



## The Bearing of Empirical Research upon the Development of Social Theory

Robert K. Merton

*American Sociological Review*, Volume 13, Issue 5 (Oct., 1948), 505-515.

Stable URL:

<http://links.jstor.org/sici?sici=0003-1224%28194810%2913%3A5%3C505%3ATBOERU%3E2.0.CO%3B2-F>

---

Your use of the JSTOR archive indicates your acceptance of JSTOR's Terms and Conditions of Use, available at <http://www.jstor.org/about/terms.html>. JSTOR's Terms and Conditions of Use provides, in part, that unless you have obtained prior permission, you may not download an entire issue of a journal or multiple copies of articles, and you may use content in the JSTOR archive only for your personal, non-commercial use.

Each copy of any part of a JSTOR transmission must contain the same copyright notice that appears on the screen or printed page of such transmission.

*American Sociological Review* is published by American Sociological Association. Please contact the publisher for further permissions regarding the use of this work. Publisher contact information may be obtained at <http://www.jstor.org/journals/asa.html>.

---

*American Sociological Review*  
©1948 American Sociological Association

JSTOR and the JSTOR logo are trademarks of JSTOR, and are Registered in the U.S. Patent and Trademark Office. For more information on JSTOR contact [jstor-info@umich.edu](mailto:jstor-info@umich.edu).

©2002 JSTOR

# American SOCIOLOGICAL Review

October  
1948

Volume 13  
Number 5

The Official Journal of the American Sociological Society

---



## THE BEARING OF EMPIRICAL RESEARCH UPON THE DEVELOPMENT OF SOCIAL THEORY\*

ROBERT K. MERTON

*Columbia University*

HISTORY has a certain gift for outmoding stereotypes. This can be seen, for example, in the historical development of sociology. The stereotype of the social theorist high in the empyrean of pure ideas uncontaminated by mundane facts is fast becoming no less outmoded than the stereotype of the social researcher equipped with questionnaire and pencil and hot on the chase of the isolated and meaningless statistic. For in building the mansion of sociology during the last decades, theorist and empiricist have learned to work together. What is more, they have learned to talk to one another in the process. At times, this means only that a sociologist has learned to talk to himself since increasingly the same man has taken up both theory and research. Specialization and integration have developed hand in hand. All this has led not only to the realization that theory and empirical research *should* interact but to the result that they *do* interact.

As a consequence, there is decreasing need for accounts of the relations between theory and research to be wholly programmatic in character. A growing body of theoretically oriented research makes it progressively possible to discuss with profit the actual relations between the two. And, as we all know, there has been no scarcity of such discussions. Journals abound with them. They generally center on the role of theory in research, setting forth, often with admirable lucidity, the functions of theory in the initiation, design and prosecution of empirical inquiry. But since this is not a one-way relationship, since the two *interact*, it may be useful to examine the other direction of the relationship: the role of empirical research in the development of social theory. That is the purpose of this paper.

### THE THEORETIC FUNCTIONS OF RESEARCH

With a few conspicuous exceptions, recent sociological discussions have assigned but one major function to empirical research: "testing" or "verification" of hypotheses. The model for the proper way of performing this function is as familiar as it is clear. The investigator begins with a hunch or hypothesis, from this he draws various in-

---

\* Paper read before the annual meeting of the American Sociological Society, Cleveland, Ohio, March 1-3, 1946. This may be identified as Publication No. A-89 of the Bureau of Applied Social Research, Columbia University. Manuscript received April 19, 1948.

ferences and these, in turn, are subjected to empirical test which confirms or refutes the hypothesis.<sup>1</sup> But this is a logical model, and so fails, of course, to describe much of what actually occurs in fruitful investigation. It presents a set of logical norms, not a description of the research experience. And, as logicians are well aware, in purifying the experience, the logical model may also distort it. Like other such models, it abstracts from the temporal sequence of events. It exaggerates the creative role of explicit theory just as it minimizes the creative role of observation. For research is not merely logic tempered with observation. It has its psychological as well as its logical dimensions, although one would scarcely suspect this from the logically rigorous sequence in which research is usually reported.<sup>2</sup> It is both the psychological and logical pressures of research upon social theory which we seek to trace.

It is my central thesis that empirical research goes far beyond the passive role of verifying and testing theory: it does more than confirm or refute hypotheses. Research plays an active role: it performs at least four major functions which help shape the development of theory. It *initiates*, it *reformulates*, it *deflects* and *clarifies* theory.<sup>3</sup>

### 1. The Serendipity Pattern

(The unanticipated, anomalous and strategic datum exerts a pressure for initiating theory.)

