion conceived as an interpersonal process would undoubtedly qualify, whereas Triplett's study would have difficulty meeting even this criterion. That is what Haines and Vaughan were able to demonstrate. But such demonstrations still leave unanswered the more fundamental question about the origins of "experimental social psychology." Armed with our current understanding of social psychology, we might discover any number of singular experiments with social psychological relevance in the past without uncovering the historical roots of the field. This is because the field was not constituted by an *aggregation* of separate studies, each of which just happened to employ experimentation. At a particular juncture the use of experimentation became *programmatic* and potentially *normative.* What is meant by these distinctions?

As Stam, Radtke, and Lubek (2000) show, after World War II, experimentation ceased to be a method like any other; it was advocated as the method of choice for the field of social psychology as a whole and held up as the methodological norm that ought to be respected by all who worked in it. This is when experimentation in social psychology became normative. The formation of recognizable social psychological research traditions, however, did not depend on the emergence of such a pan-disciplinary norm. Such traditions, for example in research on social attitudes or on work groups, existed several decades before this normative phase. During this earlier period, experimentation, though not yet normative, was practiced programmatically in several sub-areas of social psychology. Experimentation therefore was no longer something to be found in isolated studies but accepted as the common method for a coherent set of studies linked by similar research interests, research questions, and theories.[1]

Beginning with the advent of programmatic experimentation, the history, at least of psychological social psychology, shows a degree of continuity that was unlike anything that had gone before. Allport was mistaken in assigning any special significance to Triplett's experiment, not only because he chose the wrong experiment, but, more importantly, because his focus on a specific study trivialized the historically important question regarding the origins of experimental social psychology. That question has to be addressed on another level, namely, by an analysis and historical contextualization of programmatic social psychological experimentation.

1. This use of "programmatic," like the closely related "research tradition," derives from the practice, frequently adopted in science studies, of taking, not specific experiments, but interlinked sets of experiments as the unit of historical analysis. The rationale for this usage was provided by Imre Lakatos' (1978) critique of falsificationism, which pointed out that the historical fate of a scientific theory depended, not on unique experimental outcomes, but on the general trend of "research programs." The existence of such programs is not a matter of rhetoric but of practice. Rhetoric may or may not play a role in establishing practice in specific cases. Thus the "explicit announcement of a new program" is not a criterion for establishing the existence of a research program, as Samelson's (2000) comments imply.

Two historical questions arise out of the distinction between a post-World War II period, during which experimentation became the preeminent method advocated for social psychology as a whole, and an earlier period during which experimentation coexisted with other methods but had become the method of choice for parts of the discipline. First, there is the question of how the transition from the earlier to the later state of affairs was accomplished. This is the question addressed by Stam et al. (2000) and by MacMartin and Winston (2000) in this issue. The other question is the one on which I wish to focus here, namely, how experimentation entered social psychology in the first place. From the perspective of the present, it is easy to take the existence of social psychological experiments for granted. But if we go back to the beginnings of the field, and to the beginning of the last century, the very notion of social psychological experimentation would have seemed dubious. Undeniably, social psychology is concerned with social phenomena, and social phenomena were not obvious candidates for the application of an experimental methodology borrowed from the natural sciences. For about half a century before the emergence of the label "social psychology," a related field, *Völkerpsychologie,* roughly translatable as "cultural psychology," had existed in Germany (Danziger, 1983). This field had had its own journal and literature, but it was never experimental. In fact, its last major representative, Wilhelm Wundt, a luminary of experimental psychology, had repeatedly argued against the appropriateness of the experimental method for investigating topics in this field (most recently in Wundt, 1907, 1908). More generally, and more tellingly, none of the social sciences emerging at the end of the nineteenth and the beginning of the twentieth century were experimental. What were the circumstances that made it possible to propose a radical break with this kind of precedent?

As soon as one addresses this question it becomes clear that it cannot be divorced from the further question of what was meant by experimentation. Before the era of social psychological experimentation Utopian communities represented prime examples of "social experiments" (Pagès & Vacher, 1993). Historically, the meaning of "experimentation" has varied widely, even in the natural sciences (Hacking, 1983). In psychology, Wundt, in spite of being a major promoter of experimental psychology, had a very restricted notion of what constituted a genuine psychological experiment, a notion that was not shared by his successors (Danziger, 1990). Later, the definition of a psychological experiment underwent further fundamental changes (Winston, 1990; Winston & Blais, 1996). Social psychology was no exception. The first textbook of "experimental social psychology" (Murphy & Murphy, 1931) adopted an extremely broad definition of what constituted an experimental investigation—most of the studies the authors surveyed would probably not have qualified as "experimental" a quarter century later because the conception of what an experiment was had changed. Clearly, an inquiry into the introduction of experimentation into social psychology cannot proceed from a fixed conception of experimentation. Rather, the historical changes in this conception must themselves become the focus of such an inquiry (MacMartin & Winston, 2000).

When that is done, what emerges is not a discipline that was invented just once and subsequently grew in an incremental manner. Both the theory and the practice of social psychological experimentation underwent profound changes during the first half century of the existence of the discipline, that is, between 1920 and 1970. Although experimentation was advocated and practiced programmatically throughout this period, there was profound change in what a social psychological experiment should and did look like. Thus, during the earlier part of this period, the term "experimental" was commonly used to cover correlational studies, whereas these were later sharply distinguished from investigations that involved actual experimental control and manipulation. A relative devaluation of "merely" correlational studies accompanied this change. However, this was a development that was not peculiar to

social psychology, and it does not form part of the present study. What I wish to concentrate on here are the most influential models of manipulative social psychological experimentation that appeared during the first half century of "experimental social psychology."

There have been three such models. All of them were explicitly programmatic and quite successful in their time, but they differed profoundly from each other. The first model was that associated with the name of Floyd Allport.[2] That model had major importance because it was the first. It gave concrete content to the very idea of an experimental social psychology. The second model was closely associated with the name of Kurt Lewin. Others were moving in the same direction, but Lewin was preeminent in providing a sophisticated rationale for the new model. The third model became prominent in the 1950s and after and has no eponymous hero. It was characterized by the fact that specific statistical techniques assumed a governing role in social psychological experimentation, establishing a kind of methodological regime. In what follows I will analyze essential features of each model that made it different from the others. Though my focus here is on the historical discontinuities in conceptions of social psychological experimentation, this does not imply the absence of any continuity.

The Invention of an Experimental Social Psychology

It is well known that a field called "social psychology" was variously described and identified several years before there was any recognition of social psychology as an experimental science (Apfelbaum, 1986; Lubek & Apfelbaum, 2000). The first deliberate and systematic attempts to apply experimental methods to social psychological problems were only published 12 years later by Wolfgang Moede (1920) in Germany and Floyd Allport (1920) in the United States. Of course this did not mean that experimentation immediately took over the entire field or that such a thing was even contemplated by these early experimenters.[3] For about two decades after 1920, much of what was considered social psychology remained closed to experimentation (see, e.g., Murchison, 1935), and of course there has long existed a sociologically oriented social psychology that is non-experimental (Collier, Minton, & Reynolds, 1991). How then did the notion of an experimental social psychology arise in the first place?

