

## CHAPTER

# 15

## TESTING THEORY IN ACTION

When America entered World War II, the resources of the social and behavioral disciplines became an important part of the mobilization effort. The demand by many military and other government programs for professional advice forced the experts to evolve new ways of tackling problems and analyzing and reporting results. Moreover, these data and recommendations had to be communicated in a way which could be understood and used by laymen who had to act on them.

For many social scientists, the transition from safe academic work to an immediate concern with the hazardous application of research findings in making major policies was radical. But for Lewin and the majority of his colleagues, the transition was a logical consequence of the main thrust of their previous work. Thus they were readier than most others in the hastily improvised circumstances to investigate "real life" problems with more scientific rigor and confidence.

Lewin was limited in his early efforts because, not yet an American citizen, he could not get security clearance. But on January 5, 1940, he sent me a handwritten note saying, "I am a citizen! Hurrah!"

Soon Lewin and his group were taking up such questions as:

What was the state of morale and its probable future course both in enemy countries and on the home front? What techniques of psychological warfare would most effectively weaken the enemy's will to resist? What kind of leadership in military units was likely to be the most successful? How could more such leaders be found and trained? How could home-front consumption of foods in short supply be cut back and the use of more available foods be encouraged? How did human relations in office and factory affect war production in America's industries? What measures could be taken to care for and psychologically rehabilitate those injured in combat?

Solid answers to such questions, when translated into decisions, policies, and programs, might exercise a profound influence on the course and outcome of the war. All of them required seasoned judgment, based on factual knowledge in the behavioral sciences. Team effort was important, for, by its nature, the program usually required the contribution of more than one specialty or discipline. Little wonder, then, that the earliest, most continuous, and heaviest demand for aid was made on those who had had the most experience with psychological research in real-life situations and who were accustomed to working as teams: cultural anthropologists, such as Margaret Mead; public-opinion researchers such as Samuel Stouffer, Paul F. Lazarsfeld, and Rensis Likert; psychologists such as Murray and MacKinnon, who had worked in the Harvard Psychological Clinic and later played such an important role in the Office of Strategic Services (OSS); and Lewin and his Iowa group.

John MacMillan, of the Office of Naval Research, invited a group of social scientists—Lewin among them—to review research proposals and discuss general ONR policy. According to Rensis Likert, another member of the committee that was formed, "Lewin's ability to identify the major problems on which research was needed made him an invaluable member. Frequently when a methodology was inadequate he devised a procedure as well as a general theory of conceptualization to deal with the particular problem on which the research was being done. Lewin was much more interested in having significant research started on major social problems, even if the approach was crude, than on unimportant problems with nice,

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neat, precise methods. His willingness to move ahead even if the methods were tentative was a factor in the criticism aimed at him by some psychologists who held that his research had not adequately produced the large body of quantitative data required for his major conclusions. The soundness of his work, however, is amply demonstrated by the extent to which his central concepts have stood up as research on them and related problems has been undertaken in the two decades since his death."

All the experimental and other studies done for the OSS and ONR were related to the national emergency, to the practical requirements of the government departments which underwrote the research, and to the life of people in the local communities—a life vastly different from those psychological laboratories where abstractions called "subjects" performed tasks stripped of direct relationship to everyday life problems.

"There is little doubt," says Margaret Mead of this period of inquiry, "that coming to grips with the non-arrival of a shipment of lettuce involved in an experiment was an excellent setting for cooperation between anthropologists and psychologists and for the concept of 'action research.' The studies also offered many significant theoretical implications, and Lewin and his students and colleagues were on the alert for them. On some of these theoretical leads, such as those developed by Bavelas and French's studies of group performance at the Harwood plant, would someday be built many useful social procedures." She also remarked that Lewin's general program of action, plus his knowledge of how and why to experiment and of the relationships between experiments and theory, tied together the many and diverse topics which were investigated by the Iowa group.

Similarly, such seemingly trivial questions (from a research point of view) as to what would make a new drinking container most acceptable led to a series of experiments which defined the relationships between the beverage preferences of the user, various types of containers, and beverages.

One finding of this brief study, conducted by Festinger, was that the use of a new container is more acceptable if the type of beverage

it contains is also new. This provided an interesting confirmation of a general principle in field theory and action research: that change of one kind of behavior is associated with changes of other kinds, and that it is important in bringing about change to look at the whole behavior system involved, rather than isolated actions or decisions.

During the war Lewin traveled frequently from Iowa City to Washington; he participated in many committees of social scientists, administrators, and military personnel working on a wide range of problems. Much of his labor—for example, advising the OSS on psychological warfare programs—was never documented in detail; some of the information concerning these activities is still classified. “But it is known,” according to Lippitt, “that he made very creative contributions to the working out of the proper relations between psychological warfare, target setting, field operations, and evaluative reconnaissance.”

His ability to grasp complicated ideas and to see complex relations was combined with a power of sustained concentration and an enormous enthusiasm for his work. The capacity of great scientists and artists to labor long hours is legendary; Lewin was one of them. He had a remarkable ability to keep a variety of projects, committees, experiments, and writing assignments going at once.

Those who worked with Lewin at that time believe that he was convinced that any psychological problem could be examined by means of experiment. He rarely failed to devise a way to test any thesis or to try out any hypothesis in the laboratory or in the field.