Under certain conditions, a research finding gives rise to social theory. In a previous

paper, this was all too briefly expressed as follows: "Fruitful empirical research not only tests theoretically derived hypotheses; it also originates new hypotheses. This might be termed the 'serendipity' component of research, *i.e.*, the discovery, by chance or sagacity, of valid results which were not sought for."<sup>4</sup>

The serendipity pattern refers to the fairly common experience of observing an *unanticipated, anomalous and strategic* datum which becomes the occasion for developing a new theory or for extending an existing theory. Each of these elements of the pattern can be readily described. The datum is, first of all, unanticipated. A research directed toward the test of one hypothesis yields a fortuitous by-product, an unexpected observation which bears upon theories not in question when the research was begun.

Secondly, the observation is anomalous, surprising,<sup>5</sup> either because it seems inconsistent with prevailing theory or with other established facts. In either case, the seeming inconsistency provokes curiosity; it stimulates the investigator to "make sense of the datum," to fit it into a broader frame of knowledge. He explores further. He makes fresh observations. He draws inferences from the observations, inferences depending largely, of course, upon his general theoretic orientation. The more he is steeped in the data, the greater the likelihood that he will hit upon a fruitful direction of inquiry. In the fortunate circumstance that

<sup>1</sup> See, for example, the procedural review of Stouffer's "Theory of intervening opportunities" by G. A. Lundberg, "What are Sociological Problems?", *American Sociological Review*, VI (1941), 357-359.

<sup>2</sup> See R. K. Merton, "Science, Population and Society," *The Scientific Monthly*, XLIV (1937), 170-171; the apposite discussion by Jean Piaget, *Judgment and Reasoning in the Child*, London, 1929, Chaps. V, IX, and the comment by William H. George, *The Scientist in Action*, London, 1936, p. 153. "A piece of research does not progress in the way it is 'written up' for publication."

<sup>3</sup> The fourth function, clarification, will be elaborated in a complementary paper by Paul F. Lazarsfeld.

<sup>4</sup> R. K. Merton, "Sociological Theory," *American Journal of Sociology*, L (1945), 469n. Interestingly enough, the same outlandish term 'serendipity' which has had little currency since it was coined by Horace Walpole in 1754 has also been used to refer to this component of research by the physiologist Walter B. Cannon. See his *The Way of an Investigator*, New York: W. W. Norton, 1945, Chap. VI, in which he sets forth numerous instances of serendipity in several fields of science.

<sup>5</sup> Charles Sanders Pierce had long before noticed the strategic role of the "surprising fact" in his account of what he called "abduction," that is, the initiation and entertaining of a hypothesis as a step in inference. See his *Collected Papers*, Harvard University Press, 1931-35, VI, 522-528.

his new hunch proves justified, the anomalous datum leads ultimately to a new or extended theory. The curiosity stimulated by the anomalous datum is temporarily appeased.

And thirdly, in noting that the unexpected fact must be "strategic," *i.e.*, that it must permit of implications which bear upon generalized theory, we are, of course, referring rather to what the observer brings to the datum than to the datum itself. For it obviously requires a theoretically sensitized observer to detect the universal in the particular. After all, men had for centuries noticed such "trivial" occurrences as slips of the tongue, slips of the pen, typographical errors, and lapses of memory, but it required the theoretic sensitivity of a Freud to see these as strategic data through which he could extend his theory of repression and symptomatic acts.

The serendipity pattern, then, involves the unanticipated, anomalous and strategic datum which exerts pressure upon the investigator for a new direction of inquiry which extends theory. Instances of serendipity have occurred in many disciplines, but I should like to draw upon a current sociological research for illustration. In the course of our research into the social organization of Craftown,<sup>6</sup> a suburban housing community of some 700 families, largely of working class status, we observed that a large proportion of residents were affiliated with more civic, political and other voluntary organizations than had been the case in their previous places of residence. Quite incidentally, we noted further that this increase in group participation had occurred also among the parents of infants and young children. This finding was rather inconsistent with commonsense knowledge. For it is well known that, particularly on the lower economic levels, youngsters usually tie parents down and preclude their taking active part in organized group life outside the home. But Craftown parents themselves readily ex-

plained their behavior. "Oh, there's no real problem about getting out in the evenings," said one mother who belonged to several organizations. "It's easy to find teen-agers around here to take care of the kids. There are so many more teen-agers around here than where I used to live."

