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SOME HISTORICAL NOTES ON THE STUDY OF ACTION

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A Description of an Intellectual Odyssey

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Some Historical Notes On The Study of Action

It has often been said that each generation rewrites history in the light of its own contemporary problems. In the same way, one might say that each generation of psychologists has rewritten "theories of action." At some periods and in some contexts, the study of action has been considered the very center of psychological analysis; at other times and in other contexts it has received no attention at all.

One very good indication of varying interests in the study of action can be found in the psychology textbooks of different periods. Invariably, American textbooks written during the 1920's included a chapter on the "psychology of action." Such a section usually contained a graph in which a line from left to right represented the passage of time. This line would be divided into a number of sectors denoting different phases of the "activity in progress." (M. Bentley, 1925; R. Woodworth, 1923.) Although different authors distinguished different numbers of phases, there were always at least three: one indicated the beginning of the action -- a task is taken on, or an intention is acquired; the middle phase dealt with elements which push the intention forward; and the final phase referred to the resolution or consummation of the act. Usually, also, the relative importance of various factors at different phases of the activity were considered and discussed -- the motives of the actor, outside influences impinging on his, and his information about

the world around him. It was taken for granted that one or two examples would make clear what the word "acting" referred to: the man going to town to mail a letter, the child exploring a toy, the bird flying to a rock. By the middle of the century (with the exception of Woodworth's textbook on which we will comment later), the examples, the topic, let alone sections devoted to it, completely disappeared. The term "action" does not seem to appear in the recent textbook of H. Garrett (1950), or Stegner-Kominsky (1952), or Hilgard (1953). It may be that we have up to now looked into the wrong textbooks and that the social psychologists have maintained interest in this problem. But this, again, is not the case. If we look at Krech and Crutchfield (1948), Sherif (1948), and T. Newcomb (1950), nowhere do we find an explicit treatment of human action or a mention of the term in the index.

This is a puzzling situation, and it raises the question of what has happened to the study of action. One answer might derive from the nature of the problem and the intellectual mood of psychologists in the period between the two World Wars. Anyone who has tried to define action has had reference to its goal-directed character. Whichever writer discusses action, it is defined as a behavior sequence deriving from a purpose of the actor which he or at least the investigator understands as meaningful. Even Warren's Dictionary of Psychology (1934), which attempts to do justice to all possible theoretical positions, defines action as "a general term for all movements or patterns of movements

with which certain conscious patterns are or have been correlated." If the notion of action requires some reference to consciousness, then it is not surprising that in the decades which saw the intellectual dominance of behaviorism there were not many scholars in the United States willing to approach such an apparently unrewarding subject matter.

Correlative with the general character of this trend was a more specific one. When Karl Bühler commented (1926) on the ambivalent attitudes of social scientists to the problem of action, he noted:

> "Action is a historical, or more precisely a biographical concept.... Every human action has its history (aktgeschichte) -- sometimes a long and rich, sometimes a short and poor one. Raskolnikov needed weeks from the first emergence of the idea to the final (consummatory) act. (In other cases) the theoretical psychologist has to consider seconds and fractions of seconds, if he wants to analyze a quick decision, which immediately turns into execution." (p.52)

Again during a period of stress on an oversimplified image of operational definitions, aversion against such a seemingly amorphous concept should not surprise anyone.

The situation would be very simple if it were all a matter of intellectual fashion. Now that most of us concede that introspection in one form or another is here to stay (E. Boring, 1953), and that "partial definitions" are alleviating our logical conscience (A. Kaplan, 1946") we would expect that the study of action will slowly be resumed. And, indeed, a president of the American Psychological Association, in his

presidential address of 1951, placed the matter on the top of the agenda for the profession. Robert Sears (1951) states his conviction in not uncertain terms:

> "... What is needed at present is a single behavior science, with theoretical structure that will account for the actions and the changes of potentiality for actions both of individuals and of groups ... action is clearly more significant than perception or traits ... actions are the events of most importance, and actions are most available to observation and measurement ... there is no virtue in a descriptive statement that a person or a class of persons possesses such-and-such a trait or need unless that statement is part of a larger one that concludes with a specification of a kind of action to be performed."
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> (467-468)

And clearly Sears' concerns are with just the kinds of situations which interest us here. What he wants is a theory of action to deal, for example, with "whether a marriage will terminate in divorce or whether citizens will buy bonds or vote for a Congressman" etc. He stresses that "even those changes commonly interpreted as perceptual, such as art and music appreciation, are evidence in the form of choices as to where to go, what to look at, what to listen to."

In this connection, we advisedly speak of a concern, and not of a problem. The topic of how people make choices between available alternatives is such a broad one, having the character of a general intellectual program, that we cannot derive from it directly what might be the appropriate ways of empirical study and theory formation. This

raises a second and more important question: what can really be meant by a theory of action? Perhaps no one can answer this question at the moment. But we certainly cannot overlook the fact that the need for an answer was once very paramount and has somehow been thwarted. Now that we seem to have a second chance, we might easily be in for another disappointment if the lessons of the past are not taken to heart. Indeed, as we shall try to show, there seems to have been a set of additional difficulties beyond the academic temper of the times which have stood in the way of successful and continuous development. These have been partly accidental in nature, connected with the disruption of two World Wars and the transfer of the center of gravity from European to American locations of research. In addition, however, an interesting intellectual difficulty has appeared.

After an initial frontal attack on the study of human action, the main students concerned each selected a different aspect of the total concern, and the work of each has developed without any careful effort at integration. It is true that in learning theory, as well, there are various schools and controversies, but these controversies themselves show the existence of a common core of interest and an effort to reconcile available facts and methods. This has not been the case in the empirical study of action. Therefore, as we follow these different trends, we shall organize our attention roughly around three questions. Which elements in the general concern with human action have been selected by an author or school as a specific problem? What contributions were they able to make?

What prevented them from integrating their work with that of others into a more general whole? Because our review covers the turbulent times of the last few decades, we will find that the difficulties which were met were partly intellectual in nature and partly social. The slowdown of academic work in Europe and the shift of general emphasis to the American scene will turn out to be important in various forms. We shall, therefore, give more space to writings which are not available in English translation and treat with more brevity literature which is easily available here and which has become part of American textbooks.

The Wurzburg School as an Early Matrix for the Study of Human Action

The empirical study of action began very vigorously at the start of this century when a group of German psychologists, known later as the Wurzburg school, went about finding out "what happens when people do something." This was part of a general tendency to transcend the tradition of Wundt and to submit the "higher mental processes", especially thought and will, to experimental investigations. The accepted method was still the sharply controlled laboratory experiment. The stimuli, however, were intended to elicit complex reactions and free rein was given to detailed introspective reports of the subjects and interpretation of these reports by the experimenters.

The basic pattern of the experiments was as follows

(1) The subjects were informed of the task they were to perform.

(2) A 'ready' signal was given, indicating to the subject that the experiment was about to proceed.

(3) One or a few seconds after the 'ready' signal, a stimulus was given to the subject and he had to carry out the task which had been agreed upon in advance.

The stimuli to which the subjects were exposed were of various kinds. In the early experiments the subjects were instructed only to add two simple figures which would appear before them or to find a rhyme to a nonsense syllable. Later the number of stimuli, and the possible responses, were increased and varying combinations of uncertainties between stimulus and action were introduced. The whole field was so new at this time that any finding was of interest. The reaction-time was found to be clearly related to the complexity of the task. If letters were to be substituted in nonsense syllables, some group of letters seemed to have greater propinquity than others. However, most of the attention of these investigators was given to analyzing the reports of the subjects. It turned out, for instance, that the intention to solve the problem was most clearly experienced in the "before period" between the 'ready' signal and the actual exposure to the stimulus (step 2). Once the stimulus was exposed, the reaction followed automatically without the intention being experienced again. From this derived the famous doctrine of the determining tendencies, which is so closely related to the subsequent history of the concept of attitudes (G. Allport, 1935). It is not easy today to reconstruct what the members of the Wurzburg school would have

considered the main purpose of their work. The transfer of experimental procedures to higher processes was certainly one main motive, as mentioned before. There were also involved discussions around the question of what are the basic elements of mental experiences and we would not be too interested in these discussions today. Very characteristic and fruitful for our present discussion, however, was the intense interest of the Wurzburg group in the sequences of concrete acts and the laws by which their course is regulated. It is this broad approach which made the Wurzburg school the matrix for a number of the specific developments which we are about to describe. And it is this unity of approach which has regained significance.

Because of the important historical position of the Wurzburg school, it is regrettable that its work is accessible to the American reader only in fragmentary form. Boring (1950) has given a very insightful, brief description of their work. There he makes the important statement as to how "it became clear that the problems of thought and action are essentially the same ... the key to thought as well as to action is to be found in the preparation of the subject." However, this dual interest of the Wurzburg school in thought and in action has led to a singular difficulty in most other secondary reports of their work. They can almost be considered projective tests reflecting the interests of the reporter. For example, we have the first textual translation of some of the original writings by Ach and Bühler in a recently published

annotated reader on Organization and Pathology of Thought (D. Rapaport, 1952). Since the editor's interest is centered on thinking, he apologizes for one of his authors, pointing out that although his "discussion is couched in terms of action peculiarly, the appearance in consciousness and the effects of thoughts are the subject matter of this discussion."

On the other hand, the American reader might turn to Lindworsky's text book on Experimental Psychology (1930); it is the only translated text which was written in the spirit of the Wurzburg experiments, even if the author did not quite belong to this tradition. Lindworsky's own work was mainly in the field of will, and therefore he includes the Wurzburg school in the section of his book dealing with "volitional life." (pp. 303-338) He, too, apologizes, indicating that the reader can hope "to gain a better understanding of the experiences of attention, since the most important of these experiences may be explained only as expressions of volition." As far as thinking is concerned, the most detailed secondary source at this moment, as far as we know, is Humphrey. The most thorough review of the volitional aspect of the whole tradition is available only in German. N. Ach (1935) published a 200-page review of the experimental work on the psychology of will, carried on during the first third of this century. We shall repeatedly refer to this comprehensive text.

Beginning about 1910 the Wurzburg school tradition was developed essentially in three directions, carried on partly by the members of the group itself, their students, and those who were indirectly influenced,

and partly by scholars who wrote in opposition and yet were clearly marked by the Wurzburg stamp.

The main three trends can be easily understood if the original experimental scheme is meaningfully enlarged. The first step, the reader will remember, consisted in the subject being given a task by the experimenter. If we now substitute for the experimenter other sources of goals -- society, the past experience of an individual -- this branches out into what is usually referred to today as the problem of motivation. In this the main attention of the student is directed toward the question of the nature and origin of goals. The second step was initiated by the 'ready' signal of the investigator. At this point, the intention to act was set up in the subject, and the determining role of this intention all through the rest of the act was one of the main topics of the Wurzburg school. A considerable amount of subsequent work remained concentrated on studying what happens with intentions, once they are set up, whatever the broader motivation behind them might be. In the third step, the final reaction of the subject was released through exposure to the stimulus. This is easily broadened in the study into the influences and occasions which transform an intention into a final consummatory phase of an action. The three trends, then, which we shall now exemplify, are characterized by their emphasis either on the general problem of goals (motivation) or on the successive transformation of intentions or, finally, on the influences which seem to push the act forward to its final conclusion.

The three case studies which seem to illustrate best these three points are Karl and Charlotte Bühler, for the first, Kurt Lewin, especially in his relation to Ach, for the second, and, for the third, a type of research which is not easily identified but which, as applied psychology and the study of propaganda, has placed special emphasis on problems of manipulation in its various forms.

