ON SOME OF THE DIFFICULTIES OF DOING SOCIAL WELFARE RESEARCH*

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IT has become almost fashionable to pay at least lip service to the need for research in the various fields of social work. There is a growing momentum of interest in research and some of this interest is finding expression in action. An earlier session at this conference was devoted to reviewing some of the research that has already been done or is in some advanced stage of progress. Yet there can be no denying that the amount of research is still pitifully small in comparison to the need. It behooves us, therefore, to take stock of the difficulties in and obstacles to social work research if we would see work in this field develop.

Let me start with fundamentals, for the difficulties go deeper than those inherent in our subject matter. For all of our enthusiasm about the possibilities of research, many of us have numerous reservations and resistances—rationalizations, perhaps—which constitute the initial obstacle to actually doing research.

First among these I would list a spirit of perfectionism, an undue intolerance for research studies which do not definitely answer particular questions for all time. Curiously this spirit of perfectionism creates a barrier for many competent scientists as well as for many others who are totally unskilled in research techniques. What happens to the scientists moved by this spirit may well be illustrated by a vaudeville skit I saw as a child. The curtain rises on a man on hands and knees and evidently looking for something under a lamp post. A street sign on the lamp post says, "Third St." A second man comes along and asks, "Lose something?" "Yep," answers the first, "my watch." "Where did you lose it?" "On Fourth Street." "But this is Third Street." "Yes, I know. But there is more light here." There is a school of scientists who look for answers only where there is lots of light—i.e., where there are clear-cut methods for finding specific answers to specific questions. It matters little to them that the answers are too often to questions that make no difference to anyone, possibly not even to science.

Those less skilled in research techniques, but moved by the same spirit, do not even turn to the light. They simply stand still and fervently hope that *somebody*—somebody else!—will start doing some research.

To this spirit of perfectionism, I have but one answer. I take it as an axiom that bad and blundering research is better than none. Mind you, I am not pleading for bad research. I am simply advancing the proposition that, if there is no alternative, it is better to do bad research than none at all. There are dangers, to be sure, but at the very least even bad research sets something going. For out of the criticism of bad research come the designs and formulations for better research. And this is in accord with the essence of science: Contrary to popular conceptions, science is not a body of established and unchallengeable facts and principles; it is rather an approach that is, in its very nature, self-correcting.

Let us, therefore, be as critical as we can be of our study designs and of the interpretations built on the findings, and let us not be naive and pretentious about our research. But let us also not be unduly inhibited by a spirit of sterile perfectionism. Due humility and critical-mindedness will eliminate any danger—if indeed any such danger really exists—that people will do bad research, mistake it for good research, and over-hastily shape program in terms of the findings. After all, not even the best of research provides answers to *all* questions. Even top-notch research, therefore, must—at least until the millenium arrives when all questions are definitely answered—be evaluated and supplemented by good sense and sound judgment in determining program. Research, at best, corrects only *some* of our misconceptions, or demonstrates that we were right all along in *some* of our beliefs, or gives us entirely new food for thought. There is, I sincerely believe, little danger that investigations undertaken in the spirit of scientific inquiry will become a tyrant to be feared.

Last year, when I appeared before you, I urged that some curb be placed on the enthusiasm for research. I warned, as some of you will recall, that there will be many mistakes, that many false leads will be followed, that much of the work will be superficial. I freely repeat this warning today. I will even add that undoubtedly many false conclusions will be drawn. The point of my warning, however, was that we should not make too many or too grandiose promises—to ourselves or to others. I went on to say, and it bears repetition, that there is no alternative to useful research and that it is only through the promotion of such research that we can hope to establish the various fields of social welfare on a solid foundation of scientifically established principles.

So much for perfectionism as an obstacle to research. There are other psychological barriers. There are various cherished illusions which inhibit research.

Many people still share the illusion that the uniqueness of each individual makes research in matters involving human behavior hopeless and meaningless. For if each person is unique, how can the study of one case yield information that is applicable to another? And how can conclusions arrived at in the study of one group of individuals be transferred to another?

Let us grant, for the sake of argument, the manifestly untrue proposition that all individuals are *in all respects* unique. Not even such an extreme state of affairs would preclude the possibility of useful research. An elementary knowledge of the principles of mathematical statistics, for example, would show how—simply as a result of chance combinations—order must emerge out of chaos. In other words, the behavior of groups can be lawful even though the behavior of

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every individual member of these groups is lawless. But there is a more fundamental answer to the issue of individual uniqueness. As Kurt Lewin and many others have often reminded us, it is precisely the central task of all of the sciences of human behavior to arrive at a set of basic principles which in unique combinations can account for the unique individual case.