The explanation appears adequate enough and would have quieted the investigator's curiosity, had it not been for one disturbing datum: like most new housing communities, Craftown actually has a very small proportion of adolescents—only 3.7%, for example, in the 15-19 year age group. What is more, the majority of the adults, 63%, are under 34 years of age, so that their children include an exceptionally large proportion of infants and youngsters. Thus, far from there being many adolescents to look after the younger children in Craftown, quite the contrary is true: the ratio of adolescents to children under ten years of age is 1:10, whereas in the communities of origin, the ratio hovers about 1:1.5.<sup>7</sup>

We were at once confronted, then, by an anomalous fact which was certainly no part of our original program of observation. This should be emphasized. We manifestly did not enter and indeed could not have entered upon the field research in Craftown with a hypothesis bearing an illusory belief in the abundance of teen-age supervisors of children. Here was an observation both unanticipated and anomalous. Was it also strategic? We did not prejudge its "intrinsic" importance. It seemed no more and no less trivial than Freud's observation during the last war (in which he had two sons at the front) that he had mis-read a newspaper headline, "*Die Feinde vor Görz*" (The *Enemy* before Görz), as "*Der Friede von Görz*" (The *Peace* of Görz). Freud took a trivial incident and converted it into a strategic fact. Unless the observed discrepancy between the subjective impressions of Craf-

<sup>7</sup> Essentially the same discrepancies in age distribution between Craftown and communities of origin are found if we compare proportions of children under ten with those between 10 and 19. If we make children under five the basis for comparison, the disproportions are even more marked.

<sup>6</sup> Drawn from continuing studies in the Sociology and Social Psychology of Housing, under a grant from the Lavanburg Foundation.

town residents and the objective facts could undergo a somewhat similar transformation it had best be ignored, for it plainly had little "social significance."

What first made this illusion a peculiarly intriguing instance of a general theoretic problem was the difficulty of explaining it as merely the calculated handiwork of vested-interests engaged in spreading a contrary-to-fact belief. Generally, when the sociologist with a conceptual scheme stemming from utilitarian theory observes a patently untrue social belief, he will look for special groups in whose interest it is to invent and spread this belief. The cry of "propaganda!" is often mistaken for a theoretically sound analysis.<sup>8</sup> But this is clearly out of the question in the present instance: there are plainly no special-interest groups seeking to misrepresent the age-distribution of Craftown. What, then, was the source of this social illusion?

Various other theories suggested points of departure. There was Marx's postulate that it is men's "social existence which determines their consciousness." There was Durkheim's theorem that social images ("collective representations") in some fashion reflect a social reality although "it does not follow that the reality which is its foundation conforms objectively to the idea which believers have of it." There was Sherif's thesis that "social factors" provide a framework for selective perceptions and judgments in relatively unstructured situations. There was the prevailing view in the sociology of knowledge that social location determines the perspectives entering into perception, beliefs and ideas. But suggestive as these general orientations<sup>9</sup> were, they did not directly suggest

which features of "social existence," which aspects of the "social reality," which "social factors," which "social location" may have determined this seemingly fallacious belief.

The clue was inadvertently provided by further interviews with residents. In the words of an active participant in Craftown affairs, herself the mother of two children under six years of age:

"My husband and I get out together much more. You see, there are more people around to mind the children. *You feel more confident about having some thirteen-or-fourteen-year-old in here when you know most of the people. If you're in a big city, you don't feel so easy about having someone who's almost a stranger come in.*"

This clearly suggests that the sociological roots of the "illusion" are to be found in the structure of community relations in which Craftown residents are enmeshed. The belief is an unwitting reflection, not of the statistical reality, but of the community cohesion. It is not that there are objectively more adolescents in Craftown, but more who are *intimately known* and who, therefore, *exist socially* for parents seeking aid in child supervision. Most Craftown residents having lately come from an urban setting now find themselves in a community in which proximity has developed into reciprocal intimacies. The illusion expresses the perspective of people for whom adolescents as potential child-care aides "exist" only if they are well-known and therefore merit confidence. In short, perception was a function of confidence and confidence, in turn, was a function of social cohesion.<sup>10</sup>

From the sociological viewpoint, then, this unanticipated finding fits into and extends the theory that "social perception"

<sup>8</sup> To be sure, vested-interests often do spread untrue propaganda and this may reinforce mass illusions. But the vested-interest or priestly-lie theories of fallacious folk beliefs do not always constitute the most productive point of departure nor do they go far toward explaining the bases of acceptance or rejection of the beliefs. The present case in point, trivial though it is in any practical sense, is theoretically significant in showing anew the limitations of a utilitarian scheme of analysis.

<sup>9</sup> For the differences between "theory" and "general orientations," see Merton, "Sociological theory," *op. cit.*, 464.

<sup>10</sup> Schedule data from the study provide corroborative evidence. In view of the exceptionally high proportion of young children, it is striking that 54 per cent of their parents affirm that it is "easier in Craftown to get people to look after our children when we want to go out" than it was in other places where they have lived; only 21 per cent say it is harder and the remaining 25 per cent feel there is no difference. Those who come from the larger urban communities are more likely to report

is the product of a social framework. It develops further the "psychology of social norms,"<sup>11</sup> for it is not merely an instance of individuals assimilating particular norms, judgments, and standards from other members of the community. The social perception is, rather, a by-product, a derivative, of the structure of human relations.