Accent on Goal -- Karl and Charlotte Bühler

Among the members of the Wurzburg group Karl Bühler's early work (1913) went furthest in the effort to study complex thought processes. In the course of this work, however, he became the one who maintained and strengthened most the interrelation between the psychology of thought, on the one hand, and of will and action, on the other hand. In retrospect, he formulated the situtation as follows. (Free and condensed translation from Bühler, 1927, page 13):

> " In a complex arithmetical expression we have numbers, on the one hand, and operational signs, like addition and multiplication, on the other hand; in the same way, when we observe an artist or a craftsman, we notice that he works with material but that he performs certain techniques and manipulations on it. This is essentially what has slowly been brought out: in thinking, as well as in any other activities, we need a certain number of operations and these are the ones which are the crucial elements for a systematic analysis." (Italics ours)

The interrelation between materials and operations has probably remained one of Bühler's central themes. It first led him into his major

contributions on the perception of color and form. In both studies the emphasis was on the fact that perception depends not only upon what is presented to the subject but also what he does with it; these ideas led through the experiments of Brunswik and his students directly to the modern interest in motivated and directed perception, a topic which cannot properly be pursued here. But parallel to this work, Karl Bühler also turned to child psychology to a rather far-reaching development in our field of interest. The first paragraph of his book contains a sentence which is revealing in many ways. Bühler describes as his purpose the study of how the child becomes a human being (menschwerdung). The word Mensch in German has the connotation of man, with special emphasis on the spiritual and creative elements which distinguish him from non-human beings. Bühler was obviously not satisfied with merely describing the motivational elements in perception. Through the study of the child he wanted to find out how the capacity to form and reform the outside world came about. The book accordingly ends with a chapter entitled, "Elements of a General Theory of Mental Development." Here, again, we must remember that the word "geistig", translated here as mental, has a very broad connotation in German, implying everything which is characteristic of the mature human being. +

+ Linguists tell us that the root of the German word Mensch can also be found in the English word mind, the German word Meinung, and even in Minne, which is an older German term for love. It is important to remember that the German terminology has to some degree retained this consonance of meaning.

The scene was thus set about as follows. The mature human being is characterized by creative activity on physical and social objects. The main task of psychology is to study the origin and development of this faculty. A theory of action is therefore needed, the center of which would be the proper understanding of goals: the main empirical source for such a task ought to be developmental and comparative psychology. Bühler's theory of mental development was at that time a variation around the theme of three consecutive steps through which the springs of action progressed. They might be translated as satiation, play and achievement. First the child is interested only in the intake of food, it craves satiation; then the exercise of its developing faculties provides "functioning-pleasure"; finally, work on objects becomes an additional source of enjoyment. Three levels of activities, in the narrowest sense, correspond to these three basic relationships: mere reflex action, learning by trial and error, and finally, creative intelligence.

Ten years after Karl Bühler's book appeared, his wife and close collaborator, Charlotte Bühler, published her book Childhood and Adolesence (1927). In this, the idea of phases of development had become dominant, and the central theme deals with how the ability for mature human action (handlung) slowly grows out of the first primitive, and mainly physiological, actions of the newborn infant. Again, there is development from mere receptive gratifications to the enjoyment of one's own functioning, and, finally, the goal-directed organized performance is

in the center of the analysis. The theme is systematically enlarged by a distinction between the three main object areas toward which these activities can be directed: things, people, and what we would today term culture, that is, ideas, norms and institutions. Charlotte Bühler thus distinguishes "phases" which are construed from an interweaving of these steps toward maturity with these three sectors of the world. In an early phase, for example, the typical child would have work maturity in dealing with inanimate things, but would be on the level of mere functioning in talking about his future occupation. In a next phase (around 8 to 13 years), the child reaches a certain maturity in his personal relationships with people, in beginning to integrate himself into groups in the role of member. But it still "plays" at responsibilities which the later citizen has in his community. Finally, in adolescence, maturation begins to cover cultural and institutional subject matters.

Five years later this basic idea was further extended in Charlotte Bühler's book on The Course of Human Life as a Psychological Problem (1933). Her central problem was as follows. The adolescent has the first glimpses of the notion of devoting his life to an objective, transcending his own individual existence. What was the further development of this goal striving? Charlotte Bühler introduces the concept of "self-determination" meaning the striving toward a selected goal, implying the individual's intent to dedicate himself and his feeling that he is meant to accomplish certain specific things, a feeling which in the case of some people assumes even the conviction of being destined to a certain role or development.

This "self-determination" toward an end in life begins as a prepatory, experimental and preliminary one in what is called life's second phase (about 15 to 25 years); this is followed by a definite self-determination in the third phase of life (25 to 45 (50) years) in which the individual usually settles down with a definite occupation, family, home; in the fourth phase (45 (50) to 60 (65) years), the individual evaluates the success, results, products of his striving and often experiences the so-called "climacteric" self-reproaches about his failures which made him fall short of his own expectations and goals. This self-evaluation may lead on to increased feverish endeavors to bring in the harvest as envisaged in years before or it may lead on to resignation, despair, actual breakdown if it seems "too late" to change the unfavorable course of events. The final fifth phase of a contemplative retrospection on life, paralleling the initial first phase before self-determination to goals sets in, occurs in this harrassed time and civilization only with those who voluntarily or by necessity will disengage themselves in their late years from the struggle of goal-striving to live in retirement from life.

This development of the individual's self-determination through life is, in Charlotte Bühler's theoretical system, shown in relationship with a psychobiological process of maturation. Based on the biological facts of growth, reproduction and decline, she defines phases of expansive, stabilized and restrictive growth and reproduction processes which represent the underlying psychosomatic tendencies to the self-determination changes.

As far as the empirical study of action is concerned then, the work of Karl and Charlotte Bühler led to a system of categories by which goals of human action could be systematically described. Before turning to the implication of this, a digression is necessary into the source and variety of data with which they and their students worked.

Data and Categories for Analyzing Human Goals

By the end of the first World War Karl Bühler had reached the conviction that the analysis of human action would be the focal topic of a modern psychology. But while at first it seemed to grow naturally out of the Würzburg effort to study "higher mental processes", two intellectual developments had come about in the meantime which made the program appear more complicated. This is what Bühler describes, finally in 1927, as the contemporary constructive "crisis of psychology." On the one side there now were the behaviorists who made it increasingly difficult to speak of perceptions, intentions, or any of the otherconcepts of goal-directedness which were indispensable for the definition of an act. While most German psychologists were immediately hostile to behaviorism, on philosophical grounds, Bühler from the beginning hailed the contributions which might be expected from the "Americans", a term he used synonomously with behaviorists. There was still another attack, however, against classical German tradition, coming from a movement of which Bühler himself says, "No one talks about it except the Germans, and even among them it is still very controversial."

At the end of the 19th century, Dilthey had become very impatient with experimental psychology, as developed by Wundt and his students. He was one of those who wanted to see the complex problems of mental life investigated, but he wanted it done quickly, as opposed to the Würzburg scholars who were willing to slowly extend the scope of experimental work. Basically, Dilthey's suggestion was to take human products, such as art or philosophical writings, and to analyze their structure in order to derive from it psychological knowledge about those who created these works. In principle, this is the idea which, in more modest form, underlies the use of projective methods in psychology today. It seems justified, therefore, to refer to this whole movement as projective psychology, although at the time it was called "geistwissenshaftliche" psychology. Dilthey mainly wrote of intellectual and artistic products, while later on, institutions and physical objects in cultural use were included as desirable sources.+ Bühler's position was as follows: There are three aspects under which human action can be seen. The aspect of the private experience of the actor, the former introspection; the aspect of his observable behavior in a concrete situation, which meant, of course, behaviorism; and the aspect of the product brought about by the act, projective-psychology. His effort in the Crisis was to show that all three aspects are indispensable for a study of human action, and that all forces should concentrate on clarifying the relation between these

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+ We shall return to this point in a later context.

three aspects and the corresponding sources of data.+

This general approach led to a very catholic use of a broad range of data. Continuous observations were carried out, with the help of Rockefeller funds, at a central home maintained by the municipality of Vienna for families unable to take care of their children. The study of adolescents was facilitated by a large collection of diaries which, incidentally, are kept by young people in Europe much more than in this country. Biographies of adults were amply available in the literature for upper-class and professional groups. They were supplemented by elaborate case studies collected in old age homes. Questionnaires for adolescents were somewhat in the Stanley Hall tradition, collected with the official cooperation of schools and of labor unions. Students were trained to keep day-long records of the interactions and conversations within families in the course of their normal routine. The material thus collected was analyzed in terms of categories which formed what might be called today "biographical functionalism." In his early writings Karl Bühler had already begun to stress how the child's play seemed to train him for performances needed in later life. In a recent summary Charlotte Bühler(1951)

+ It would be highly desirable if Bühler's Crisis were translated into English, for two reasons. This book reflects excellently the very perceptive reaction of one who saw the clash of three intellectual traditions, and his analysis of the situation would ensure against the possibility of historical provincialism on the part of modern students. The book also contains some very important ideas on the logic of projective psychology (especially pages 137 - 165).

stressed that "a need at any given moment is not only related to that moment but also to the future ... it implies in its complex tension pattern a dynamic relationship to a partially foreshadowed future." (pp. 206 f.)

Only a few of the basic categories can be mentioned here. We have already referred to the distinction between actions performed in response to outside stimuli and expansive actions, in which the initiative comes from the acting subject -- the distinction, as it were, between satiation and self-realization. Another distinction is between those goals which are essentially taken over from the environment and those which grow out of the maturation process of the subject. Here, it will be remembered, the notion of maturation was so conceived that it could be applied throughout the life span. Actions were distinguished according to whether they grew out of the need of the moment or whether they had a conscious or unconscious anticipatory character, taking into account future situations.

Where do we locate the contributions which the Bühlers made to the empirical study of action, conceived as a broad programmatic concern? The answer can be attempted in three ways. First, looking at the matter in their own terms, the e mphasis is on broad, almost philosophical implications. When Karl Bühler, in the first edition of his book on child psychology, wrote his chapter on "The First Act of Wills", he implied the

hope that such studies would in the long run contribute something to the "foundation for a moral or legal evaluation of human activities." In the introduction to the third edition, he stated apologetically that new editions were needed so rapidly that he was still unable to add a summarizing chapter on how his analysis is related to the crucial problems of norms and values in modern society. From the biographical work of Charlotte Bühler it is fairly clear, however, where such a development would lead. We are all acquainted with efforts of social psychologists to develop tables of basic needs, close to the biological foundations of the human being. It could be said that the work of the Bühlers was partly directed toward developing a table of ultimate needs characteristic for the mature human being. Empirical studies of values have recently moved toward the center of interest for social scientists. In this country they have mainly been patterned along the lines of statistical attitude studies or along the lines of generalization from anthropological and historical material. The emphasis on the developmental and biographical approach to the empirical study of values adds an additional technical dimension which deserves careful attention.+

+ It is interesting to note that Charlotte Bühler turned to psychotherapy in this country. But even when she summarized her work in this context, her culminating section is called "The Problem of the Aim of Life."

We can look next at the empirical studies which were done at the Vienna Psychological Institute. When the Bühlers left Vienna in 1938, a considerable number of biographical studies were under way. Some of their findings were ably summarized by Else Frankel (1936) who was Charlotte Bühler's main assistant in the supervision of these investigations. Special attention should be given to the studies cited there on the appearance of the notion of duty in the diaries of adolescents, and on the statistical shift from the notion of needs to the notion of fulfillment in the correspondence of adults. A study based on interviews with unsuccessful suicides has since appeared in an English edition. The investigator used the list of ultimate needs developed under Charlotte Bühler's direction, which, for this specific purpose, were divided into those which belonged in the personal sphere of life, such as friendship, love, home, reputation, and so forth, and those which belonged into what she called the material spheres of life, including income, health, occupational success, and so forth. Her conclusion was that if losses to the individual occurred in such combinations that adequate compensations become impossible, then "the individual's living space suffers a maximum restriction and then may lead to catastrophe. Suicides usually happen when such difficulties occur in at least one subjective personal sphere and at least one objective practical one (Margaret Andicz, English edition, 1947, page 158)." Cross-analyzed against this typology of human needs was biographical information. The various difficulties were likely to show up at characteristic phases of the life cycle, and their effect could

very well be a latent one, coming into play at a later period when conjoined with added difficulties. A series of similar investigations of occupational choice was recently reviewed and further developed by Eli Ginzberg (1952). This is probably one of the main American investigations which consciously carried forward the work of the Viennese school applied to a specific decision area, and therefore it deserves special attention by the student interested in the continuity of social research.