The key to answering this question lies in the close link that always exists between preconceptions about the object to be investigated and faith in the appropriateness of particular methods of investigation. *Methodology is not ontologically neutral* (Danziger, 1988). Those who held experimentation to be inappropriate for the investigation of social life and those who held it to be appropriate are likely to have had different preconceptions of social reality. The aspects of social reality that mattered to Wundt, for example, were communal patterns (i.e., customs), symbolic products like myths, and symbol systems (i.e., language). Then as now experimentation was not the obvious method of choice for investigating these social phenomena. That was true also of the aspects of social reality studied by other fields of social investigation like economics, sociology and politics. It was impossible to advance a credible

2. As this is a *conceptual* history, the focus is on historical changes in conceptions of experimentation within the field. Authors' contributions are therefore considered only insofar as they are relevant to this framework. The intellectual biography of each author belongs to another level of discourse.

3. Nor did it mean that Allport and Moede never engaged in or encouraged any other kind of research practice, as Samelson (2000) points out. The fact that Allport as an individual did many things besides social psychological experimentation has no bearing on his role in the history of this field.

case for a methodology of scientific experimentation on any social object without redefining that object in a nontraditional way. The earliest practitioners of experimental social psychology did precisely that.

In the influential pronouncements of Floyd Allport, the most outspoken proponent of the new social psychology, the redefinition of social objects was accomplished in two complementary moves. First, he denied the reality of all traditional social objects. Only individuals were real. At the beginning of his first programmatic statement, Allport (1919, p. 297) listed "the fallacy of the group" as the first of "the factors which have impeded experiment in social science." For the experimental social psychologist "concepts which denote characteristics only of groups are of little service." More concretely, "in the present industrial conflict it is of little value to speak of conflict between groups or classes" (p. 298). In his subsequent work Allport generalized and systematized his critique of traditional conceptions of social objects. In his textbook of social psychology, he objected to "all theories which partake of the group fallacy (because they) have the unfortunate consequence of diverting attention from the true locus of cause and effect, namely the behavior mechanism of the individual" (F. Allport, 1924, p. 9). A few years later he devoted a long monograph to the most explicit formulation of his position, namely, that all institutions, social organizations, cultural symbols, and the like exist only in the social habits and attitudes of individuals (F. Allport, 1933). It followed that individual psychology was the only source of explanatory theories for the social sciences.

But this, according to Allport, did not deprive social psychology of a domain of its own. Although non-psychological *theories* of the social can be dismissed as mythical, the social is *observable,* and hence investigable, because of what Allport regarded as a real distinction between social and non-social behavior. The former involved "social stimuli" and the individual's response to them, social stimuli being simply other people. Examples of social behavior are "the reactions to language, gestures, and other movements of our fellow men, in contrast with our reactions toward non-social objects, such as plants, minerals, tools, and inclement weather" (F. Allport, 1924, p. 3).

Therefore Allport asserted that the province of social psychology was the investigation, especially the experimental investigation, of social behavior. The field was defined by a special kind of social object, one that was enormously restricted compared to the social objects that defined the more traditional fields of social investigation. Not only was the environment that humans created for themselves desocialized—even tools are not social—but the social objects that remain, that is, other people, were reduced to a concrete physical presence. "A collection of individuals, not assembled in one another's presence, but joined by some common bond of interest or sympathy" may be referred to as a group "in a sociological sense," but, insofar as it can be termed social, the behavior of individuals in such groups is based on "actual contacts" (F. Allport, 1924, p. 260). Humans functioned as "social stimuli" only insofar as they were directly present to each other. This covered a range of effects, from social facilitation to competition and suggestion.

During the earliest years of the existence of the field, social psychological experimentation with human adults was largely represented by studies based on an implicit definition of the social that was essentially Allportian (see Dashiell, 1935). But even the extension of that definition to include not only the actual but also the imagined presence of other people left the field with a rather limited domain.

Impoverished though it was, the Allportian conception of the social was tailor made for an experimental social science, given the notion of experimentation that prevailed among psychologists at the time. Psychological experiments, almost without exception, had been limited to the investigation of a very limited range of effects. In particular, they had been

limited to exploring *effects that were local, proximal, short term, and decomposable.* Effects were local in the sense that they were observed at a particular place and time. I use "proximal" to refer to effects that resulted from the immediate presence of some effective agent, known as a "stimulus." Changes in visual perception as a result of changes in visual input under laboratory conditions provided numerous examples of such local, proximal effects. Distal effects, for example of changes in the visual environment as a result of urbanization, remained outside the scope of psychological experimentation. In most cases the experimentally studied effects would also be short term, often lasting no longer than the presented stimulus conditions. Even the more lasting effects studied in some psychological experiments, for example, those on memory, were relatively short term compared to many real-life effects on human experience and conduct that might extend over a lifetime or even over generations. Finally, the effects explored by experimental psychology had been decomposable, that is to say, complex effects (and virtually all effects were complex) could be analyzed into components that were amenable to experimental isolation and separate investigation.

Although these limitations had placed some effects outside the reach of experimental psychology, they still left a broad field for fruitful investigation, especially in such areas as sensation and perception, semantic memory, skill learning, etc. Floyd Allport, and those who accepted his program, believed that psychological experimentation — as they knew it — would also provide a secure methodological foundation for a scientific social psychology. This meant they were convinced that the kinds of effects studied by means of psychological experimentation were adequate for building a science of social behavior. Such beliefs entailed a particular conception of the social that Allport made explicit. His program depended on a perfect convergence of methodological prescriptions and ontological assumptions. The kinds of effects, local, proximal, short-term, and decomposable, that psychological experimentation was able to establish were precisely the effects that counted if the realm of the social was defined as the stimulation that individuals provided for each other. Experiments on social facilitation, such as those carried out by Allport himself, delivered the paradigmatic examples for this version of experimental social psychology. Historically, what is so remarkable about this program is Allport's radical break with the more traditional conceptions of the social that were operative in the social sciences around the turn of the century. The social effects that primarily concerned such disciplines as political economy, ethnology, sociology, historical linguistics, or *Völkerpsychologie* had characteristics that were the opposite of those investigable in the psychological laboratory. These effects were largely *non-local, distal, long-term, and experimentally non-decomposable.* For example, the causes and effects of such phenomena as international markets, political structures, or historical changes in symbolic life are not local in the sense that events studied in the psychological laboratory are local. If one reduces them to proximal effects of one individual on another in some immediate situation, one loses the phenomenon. The effects that preoccupied the older social sciences were also typically long term, manifesting themselves over many years, often over generations. Moreover, with very few exceptions, such effects cannot be experimentally decomposed into components the additive recombination of which accounts for the original phenomenon.

The conspicuous novelty of Allport's program involved much more than a purely methodological beginning. His methodological innovation would not have been plausible, or even conceivable, without a radical break with conceptions of the social that formed part of the foundations of existing social sciences. This radical break poses a historical challenge. Allport surely did not construct his specific conception of the social in a historical vacuum. What were the social resources upon which he was able to draw in accomplishing this construction? Had there been any developments in the social sciences that pointed in the direction he was

going to pursue? Had there perhaps even been predecessors whose work might have been exemplary for his own?