This is perhaps sufficient to illustrate the operation of the serendipity pattern: an unexpected and anomalous finding elicited the investigator's curiosity, and conducted him along an unpremediated by-path which led to a fresh hypothesis.

## 2. *The Recasting of Theory*

(New data exert pressure for the elaboration of a conceptual scheme.)

But it is not only through the anomalous fact that empirical research invites the extension of theory. It does so also through the repeated observation of hitherto neglected facts. When an existing conceptual scheme commonly applied to a given subject-matter does not adequately take these facts into account, research presses insistently for its reformulation. It leads to the introduction of variables which have not been systematically included in the scheme of analysis. Here, be it noted, it is not that the data are anomalous or unexpected or incompatible with existing theory; it is merely that they have not been considered pertinent. Whereas the serendipity pattern centers in an apparent inconsistency which presses for resolution, the reformulation pattern centers in the hitherto neglected but relevant fact which presses for an ex-

tension of the conceptual scheme.

Examples of this in the history of social science are far from limited. Thus it was a series of fresh empirical facts which led Malinowski to incorporate new elements into a theory of magic. It was his Trobrianders, of course, who gave him the clue to the distinctive feature of his theory. When these islanders fished in the inner lagoon by the reliable method of poisoning, an abundant catch was assured and danger was absent. Neither uncertainty nor uncontrollable hazards were involved. And here, Malinowski noted, magic was not practiced. But in the open-sea fishing, with the uncertain yield and its often grave dangers, the rituals of magic flourished. Stemming from these pregnant observations was his theory that magical belief arises to bridge the uncertainties in man's practical pursuits, to fortify confidence, to reduce anxieties, to open up avenues of escape from the seeming impasse. Magic was construed as a supplementary technique for reaching practical objectives. It was these empirical facts which suggested the incorporation of new dimensions into earlier theories of magic—particularly the relations of magic to the fortuitous, the dangerous and the uncontrollable. It was not that these facts were *inconsistent* with previous theories; it was simply that these conceptual schemes had not taken them adequately into account. Nor was Malinowski testing a preconceived hypothesis—he was developing an enlarged and improved theory on the basis of suggestive empirical data.

For another example of this pressure of empirical data for the recasting of a specific theory we turn closer home. The investigation dealt with a single dramatic instance of mass persuasion: broadcasting at repeated intervals over a span of eighteen hours, Kate Smith, a radio star, sold large quantities of war-bonds in the course of the day. It is not my intention to report fully on the dynamics of this case of mass persuasion;<sup>12</sup> for present purposes, we are con-

---

greater ease in obtaining assistance in Crafttown. Moreover, as we would expect from the hypothesis, those residents who are more closely geared in with Crafttown, who identify themselves most fully with it, are more likely to believe it easier to find such aid; 61 per cent of these do so as against 50 per cent of those who identify with other communities, whereas only 12 per cent find it more difficult in comparison with 26 per cent of the latter group.

<sup>11</sup> Muzafer Sherif's book by this title should be cited as basic in the field, although it tends to have a somewhat limited conception of "social factors," *The Psychology of Social Norms*, New York, 1936.

<sup>12</sup> R. K. Merton, M. Fiske and A. Curtis, *Mass Persuasion*, New York: Harper, 1946.

cerned only with the implications of two facts which emerged from the study.

First of all, in the course of intensive interviews many of our informants—New Yorkers who had pledged a bond to Smith—expressed a thorough disenchantment with the world of advertising, commercials and propaganda. They felt themselves the object of manipulation—and resented it. They objected to being the target for advertising which cajoles, insists and terrorizes. They objected to being engulfed in waves of propaganda proposing opinions and actions not in their own best interests. They expressed dismay over what is in effect a pattern of *pseudo-Gemeinschaft*—subtle methods of salesmanship in which there is the feigning of personal concern with the client in order to manipulate him the better. As one small businessman phrased it, "In my own business, I can see how a lot of people in their business deals will make some kind of gesture of friendliness, sincerity and so forth, most of which is phony." Drawn from a highly competitive, segmented metropolitan society, our informants were describing a climate of reciprocal distrust, of *anomie*, in which common values have been submerged in the welter of private interests. Society was experienced as an arena for rival frauds. There was small belief in the disinterestedness of conduct.