Now it is characteristic for all these studies that they refine the understanding of goals but do not deal with what might be called "implementation": the way goals are transformed into specific intentions and finally, under the influence of concrete situations, into ultimate specific decision. And in this third form the question of the Bühlers' place in the history of the empirical analysis of action cannot yet be answered.[+] We shall meet this situation several times again. The political catastrophes in Europe have interrupted a number of intellectual developments which have not yet been taken up in this country. There are many indications that the continuity is about to be resumed -- as a matter of fact, the present historical notes are an effort to help bridge the gap.

[+] It has to be kept in mind that we are referring here only to their study of human goals. Presently we shall trace two other lines: Karl Bühler's insistence on phenomenological description, which greatly affected his students' work in applied psychology; and his ideas on interaction (Steuerung).

But as of this moment, the accent on goals, most richly represented by Karl and Charlotte Bühler, has remained only one section of a broader program. We now turn to a second one.

Accent on Intention -- Kurt Lewin

Early in the Würzburg period Ach developed a technique by which he attempted to bring out more clearly the course of intended activities. He introduced obstacles: he has subjects learn pairs of syllables; he then offered the first syllable in the pair, with the instruction that the subject respond not with the syllable already learned but rather with another one. This led to more explicit efforts to perform the task which, in turn, made it easier for the subject to report his experiences and for the experimenter to analyze his materials. (See Lindworsky, for example, 1930, p. 193.) This so-called combination technique was picked up just before the first World War by a Berlin student, Kurt Lewin. He reported results which were at variance with Ach's finding, and out of these followed a great deal of discussion on the theory of associations, in which we have no direct interest here (Lewin, 1917). What is relevant in our context is that Lewin spent the next fifteen years elaborating the basic idea of Ach and developing a series of influential experiments.

There were two basic changes which Lewin made in contrast to Ach. One was that his subjects did not work with meaningless materials, but

performed tasks much more "close to life." Secondly, the obstructive forces were not perseverations resulting from past training; they consisted in the interruption of tasks by the experimenter through giving subjects problems which were impossible to solve, in making them work to a point where "satiation" led to discontinuation, and so on. The basic idea, however, remains the same. An intention was set up in the subjects and an obstacle was added, the experiments having the purpose of analyzing the conditions under which the intention was not carried out. As Lewin put it himself:

> " The problem is: how does the act of intending bring about the subsequent action, particularly in those cases in which the consummatory action does not follow the act of intending?" (1926)

Lewin's experiments have become so well known in this country that they do not require summary here. However, since we shall be interested in the role they play in the general context of our problem, and especially in the course of Lewin's own development, some comments are necessary on the way they are so far available to the American public. Seventeen of these studies were published between 1927 and 1934 in the Psychologische Forschung, then the journal of the Berlin Gestalt psychologists. They are introduced by two general programmatic papers by Lewin, published there in 1926. The second, and much more important one was called "Intention, Will and Need." It is only since 1951 that this paper has become available in English translation through its inclusion in Rapaport's collection (1951). Rapaport correctly remarks that more than

any later writings of Lewin this paper gives the best understanding of Lewin's concept of dynamics, "which still bears its freshness and the earmarks of its origin." (p. 95) The papers on the experiments themselves are all of considerable length, each averaging about 100 pages, but the only place in which even an inventory is available to the American reader is a curious one. In 1935, when Lewin had come to this country for the first time, a collection of his earlier papers was published under the title, A Dynamic Theory of Personality. Lewin includes there, as an appendix, a classified summary of the main context and purposes of these German studies, and he stated there that his purpose was to investigate "the structure and dynamics of personality and of the psychological environment." This, however, was by no means indicated in the title of the series which, throughout the whole course of Lewin's work in Germany, was called "Investigation on the Psychology of Action and of Affectivity" (italics ours).

In this shift of terminology there is symbolized quite an important segment of the problem history which we are tyring to trace. It is quite simple, to begin with, to show what aspect of the total program of a theory of action Lewin selected for himself. He singled out the second step, the intention, and followed it through its course. In his programmatic paper he was still very much interested in the relation of this to the other two main elements of the original scheme, to wit, goals and occasions for consummation. He spoke of intentions as quasi-needs, because he

wanted to compare them with more permanent motivational needs, out of which specific intentions grow. He was also aware that in the original experiments by Ach the intention was carried out at one specific moment, when the experimenter exposed the material on which the subject had to perform the task. But Lewin stressed that "not in every act of intending are occasion and consummatory action so specifically defined." A variety of occasions can turn an intention toward its consummatory end; and inversely, if no such occasion occurs, substitute intentions might develop or the intention might die out. Yet this integration of goal, intention and occasion, stressed in Lewin's original program, was not maintained in his concrete studies. The accent moved to the study of the intention alone, and, furthermore, the experiments favored those situations in which nothing had to be done or could be done by the "actor." In retrospect we now can state it succinctly: the Lewinian experiments on action have laid the groundwork for a powerful theory of frustration in the broadest sense; they have nowhere led to a theory of choice or decision.

One of the two reasons might explain this situation. It might either be that the interruption of Lewin's European work, as in the case of Bühler, led to a social discontinuity in research. Or, perhaps, the accent on intention led to a point where its re-integration into a general approach turned out to be intellectually more difficult than was anticipated. The former element certainly played a role. When Lewin came to this country the behavioristic trend was still very strong and the interest in learning

theory was at its peak. It is quite possible that Lewin felt that it was a hopeless task to get American students interested in a theory of action, which had been deeply rooted in a European tradition. But there are also strong indications that Lewin became aware of the intrinsic difficulty of the intellectual situation and shifted to another subject matter. The following reconstruction suggests further scrutiny of Lewin's German action studies for confirmation. If it is correct, then it highlights a salient problem for any future efforts in action theory.

Every psychologist is acquainted with the very helpful graphic symbols by which Lewin's German students were taught to illustrate the experimental findings and their conceptual interpretations. An intention was a tension system, symbolized by a closed curve on the printed page. The system could have strong or weak borderlines; it could be homogeneous or subdivided; it could be connected in various ways with other such systems. With these symbols a number of basic ideas could be represented, such as the strength of an intention, its connections with other dynamic psychological elements, its stability, and so forth. Through a small number of such concepts Lewin was able to organize a surprisingly large array of interesting empirical findings; and even more, such a representation used in one study gave very concrete leads to further experiments. As Lewin himself said in retrospect, it slowly occurred to him "that the figures on the blackboard ... which were to illustrate some problems ... might, after all, be not merely illustrations but representations of real

concepts." (K. Lewin, 1936, VII) Out of this grew his book on Principles of Topological Psychology.[+] This book develops, in a systematic and coherent way, his general idea of how psychological relationships could be represented by a symbolism borrowed from topology, the mathematics of non-metric geometry. It is now perfectly clear that this representation had nothing to do with topology in the mathematical sense.[++] But the analogical use of some of these symbols certainly proved stimulating.

Lewin took the matter up again and published his Duke University monograph on the "Conceptual Representation and the Measurement of Psychological Forces." There are two differences between the 1936 and 1938 publications. One is unimportant in our context: in 1938 we find a heavier use of mathematical symbols; what is important for us is that we now find an intensive treatment of the notion of valence which was hardly mentioned in the 1936 book. What might be the background of this striking difference? In the seventeen German inquiries into the theory of action the word valence (aufforderungscharakter) had appeared more and more frequently. It referred to the relationship between needs and intentions, on the one hand, and the objects -- the outer world -- on the other.

[+] The preceding reminiscence was quoted from the introduction to this volume.

[++] The mathematically untrained reader who would like to get an idea of the background from which Lewin drew his concepts will find easily understandable a short introduction in Courant and Robbins, What is Mathematics (Chapter V, pages 235-272).

If a man is very hungry, then he will eat food which he usually would refuse. But also, the sight of a piece of juicy fruit might suddenly whet his appetite. If we have forgotten that we intended to mail a letter in our pocket, the sight of a mailbox might remind us of it. Objects have valence in the sense that they can strengthen our intentions, bring forgotten ones to mind, and, most of all, turn intentions into consummatory acts. Especially in the latter sense the valences correspond to the occasions of the general scheme of action. An analysis of the interrelationship between valences and intentions would probably be the crucial step in the development of an integrated theory of action.

Now, if we are not mistaken in our interpretation, it is at this point that Lewin found himself defeated. In the German experiments on action the notion of valence, however often the term appeared descriptively, never played any real role in the systematic analysis. This is, of course, a statement which would need careful checking in the text.+ We surmise that Lewin was in the midst of a very serious intellectual conflict here. On the one hand, he was very aware that the interplay over time between "need" and "occasion" might well be the central problem of any theory of action. In his programmatic paper he had already included pages of vivid examples taken partly from the studies of his students which were then in progress, and partly from everyday life experiences. He created the word valence, which, in its German form, was a linguistic achievement well worth being classified with such other portentous neologisms as "inferiority complex" or "primary group." But at

+ It will be necessary at some time to make a collection of the places at which the sequence of studies in the Psychologische Forschung used the word valence, to determine whether they could really be dispensed with; a recent rereading of most of the papers hasgheatly stressed the conviction of this writer, although documentary proof is clearly needed.

the same time he obviously realized that tagging and describing a phenomenon does not make it a manageable object of scientific inquiry. As he wrote in his programmatic paper:

> " The proposition that 'such-and-such a need exists' is to a certain extent equivalent to the proposition that 'such-and-such a region of structures has a valence for such-and-such actions' ".

The crux, of course, lies in the apologetic words, "to a certain extent." Inasmuch as valence is equivalent with needs, it does not even help conceptually to analyze further how needs affect the perception of objects and how, in turn, physical objects can affect intentions. If, however, there is an intellectual surplus value in the notion of valence, then to elicit what it consists of would be the decisive problem. In his Principles (1936), Lewin obviously took the position that he could do without the concept of valence. He includes there only a short discussion on the relation between the physical and the psychological world (Chapter 8), in which he does not do more than to reiterate some of his descriptive material. Even the terminology avoids the term valence; he only distinguishes between physical objects which are alien in the sense that they intrude upon a person's world -- such as disturbances from the outside -- and other objects which have already been embedded in the psychological and biological stream of a person's life. It must have occurred to Lewin, however, that by temporarily excluding the "valence" from his system he could not really develop a "theory of action" in which

the mutual interplay between desires and the characteristics of choice objects would be a crucial concern. In his next effort, therefore, he gave the notion of valence practically top billing. His programmatic statement now read as follows (Contributions to Psychological Theory, p.108):

> " Instead of linking the need directly to the motoric, the need is linked with certain properties of the environment. The environment then determines the motoric."

The earlier effort of systematization, then, sees action as controlled from within; objects in the physical world are either psychologized or they are treated as random interferences. The new effort treats action as controlled from without. People are visualized as acting like bodies in a field of forces. Only by adding something which would presumably be the residual of past biography would the effect of the outside objects be different from one person to another. The measurement of the valences of the outside objects therefore become as crucial now as they were negligible before. In both efforts, however, Lewin, who is so vividly aware of the problem does not see a way to really clarify the relation between needs and occasions.

Later he turned to group dynamics, greatly enriching the recent developments in social psychology. Our indebtedness for these contributions should not blind us to the fact which is so significant in our context: Lewin's program for the analysis of actions of individuals came to a rather sudden stop; his accent on intention, like the accent on goals, awaits integration into a more comprehensive approach. Still, we have to ask specific contribution Lewin's ideas on action psychology can make to the study of specific decision.

The Morphology of Action

Lewin does not state any specific propositions in this whole area of decisions and choice and, as far as we know, he has never made any systematic effort to develop this program further after coming to this country. There are two isolated efforts by early students of Lewin. One is a study by Wright (1937) which had a very pertinent topic -- whether barriers increase desires -- but the findings were inconclusive. Another series of studies showed that the more similar to us two objects are, between which one has to choose, the more time it takes to make up one's mind. These studies are only replications of early studies by Ach, and might have contributed to Lewin's feeling of how little progress has been made in four years on this crucial problem of choice. Two other writers, who were not personal students of Lewin have applied his concepts to the description of buying activities. J. Clawson (1950) and W.G. Bilkey (1951) wrote for a market research audience and therefore had to give most of their space to the mere exposition of the merchant and advertiser who wanted to find out "what makes a customer tick." Clawson mainly makes the point that a careful analysis of the process of buying, in psychological terms, would be helpful but it is not clear whether strictly

Lewinian terms would be more useful than any other. Bilkey tries, at least in principle, to make a concrete application to the problem of budgeting. He carries out an example in which the valences of various commodities are assumed to be known and where the reorganization of the budget, under the assumptions of income and price changes, is followed through. This, of course, is so close to the use of the concept of utility in traditional economics that the question can be raised whether it is a well-known approach with simply a change in terminology.