BEFORE ALLPORT

Allport clearly did not care to waste much time thinking about the possible historical roots of his work. He was far more interested in advertising the scientificity and the broad potential of his approach. Yet the few clues he dropped about the past are worth following up. We know that his larger program grew out of the experimental work he did for his doctorate, the topic of which had been suggested by Hugo Münsterberg (F. Allport, 1974). This work involved the comparison of individuals' performance when alone with their performance when in the presence of others. The difference between the two could be attributed to the influence of other people, that is, the social as defined by Allport. In his initial programmatic statement, Allport (1919) had indicated the identity of his predecessors in this work: "Prior to 1915 practically all research on this problem had been done in Germany by August Mayer, Meumann, Moede, and others" (p. 304). He then distinguished his own work from theirs, not on methodological grounds, but on the basis of their different investigative goals. Their purpose had been purely practical, namely, to assess the relative merit of having school children work in class or on their own, whereas he, Floyd Allport, clearly had a far grander vision of the implications of such experiments. The significance he claimed for his own work was based on its programmatic aspects, not on the novelty of its empirical results or his methodological superiority.

Was Allport's claim justified? In regard to the first two individuals he mentioned, as well as some others mentioned a few years later in his textbook (F. Allport, 1924), the answer is undoubtedly yes. Although experimental designs comparing the performance of individuals when alone and when in the presence of others had been systematically employed in German "experimental pedagogics" since the early years of the century, there had never been any suggestion that such experiments could provide the paradigm for a new social science. This suggestion required a redefinition of the social, a contribution to which Allport could rightfully lay claim.

But things get rather more complicated when we turn to the last name on Allport's list. Wolfgang Moede was not an educational psychologist like the others. He did have strong practical interests and after World War I all his academic appointments were in applied psychology. He edited the journal *Industrielle Psychotechnik* and was for many years the chairman of the organization of German applied psychologists (Geuter, 1986). He also had, however, what appears to have been a lifelong interest in crowd psychology. (When Le Bon's popular book on "The Crowd" was republished in Germany in 1932, Moede wrote an introduction for it.) His early experimental work was motivated by both these interests, though it was the interest in crowd psychology that provided the fundamental orientation. It is this, as well as the extensive, systematic, nature of his experiments that sets Moede's work apart from the other early German studies.

Among Moede's experiments were some in which he compared work output when alone with output in a group and others in which he investigated the effect of competition on group work. Some of these studies were similar to those of the German educational psychologists. Others anticipated Allport's work. But Moede also performed experiments to gauge the effect of group conditions on some of the most widely studied psychological phenomena, that is, sensory thresholds, attention, and memory. Although this work had been done prior to the outbreak of World War I, only a preliminary report on a small part of it had appeared (Moede,

1914) before further publication was delayed by the war. The final report (Moede, 1920) runs to over 200 pages and at the very least represents the most extensive assembly of experiments of social psychological interest up to that point. Allport (1924) cited both reports in his references but discussed only the 1914 study in his text. This is surprising because there was much that would have provided grist for Allport's mill in the longer report. Whether he actually consulted Moede's full report is therefore open to doubt.

It seems safest to regard Allport's and Moede's work as virtually simultaneous attempts at the programmatic application of the experimental method to social life. For there is no question that what Moede presented in 1920 was more than a collection of experiments that happened to have a social psychological focus. All his experiments were explicitly tied together by a problematic that was derived from a particular theoretical approach to social phenomena. His work represented a systematic extension of experimental psychology to problems generated within a certain framework of social theory. Like Allport, he presented his work as the nucleus of a new experimental social science. But there was a difference. Moede did not call his new science experimental social psychology. He called it experimental crowd psychology (*experimentelle Massenpsychologie*). He is more specific than Allport regarding the scope of the experimental method when applied to the investigation of social phenomena. It would be limited to the study of crowd phenomena, not embedded within the framework of a vast field called social psychology.

In the theoretical introduction to his experimental studies Moede discussed crowd psychology at length. Here we discover the conceptual roots of the design that run through almost all his experiments, as well as those of Allport, namely, the comparison of individuals' responses when alone with their responses to similar tasks when in a group. Crowd psychology had been built on a fundamental contrast between the supposedly rational conduct of individuals on their own and the irrational behavior of those same individuals in a crowd. "The individuals of a crowd," according to Le Bon (1960, p. 30), showed "special characteristics which are quite contrary at times to those presented by the isolated individual." Crowd psychologists had little difficulty in describing this contrast, "but," Le Bon (1960, p. 27) had to admit, "it is less easy to discover the causes of this difference." The gap had been filled by speculation. Here Moede saw an opening for the experimental method. If one were to create artificially, that is, experimentally, crowd psychology's contrast between isolated and group behavior, one could not only analyze the differences between the two with far greater precision than had hitherto been possible, one could also subject speculations about their causes to empirical test. Moede's experimental agenda was therefore set by crowd psychology. In the shape of the crowd, the social had become a problem that could be transformed into an experimental problem.

This transformation depended, however, on a particular conception of the crowd. The contrast between acting on one's own and acting in a crowd could provide the structure for a program of social psychological experimentation because the prototypical crowd conformed to the limitations of experimental situations far better than many other significant social phenomena. Although there was some temptation to extend the meaning of "crowd" to include members of institutions or even nations, the root image of the crowd, the prototype to which crowd psychology owed its existence and always returned, was that of a collection of individuals congregated in the same place for a limited period of time. It was possible to create such collections in miniature in the psychological laboratory, whereas the creation of an institution or a nation seemed impossible.

Crowd phenomena constituted one species of social phenomena that could find an experimental analogue within the already established format of psychological experimentation.

In a crowd, social influences were manifestly local and proximal—individuals who were physically present to each other in a confined space functioned as immediate social stimuli for each other. That was the kind of stimulus situation with which investigators in psychological laboratories had been working for some time, except that the stimuli would now be other people instead of disembodied visual figures, abstracted tones, etc. Second, compared to a vast range of social phenomena that had a historical dimension, the psychological effects operating in a crowd were short-term effects. One could therefore think of studying analogues of such effects experimentally. Moreover, because the crowd presented social phenomena in such a palpable physical form, these phenomena appeared to be eminently decomposable into elements that were experimentally manipulable without a loss of the total effect. Social elements in the shape of human individuals could be added to or subtracted from the social stimulus situation, the way in which these individuals related to each other could be experimentally varied, the spread of movements from one individual to another could be recorded, and so on.

For several reasons, Moede's experimental crowd psychology did not give rise to a recognized subdiscipline. In Weimar Germany, the paucity of support, even for traditional psychological research, was so extreme that such a development was not to be expected. And Moede lacked both the intellectual vigor and the international orientation that enabled a movement like Gestalt psychology to survive this limitation. The center of disciplinary development was now in America. In a sense, experimental crowd psychology did survive there, although not in an explicitly recognized form. It survived in the form of Allportian experimental social psychology, for there was a profound convergence between the two approaches.