In contrast to all this was the second fact: we found that the persuasiveness of the Smith bond-drive among these same informants largely rested upon their firm belief in the integrity and sincerity of Smith. And much the same was found to be true in a polling interview with a larger cross-section sample of almost a thousand New Yorkers. Fully 80% asserted that in her all-day marathon drives, Smith was *exclusively* concerned with promoting the sale of war bonds, whereas only 17% felt that she was *also* interested in publicity for herself, and a negligible 3% believed she was *primarily* concerned with the resulting publicity.

This emphasis on her sincerity is all the more striking as a problem for research in the molding of reputations because she herself appeared on at least six commercially spon-

sored radio programs each week. But although she is engaged in apparently the same promotional activities as others, she was viewed by the majority of our informants as the direct antithesis of all that these other announcers and stars represent. In the words of one devotee, "She's sincere and *she really means anything* she ever says. It isn't just sittin' up there and talkin' and gettin' paid for it. She's different from what other people are."

Why this overwhelming belief in Smith's sincerity? To be sure, the same society which produces a sense of alienation and estrangement generates in many a craving for reassurance, an acute will to believe, a flight into faith. But why does Smith become the object of this faith for so many otherwise distrustful people? Why is she seen as genuine by those who seek redemption from the spurious? Why are her motives believed to rise above avarice, and ambition and pride of class? What are the social-psychological sources of this image of Smith as sincerity incarnate?

Among the several sources, we wish to examine here the one which bears most directly upon a theory of mass persuasion. The clue is provided by the fact that a larger proportion of those who heard the Smith marathon war-bond drive are convinced of her disinterested patriotism than of those who did not. This appears to indicate that the marathon bond-drive enhanced public belief in her sincerity. But we must recognize the possibility that her devoted fans, for whom her sincerity was unquestioned, would be more likely to have heard the marathon broadcasts. Therefore, to determine whether the marathon did in fact extend this belief, we must compare regular listeners to her programs with those who are not her fans. Within each group, a significantly larger proportion of people who heard the marathon are convinced of Smith's exclusive concern with patriotic purpose. This is as true for her devoted fans as for those who did not listen to her regular programs at all. In other words, we have caught for a moment, as with a candid camera, a snapshot of Smith's reputation of sincerity in the process of being even further enhanced. We have

frozen in mid-course the process of building a reputation.

But if the marathon increased the belief in Smith's sincerity, how did this come about? It is at this point that our intensive interviews, with their often ingenuous and revealing details, permit us to interpret the statistical results of the poll. The marathon had all the atmosphere of determined, resolute endeavor under tremendous difficulties. Some could detect signs of strain—and courageous persistence. "Her voice was not quite so strong later, but she stuck it out like a good soldier," says a discerning housewife. Others projected themselves into the vividly imagined situation of fatigue and brave exertion. Solicitous reports by her coadjutor, Ted Collins, reinforced the empathic concern for the strain to which Smith was subjecting herself. "I felt, I can't stand this any longer," recalls one informant. "Mr. Collins' statement about her being exhausted affected me so much that I just couldn't bear it." The marathon took on the attributes of a sacrificial ritual.

In short, it was not so much what Smith *said* as what she *did* which served to validate her sincerity. It was the presumed stress and strain of an eighteen-hour series of broadcasts, it was the deed not the word which furnished the indubitable proof. Listeners might question whether she were not unduly dramatizing herself, but they could not escape the incontrovertible evidence that she was devoting the entire day to the task. Appraising the direct testimony of Smith's behavior, another informant explains that "she was on all day and the others weren't. So it seemed that she was sacrificing more and was more sincere." Viewed as a process of persuasion, the marathon converted initial feelings of scepticism and distrust among listeners into at first a reluctant, and later, a full-fledged acceptance of Smith's integrity. The successive broadcasts served as a fulfillment in action of a promise in words. The words were reinforced by things she has actually done. The currency of talk was accepted because it is backed by the gold of conduct. The gold reserve, moreover, need not even approximate the amount of currency it can support.

This empirical study suggests that propaganda-of-the-deed may be effective among the very people who are distrustful of propaganda-of-the-word. Where there is social disorganization, *anomie*, conflicting values, we find propaganditis reaching epidemic proportions. Any statement of value is likely to be discounted as "mere propaganda." Exhortations are suspect. But the propaganda of the deed elicits more confidence. Members of the audience are largely permitted to draw their conclusions from the action—they are less likely to feel manipulated. When the propagandist's deed and his words symbolically coincide, it stimulates belief in his sincerity. Further research must determine whether this propaganda pattern is significantly more effective in societies suffering from *anomie* than in those which are more fully integrated. But not unlike the Malinowski case-in-point, this may illustrate the role of research in suggesting new variables to be incorporated into a specific theory.