At this point, an insertion might be appropriate on the interesting role which Lewin's terminology and graphic symbolism has played over the last twenty or thirty years. No one can deny their original suggestive character; but no one has made a serious effort to pinpoint what this suggestiveness consists of. The problem would nevertheless be very important, because it is quite likely that in a new field of research a productive terminology might be very important, even if it does not immediately lead to coherent systems of propositions. There exists an interesting possibility that a contribution can be made to this problem, in connection with Lewin's work, by turning the strange duet performed by Lewin and Ach for a number of decades. Lewin has, throughout his German period, felt that he had to fight against associationism. His programmatic paper, as well as the work of his German students up to the end, are full of explicit or implicit references to the progress they have made beyond Ach's early efforts. Inversely, Ach has obviously followed Lewin's work with

great ego involvement. When in 1935 he published his great review of the psychology of will -- which he considered an appraisal of his own life work -- he commented in very great detail on practically every study included in Lewin's theory on the psychology of action. On a few of them, especially those related to satiation and to level of aspiration, he was highly complimentary. But on most of them he was highly critical on three points. First, quite contrary to Lewin, he felt that making experiments close to life, clouds their psychological implications. He therefore had his Ph.D. candidates in Göttingen repeat most of Lewin's studies, especially those collected on interruption and forgetfulness, but in his studies the materials were always meaningless syllables on which, rather ingeniously, the same operations were repeated which Lewin's students carried out on meaningful tasks. By and large, Ach found most of Lewin's empirical results corroborated, so that at this point a test cannot be made.

On a second point, however, the long distance dialogues between the two scholars would be very revealing. Ach translated all experimental findings into a very simple terminology, making them ever more specific "laws of determination." He claimed that Lewin's elaborate terminology and symbolism obscured the meaning of the results and made it more difficult to systematize them. For the purpose of the present paper it was not possible to analyze carefully whether Ach's position has merit. But it is probably the only time in the history of action analysis that the same set of experiments are available with very different kinds of materials and in very different terminology. One impression can be reported, as a result even of a preliminary comparison. It is quite likely that the choice

of specific problems and the experimental ideas were definitely due to Lewin's richer imagery, after all, it is not likely to be a coincidence that the Gottingen group only repeated the studies but did not think of them. On the other hand, it is not unlikely that the cataloging and interrelating of the findings could be more economically done in terms of Ach's simpler schemata.

However this may be, an accent on intention is likely to stress the various ways in which a course of action can develop through time. This is meant when we speak of the morphology of action. Lewin has not developed a system of pertinent categories as the Buhlers have done in regard to goals. But especially in his early writings stimulating suggestions can be found. Thus in his programmatic paper, Lewin suggested an interesting classification of actions according to whether they were controlled and uncontrolled, on the one hand, and intended or not, on the other hand (P. 1hh ff., D. Rapaport). An uncontrolled action is one in which a person behaves like a body tossed through a field of forces, there are uncontrolled, intended actions -- a child who decides to pass by a dangerous dog, and then runs for his life. Inversely, there are controlled actions which are not preceded by a phase of intending, such as conversation or certain (non-automatized) occupational activities. It is easy to think of the other two combinations: intended and controlled is exemplified by the carrying out of a plan, unintended and uncontrolled is what we normally refer to as an impulsive act. "To establish the action type to which a psychological event belongs" is a program which Lewin sketches in these few pages and which is still waiting to be carried out.

Anyone who has done empirical studies knows how crucial certain of these morphological distinctions are. Voting for example has to be done on a specific day, while the decision to buy a car can be postponed endlessly. Action with prescribed and with free terminal date must thus be distinguished. Whether or not we bought a good meal, we know as soon as we taste it, whether or not we bought a good suit takes many months to determine, whether or not we voted for the right candidate, we may never know. The testability of an action is another category which plays a great role in empirical research. Here is a juncture of theory and field work which deserves careful attention.

We have now followed two directions in which an original concern with "why people act the way they do" became specialized. Both the accents on goals and on intentions were prepared in the early Wurzburg experiments and their subsequent generalization. A third element has still to be traced: the occasion which brings the intention to consummation, or in generalized form the "influences."

Accent on Influences -- the Empirical Study of Propaganda, Advertising and Other Manipulative Efforts

In the Würzburg experiments the interviewing of the subjects who had performed his simple task played a very important role. Often an hour-long "protocol" was held regarding a decision which might have been reached in one or two seconds. These interviews were made the object of a savage attack by W. Wundt (1907), and, as a consequence, a considerable

amount of attention has been given to their methodology over the years. Karl Bühler (1908) wrote the first systematic defense of the method of "Ausfrage" -- a term which Wundt created as a derogatory one, but which was accepted as appropriate by its proponent. Ach (1935) wrote a very useful summary of the literature which ensued (pp. 40-59). The main aspects of the technique used were the following:

(a) It was definitely a matter of retrospection, not introspection, the subject reported his experiences after the completion of the act and did not try to observe himself while he performed the task.

(b) The interviewer made maximum use of the time sequence character of the experience. Occasionally, the method of "fractionization" was used. this meant that in a series of interviews the interrogator singled out varying phases for emphasis, according to where clarification was needed.

(c) An idea which we might call synthetic structure played a considerable role. It turned out that the experiences of a number of subjects would differ in detail but that certain broad features were common to all. The purpose of the interview, therefore, was a two-fold one. on the one hand, to bring out the broad common structure by cumulation from one interview to the next. on the other hand, to facilitate the discovery and reporting of specific individual reactions to be accounted for later by the interpretation of the analyst.

(d) As the studies went on, the purpose of the interviews was not to serve as a catch-all for whatever the subject might have experienced. Rather, the role and location of specific elements in the course of the experience was the main purpose, for example, to see at which point the determination through the task did and did not play a role.

A reading of these old discussions, including careful scrutiny of the original protocols, would still be worthwhile today. For this methodological

heritage of the Wurzburg school is in several ways related to another development in the study of action for which it is difficult to find an existing text. Perhaps the best way to proceed is with a reminder of a well-known type of research problem.

In the area of modern manipulation, such as propaganda and advertising, we are often confronted with a peculiar situation. A political party or a major manufacturer might have invested a large amount of money on a campaign, making use of some specific medium such as speeches over television, or a series of ads placed in a few magazines. The manipulator knows that social and applied psychology are not yet in a position to tell him in advance whether the campaign will be successful or not. But at least he would like to know at the end of the campaign, at least, whether this effort helped him to reach his goal -- an increased number of voters or of consumers. Even this retrospective information, however, is not easily collected. In certain specific situations, in mail order campaigns for example, success may be measured fairly easily. Still most of the time the contigencies and ramifications of a campaign are so diversified that measurement of effectiveness is, to say the least, very blurred.

Outsiders often feel that traditional laboratory experiments should be helpful. But this is by no means always the case. In an experimental situation, for example, it will always turn out that an educational radio or television program will affect the attitudes of those who are exposed to it. In real life situations, however, the people who are most likely

to be influenced are those who do not expose themselves to educational programs. Due to this self-selection of the audience, the actual effect might be zero, in spite of the results found in the laboratory.

The following trend of thought suggests itself in the foregoing situation. The difference, after all, between human beings and animals is that human beings are aware of some of the influences which are exerted upon them and should be willing to give retrospective accounts of what led them to perform a certain action. Why, therefore, should it not be possible to check the effectiveness of campaigns by interviewing the people who were reached, and who might or might not have acted as a result of them? Whether the testing of propaganda effects by direct interviews will, in the end, be feasible is still a matter of considerable debate. But it is quite clear that a necessary, although perhaps not a sufficient, condition is very systematic insight into the kind of interviews which would be necessary for such a purpose. Thus we can understand easily that one branch in the development of the empirical study of action led to the problem of tracing influences by retrospective interviews. For a number of obvious reasons -- multiplicity of situations and availability of research funds, among others -- most of the pertinent writing has been done in the commercial field of market research.

The whole situation is best exemplified in a monograph entitled "The Technique of Market Research from the Standpoint of the Psychologist" (A. Kornhauser and P.F. Lazarsfeld, 1937). The authors pointed out that

specific research techniques could be better understood, better integrated and better carried out if there were a "master technique" from which particular procedures could be derived. And, in their view, "psychological analysis of action is a master technique in market research." In their monograph they attempted to clarify the meaning of action analysis, and then to exemplify specific applications. They pointed out that the situation in which an individual found himself at any given moment determined the next stage of his action. For example, a person with an interest acquired in the past talks with a friend, he is changed by that conversation, and moves, so to speak, to a new stage, this newly oriented person is then confronted with a new situation, for example, an advertisement, from this new situation he takes a new step toward the final action. The relevance of this traditional scheme to market research can be seen in the following quotation:

> " This longitudinal analysis of action may begin at stages near to or remote from the final purchase ... Whatever the starting point, a complex background already exists in the makeup of the person himself, a summary of preceding experiences. It is necessary, in consequence, to begin at the point chosen with whatever then existing understanding of the individual one may possess and to proceed by tracing the most significant steps leading from that point to final buying habits... Beginning with the psychological background, the problem is to analyze the steps by which people move to the attitudes and buying responses in which the interest of the particular investigation cen-

> ters ... Of course in a specific investigation one cannot ask all the hundred questions which are required by some scheme. But this is not necessary. Only in general the explorer must know far more of the structure of the action than what he is actually then asking about. You may survey tentatively scores of points that might be inquired into, and you may end with an interview schedule of five questions. But these five questions will be vastly different from the ordinary ones. You will have explored and decided the best spots at which to pitch your interrogation. By analyzing the whole act, one sees what part is significant for a given purpose."

The authors then go on to indicate the technical implications of this general analysis. If people are asked why they use a particular brand of merchandise, for example, only 2 or 3 per cent will mention that they were influenced by advertising. In terms of a general action analysis, however, it is clear that individuals who say that they switched to a new brand because of its good quality -- and this is maintained by some 60 per cent of the respondents usually -- really condense their experience in retrospect. Obviously, before using the product they could not have known about its quality from their own experience. It is therefore appropriate to ask them how they learned about these features of the product. Experiments have shown that, when such specifying questions are used, there will be a marked increase (to say, 20 %) in the number of respondents saying that advertising was an effective source of knowledge guiding their behavior.

If one wanted to go further, one could use the method of fractionalization, and employ a variety of such specifying questions. One could ask purchasers how they first learned about the product; whether they looked for information, and if so, where; what were the crucial episodes which finally made them buy, and so forth. Again, it has been shown that the additional efforts to locate the role of advertising at different phases of the purchase make it easier for subjects to give relevant retrospective reports.