Not only were Allport's paradigmatic experiments on social facilitation very similar to several of the experiments that formed the cornerstone of Moede's experimental crowd psychology, but Allport's definition of the social was entirely congruent with the image of the social that was built into the foundations of crowd psychology. His distaste for the "group mind" as an explanatory concept should not be allowed to obscure the profound affinity between his image of the social and that of crowd psychology. Their disagreement existed only on the level of the explanans, the group mind in the one case and essentially biological processes in the other. About the explanandum, the nature of the social, there was no significant disagreement. Allport explicitly denied any sharp distinction between group and crowd. A group consisted of some individuals "assembled to perform some task, to deliberate upon some proposal or topic of interest, or to share some affective experience of common appeal" (F. Allport, 1924, p. 260). What distinguishes such assemblies from crowds is the presence among the latter of "emotional excitement and the replacing of deliberate group activities by drives of the more primitive and prepotent level." In both cases, the social was constituted by the immediate presence of other people and the direct influence that this exerted on individuals. Like Moede, Allport perceived that such a conception of the social would lend itself to experimentation. But while Moede limited this insight to the field of crowd psychology, Allport generalized it to the social as such. He could therefore present his experimental program as a contribution to the much broader field of social psychology.

In this respect, Allport showed himself to be in tune with certain broader trends in the analysis of the social that had been developing since the closing years of the nineteenth century. Crowd psychology was only part of a more general shift of focus among some social theorists who were much less interested in such traditional topics as political domination, social revolution, and institutional structure, than in the micro-analysis of social interactions among individuals. The work of Gabriel Tarde is representative of this trend (Lubek, 1981). It is in this context that concepts like imitation and suggestion achieved prominence as ex-

planatory principles in the realm of social phenomena. Two general features of this change to a new image of the social were particularly important for the subsequent development of an experimental social psychology. First, there was now an emphasis on what Moscovici (1985, p. 9) calls "the primacy of the psychic element in collective life." This put on the agenda the question of investigating the operation of psychological factors in a social context, though the means were not yet apparent.

Second, the shift of attention to the interpersonal level of the social entailed a change in the preferred mode of understanding social phenomena. Traditionally, these had been understood in moral and religious terms. In the wake of the Enlightenment, this was replaced by secular perspectives that tended to interpret social events in terms of institutionalized power, mostly political or economic. By the end of the nineteenth century, however, a change of direction had become recognizable, especially in France. Writers like Tarde and Le Bon effectively disqualified both a moral perspective on human affairs in terms of social justice and the analysis of social effects in terms of power relationships. They did this by reformulating social issues in terms of one or other kind of *social influence.* They derived their models for social relationships, not from economics or politics, but from certain kinds of individual interaction, imitative in the case of Tarde, and hypnotic in the case of Le Bon and others (Apfelbaum & McGuire, 1986). That removed a major barrier to the possibility of social experimentation. Controlling and manipulating real social power for purposes of experimentation not only raises serious ethical problems but is likely to be impossible for practical reasons. Playing around with real social power would itself require the kind of social power that mere social scientists could hardly aspire to. But the investigation of social influence on the interpersonal level did not face such barriers. Once these influences had been accorded a major theoretical importance, their experimental investigation was perhaps only a matter of time.

The First Two Decades

Although the Allportian approach had placed experimental social psychology on the map, it did not suffice to establish it as anything more than a disciplinary backwater. Although a continuous tradition of experimental research on "the influence of social situations on the behavior of individual human adults" (Dashiell, 1935) existed in the 1930s, the scope of this research program was quite limited relative to the broader ambitions of people in the field. Hence the authors of newer texts of social psychology attempted to beef up their experimental component with other lines of work. Thus, Carl Murchison in his 1935 *Handbook of Social Psychology* devoted most of his experimental chapters to animal behavior. But in the furtherance of social psychological experimentation with humans, animal experimentation played at best an indirect role in that it tacitly supported a definition of the social in essentially Allportian terms. Animals have no symbolic culture, no institutions, and no history. For them the social does indeed reduce to the presence of conspecifics and their physico-chemical traces. Insofar as a biologically reductionistic social psychology had any effect at all on human experimentation, it would favor the investigation of local and direct interactions that coincided exactly with the agenda of Allportian social psychology. A biological definition of social behavior and its causes had already been incorporated into behavioristic social psychology.

During the 1920s, the experimental approach was extended to investigations of social aspects of child development, a trend that gathered momentum in the following decade. Much of the content of the first text with the title *Experimental Social Psychology* (Murphy & Murphy, 1931; Murphy, Murphy, & Newcomb, 1937) was based on these investigations. As

in the case of animal experiments, studies of the social life of children were eminently compatible with a definition of the social in terms of influences that were proximal, local, immediate, and easily decomposable. They were therefore obvious candidates for the introduction of the experimental method as practiced by psychologists.[4]

More significant for the development of experimentation in social psychology than work with animals or children were studies of social attitudes that began in the 1920s and gathered momentum subsequently. The authors of the earliest compendium in the sub-discipline (Murphy & Murphy, 1931) already devoted a chapter to this topic. Floyd Allport had made a pioneering contribution to this field too (Allport & Hartman, 1925), although in this case his role was less central.

The concept of social attitude that American social psychology had adopted had little in common with earlier concepts of attitude or with alternative ways of conceptualizing social consciousness (Danziger, 1997, chap. 8). Early studies in this field often treated attitudes and opinions as features of individual personalities. Others, however, grew out of a concern with the effects of propaganda and of mass media like film and radio. Such studies typically exposed experimental subjects to various messages in order to assess the nature and magnitude of these effects. What was "social" in these experiments was not interaction with other people, not the fate of social groups or their structure, not the creation of cultural symbols, but individuals' responses to messages that carried some kind of socially relevant information. Subjects in these experiments typically did not interact with each other—at most, they responded to unidirectional messages that contained information about (often hypothetical) others—and they did not control the nature of the messages that reached them. In these ways they resembled an *audience* more than any other social constellation.

Like crowd psychology at an earlier stage, mass media audiences provided a model of social life that easily accommodated to the constraints of psychological experimentation.[5] What such audiences had in common was their exposure to a particular set of messages that they received individually while in a certain place. The effects of such exposure were conceived as proximal and local as well as short term. This model began with specific messages presented here and now and ended with more or less immediate responses to these messages. What was excluded was any consideration of distal and long-term effects, the decisions and the power that lay behind the presentation of any particular message, and the cumulative effects of audience membership. The messages themselves, being part of a deliberate program,

4. Experimentation with children did show one novel feature that might have changed experimental social psychology rather profoundly. In standard practice, the behavior of subjects observed under experimental conditions was rigorously directed into specific channels, so-called experimental responses, and only this aspect of their total reaction to the experimental situation formed part of the experiment proper. Those who worked with children, however, often placed great value on careful observational records that allowed for a more complex understanding of experimental responses. In the long run, neither this feature, nor the general extension of experimental studies to children, had any profound effect on the development of experimental social psychology as a discipline. The lowering of experimental control, entailed by the use of observational methods in experiments with children, ran counter to the prevailing ideology within the discipline of psychology. In fact, the use of such methods virtually disappeared during the following decades. Moreover, the entire field of child psychology, including the experimental study of children's social life, ranked extremely low on the informal prestige hierarchy that operated within psychology as a whole. Gender bias played a large role here (Sherif, 1979). This field was virtually a female ghetto, and the chances of it influencing mainstream experimentation were probably nonexistent.