### 3. *The Re-Focussing of Theoretic Interest* (New methods of empirical research exert pressure for new foci of theoretic interest.)

To this point we have considered the impact of research upon the development of particular theories. But empirical research also affects more general trends in the development of theory. This occurs chiefly through the invention of research procedures which tend to shift the foci of theoretic interest to the growing points of research.

The reasons for this are on the whole evident. After all, sound theory thrives only on a rich diet of pertinent facts and newly invented procedures help provide the ingredients of this diet. The new, and often previously unavailable, data stimulate fresh hypotheses. Moreover, theorists find that their hypotheses can be put to immediate test in those spheres where appropriate research techniques have been designed. It is no longer necessary for them to wait upon data as they happen to turn up—researches directed to the verification of hypotheses can be instituted at once. The flow of relevant data thus increases the tempo of advance in certain spheres of theory whereas in others,



theory stagnates for want of adequate observations. Attention shifts accordingly.

In noting that new centers of theoretic interest have followed upon the invention of research procedures, we do not imply that these alone played a decisive role.<sup>13</sup> The growing interest in the theory of propaganda as an instrument of social control, for example, is in large part a response to the changing historical situation, with its conflict of major ideological systems; new technologies of mass communication which have opened up new avenues for propaganda; and the rich research treasures provided by business and government interested in this new weapon of war, both declared and undeclared. But this shift is also a by-product of accumulated facts made available through such newly developed, and confessedly crude, procedures as content-analysis, the panel technique and the focussed interview.

Examples of this impact in the recent history of social theory are numerous but we have time to mention only a few. Thus, the increasing concern with the theory of character and personality formation in relation to social structure became marked after the introduction of new projective methods; the Rorschach test, the thematic apperception test, play techniques and story completions being among the most familiar. So, too, the sociometric techniques of Moreno and others, and fresh advances in the technique of the "passive interview" have revived interest in the theory of interpersonal relations. Stemming from such techniques as well is the trend toward what might be called the "rediscovery of the primary group," particularly in the shape of theoretic concern with informal social structures as mediating between the individual and large formal organizations. This interest has found expression in an entire literature on the role and structure of the informal group, for example, in factory social systems, bureaucracy and political or-

ganizations. Similarly, we may anticipate that the recent introduction of the panel technique—the repeated interviewing of the same group of informants—will in due course more sharply focus the attention of social psychologists upon the theory of attitude formation, decisions among alternative choices, factors in political participation and determinants of behavior in cases of conflicting role demands, to mention a few types of problems to which this technique is especially adapted.

Perhaps the most direct impact of research procedures upon theory has resulted from the *creation* of sociological statistics organized in terms of theoretically pertinent categories. Talcott Parsons has observed that numerical data are scientifically important only when they can be fitted into analytical categories and that "a great deal of current research is producing facts in a form which cannot be utilized by any current generalized analytical scheme."<sup>14</sup> These well-deserved strictures of a scant decade ago are proving progressively less applicable. In the past, the sociologist has largely had to deal with *pre-collected series* of statistics usually assembled for non-sociological purposes and, therefore, not set forth in categories directly pertinent to any given theoretical system. As a result, at least so far as quantitative facts are concerned, the theorist was compelled to work with makeshift data bearing only a tangential relevance to his problems. This not only left a wide margin for error—consider the crude indexes of social cohesion upon which Durkheim had to rely—but it also meant that theory had to wait upon the incidental and, at times, almost accidental availability of relevant data. It could not march rapidly ahead. This picture has now begun to change.

No longer does the theorist depend almost exclusively upon the consensus of administrative boards or social welfare agencies for his quantitative data. Tarde's programmatic

<sup>13</sup> It is perhaps needless to add that these procedures, instruments and apparatus are in turn dependent upon prior theory. But this does not alter their stimulating effect upon the further development of theory. Cf. Merton, "Sociological Theory," 463n.

<sup>14</sup> Talcott Parsons, "The Role of Theory in Social Research," *American Sociological Review*, III (1938), 19; cf. his *Structure of Social Action*, New York, 1937, pp. 328-329n. "... in the social field most available statistical information is on a level which cannot be made to fit directly into the categories of analytical theory."

sketch<sup>15</sup> a half century ago of the need for statistics in social psychology, particularly those dealing with attitudes, opinions and sentiments, has become a half-fulfilled promise. So, too, investigators of community organization are creating statistics on class structure, associational behavior, and clique formations, and this has left its mark on theoretic interests. Ethnic studies are beginning to provide quantitative data which are re-orienting the theorist. It is safe to suppose that the enormous accumulation of sociological materials during the war—notably by the Research Branch of the Information and Education Division of the War Department—materials which are in part the result of new research techniques, will intensify interest in the theory of group morale, propaganda and leadership. But it is perhaps needless to multiply examples.