There is no space here to develop in detail the general code of research techniques into which this approach has evolved over the years. But the propinquity to the Wurzburg ideas about method of "Ausfrage" should be obvious even after this short description. A direct link was established by a division of applied psychology which was established in the Bühler Institute at the University of Vienna under the direction of this writer. A systematic summary of the ensuing studies was published for American readers under the title, "Psychological Aspects of Market Research" (1934). The approach is, of course, by no means restricted to marketing studies. Influences on voting, on migration, on committing of crimes can be studied in this way. A survey of the main results obtained in recent years can now be found in two chapters on Reason Analysis in the fourth edition of "Say It With Figures" (Zeisel -- 1957). There, also, the notion of "accounting scheme" is discussed as central to the efforts of tracing influences by retrospective interviews. Prior to any such study we have to decide what group of factors are to be considered. In a study of migration, for example, a respondent should tell at least what "pushed" him away from his former

place, what determined the specific time of the migration, what "pull" did the new place exercise, and how did he know about this attraction before migrating. This should provide an accounting scheme consisting of four "categories" and for each category a specific piece of information has to be collected. This makes for a much richer inventory of influences than if we ask the respondent only "why did you migrate." And inversely such an accounting scheme permits a more systematic statistical treatment as compared with a loose "case study." Many logical aspects of this accounting scheme procedure still await clarification.*

How should we assess this development as it grew out of the general matrix of an integral study of human action? Much of the relevant impetus came, undoubtedly, from the desire to manipulate people -- propaganda and advertising. But in its application to criminology, for example, it was the reverse side of the coin and takes a remedial turn. What is common to all of these studies is that their findings were most likely to be useful for institutional purposes. If they are successful, they can lead to improvements in crime prevention, to better organization of occupational counseling services -- and, for better or worse -- more successful propaganda. In this connection a serious word should be said about neglect of market research as a legitimate part of general social research. For understandable reasons, the social scientist seems to feel that he demeans himself if he investigates the influences under which people buy one type of merchandise rather than another. But this is a shortsighted prejudice. It is difficult to see why a man comparing weights, or a rat

*There exists a very interesting parallel between these accounting schemes and a recent schematization by Tolman (1951, in which he coordinated the Lewinian pictorial devices with his own theoretical concepts. Characteristically enough, the main example which Tolman carries through his paper is that of a man who goes to a restaurant for a meal and whose choice of eating place is to be explained.

running through a maze should have more scientific dignity than individuals repeatedly running through the maze of the modern market economy, weighing the merits of one purchase against another. For the systematic study of influences we have to take advantage of any situation where acts are performed repeatedly and by many people under conditions which can be clearly specified.

The shortcomings of this approach lie not in the "triviality" of the material but at another point. Skillful retrospective interviewing can isolate the role of outside influences; but it cannot easily connect them with the dispositions and sentiments which prevailed at the time of the act. It is unlikely that a respondent can remember -- or even know -- that he followed a neighbor's advice, because he reminded him of a person whom he trusted when he was a child. Thus the post hoc reconstruction of an activity in progress is likely to be fairly complete on the side of the "stimulus" -- and this is enough for many practical purposes; but it will miss much of the "inner" elements of the process -- which are needed for a more systematic analysis. Only a repeated set of observations, made at the time of the event can get this full picture -- an idea to which we shall return at the end of our review.

Let us now return to our starting point. The full course of human action extends from a broad set of motivating goals, out of which specific intentions emerge. These, in turn, evolve through a variety of situations, one of which will provide the occasion for a final consummation of the act. We have followed the development of three major trends in research, which we have dubbed , respectively, the accent of goals, intentions and influences. We have lamented the fact that these developments have not only been unintegrated, but that, due to a series of external circumstances, and perhaps to the intrinsic difficulty of any program of integrated action research, they have moved more and more away from each other. We could at this point close our historical note and turn to our main positive suggestion. But the term action [Handlung] played a very great role with another group of German writers. Their concern was very different from that of the laboratory workers. In recent American writings a lack of distinction between the two traditions has created confusion. This justifies some additional browsing in the past

The "Action-Language" in the German Tradition of "Human Studies"

There exists a body of inquiry which the French call "sciences morales" and the Germans "Geisteswissenschaften." An English writer suggested the name "human studies" (Hodges, 1952). We shall adopt this term for brevity, and the reader should keep in mind that in its origin it covered mainly jurisprudence, economics and history, with occasional inclusion of linguistics and analysis of art. In Germany the foundations of these human studies were made the topic of special analysis. Since the time of Kant, the question "How is natural science possible?" had remained a central theme for all philosophers. Toward the middle of the

autonomized projection

century a similar question became paramount for those studies which dealt with the major social institutions, their interrelationships and their development. The object of these studies also acquired a standard term "Objektiver Geist." We might translate it as "autonomized projection," conjoining two terms which other authors have introduced. Kardiner speaks of projective systems, when he studies the psychological roots of primitive beliefs. But the German tradition of "human studies" included technology and artistic products, and it stressed that they had all taken on functional autonomy in G. Allport's sense -- they had intrinsic characteristics which could be deduced by looking at them as complex extensions of a basic notion of "human action."
But it was not an empirical study of action which was under consideration, as it was with the psychologists. The idea was rather that the notion of Handlung properly conceived would be a kind of systematic frame of reference within which available knowledge and prevailing procedures could be located. It is not simple, but it is rather important for our purpose, to get a feeling for this use of action schemes as an ordering device. It so happens that a young jurist, who, twenty years later was to become Minister of Justice in the first Weimar Republic, provides us with a very good example. He wrote a review and reanalysis of all the available literature (more than fifty references) on "The Concept of Action in Its Importance for Legal Systematics" (Radbruch, 1903). He was very explicit in stating that the problem was not to study empirically "what happened when people did something" but to construe a concept so that the salient legal distinctions could be taken account of.

> "The concept of action has to be the solid structure upon which the doctrine of crime is to be based." (p. 96)

It appears that the legal philosophers were confronted with problems of the following kind: On the one hand, a punishable crime was an act; on

content analysis

the other hand, you could also be punished for negligence (for failing to do something), or for an intended crime, even if it had not led to the final consummation. The task of a theory of action was to give it all those characteristics, and not any more, which were necessary to subsume under it everything for which the law provided punishment. Correspondingly, the starting point of Radbruch's analysis is an inventory (this would be referred to as content analysis today) of all the places where the penal code uses the term action (p. 73). And therefrom he went on to construe "a concept of action which would satisfy the requirements of a legal systematics" (p. 75).

This writer does not know whether the problem is still important in contemporary German jurisprudence or whether it ever played a similar role in the Anglo-Saxon tradition. But it certainly makes fascinating reading to learn about what Radbruch calls the doctrinal history ("Dogmengeschichte") of the concept of action (p. 78, p. 131), the way it was discussed and redefined over and over again to adapt it to changing ideas about legal responsibility and criminal guilt.* Radbruch for his own solution takes into account some introspective observations suggested by what he calls "psychological jurisprudence" (p. 112 f). But he never has any doubt that action as the central concept of criminal law has to be analyzed in the light of classificatory needs, and not as a result of empirical observations.

*It seems that even early ideas about unconscious purposes emanated from the vexing problem of omission as action.

in the light of classificatory needs, not as a result of empirical observations --

Geisteswissenschaften

The specificity of the legal problem and the clarity of Radbruch's presentation is not paralleled by more ambitious efforts to derive general categories for the human sciences from a formal notion of action. Dilthey was most specific as to his goal. The life of an individual was action on the largest scale; history was a system of such lives. The analysis of action would therefore provide a "critique of historical reason" (Vernunft); it would answer the question, "How is history possible" in a way similar to what Kant tried for the natural sciences in his "Critique of Pure Reason." A perceptive British philosopher has traced Dilthey's ideas on this matter through the many volumes of his collected works. (Hodges (1952), especially Chapter IX) A later attempt, but belonging to the same tradition, has been made by Fryer (1933). He distinguishes five major types of autonomized projections, difficult to understand and impossible to translate. They are characterized according to different elements of an action scheme: one, exemplified by art (Gebilde) corresponds to the expressive function of actipn; another, exemplified by tools (Geraete), to its instrumental aspect. In addition the very nature of all "objectiver Geist" is "derived" in several steps from the action of concrete individuals to the permanent traces they result in: from a man pointing the way, to the road sign and finally to, say, the system of musical notation.

The Germans who wrote in this tradition seem to have taken it for granted that some kind of "action - language" is mandatory for a discussion of the human sciences. The origin of this tradition goes at least back to Fichte, as one can gather from occasional references. It is like the case of a modern theoretical physicist who as a matter of course writes in mathematical terms. Actually these efforts can well be considered the precursor of the modern mathematical models in the social sciences -- which so far are essentially a device for organizing concepts and assumptions (Lazarsfeld, 1956).

"reductionist"

The compulsion of using action language -- without justifying its merits -- becomes especially clear in a paper by Max Weber (1913) on some basic categories of sociology.* Its main purpose is to define a number of concepts, which might -- or might not, as Weber himself stresses -- turn out useful for a systematic sociology. The task at hand is to take notions from every-day language and to make them more precise by showing in what respect they represent "the course of human action of a special kind." He states what today one would call a strictly reductionist program. Concepts like state, feudalism, etc., should "without exception be reduced (reduziert) to the actions of the single individuals involved." The paper was written for a philosophical journal (M. Weber, 1913) and contains many digressions and inconsistencies within its fifty pages. Its central distinctions refer to three types of actions. In one type people act together but are controlled only by the requirement of a concrete situation: the example he gives is a group of streetcar passengers trying jointly to help a fellow passenger who is suddenly stricken by illness. Another type consists of those actions in which the conduct of the actor and other people is prescribed by definite rules: the most extreme example would be an army. In between there is the type of action in which the interplay is governed by some kind of informal understanding which is less binding than the rules relevant to the latter type above but more stable and probably more explicit than the haphazard "orientations" of the first type.**

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*This paper has to our knowledge never been translated. It is in many respects quite different from the introduction to his great posthumous work, which forms the first chapter in the Parsons - Henderson translation of "Theory of Social and Economic Organization."

** The three terms could be translated "joint action," (Type I) "agreed action" (Type II), and "organized action" (Type III). The last two seem sometimes subdivisions of the first and sometimes coordinate with it; in any case, Weber stresses repeatedly the "fluid transitions" existing between the major types.

A variety of social phenomena like market, associations, organizations and of "social relations" like competition, domination are analyzed in terms of these types of action. At first glance the circular character of this "casuistic" (Weber himself uses this legal expression) is surprising. The types of action are described in terms of the social situation in which they occur; in turn these "configurations" are defined in terms of action types. Thus the action language seems to force one to say everything twice. This is, however, not a cogent objection. It could well be that through this procedure one could arrive at the minimum of elements, the combination of which would permit organizing a large number of concrete phenomena. Whether Weber could have achieved this we shall never know, because he did not pursue this plan in its original form. *

What is important for our present purpose is Weber's awareness and insistence that he is not concerned with an empirical study of human social action. His purpose is to develop an action scheme through which sociological concepts can be organized. He is aware of the parallel to Radbruch's efforts; he refers to him in the introduction to this paper as well as at other points of his methodological writings. Empirical action studies he explicitly considers a different task assigned to the psychologist. For seven pages he dwells on this distinction, and the following quotation is most characteristic (p. 412,

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* In his subsequent work along this line (M. Weber, 1920), he did not try a coherent systematization but was satisfied with defining only a specific list of concepts.

condensed translation):

> From the notion of a mystic-contemplative religiosity one can deduce logically a lack of concern with the well-being of others. And yet psychologically it may happen that this type of religiosity leads to a kind of Euphoria, which is experienced as a peculiar feeling of love for everyone.
>
> (Italics Ours)

Here psychology is opposed to logic. From the context it is clear that by logical deduction he means the kind of systematization he tried in this paper. Weber is not stopped by any doubt whether logic can derive one sentiment (Unbekuemmertheit) from another one (Religiositaet). For this is exactly the conviction he has: from the elements of an action-schema one should be able to build up concepts which permit derivations of this kind. Or, to put it differently and more appropriately: this is the intellectual ritual through which he felt obliged to go when he reflected on the methodological foundations of his creative work.

It is clear that even for such a reductionist program "action of the individual" is by no means the only formal scheme conceivable to serve as a starting point. Other efforts are well known in sociology: Wiese's reduction to a basic number of social relations, like cooperation and antagonism; W.I. Thomas' emphasis on a set of basic attitudes, like his four wishes. But Weber was trained as a jurist, had worked on economic problems and was well acquainted with the general concern regarding the foundations of the human sciences. If his new preoccupation with sociology was to have status, it too had to be linked

up with the notion of Handlung.* This would have been all right if Weber had not deeply felt and dimly known that the use of this formal action language needed reconciliation with the empirical study of concerbely observed actions. From the writings available in English it would seem as if he had brushed the problem aside by casual references to an imaginary psychologist who will take care of it. But from untranslated sources it becomes clear that Weber was, at least once, very close to such empirical work and that it caused him considerable anguish. Thereby hangs a tale which deserves a special place in our historical sketch.