As the end of World War II seemed to be approaching, Lewin was completing nearly a decade of productive work at Iowa. The research output of his own students at Iowa, and students of his former students at other colleges and universities, on problems stemming from his own theory and research was enormous and diverse. Some idea of this can be gained from a glance at some of the titles of the published studies. In 1937, Herbert Wright published “The Influence of Barriers upon the Strength of Motivation,” and Mary Elizabeth Keister, “The Behavior of Young Children in Failure,” an experimental attempt to discover and modify undesirable

responses to failure in pre-school children. In the following year, I published "Goal Tension and Recall," and 1939 saw the publication of Daniel Adler's "Types of Similarity and the Substitute Value of Activities at Different Age Levels," Adler and Jacob Kounin's "Some Factors Operating at the Moment of Resumption of Interrupted Tasks," and Lewin, Lippitt, and White's "Patterns of Aggressive Behavior in Experimentally Created 'Social Climates.'" In 1940 Lippitt brought out "An Experimental Study of the Effect of Democratic and Authoritarian Group Atmosphere"—a preliminary to the classic "autocracy-democracy" study; C. Anderson published "The Development of a Level of Aspiration in Young Children," and Sybille Escalona, "The Effect of Success and Failure upon Level of Aspiration and Behavior in Manic-Depressive Psychoses."

The following were published in 1941: Dorwin Cartwright's "Decision Time in Relation to the Differentiation of the Phenomenal Field," Jacob Kounin's "Experimental Studies in Rigidity," Barker, Dembo, and Lewin's "Frustration and Regression," and John R. P. French's "Disruption and Cohesion of Groups." In 1942, Roger Barker's "An Experimental Study of the Resolution of Conflict in Children" appeared, along with Beatrice Wright's "Altruism in Children and the Perceived Conduct of Others," Mary Henle's "An Experimental Investigation of Dynamic and Structural Determinants in Substitution," and Alex Bavelas's "Morale and the Training of Leaders."

In the last two years that Lewin was at Iowa, there came Lippitt and White's "The 'Social Climate' of Children's Groups," Cartwright and Festinger's "Quantitative Theory of Decision," Erik Wright's "Influence of Frustration upon the Social Relations of Young Children," and Jerome Frank's "Experimental Studies of Personal Pressure and Resistance"—the study on which he had been working when he first encountered Lewin at Cornell. These appeared in 1943. John R. P. French's "Organized and Unorganized Groups under Fear and Frustration" and "Level of Aspiration," by Lewin, Festinger, Sears, and Dembo, came out in 1944.

But Lewin was becoming increasingly restless. His attention centered more and more on group dynamics, experimental social psy-

chology, and the process he termed action research. Psychology, he decided, needed to do more than just seek explanations of behavior. "We must be equally concerned," he said, "with discovering how people can change their ways so that they learn to behave better." This, he held, required experiments. He pointed out that "for thousands of years man's everyday experience with falling objects did not suffice to bring him to a correct theory of gravity. A sequence of very unusual, man-made experiences, so-called experiments, which grew out of the systematic search for the truth, was necessary to bring about a change from less adequate to more adequate concepts."<sup>1</sup> The same situation, he believed, was true in human affairs. Systematic scientific experimentation was needed to study social problems in small and large settings. But he doubted that such controlled field experiments could be carried out at Iowa.

Other factors were at work affecting Lewin's mood. His wartime travels produced psychological strains, since some Iowa colleagues showed resentment at his frequent absences and his "starring role." Some colleagues, besides disagreeing with his theories, criticized his consultant activities which forced him to neglect routine academic duties, such as attending faculty meetings. Finally, there were many Americans—"corn-fed, dust-bowl empiricists," in Allport's phrase—who were anti-theoretical and especially opposed to Lewin's emphasis on a philosophy of science. Many of these rejected Lewin because his approach did not emphasize large-scale testing and statistical analysis.

For his part, Lewin became skeptical about remaining in a conventional academic setting as his concept of the organization of action research developed. It could be best developed, he felt, if he presided over an autonomous institute affiliated with a university, but not subject to its routines. Of course, group dynamics and action research were debatable projects among his academic peers. Most preferred the non-activist tradition of academic psychologists and yearned to return to teaching, writing, and research in the security of Academe. So Lewin went his own way on his own work.

<sup>1</sup> K. Lewin and P. Grabbe, "Conduct, Knowledge, and Acceptance of New Values," *Journal of Social Issues*, 1945, 1, 53-63.

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His Washington assignments had placed him in a collaborative relationship with many non-academic people. The roles of practitioner and administrator (though he was not particularly efficient) gave him certain satisfactions. They offered a means of keeping in close touch with practical affairs and social issues. Moreover, certain problems were of great personal concern to him. Among them was a situation rather than a problem. He had been proposed by Horace Kallen, then Dean of the Graduate Faculty at the New School, as successor to the late Max Wertheimer who—together with Köhler—had been a colleague of Lewin's in Berlin.

To Kallen, it seemed that a fellow Gestaltist such as Lewin, who was a theoretician of great originality, an experimentalist, and a philosopher of the mind, would be the appropriate "*Nachwuchs*" to Wertheimer at the New School. Outsiders, too, when Kallen broached the idea, were enthusiastic; but not Köhler, the other Berlin Gestaltist, then at Swarthmore. Lewin, the innovator, was too heretical for Köhler. Köhler's counsel was followed, and in spite of the efforts of Kallen and Gardner Murphy, Lewin was not invited to join the New School faculty.

The rejection caused Lewin a good deal of self-questioning—not because he was greatly concerned about the post, but because of the personal attitudes that came into play. He felt that for progress in his projects of action research and the dynamics of groups, Iowa was no longer an appropriate location. It was necessary to find a more fitting place and better opportunities, and he so advised several of his friends. He was especially concerned about his chance for unhampered inquiry and pursued his idea of an autonomous institute loosely attached to some university. The financing of such an institute would not, he realized, be undertaken by university authorities; it needed sources outside the conventional academic budget—from foundations, perhaps, and through personal contributions. And thus Lewin next turned his efforts toward a pursuit of funds for his institute.