What we have said does not mean that the piling up of statistics of itself advances theory; it does mean that theoretic interest tends to shift to those areas in which there is an abundance of *pertinent* statistical data. Moreover, we are merely calling attention to this shift of focus, not evaluating it. It may very well be that it sometimes deflects attention to problems which, in a theoretic or humanistic sense, are “unimportant”; it may divert attention from problems with larger implications onto those for which there is the promise of immediate solutions. Failing a detailed study, it is difficult to come to any overall assessment of this point. But the pattern itself seems clear enough in sociology as in other disciplines: as new and previously unobtainable data become available through the use of new techniques, theorists turn their analytical eye upon the implications of these data and bring about new directions of inquiry.

#### 4. *The Clarification of Concepts*

(Empirical research exerts pressure for clear concepts.)

A good part of the work called “theorizing” is taken up with the clarification of

concepts—and rightly so. It is in this matter of clearly defined concepts that social science research is not infrequently defective. Research activated by a major interest in methodology may be centered on the *design* of establishing causal relations without due regard for analyzing the variables involved in the inquiry. This methodological empiricism, as the design of inquiry without correlative concern with the clarification of substantive variables may be called, characterizes a large part of current research. Thus, in a series of effectively designed experiments, Chapin finds that “the rehousing of slum families in a public housing project results in improvement of the living conditions and the social life of these families.”<sup>16</sup> Or through controlled experiments, psychologists search out the effects of foster home placement upon children’s performances in intelligence tests.<sup>17</sup> Or, again through experimental inquiry, researchers seek to determine whether a propaganda film has achieved its purpose of improving attitudes toward the British. These several cases, and they are representative of a large amount of research which has advanced social science method, have in common the fact that the empirical variables are not analyzed in terms of their conceptual elements.<sup>18</sup> As Rebecca West, with her characteristic lucidity, put this general problem of methodological empiricism, one might “know that A and B and C were linked by certain causal connexions, but he would never apprehend with any exactitude the nature of A or B or C.” In consequences, these researches further the procedures of inquiry,

<sup>16</sup> F. S. Chapin, “The effects of slum clearance and rehousing on family and community relationships in Minneapolis,” *American Journal of Sociology*, XLIII (1938), 744-763.

<sup>17</sup> R. R. Sears, “Child Psychology,” in Wayne Dennis, ed., *Current Trends in Psychology*, University of Pittsburgh Press, 1947, pp. 55-56. Sears’ comments on this type of research state the general problem admirably.

<sup>18</sup> However crude they may be, procedures such as the focused interview are expressly designed as aids for detecting possibly relevant variables in an initially undifferentiated situation. See R. K. Merton and P. L. Kendall, “The Focused Interview,” *American Journal of Sociology*, LI (1946), 541-57.

<sup>15</sup> Gabriel Tarde, *Essais et mélanges sociologiques*, Paris, 1895, pp. 230-270.

but their findings do not enter into the repository of cumulative social science theory.

But in general, the clarification of concepts, commonly considered a province peculiar to the theorist, is a frequent result of empirical research. Research sensitive to its own needs cannot avoid this pressure for conceptual clarification. *For a basic requirement of research is that the concepts, the variables, be defined with sufficient clarity to enable the research to proceed*, a requirement easily and unwittingly not met in the kind of discursive exposition which is often misnamed "sociological theory."

The clarification of concepts ordinarily enters into empirical research in the shape of establishing *indices* of the variables under consideration. In non-research speculations, it is possible to talk loosely about "morale" or "social cohesion" without any clear conceptions of what is entailed by these terms, but they *must* be clarified if the researcher is to go about his business of systematically observing instances of low and high morale, of social cohesion or cleavage. If he is not to be blocked at the outset, he must devise indices which are observable, fairly precise and meticulously clear. The entire movement of thought which was christened "operationalism" is only one conspicuous case of the researcher demanding that concepts be defined clearly enough for him to go to work.

This has been typically recognized by those sociologists who combine a theoretic orientation with systematic empirical research. Durkheim, for example, despite the fact that his terminology and indices now appear crude and debatable, clearly perceived the need for devising indices of his concepts. Repeatedly, he asserted that "it is necessary . . . to substitute for the internal fact which escapes us an external fact that symbolizes it and to study the former through the latter."<sup>19</sup> The index, or sign of

the conceptualized item, stands ideally in a one-to-one correlation with what it signifies (and the difficulty of establishing this relation is of course one of the critical problems of research). Since the index and its object are so related, one may ask for the grounds on which one is taken as the index and the other as the indexed variable. As Durkheim implied and as Suzanne Langer has indicated anew, the index is that one of the correlated pair which is perceptible and the other, harder or impossible to perceive, is theoretically relevant.<sup>20</sup> Thus, attitude scales make available indices of otherwise not discriminable attitudes, just as ecological statistics represent indices of diverse social structures in a given area.