A Dualism in Max Weber's Writings on Action

Weber repeatedly emphasized the difference between formal action models which were the business of the social sciences and empirical studies of action which belonged to the psychologist. One of his untranslated methodological papers deals with the relation between the economic doctrine of marginal utility and the Weber-Fechner law (M. Weber, 1908). A colleague, Brentano, had pointed to the geometric

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* How much Weber is influenced by his legal background can be seen by his definition of action (M. Weber, 1913):

> Action (including willful neglect and acquiescence) is defined as an understandable ... behavior toward objects.

The clause in parenthesis echoes almost automatically the kind of writing we have exemplified by Radbruch.

similarity between the law that a constant increase of the stimulus makes for a relatively decreasing increment in sensation, on the one hand, and the law that a constant increase in the supply of the same good makes for a relatively small increase in its utility, on the other hand. Max Weber stressed that the two things should never have any relation to each other. Theoretical economics is not concerned with how people behave but with how to draw up an action scheme from which the idea of marginal utility and other economic concepts could be derived*.

But he was far from deprecating empirical psychological studies. In Germany since 1872 there existed an "Association for social policy." (Thereafter referred to as the Association.) Its core was formed by a group of university professors who were worried about the growing antagonism of the German workers, organized in socialist unions, toward the German state. They wanted on the one hand to impress upon

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* It would be interesting to trace the role of the action language tradition in economics. No one can escape a feeling of pleasure if he reads the detailed introspections of a leading economist of the Austrian school who attempted to use his experience as a mountaineer when he discussed "The Motivation of Present Economic Action Through Future Needs" (E. von Böhm-Bawerk, 1907). How was this translated into the "wirtschaftliche Handlung" of the economic man? And how has this schematization finally been replaced by indifference curves and mathematical formulae? Several authors have stressed the sociological elements in the writings of economists, e.g. Marshall (Parsons, 1932) or Keynes (Lekatchman, 1957). We are raising here the reverse question. The economists at the turn of the century started with vivid psychological descriptions and from them abstracted schemata to explain why capital bore interest and similar matters. What is precisely this procedure, which moves, as Weber put it, "from trivial but uncontestable facts of the daily experiences by many people to theoretical conceptions"?

industrialists the need for social reforms and on the other hand remove the workers from the influence of Marxist thinking. Their procedure was to organize studies of social problems, like cost of living, taxes, tariff, and they discussed the findings at yearly meetings. Representatives of union, industry, and government were invited to participate. Their hope was that studies and discussion would lead to legislation and what today we would call improved labor-management relations.

In 1908 upon the previous suggestion of Alfred Weber, Max' brother, it was decided to start a series of studies on "selection and adjustment (occupational choice and experience)" of workers in large industries. The parenthesis is part of the official title. The investigations were to be based on data available in the offices of selected factories and on direct interviews with their workers. It was to be a collective enterprise of collaborating university institutes, represented by their heads in the association. To guarantee a unity of purpose, three documents were made available. The first is a "methodological introduction" written by Max Weber and only subsequently published in his collected papers on sociology and social policy. He formulates the two problems implied in the title of the program as follows:

> a) What is the effect of big industry on the personal characteristics, the occupational destiny and the private style of life of its workers; what physical and psychological qualities do they develop; and what role do they play in the course of the worker's life?
>
> b) To what extent is the potential and actual development of large industries determined by characteristics of the

labor force rooted in its ethnic, social and cultural origin, its traditions and living conditions? *

The second document is a working plan of about 2,000 words, probably developed by Max Weber in collaboration with the committee of three professors (Herkner, Schmoller and Alfred Weber) who were to coordinate the various studies.** It instructed the collaborators along the following line. They should first describe the technological features of the factories under study. Then they should analyze the composition, geographic origin and work history of the labor force, required qualifications and difficulties to satisfy them. Special emphasis should be put on recent changes. The third and largest block of problem units is devoted to the activities of the worker in the factory: chances for advancement and for satisfaction of special work interests; experience with various wage systems; mobility; training facilities; effect of aging, etc. A final block is clearly meant to refer to sociological problems: social distinctions among types of workers, degree of cohesion, features of daily life different from other population groups with similar incomes, aspirations for their children, etc.

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* Weber's methodological introduction has interesting features. They are discussed in a paper by this author on Max Weber and German industrial sociology. There the history of the whole investigation is traced in considerable detail with special emphasis on Weber's thinking.

** Herkner was the leading labor economist of the period; Schmoller was the great old man among the social scientists, the middle-of-the-roader who, from the beginning, had been president of the association, his main field being economic history; Alfred Weber was Max' younger brother and later became, like him, a sociologist.

Much of this information is to be gotten from experts or by participant observation. But attached to the general plan is a formal questionnaire of twenty-seven questions to be answered by the workers in personal interviews. The questions pertain either to topics on which statistics are desired (like occupation of parents and leisure time activities), or to topics on which, supposedly, only direct interrogation can provide information, such as reasons for their occupational choice and goal in life. Altogether most of the sixteen studies finally published are organized along the outline and give numerical results derived from the questionnaires.*

The greatest attention was given by everyone to a third document prepared by Max Weber. He obviously attached much importance to it. Its two-hundred pages were published at once, and on a later occasion, he described how hard he had worked on it; he made endless computations himself "because only during the continuous and personal analysis of figures will the investigator hit upon those ideas which he needs to interpret his findings and to develop new problems." (Assoc. vol: 138, 1911) The title of the paper is "Regarding the Psycho-Physical Aspects of Industrial Work." Its starting point is the following problem. Differential qualifications of various types of workers are obviously of interest to the social scientist. Can they be measured? Does contemporary work in experimental psychology give some leads for ways of doing so? What other procedures could be used to answer, in the

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*The whole enterprise will hereafter be referred to as "Enquete."

frame of the Association's Enquete, such questions as: are there innate abilities to work? Do they differ among various races? How are they affected by sex and age of the workers, their social origin, etc.? (M. Weber, 1909)

The first hundred pages of the paper (Sections 1-5) are devoted to a detailed review of the existing literature with special emphasis upon the work of Weber's colleague, Kraepelin. He had published five volumes of papers, which reported laboratory experiments dealing with concepts which today still form the foundation of industrial psychology: learning curves, fatigue, monotony, effect of interruptions, etc.*

In a sixth section on "methodological problems," Weber raises the question, based mainly on pencil and paper tests, whether the results of laboratory studies can be applied to the much more complex factory situation. He is doubtful and wishes that experiments on real working machines could be carried out. This he considers financially impossible, because such experiments would cost at least twenty dollars a day. [sic; p. 119] So the next best thing to do is to look at records of piece-rate earnings and of production records as they are kept in the natural course of industrial production. The next 120 pages are devoted to a secondary analysis of such data, which Weber got from the textile factory of a relation.

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* This writer is not aware of any material which would permit tracing the relation between the contemporary work of Kraepelin and the Würzburg school. In Weber's writings and letters one finds frequent reference to publications by contemporary psychologists; we could not find a reference to the Würzburg group, who by all expert agreement turned out to be the most important one.

There is no space to describe here the brilliance of his procedure. He begins by looking for variations during the day, in the course of the week and over longer periods. The findings are partly interpreted with the help of Kraepelin's psychological categories. However, when it comes to discussing individual variations, he introduces the workers' desire to influence the piece rate, the role of the rate booster, the attitude of organized workers and of pietistic women (section 9). In analyzing the role of marriage as a stabilizing influence, the similarity to Durkheim's interpretation of suicide rates is striking, although the latter is not mentioned. (Section 10) At one point he analyzes the behavior of a few workers who attend simultaneously two weaving machines which differ in regard to difficulty and to piece rate. He shows that after a period of trial and error the worker finds an optimum balance between effort and earning; his data and discussion could be directly translated today into a mathematical learning model (pages 209 - 217).*

The monograph anticipates in every respect the approach which today would be taken for an analysis of voting, radio listening, buying

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* Weber at that time was obviously very much interested in the use of statistical methods in the study of more complex social phenomena. A series of books had appeared on the world of manual workers, written by Levenstein and based on questionnaires and letters. Weber wrote a ten page review hailing the idea but criticizing the impressionistic procedures of the author. He gives detailed advice about what kind of tables should have been run and explains the use of correlation coefficients, which were practically unknown in the Germany of that time. He expresses the hope that the problem of classconsciousness could be approached in this way. From the review one gathers that he had spent some time inspecting the original material (M. Weber, 1909).

or any other action performed by large numbers of people under comparable circumstances. Careful statistical analysis is used to deduce as many generalizations from the data as possible and to interpret them in the light of the appropriate conceptualization, either available already or newly advanced for the purpose at hand. The paper was much admired by everyone who was connected with the Enquete. At the 1911 meeting of the Association for Social Policy, when Herkner reviewed the whole Enquete, a special report was given by an assistant of Weber who had collected some additional data, using his approach and corroborating his findings.* What influence did it have on Weber himself? How did he link this whole experience to his and his contemporaries' concern with "Handlung"? The answer is surprising and significant for the development we are tracing in this paper.

Max Weber, after having thrown himself so vigorously into this whole effort, completely deserted it after a while, and as far as we know, never connected it with any of his sociological writings.** As a matter of fact, beyond his original directions, he seems not even to have remained in contact with the actual studies. None of them

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*Association report, page 139 ff. vol. 138.

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There is one cross-reference to the paper on human work in a later edition of the Protestantic Ethic and inversely in section 9 of the paper, a reference to "the larger context in which I have tried to analyze these things elsewhere," (p. 162)

refer to his personal sponsorship. At the general discussion of 1911 he was not listed among the discussants whom the chairman announced in advance. But in the end he did intervene at length. The leading German statistician, Bortkiewich, criticized politely but incisively various shortcomings in the studies. Thereupon, Max Weber got up, acknowledged some mistakes in the statistical procedures, and defended others in considerable detail. True to his great interest in probability literature, he was especially reverent to Bortkiewich, an early leader in the application of mathematics to social data. Only at one point was he flippant. Bortkiewich had very aptly criticized the naive categories which the studies used when they classified the reasons for occupational choice. Of the eight pages in the steneographic report given to Weber's remarks the following lines cover his reaction to one of the basic problems in the empirical study of action (report of 1911 meeting, p. 193:

> I agree that [these data] have no value for the question: why have these people really chosen their occupation? Possibly the answers are quite useless. I consider it possible, however, that they are worthwhile under another aspect: what do people answer to such an -- if you please -- stupid question?
>
> (great hilarity)
>
> Sometimes stupid questions provide quite valuable answers.
>
> (great hilarity)

The ambivalence of this comment is characteristic for his whole attitude during the discussion.* All other participants reiterated their admiration

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* In discussing the occupational choice of women, Bortkiewich pointed out that really two questions are in order: why do they work at all? Why in a specific occupation? Only during the present review did the present writer become aware of Bortkiewich's priority. In a previous publication (Lazarsfeld, 1934) he thought that he was the first to develop such distinctions. No one today doubts their significance; it is surprising, therefore, that Weber overlooked the significance of Bortkiewich's remarks, as he himself made very astute remarks on questionnaire construction in his discussion of Levenstein, cited above.

for the great importance of the Enquete. Weber , while defending many of its details, stressed that in his opinion nothing much came of the whole affair, at best a few hypotheses and the "high probability that with the help of future material after a long, long while valuable and crucial results might ensue." (Report of 1911 meeting, p. 190)

Having pointed to a strange reaction of Weber, we might as well go a step further and venture a more general interpretation. Why did Weber so emphasize the separation of empirical psychology from his own sociological work? There is indirect evidence that real resistance was at work. Let us look at a sequence in his life, as reported by his wife. He was deeply devoted to his mother, from whom he acquired his interest in social amelioration, developed in sharp contrast to the political conservatism of his father, a member of the German parliament. At twenty-nine he married after having been greatly worried whether he would ever be able to "satisfy a woman," (Marianne Weber, p. 171 and 195) When he was thirty-three there was an open and violent break with his father over the question whether his mother would be permitted to visit him alone each year for a month. The quarrel led to a separation of his parents; the father went alone on a trip and died two months after the scene without a reconciliation with his son Max. Two more months later the latter had his famous nervous breakdown, which for almost five years made all intellectual work impossible. Sketching the course of events in four pages, Marianne Weber refers to the sense of tragedy everyone felt at the funeral of Weber senior and adds:

> But the oldest son (Max) has no feeling of guilt; the quarrel of seven weeks before was clearly inevitable.