5. Ian Lubek has pointed out to me that the French crowd psychologists tended to stretch the meaning of "crowd" to include "publics," understanding the latter as a collection of individuals exposed to the same social message, e.g., a newspaper article, at more or less the same time. Thus, crowd psychology prefigured conceptions of the social that were crucial for both the major early research paradigms of experimental social psychology.

whether in a mass media context or an experimental context, were also easily decomposable for the investigation of effects that were highly specific. Such programs, however, derived their meaning from larger cultural patterns, the culture of the media, or the culture of social psychological experimentation that had by now acquired a distinctive character. But this kind of thing necessarily remained beyond the horizon of the audience model. Groups of experimental subjects, of course, had no culture by definition—their role was that of an audience, not that of a public. Social aspects of human life could be made to submit to the requirements of rigorous experimentation, provided one worked with suitably reduced conceptions of the social.

The Reinvention of Experimental Social Psychology

By the end of the 1930s, it was possible to detect an opening up of social psychological experimentation. In the wake of the Great Depression, collective social action had become more acceptable and gained a certain intellectual respectability, a development that was greatly strengthened by the social demands of the war effort. As a result, the taboo that had been placed on what Allport called "the group fallacy" became less effective and a space opened up for the experimental study of groups. Whereas previous studies had worked with group phenomena only insofar as they could be reduced to direct interindividual influences, for example, rivalry or suggestion, the new social psychology admitted irreducible group properties. Perhaps the first of these was represented by Muzafer Sherif's (1936) concept of "social norms." Subsequently, many more such concepts were introduced by Kurt Lewin, for example, "group climate," "group cohesion," "group decision."

What difference did this make to social psychological experimentation? At one level, there was no difference at all, in that experiments were still limited to the investigation of effects that were essentially direct and local, decomposable, and relatively short term. However, the new departure entailed a reinterpretation of all but the last of these features, and this promised a considerable broadening of the scope of social psychological experimentation.

This reinterpretation was most explicit in the work of Kurt Lewin. The programmatic quality of his work, as well as his reputedly large influence (Festinger, 1980; Patnoe, 1988), gives his contribution a historical significance that is comparable to that accorded to F. H. Allport in an earlier period. Others made important contributions to the reinvention of experimental social psychology around midcentury, but Lewin had no peers in the way he combined a new practice with a profound analysis of the nature of scientific experiments in general and social psychological experiments in particular.

Lewin drew on two major sources in his conceptualization of experimentation, Gestalt psychology, and the philosophy of Ernst Cassirer. The former led to an emphasis on the priority of whole situations as objects of investigation. Thus, social psychological experimentation would be directed at patterns of interaction characterizing groups rather than at the effects of specific stimuli on individual responses. Specific observations would still be made, but they had little significance in their own right. They had to be considered in the context of the "total situation," or "field," within which they were embedded (Lewin, 1952a). An early and paradigmatic example of this approach was provided by an experiment on "group climates" (Lewin, Lippitt, & White, 1939) in which a complex set of experimental interventions was employed to create "authoritarian," "democratic," and "laissez-faire" groups whose differences could then be explored. This constituted a significant shift in the favored experimental paradigm for social psychology. Experiments remained instruments of comparison. But what was being compared now were the properties of different kinds of groups, not the responses of individuals to specific influences emanating from other individuals.

Quite clearly, this departure implied a different conception of the social from the All-

portian one as well as a concomitant methodological change. From an Allportian perspective, concepts like group climate or group cohesion were horrible examples of the group fallacy. Only individuals had a physical presence and could be scientifically observed, not groups; hence only individuals were real. But for Lewin (1952a) instances of individual behavior had no intrinsic meaning. Their significance could be assessed only in terms of the situation in which they occurred. Group phenomena were part of that situation. They could be observed directly, just as Gestalt patterns could be observed in visual fields. Lewin's field theoretical conception of the social was one that allowed for the reality of groups. Of course, Lewin was not the only social psychologist of his time to be influenced by Gestalt psychology, but in setting out the conceptual and research implications of such a position, he had no equal.

The field theoretical approach affected the nature of the effects investigated by means of experimentation. These effects were no longer decomposable in the traditional, ontological, sense. Social psychological experiments would do more than establish functional relationships between specific stimulus and specific response variables; experiments would be the means for constructing complex social realities and exploring the complex effects of these realities. Such effects need not be direct, from one present individual to another. They were often indirect, understandable only in terms of differences in group structure. For example, the absence of the group leader would have very different consequences for observed behavior in "authoritarian" and in "democratic" groups.

Lewin drew on the philosophy of Ernst Cassirer for a perspective on scientific experimentation that could provide a way of transcending the purely local nature of experimental effects. Traditionally, one hoped to achieve this inductively—by the accumulation of more and more experiments the results of which confirmed each other. A major problem for that approach was the question of experimental realism: No matter how many studies are conducted under artificial laboratory conditions one still remains ignorant of the relevance of these studies for social conduct outside the laboratory. Cassirer's philosophy of science suggested another way around this dilemma. In a famous passage, quoted by Lewin (1949/1999), Cassirer (1910/1923, p. 254) pointed out that "the experiment never concerns the real case, as it lies before us here and now in all the wealth of its particular determinations, but the experiment rather concerns an ideal case, which we substitute for it." Lewin's (1935, 1927/1992) conception of psychological experimentation was based on this fundamental point. Experiments are local events, yes, but they are local events that have been artificially constructed so as to constitute a "pure case," concrete exemplars of the general laws underlying human interaction. Experiments had an illustrative function, much like the perceptual demonstrations of Gestalt psychology. They revealed the existence of general patterns and relationships that were also present, though confounded and obscured, in everyday situations. Ultimately, the function of experiments was not empirical but theoretical: The empirical relationships established by experiments were significant insofar as they provided instantiations of theoretical concepts.

Obviously, this was a view of experimentation that placed tremendous weight on good theory. Cassirer had used physics as the paradigm science, and Lewin (1936, 1938) attempted, quite unsuccessfully, to introduce a relatively high level of formalization into psychological theory, including social psychological theory. Lewinian concepts were popular for a time, but in practice they worked only insofar as they had not been formalized. Social psychological theory was simply unable to bear the weight that a Lewinian approach to experimentation would place on it. Consequently, Lewinian grand theory never really got off the ground. However, imperfections did not seem to have impaired the inspirational effect that Lewin's contributions had on a younger generation of experimental social psychologists. The decade following the end of World War II was an exciting and innovative one for this field, and

Lewin's example was often invoked. But this did not last. Experimental social psychology soon settled into a form of orthodoxy that owed little or nothing to Lewinian precepts. That development cannot be attributed simply to the weaknesses of Lewinian theory. Far more weighty factors were involved.