What often appears as a tendency in research for quantification (through the development of scales) can thus be seen as a special case of attempting to clarify concepts sufficiently to permit the conduct of empirical investigation. The development of valid and observable indices becomes central to the use of concepts in the prosecution of research. A final illustration will indicate how research presses for the clarification of ancient sociological concepts which, on the plane of discursive exposition, have remained ill-defined and unclarified.

A conception basic to sociology holds that individuals have multiple social roles and tend to organize their behavior in terms of the structurally defined expectations assigned to each role. Further, it is said, the less integrated the society, the more often will individuals be subject to the strain of incompatible social roles. Type-cases are numerous and familiar: the Catholic Communist subjected to conflicting pressures from party and church, the marginal man suffering the pulls of conflicting societies, the professional woman torn between the demands of family and career. Every sociological textbook abounds with illustrations of incompatible demands made of the multiserved person.

Perhaps because it has been largely confined to discursive interpretations and has seldom been made the focus of systematic

<sup>19</sup> Émile Durkheim, *Division of Labor in Society*, New York: Macmillan, 1933, p. 66; also his *Les règles de la méthode sociologique*, Paris, 1895, pp. 55-58; *Le Suicide*, Paris, 1930, pp. 356 and *passim*. Cf. R. K. Merton, "Durkheim's Division of Labor in Society," *American Journal of Sociology*, XL, 1934, esp. 326-7 which touches on the problem of indices.

<sup>20</sup> Suzanne K. Langer, *Philosophy in a New Key*, New York, Penguin Books, 1948, pp. 46-47.

research, this central problem of conflicting roles has yet to be materially clarified and advanced beyond the point reached decades ago. Thomas and Znaniecki long since indicated that conflicts between social roles *can* be reduced by conventionalization and by role-segmentation (by assigning each set of role-demands to different situations).<sup>21</sup> And others have noted that frequent conflict between roles is dysfunctional for the society as well as for the individual. But all this leaves many salient problems untouched: on which grounds does one predict the behavior of persons subject to conflicting roles? And when a decision must be made, which role (or which group solidarity) takes precedence? Under which conditions does one or another prove controlling? On the plane of discursive thought, it has been suggested that the role with which the individual identifies most fully will prove dominant, thus banishing the problem through a tautological pseudo-solution. Or, the problem of seeking to predict behavior consequent to incompatibility of roles, a research problem requiring operational clarification of the concepts of solidarity, conflict, role-demands and situation, has been evaded by observing that conflicts of roles typically ensue in frustration.

More recently, empirical research has pressed for clarification of the key concepts involved in this problem. Indices of conflicting group pressures have been devised and the resultant behavior observed in specified situations. Thus, as a beginning in this direction, it has been shown that in a concrete decision-situation, such as voting, individuals subject to these cross-pressures respond by

delaying their vote-decision. And, under conditions yet to be determined, they seek to reduce the conflict by escaping from the field of conflict: they "lose interest" in the political campaign. Finally, there is the intimation in these data that in cases of cross-pressures upon the voter, it is socio-economic position which is typically controlling.<sup>22</sup>

However this may be, the essential point is that, in this instance as in others, the very requirements of empirical research have been instrumental in clarifying received concepts. The process of empirical inquiry raises conceptual issues which may long go undetected in theoretic inquiry.

There remain, then, a few concluding remarks. My discussion has been devoted exclusively to four impacts of research upon the development of social theory: the initiation, reformulation, refocusing and clarification of theory. Doubtless there are others. Doubtless, too, the emphasis of this paper lends itself to misunderstanding. It may be inferred that some invidious distinction has been drawn at the expense of theory and the theorist. That has not been my intention. I have suggested only that an explicitly formulated theory does not invariably precede empirical inquiry, that as a matter of plain fact the theorist is not inevitably the lamp lighting the way to new observations. The sequence is often reversed. Nor is it enough to say that research and theory must be married if sociology is to bear legitimate fruit. They must not only exchange solemn vows—they must know how to carry on from there. Their reciprocal roles must be clearly defined. This paper is a brief essay toward that definition.

<sup>21</sup> W. I. Thomas and F. Znaniecki, *The Polish Peasant*, New York: Knopf, 1927, pp. 1866-70, 1888, 1899 ff.

<sup>22</sup> P. F. Lazarsfeld, Bernard Berelson and Hazel Gaudet, *The People's Choice*, New York: Duell, Sloan & Pearce, 1944, Chapter VI.