Even for the layman this does not appear to be a very insightful statement. Given the intellectual closeness of Weber and his wife, we may assume that this resistance to psychological interpretation was common to both. One bit of corroboration comes from a brief reference to Freud in the paper on industrial work (p. 249): "These theories are becoming increasingly outmoded." It is greatly to Weber's credit that he knew at all about Freud in 1908; but the diagnosis as to Freud's chances for intellectual survival is surprisingly wrong for a man who, in connection with other innovations of the time -- the role of probability, e.g. -- was so farsighted.*

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* Had Max Weber been a poet or a prime minister some psychoanalyst would undoubtedly have written his biography. (The older Pitt, Lord Chatham, was a victim of similiar cycles of powerful activities and paralyzing depressions; several biographies of him exist.) Such an analyst certainly would point to the way Marianne reacted to her husband's illness (p. 249):

> Weber's supreme self-sufficiency had often raised, for her, the question whether he really needed her. [Mrs. Weber always refers to herself in the third person.] Now she doesn't need to doubt any more. Out of the dark abyss, which his illness creates, great happiness emanates for her: the strong man needs her continuous care and presence; she is permitted to serve him.

The question could be raised, incidentally, why the two students of politics who were so proud of their realistic approach -- Machiavelli and Weber -- were both so defeated in their desire to obtain a major public office.

There is another aspect to this almost compulsory tendency to keep the reality of human action separated from the constructs of the human sciences. In the papers connected with the Enquete, Weber uses terminology which does not appear in his historical writings. The workers in the textile factory have moods, dispositions and, most of all, attitudes. The latter term appears textually about a dozen times (Attituede).* Now none of this, as far as we can see, ever appears in his historical writings. We have selected Parsons translation of the Protestant Ethics as a test. The term "attitude" appears about forty times in the translation, and the context makes its appearance seem quite natural. Still, in none of these places did Weber himself use the word in the German text. He has a plethora of nouns instead: Anschauung Gesamtstimmung, Gesinnung, Art des Empfindens, etc. He obviously felt that action of historical persons or groups, which were reconstructed from letters, recorded customs, etc., should be described in a terminology different from one which the "psychologist" uses if he studies real human beings. There is, indeed, an important methodological problem here. But Weber did not solve it and, as a matter of fact, never even formulated it.

He was, as we just saw, acquainted with, and for a while strongly attracted by, the empirical study of action which had started at the beginning of the twentieth century. And yet he strained to link the new science of sociology exclusively to the formal action language of the traditional German human sciences. Why? We suggest that an

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* It also appears in his general discussion of the use of questionnaires in social research, mentioned above.

unconscious resistance to an alternative solution was partly responsible; he felt personally endangered by material which came too close to the reality of human action without historical distance or pseudo-logical terminology to shield him.* The matter has been well formulated by Paul Kecskemeti. Handlung stands in the center of Weber's methodological writings, only tangential to his thinking, and played no role at all in his substantive work (personal communication).

All this would only have historical interest if it were not for a young American traveller who came to Heidelberg a few years after Weber's death. Everyone there was still living under the impact of this intellectual giant. His historical writings were the pride of German sociology. His work for the Association probably was rarely mentioned--maybe partly because he had there engaged in many very ill-tempered fights (including court trials) with colleagues and de mortuis nil nisi bene. If one wanted to know the methodological foundations of his work, where could one better turn than to his own declarations. There one met something of highest prestige in Germany -- Handlung. And it sounded like something which in the American scene had been badly neglected.

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* In this connection one should take into account the difference in style which is so striking in Weber's writings. Repeatedly it has been remarked that his methodological writings are unnecessarily involved as compared to his substantive work, and even more to his verbal presentation (Marianne Weber, p. 322 and her report, p. 683; that Weber had to drop a course on sociological categories, because students could not understand him; also M. Rheinstein, 1957 , Preface.) His most turgid writing occurs undoubtedly in the above mentioned paper, where sociological concepts are defined in terms of action language. In contrast, the empirical paper on human work is written with lucid simplicity. We have no explanation for this contrast.

The visitor did not need to know that a formal "action-language" had become traditional within the human studies and had a very different purpose than the empirical study of human action. So Weber came to this country in terms of a "theory of action" which -- whatever positive contribution it made -- blocked American sociologists from serious concern with human action as a topic of empirical inquiry.

Parsons' Revival of Max Weber's Action-Language

It is well known how T. Parsons tried to build up the notion of "social system" from a set of "unit acts" and their interconnections over collectives and over time. He was aware of, and pointed out, the many contradictions and obscurities in Weber's methodological writing; nevertheless he was convinced, and stated so explicitly, that Weber's concept of action is substantially the one he himself wanted to make the basis for a modern program of sociology (Parsons, 1937, p. 642).

Parsons' general contributions are not under discussion here. What matters to us is the role the "Harvard movement" plays in the history of scientific concern with human action. We have by now seen that it has two traditions; one is the empirical approach, dominant among prebehavioristic psychologists. The other is a formal one characteristic for the German human studies and well exemplified by some of Weber's methodological papers. In order to locate Parsons properly, his relation to Weber needs to be clearly understood. He distinguishes four elements of the "unit act": the actor, the goal, the situation (means) and the normative orientation. He introduces these distinctions with the sentence: "An act involves logically the

following (elements)." (Parsons, 1937, p. 44) Now we saw above, page 51, that Weber, too, used "logical" in such contexts. The distinctions are logical in the sense that they are made for a definite purpose: to focus attention on the roles of norms and expectations. It would be as logical to make other distinctions: e.g., actor, goal, means and influences which help the actor select the means appropriate for his goal. This scheme would focus attention on technical and social problems of manipulation, characterize social system by the prevailing type of communication, and so on. An action-scheme proposed for classificatory purposes is necessarily suited to the main substantive interest of the proponent. Parsons has never denied the classificatory character of his system and the discussion in the literature has followed his lead (B. Smith, 1952; Swanson, 1953; Ramsoy, 1957).*

But Parsons has added an utilized one important methodological advance which has been made in recent years. Weber, as we saw, distinguished types of action and related to each a corresponding sociological concept. Parsons, however, proceeds in two steps. He first "deduces" from an action scheme his so-called pattern variables and then combines those to characterize various types of social configurations. The notion of variables and the understanding of types as combinations therefrom, Weber probably missed because in his comparative sociology/was restricted to a few cases and scarce historical material.**

*Just as the legal philosopher desires to organize and relate the various punishable acts, so Parsons himself says explicitly "the discrimination of various possible modes of normative orientation is one of the most important questions with which this study will be confronted" (Parsons 1937, p. 47). In the end this leads to the derivation of the notion of "culture" more elaborate but in principle similar to the derivation of the German "objektive Geist" from an appropriate notion of "Handlung."

**For the history and the importance of the notion of "property space" see Lazarsfeld and Rosenberg, 1954 (p. 40).

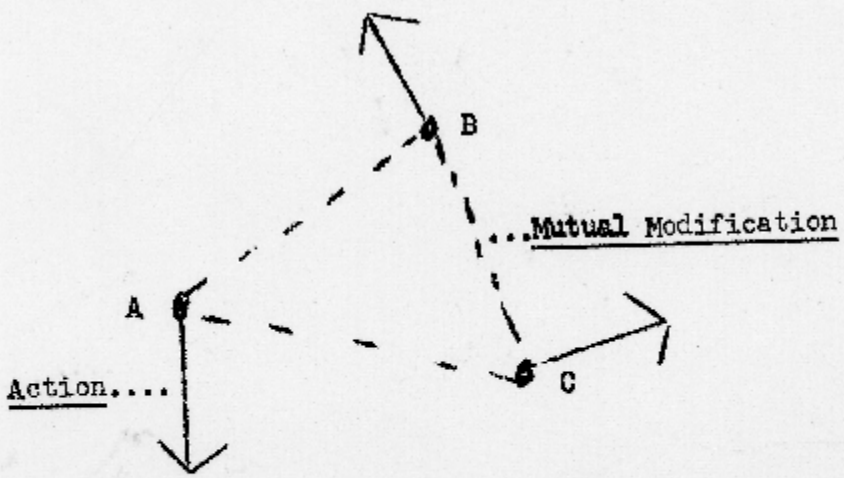
Suppose he could have visualized the Yale cross-cultural files or UNESCO's comparative attitude surveys and thus conceived the application of variables to the description of collectives. Could it be that then the methodological contradictions between the clarity of his statistical insights and his ritual use of the German action language would somehow have been resolved? The possibilities can be seen in an instructive revue by Peter Blau (1957) of the procedures available to study comparatively a large number of organizations. He mentions in passing that Weber's notion of "Verstehen" in sociological research can be seen as the use of intervening variables in a statistical analysis, the units of which are collectives.

It may be, however, that for Parsons the reverse question can be raised: Given that he himself says that his pattern variables are his main contribution (Parsons, 1953), why do they have to be "derived" from an action scheme, the only justification of which is that it helps to develop various classifications for the pattern variables? The question is welcome to remain rhetorical, because our task is to point to the consequences of Parsons' move, whatever it merits.

And here one is struck by the paradoxical convergence of two trends moving in opposite directions. In Germany the tradition of action schemes in the human sciences impeded the incorporation of empirical studies of action into the body of legitimate sociological endeavor. In the United States, however, this was just what would have been the relief for latent intellectual tensions: protest against primitive behaviorism, desire to overcome the static side of early community surveys, opposition to irrelevant minutiae. The mere term "action" was

bound to start a crusade in the USA, and hardly anyone knew or noticed that here were two brothers with the same name and maybe the wrong one was anointed.

At this point a linguistic element enters which should not be underrated: the ambiguity of the English term "interaction." Let us construct an extremely simple scheme which can bring out the main idea. We have three persons, A, B, and C, who each pursue a goal in an area of activity, say politics or occupational advancement. We indicate this goal by a solid arrow attached to the name of each person, and one might think of this arrow going into the third dimension outside the paper. The three have some social relations with each other, and these are indicated by broken lines. The lines may symbolize a large variety of things: the expectations A,B, and C have of each other; the extent to which they depend upon each other's approval; the practical help or difficulties they do or may provide for the pursuit of the three groups.



The German language would provide two very different terms for the arrows and the broken lines, even on the highest level of generalization. The goal pursuing activities are, of course, our old friend Handlung. For the various ways in which people modify each other's action there is no equally standardized word. But there was much acceptance of a term which Karl Bühler proposed: gegenseitige Steuerung. One possible translation would be "mutual control," if the term control is taken in its weakest connotation, just indicating that people somehow modify each other's pursuits. The word was taken from the then newly developing field of electronics. If we use a public address system, then the main source of electric energy comes somewhere from a central power house; our voice, when we speak into the microphone, only modifies this energy by moving light pieces of steel back and forth in the electric field. Maybe, for the present purpose, the best term to use is "mutual modification." It, together with the individual actions themselves, describes the social system in its most primitive form. (Be it remembered that we include under mutual modification what goes on in the mind of A as he takes into account the expectations of B, even if B is not aware of his own role in A's stream of activity; influence is a special case of modification.)

The distinction between action and mutual modification is, of course, a schematic one, and there are continuous interrelations which come about over time. From moment to moment the actions of a group of individuals are changed by mutual modification; in turn, the modifications shift as the goal pursuing activities change in their character. But this is a well known situation which occurs whenever we deal with a number of factors, and in its most precise form can

be represented by a system of simultaneous differential equations. This of course is not the line to be pursued here. What matters is the way in which the English language comes into the picture.

There is a great temptation to call the arrows in our scheme actions and the broken lines interactions. The danger with this terminology is that the basic difference between the two conceptual ideas gets lost because of the linguistic similarity of the two terms. In German the word interaction does not exist. Max Weber used the term "social action" to indicate the combined phenomenon, the action of individuals and the mutual modifications they bring to bear upon each other. German sociology after Weber didn't follow his conceptualization and, in one way or another, stressed the distinction between the action, on the one hand, and modification, on the other.