The Methodological Regime

There were large sections of experimental social psychology that remained totally unaffected by Lewinian considerations. Most prominent among these was the area of attitude research, one of the oldest in the field. Attitude research emerged from World War II with enhanced status due to the work of psychologists in the U.S. War Department's Information and Education division (Hovland, Lumsdaine, & Sheffield, 1949). A generation later, this work, and a postwar continuation, could still be referred to as "the most important fountainhead of contemporary research on attitude change" (Insko, 1967, p. 1). In its underlying conception of the social, this research tradition relied on a model that had already been in vogue before World War II and had remained impervious to Lewinian influences.

Experimental manipulation was conceived in terms of the features of a communication or message, and the effects of other people appeared as the effects of one of the features of communications, namely, their "source." The social individual was not investigated as one who responds to the actions of other individuals but as one who is bombarded by communications containing information relevant to his or her adaptation to the world. Such conceptions proved to be rather congenial to the information processing approach that was then affecting many branches of experimental psychology. However, the distinction between the effects of "social" conditions targeted in social psychological experiments and the effects targeted in other psychological experiments was somewhat tenuous. The distinction depended on a largely implicit (and ultimately arbitrary) classification of certain messages as "social" in nature.

As a continuation of prewar practices, this research tradition was quite conservative and was not easily reconciled with Lewinian conceptions of social psychological experimentation. Postwar changes in these conceptions, however, resulted in a new paradigm that could comfortably accommodate experimental practices that treated experimental subjects as individual reactors to messages.

Some ambivalence about the paradigmatic value of Lewin's empirical studies seems to have developed quite early. On the one hand, his dramatic manipulation of complex social situations was admired, but at the same time, his methodology was regarded as highly dubious. There was little sympathy for the Gestalt psychological aspects of Lewin's thought and total incomprehension of the neo-Kantian roots of his philosophy of experimental science. At the time of Lewin's death, at the beginning of 1947, the current of methodological precepts in American psychology was running strongly in another direction. Experiments were conceptualized in terms of the demonstration of functional relationships between specific stimulus elements, now known as independent variables, and specific response elements, known as dependent variables (Danziger, 1997, chap. 9; Winston, 1990). For such demonstrations, the variables needed to be defined unambiguously and measured along a single scale, that is, treated as unidimensional. Complexity would be represented by the multiplication of variables and their essentially additive interaction.

This ideology of experimentation was incompatible with the Gestalt assault on the decomposability of psychological patterns that had made it possible for Lewin to theorize about groups as groups. Referring to Lewin's experiments on "group climates," Leon Festinger, the most effective representative of the new methodological orthodoxy, later wrote: "I still have

no conceptual understanding of what all the differences were between these procedures" (Festinger, 1980, p. 239). This failure of comprehension was only to be expected because to Festinger "the independent variable to be manipulated was autocratic versus democratic atmospheres." If experiments are only to be thought of in terms of a framework of independent and dependent variables, then Lewin's procedures would indeed be incomprehensible. Many years earlier, in an influential treatment of laboratory experiments as tools for "behavioral research," Festinger (1953, p. 138) had contrasted Lewin's experiments with one of his own. The Lewinian approach was faulted because "rather than isolating and precisely manipulating a single variable or small set of variables, the experimenters attempted a large and complex manipulation. There was also little attempt at control . . . ." In Festinger's own experiment, cited as the counter-example, the effects of only a single independent variable were investigated. That way one could be more certain "that the results obtained are due directly to the variable manipulated by the experimenter."

The new experimentalism in social psychology did take over one Lewinian idea, namely, that there was a disjunction between artificially created experimental situations and social situations in everyday life. But there was a very profound difference in the reasons given for this disjunction. As already indicated, for Lewin, the special conditions obtaining in an experimental situation constituted an attempt at achieving a "pure case" from the point of view of a particular *theory*. Festinger (1953, p. 139), however, advanced a very different rationale: "The laboratory experiment should be an attempt to create a situation in which *the operation of variables will be clearly seen* under special identified and defined conditions. It matters not whether such a situation would ever be encountered in real life . . . . In the laboratory, however, we can find out *exactly how a certain variable affects behavior or attitudes* under special, or 'pure' conditions" (italics added). Instead of the ideal of theoretical purity which Lewin had adopted from Cassirer we now encounter a very different ideal, that of empirical purity.

The artificial conditions of the social psychological experiment are not justified by the need to provide an effective instantiation of a theoretical model, but by the need to demonstrate the unadulterated effect of singular manipulable variables. For Lewin (1952b) experiments had the function of revealing "essential structural characteristics" that could be "transposed" (in the Gestalt sense of that term) to other situations. But the experimentalists who succeeded him had a very different conception of what experiments were for. They were for isolating specific manipulable elements and their directly denotable effects (Festinger, Back, Schachter, Kelley, & Thibaut, 1950).

Another example of this kind of change was provided by the conformity experiments that became popular in the wake of Solomon Ash's influential studies. His experiments of the 1940s had still been dominated by Gestalt conceptions according to which social psychological experiments were conducted in order to create conditions that provided insights into the ways people cognitively structured certain situations. Accordingly, a crucial role was assigned to the accounts given by experimental subjects during the interviews that took place after the completion of the experimental manipulation. For an experimenter like Ash (1952, 1956), establishing the significance of any particular experimental effect meant placing it in the context of the subject's structured understanding of what was happening in the experimental situation. The reasons for the great variability in the ways, both overt and covert, in which individuals responded to conformity pressures therefore became the major focus of the inquiry. However, in most of the studies that followed Ash's lead, this focus was replaced by attempts at establishing general effects of manipulating specific situational and personality variables with little regard for the cognitive structuring of the experimental situation by either subject or experimenter (see the reviews by Allen, 1965, and Steiner, 1966).

Whereas Festinger had departed from the spirit of the Lewinian conception of experi-

mentation in several significant respects, the generation of his followers took social psychology much further in this direction (Stam et al., 2000). By the 1960s, the very notion of theory as structural model had dropped out of standard discourse on experimentation in social psychology, to be replaced by an entity called a "conceptual variable" (Aronson & Carlsmith, 1968).

This reorientation enabled experimental social psychology to participate in the wave of methodological innovation that was sweeping through psychology as a whole. A major manifestation of this development is to be found in the widespread adoption of analysis of variance as the primary technique of data analysis (Rucci & Tweney, 1980) and the subjection of experimental design to the requirements of this technique. The conception of social psychological experimentation promoted by the post-Lewinians was in perfect accord with this development, and analysis of variance was rapidly adopted as the technique of choice during the 1950s and 1960s (Higbee & Wells, 1972). Because this reorientation brought experimental social psychology, into line with well-respected core areas of experimental psychology it probably helped to make the field more acceptable within the discipline as a whole. Certainly during World War II, and in spite of the useful contributions by several social psychologists, there was still significant resistance to accepting social psychology as a scientific discipline (Capshew, 1999).

As in previous periods, developments during the postwar years were marked by a close link between methodological preferences and ontological presuppositions. The kinds of experiments that were now in the ascendant implied a model of social situations and of social conduct as composed of a multitude of separate, unambiguously denotable, elements and their essentially additive interconnections. This model could not accommodate the reality of social groups as other than the sum of their separate components. Moreover, because, under this model, experimental effects were invariably unidirectional effects, there was a strong temptation to banish any form of mutuality from the social world.

Inevitably, the study of social groups, which had flourished in the immediate postwar period, declined precipitously (Steiner, 1972) under this "regime of truth" (to use Michel Foucault's apt phrase). No doubt, there were broader historical factors that favored this outcome, but within the discipline their effect was refracted through the prism of prevailing concepts and practices of experimentation.[6] However, the new methodological regime proved lethal only for the social psychological study of real groups.

The most profound effect of the new methodological regime on social psychological conceptions of the social occured as actual social groups were gradually replaced by hypothetical groups that had a purely statistical reality. The random assignment of individuals to different groups defined only by their experimental treatment constituted a fundamental and inescapable part of this methodological regime. Through such an assignment individuals could be thought of as members of hypothetical statistical populations defined by a particular vari-

6. There was one area of social psychological experimentation for which the new methodological regime was particularly appropriate. This was the area of attitude research, one of the oldest in the field. In the heyday of the new methodological dispensation, the area came close to dominating the field as a whole. It was closely tied to the use of specific measurement scales the products of which easily lent themselves to treatment as precise denotations of psychological variables. In the experimental paradigm of attitude change, these became the dependent variables in a process that was, as usual, conceived as unidirectional. But it is the nature of the independent variable that illuminates a profound change in conceptions of the social underlying these experiments. The agents of experimental attitude change might have been, and occasionally were, conceived in Allportian or Lewinian terms as the behavior of other people or as features of group life. But in the most common experimental design, the social stimulus the effects of which were being investigated consisted, not of other individuals or of group interaction but simply of *messages*.

able attribute. Predictions of social behavior would have to be made in terms of shifts in the values of such attributes. Because individuals randomly assigned to a certain experimental treatment were regarded as a sample of a statistical population statistical generalizations could be made on the basis of data provided by this sample. Social behavior outside controlled experimental situations could be thought of as governed by an individual's simultaneous membership in a multitude of hypothetical statistical populations.

The new methodological dispensation did not replace the study of groups with the study of individuals. On the contrary, it relied totally on the study of groups. The crucial difference was that these were not socially constituted groups but statistically constituted groups. That substitution, however, implied a profound shift in conceptions of the social. The key to that shift was provided by the ideal of statistical randomization that underlay the methodological practices that were coming to dominate experimentation. Before they became subjects in a social psychological experiment individuals would be involved in all kinds of social relationships, would be members of various social groups, would have differing social histories. In the now dominant paradigm all this would be controlled for by their random assignment to various experimental groups. But that assumed that all these factors were carried by individuals independently of other individuals, something that was true only of multi-individual formations that had no internal structure, that is, *populations.* Only in this case will the random selection of individuals produce a sample that is representative of the whole. In the case of social formations structured by inter-individual and inter-group relations, for example, kinship groups, economic organizations, or administrative bureaucracies, a sampling theory based on the random selection of *individuals* would be inapplicable. An experimental practice based on the randomized assignment of individuals to treatment groups has an implicit social ontology, one that operates with populations rather than societal formations.

Fortunately for the new methodological regime there were real-life groups that approximated the ideal of a population consisting of independent individuals and having only an amorphous internal structure. Groups composed of individuals who are strangers or near strangers to each other, individuals whose ties to each other are shallow and superficial, would fit the model fairly well. Restricting the random selection of experimental subjects to such groups protects the plausibility of the underlying model. That may help to explain the overwhelming overrepresentation of college students among the populations providing subjects for social psychological experiments in the 1950s and 1960s (Higbee & Wells, 1972). Of course, there were strong practical considerations at work, but in the absence of some face plausibility, it is doubtful that the procedure of random selection from a pool of notionally unconnected individuals would have been accepted quite so unquestioningly.

On a more general level, it is worth noting that the model of social life presupposed by the most popular procedures of the social psychological laboratory seemed to approximate an anomic state in which isolated individuals without historical ties drift from one brief encounter to another. It is under such conditions that the random selection of individuals for different experimental treatments and their notional assignment to hypothetical populations would provide a rational basis for predicting social behavior (Danziger, 1992).

REFERENCES

Allen, V. L. (1965). Situational factors in conformity. In L. Berkowitz (Ed.), Advances in experimental social psychology (Vol. 2, pp. 133–175). New York: Academic Press.

Allport, F. H. (1919). Behavior and experiment in social psychology. Journal of Abnormal Psychology, 14, 297–306.