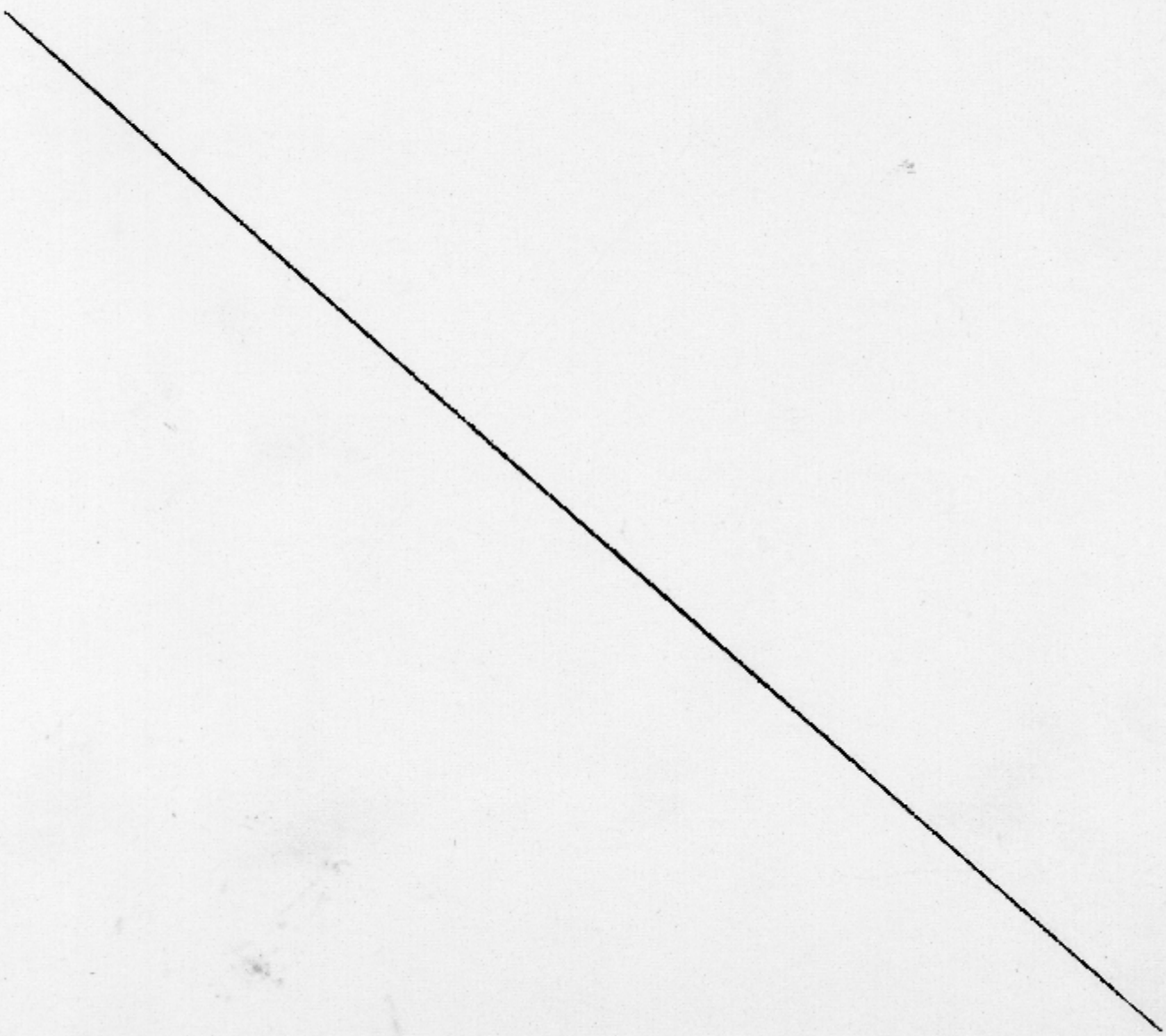
For a more extended analysis, much would have to be added. B is not only a source of modification for A's action. He can be the object, for instance, when A's goal is to get a favor from B. We also don't need to leave the matter on an atomistic level; out of the congery of actions of A,B, and C and their mutual modifications norms will develop which affect all of them. But our main point can already be made on the most primitive level. We submit that the following happened. The English terminology of action and interaction made many people forget the very different conceptual purposes which the two terms were supposed to serve. It is quite understandable that sociologists are more interested in mutual modification of individual actions than in the study of those actions themselves. But it is a great loss that, as a result, the "theory of action" became co-equal with the theory of mutual modification and crowded out the other half of the whole problem. A recent review of German sociology (Becker et al,1957)

pointed out that what German sociologists called the socius, the human being in his social context, is now translated in America by the term actor. But a full theory of human behavior really requires both terms. What the new Harvard tradition has in mind is, indeed, the socius. Now a second kind of conceptualization is needed which looks at the human being as a goal pursuing entity whose activities are modified by his socii. It is probably too late to reclaim the term actor for this other purpose; but one should at least be aware that a terminological gap has been created which can very easily become an intellectual pitfall.

Terminologies always have a bearing upon the course of scientific endeavors. A great desire for a "new learning" is abroad and Parsons has, in a way, become its symbol. But, unfortunately, he took as his coat of arms a term which he really does not need and which is now surely missed by those who want to continue another tradition: the empirical study of Handlung -- be it in terms of activities which many people perform under similar circumstances or in terms of the conditions which make for the change of rates, by which these activities are recorded. In Europe until the disintegration of the academic community in the thirties, one could still have assumed that psychologists would take care of it. But, as we have tried to show, a number of circumstances brought this development to a standstill. And American psychologists, with their great emphasis on laboratory experiments, are not likely to study complex actions characteristic of life in modern society. At the same time sociologists are urged from Harvard to reserve the impelling symbol of a theory of action for schematic classificatory purposes. Who, thus, will speak for the concrete actor, the one who in all spheres of life has to choose between alternatives, is object and

relayer of advice and who when he matches an experience with his expectations changes his mind and moves on to new goals.

We have no definite answer to offer. But the technique to which this volume is devoted holds out certain promises. By its very nature it studies activity in progress. And it seems to solve at least some of the difficulties which other procedures have encountered.



Editorial Insert for the Present Draft

The present draft of this paper will circulate among colleagues prior to its inclusion as a final piece in a book on the so-called panel technique. For the present purpose it is enough to know that this means the statistical analysis of repeated observations on the same people. The data can be of any kind: objective behavior, personal characteristics, attitudes, exposure to mass media, presence at events, contact with other people, etc. The analysis consists in relating such data across time as well as cross-sectionally. The statistical unit may be individuals, pairs, or larger collectives. In the last two cases, the data would include sociometric information (the pair might choose each other as friends or have an asymetric relation) or rates (counties with Democratic or Republican majority). In principle the panel technique is time series analysis for qualitative data. For a general discussion see P.F. Lazarsfeld, "The Use of Panels in Social Research" Proceedings of the Amer. Philosophical Soc., vol. 92, No. 5, 1948 For a description of some basic analytical procedures, H. Zeisel "Say It With Figures" 4th Edition, Chicago, Harper, 1957.

Panel Technique and Analysis of Action

One of the traditional difficulties in the schematic discussion of actions is their delineation. If I write an application in order to get a fellowship, is the letter writing an action or only a means? (Schuetz, 1953). This is probably a pseudo-problem. What we can observe is a stream of activity for the purpose of conceptualized inquiry.

The panel analyst can, so to speak, have his cake and eat it too. He might at time t_1 characterize his subjects according to their scientific productivity; at time t_2 whether they applied for a fellowship and at time t_3 whether they obtained it. Panel analysis leaves him free to choose as the object of his analysis (a) whether more productive people are more likely to apply or B) whether they are more likely to be successful <u>if</u> they apply.

Furthermore we can trace all the determinants of a decision. At time t_1 we might ask whether the respondents <u>intend</u> to apply and whether one of their friends has the same intention; at time t_2 we can ascertain whether our respondent has actually applied. This would permit us to find out whether a plan prevailing in a group of friends is more likely to be carried out than an isolated one.

If we are interested in influences, we can use a technique, which in a way solves the tension-valence problem of Lewin. Suppose there is a high correlation between wanting a fellowship and noticing announcement of opportunities in professional magazines. Do the announcements stimulate the desire, or does the interest alert the perception? The sixteen-fold tables discussed in another section of this volume provide, in principle, the answer. We can find at time t_1 people who want a fellowship but have not seen an announcement as well as people who show the opposite combination. At time t_2 many will have "harmonized": they will either be affirmative on both points or will still not have seen an announcement and have lost their desire for a fellowship.

According to whether the equilibrium comes about in the direction of tension or valence (interest or stimulation), we shall have an answer to the preceding question. Of course the relative weight of inner and outer factors might be different for different people. Well, nothing keepsus from plugging in a personality test at the first interview; the analysis can then be carried out separately for suggestible and non-suggestible respondents.

We don't want to deny the technical difficulties of such a program. Still, in certain areas, it has been well advanced. This is especially true for the role of expectations in economic decision, as exemplified in this volume by the panel studies of Franco Modigliani and his group. Another case in point are various studies of election campaigns. The "implementation" of a voting decision, its course from a vague beginning to the final act at the polls has been used as a standard for the panel analysis of an action in Chapter XIII pf Berelson, et al (1956).

So far the "others" have only appeared as possible influences or as representatives of a primary group. But this still makes for an artificial assymetry. For the respondent himself is a member of his group and obviously influences the other members. This can be taken care of by "sociometric panels" where the units are pairs or groups rather than individuals. If we want to weigh the relative influence of husbands and wives upon each other, we can keep couples under observation over a period of time. Again using the sixteen-fold table technique, we can start at time t_1, with couples in disagreement on an issue; how many will have reached agreement at time 2 ?

Whatever agreement is reached, in which direction will it be resolved, the wife agreeing with the husband or the other way around? How does the resolution vary according to the topic under scrutiny and the social class of the respondents? Of course the use of pairs (or groups) as statistical units of a panel analysis rapidly increase the number of variables and therefore the complexity of the task. Still the discussion and examples of sociometric panels reported in this volume show a promising beginning.

The title of the present series of studies refers to the study of "short-range change." The promise of the panel techniques lie so far mainly with studies extended over a few months. But one can visualize future efforts where much larger periods are covered. There are available data on human development over periods of more than twenty years. Usually they are presented in the form of "growth curves" showing each variable separately as it changes over time. One day this material can be subject to a more "panel-like" analysis. Do children who develop their social contacts slowly up to adolescence show, subsequently, a more rapid intellectual growth? Is a rapid intellectual development usually followed by difficulties with the other sex later on? This is one direction for future inquiries. The other points to historical studies. Lipset, in this volume, gives an example from the Civil War period, where changes in vote across southern counties provide a new kind of understanding by a simple transfer of panel analysis to data which were available but unexplored for decades.

These concluding remarks should not be understood as offering the panel technique as a panacea for all the problems encountered in our historical review. The stress, rather, is on the hospitality which the procedure offers to a variety of ideas. The formal schemes of action, e.g., can serve as important sources for variables to be plugged into panel studies. Weber stresses the expectation people have of each other, Kluckholm (1954) the difference between two experiences; that of a desired and of a desirable object. No reason why these sentiments should not be ascertained at one time and studied in their relation to overt behavior at a subsequent period. Another application comes from the idea of "mental experiments." To understand why a person acted in a certain way, we should visualize how he would have acted under different circumstances. If we have collected observations over a period of time, we can compare actors and non-actors regarding their situation and their outlook at preceding periods. Finally panel data permit giving operational form to certain concepts. Charlotte Bühler stresses the difference between "specific minded people" who do pursue a goal consistently and those who are tossed around by outside factors; what better way is there, then, to compare their intentions and their experiences over a period of time?

All this does not lead to a "theory of action." It does not even clarify whether this is a slogan or a serious intellectual concern. But it promises to unify some of the data, ideas and problems which have accumulated over the last decades.

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