Allport, F. H. (1920). The influence of the group upon association and thought. Journal of Experimental Psychology, 3, 159–182.
Allport, F. H. (1924). Social psychology. Boston: Houghton Mifflin.
Allport, F. H. (1933). Institutional behavior. Chapel Hill: University of North Carolina Press.
Allport, F. H. (1974). Autobiography. In G. Lindzey (Ed.), A history of psychology in autobiography (Vol. 6, pp. 1–29). Englewood Cliffs NJ: Prentice-Hall.
Allport, F. H., & Hartman, D. A. (1925). Measurement and motivation of atypical opinion in a certain group. American Political Science Review, 19, 735–760.
Allport, G. W. (1954). The historical background of modern social psychology. In G. Lindzey (Ed.), Handbook of social psychology (Vol. 1, pp. 3–56). Reading MA: Addison-Wesley.
Apfelbaum, E. (1986). Prolegomena for a history of social psychology: Some hypotheses concerning its emergence in the 20th century and its raison d'être. In K. S. Larsen (Ed.), Dialectics and ideology in psychology (pp. 3–13). Norwood NJ: Ablex.
Apfelbaum, E., & McGuire, G. R. (1986). Models of suggestive influence and the disqualification of the social crowd. In C. P. Graumann & S. Moscovici (Eds.,) Changing conceptions of crowd mind and behavior (pp. 27–50). Cambridge: Cambridge University Press.
Aronson, E., & Carlsmith, J. M. (1968). Experimentation in social psychology. In G. Lindzey & E. Aronson (Eds.), Handbook of social psychology (2nd ed., Vol. 2, pp. 1–79). Reading MA: Addison-Wesley.
Ash, S. E. (1952). Social psychology. New York: Prentice-Hall.
Ash, S. E. (1956). Studies of independence and conformity: I. A minority of one against a unanimous majority. Psychological Monographs, 70 (9, whole No. 416).
Binet, A. (1900). La suggestibilité. Paris: Schleicher.
Capshew, J. H. (1999). Psychologists on the march: Science, practice, and professional identity in America. New York: Cambridge University Press.
Cassirer, E. (1923). Substance and function. Chicago: Open Court. (Reprinted Dover Publications, 1953. German original published 1910)
Collier, G., Minton, H. L., & Reynolds, G. (1991). Currents of thought in American social psychology. New York: Oxford University Press.
Danziger, K. (1983). Origins and basic principles of Wundt's Völkerpsychologie. British Journal of Social Psychology, 22, 303–313.
Danziger, K. (1988). On theory and method in psychology. In W. J. Baker, L. P. Mos, H. V. Rappard, & H. J. Stam (Eds.), Recent trends in theoretical psychology (pp. 87–94). New York: Springer-Verlag.
Danziger, K. (1990). Constructing the subject: Historical origins of psychological research. New York: Cambridge University Press.
Danziger, K. (1992). The project of an experimental social psychology: Historical perspectives. Science in Context, 5, 309–328.
Danziger, K. (1997). Naming the mind: How psychology found its language. London: Sage.
Dashiell, J. F. (1935). Experimental studies of the influence of social situations on the behavior of individual human adults. In C. A. Murchison (Ed.), Handbook of social psychology (pp. 1097–1158). Worcester MA: Clark University Press.
Festinger, L. (1953). Laboratory experiments. In L. Festinger & D. Katz (Eds.), Research methods in the behavioral sciences (pp. 136–172). New York: Holt, Rinehart & Winston.
Festinger, L. (1980). Retrospections on social psychology. New York: Oxford University Press.
Festinger, L., Back, K., Schachter, S., Kelley, H. H., & Thibaut, J. (1950). Theory and experiment in social communication. Ann Arbor MI: Research Center for Group Dynamics.
Geuter, U. (1986). Daten zur Geschichte der deutschen Psychologie. Vol. 1. Göttingen: Hogrefe.
Hacking, I. (1983). Representing and intervening. New York: Cambridge University Press.
Haines, H., & Vaughan, G. M. (1979). Was 1898 a 'great date' in the history of social psychology? Journal of the History of the Behavioral Sciences, 15, 323–332.
Higbee, K. L., & Wells, M. G. (1972). Some research trends in social psychology during the 1960's. American Psychologist, 27, 963–966.
Hovland, C. I., Lumsdaine, A., & Sheffield, F. (1949). Experiments on mass communication. Princeton: Princeton University Press.
Insko, C. A. (1967). Theories of attitude change. New York: Appleton-Century-Crofts.
Lakatos, I. (1978). The methodology of scientific research programmes. Cambridge: Cambridge University Press.
Le Bon, G. (1960). The crowd: A study of the popular mind. New York: Viking. (French original published 1895)
Lewin, K. (1935). The conflict between Aristotelian and Galilean modes of thought. In K. Lewin, Dynamic theory of personality (pp. 1–42). New York: McGraw-Hill.
Lewin, K. (1936). Principles of topological psychology. New York: McGraw-Hill.
Lewin, K. (1938). The conceptual representation and measurement of psychological forces. Contributions to Psychological Theory, 1(4).
Lewin, K. (1952a). Field theory and experiment in social psychology. In K. Lewin, Field theory in social science (pp. 130–154). London: Tavistock (Original published 1939).

Lewin, K. (1952b). Problems of research in social psychology. In K. Lewin, Field theory in social science (pp. 155–169). London: Tavistock (Original published 1943/44).
Lewin, K. (1992). Law and experiment in psychology. Science in Context, 5, 385–416. (German original published 1927)
Lewin, K. (1999). Cassirer's philosophy of science and the social sciences. In M. Gold (Ed.), The complete social scientist: A Kurt Lewin reader (pp. 23–36). Washington DC: American Psychological Association. (Original work published 1949)
Lewin, K., Lippitt, R., & White, R. K. (1939). Patterns of aggressive behavior in experimentally created "social climates." Journal of Social Psychology, 10, 271–299.
Lubek, I. (1981). Histoire des psychologies sociales perdues: le cas de Gabriel Tarde. Revue française de Sociologie, 22, 361–395.
Lubek, I., & Apfelbaum, E. (2000). A critical gaze and wistful glance at Handbook histories of social psychology: Did the successive accounts by Gordon Allport and successors historiographically succeed? Journal of the History of the Behavioral Sciences, this issue.
MacMartin, C., & Winston, A. (2000). The rhetoric of experimental social psychology, 1930–1960: From caution to enthusiasm. Journal of the History of the Behavioral Sciences, this issue.
Moede, W. (1914). Der Wetteifer, seine Struktur und sein Ausmass. Zeitschrift für pädagogische Psychologie, 15, 353–368.
Moede, W. (1920). Experimentelle Massenpsychologie. Leipzig: Hirzel.
Moscovici, S. (1985). The age of the crowd: A historical treatise on mass psychology. Cambridge: Cambridge University Press.
Murchison, C. (1935). A handbook of social psychology. Worcester MA: Clark University Press.
Murphy, G., & Murphy, L. B. (1931). Experimental social psychology. New York: Harper.
Murphy, G., Murphy, L. B., & Newcomb, T. M. (1937). Experimental social psychology (Rev. ed.). New York: Harper.
Pagès, R., & Vacher, O. (1993). Construction biséculaire de l'expérimentalisme social: de Fourier et Laplace à Godin et à l'expérimentation sociopsychologique. Sociétés Contemporaines, 13, 129–153.
Patnoe, S. (1988). A narrative history of experimental social psychology: The Lewin tradition. New York: Springer.
Rucci, A. J., & Tweney, R. D. (1980). Analysis of variance and the "second discipline" of scientific psychology. Psychological Bulletin, 87, 166–184.
Samelson, F. (2000). Whig and anti-Whig histories—and other curiosities of social psychology. Journal of the History of the Behavioral Sciences, this issue.
Sherif, C. W. (1979). Bias in psychology. In J. Sherman & E. T. Beck (Eds.), The prism of sex: Essays in the sociology of knowledge (pp. 99–133). Madison WI: University of Wisconsin Press.
Sherif, M. (1936). The psychology of social norms. New York: Harper.
Stam, H., Radtke, R., & Lubek, I. (2000). Strains in experimental social psychology: A textual analysis of the development of experimentation in social psychology. Journal of the History of the Behavioral Sciences, this issue.
Steiner, I. D. (1966). Personality and the resolution of interpersonal disagreements. In B. A. Maher (Ed.), Progress in experimental personality research (Vol. 3, pp. 195–240). New York: Academic Press.
Steiner, I. D. (1972). Whatever happened to the group in social psychology? Journal of Experimental Social Psychology, 10, 94–108.
Triplett, N. (1898). The dynamogenic factors in pacemaking and competition. American Journal of Psychology, 9, 507–533.
Winston, A. S. (1990). Robert Sessions Woodworth and the "Columbia Bible": How the psychological experiment was redefined. American Journal of Psychology, 103, 391–401.
Winston, A. S., & Blais, D. J. (1996). What counts as an experiment?: A trans-disciplinary analysis of textbooks, 1930–1970. American Journal of Psychology, 109, 599–616.
Wundt, W. (1907). Ueber Ausfrageexperimente und ueber die Methoden zur Psychologie des Denkens. Psychologische Studien, 3, 301–360.
Wundt, W. (1908). Logik (Vol. 3, 3rd ed.). Stuttgart: Enke.