I hope you will forgive me an *ad hominem* reflection. It is curious how those who are most pessimistic about the possibilities of research are so often the very ones who, in their daily professional and personal lives, are the very ones who indulge most freely in the brashest of generalizations.

This brings me to another illusion which frequently gets in the way of research, the guinea pig illusion. This is the illusion that if we try to test our hypotheses we are violating basic principles of social work by making guinea pigs of our clients, but that if we do not test our hypotheses we are treating them as human beings. Again, this guinea pig argument is so often advanced by the very people who treat their professional operating principles as though they expressed the gospel truth. Yet, I would like to be reminded of a single principle of group work, or case work, or education which is so firmly established that it dare not be challenged. Of course social work research may make guinea pigs of the clients—if we must use unpleasant words—but, the point is, does the blind application of many programs treat the clients any less as guinea pigs?

I regret that I cannot take the time to explore further the psychological barriers to research. I shall content myself with only one more, one which I am told goes to the root of all evil—money. Good research is costly and the risk of failure is great. But if we consider the vast expenditures that go into program —program that rests on principles of more or less dubious validity—and the comparatively minute sums that go into research, we may well reflect on the possibility that this distribution of finances expresses a policy that is in the long run penny wise and pound foolish. I suspect that we have here a job of educating not merely our boards and lay financial supporters, but ourselves as well. Our standards and expectations with regard to financial support for research are set so low that we are appalled at asking for sums that are large in absolute terms, to be sure, but paltry in comparison to the totality of the program budget. Let us not, as I have already stated, deceive our supporters with grandiose promises. But we must also learn not to be overly shy and to raise our voices in righteous indignation when our research budgets do not go high enough.

Let me use this matter of money as a bridge to difficulties in doing research which are not primarily psychological in character. When we do research, we are exploring the unknown; and the unknown is, in its very nature, unpredictable. At the same time, since research does cost money, we are expected to submit our budgets in advance. Of course, any advance budget estimates are expected to be more or less flexible. The point I am making, however, is that research budgets should be expected to be more flexible rather than less. Otherwise there is a danger that a major criterion for selecting research problems will become the degree to which we can precisely predict the budget. This means, for example, that a relatively vague but potentially useful idea for research may never get into the budget: It has not crystallized enough to be carefully

budgeted and, when the budget we have submitted is approved, we are too busy with the approved items to think the idea through in time for the next budgetary submission. One of our tasks, I fear, is to educate our administrators —at the risk of evoking the displeasure of the accounting department—to a considerable measure of irregularity in research budgets.

A closely related problem is that of emergency expenditures. Since timing is often of the essence in research, it is highly desirable that emergency funds be made available with a minimum of red tape.

Another kind of difficulty has to do with the preparation for research of workers in the various fields of social welfare. It is, I believe, unfortunately true that even now our training schools do mighty little by way of preparing practitioners for participation in research, to say nothing of preparing them for carrying on research by themselves. They teach little, if anything, about such matters as experimental design, standardized recording, measurement techniques, or modern developments in handling qualitative data. I still find great difficulty, for example, in talking to social workers—and I am not saying this disparagingly, but am only trying to be realistic—to put across the idea that when we talk of random samples, we are not talking about the first few cases that come to hand nor even about every fifth or every tenth case.

You will notice that I have not explicitly mentioned statistics or the powerful analytical tools that contemporary statisticians have developed. Social workers, I have by now learned, tend to have a profound distrust of statistical analysis. And properly so, where important qualitative data are sacrificed for the sake of pretty tables. But I have seen social workers rear up and object to statistical treatment even before they have had time to examine the question of whether important qualitative data would actually be sacrificed by the treatment—to say nothing of the question of whether they were prepared to deal with these important qualitative data in some other way. In any event, the proper response to the abuse of statistics is the development of an appreciation of its proper use, its strengths and limitations, rather than the perpetuation of ignorance about or stereotyped conceptions of it.

I have already affirmed that better bad research than none. But there can be no gainsaying that we would prefer good research to bad. This means that we must concentrate on the better preparation of the profession for research. I repeat that it is as important to raise the level of preparation for cooperating with research activity as it is to raise the level of preparation for doing or assessing research. I shall return to the problems of cooperation later, but let me for the moment content myself with one primitive example of the horrible things that can happen when practitioners do not understand their role as cooperators in research projects.

CCI and NJWB have been collaborating on a study one aspect of which required the administration of a simple one-page questionnaire to a large number of boys ranging in age from 7 through 17 and attending various Jewish Centers in the metropolitan area of New York City. I don't want to sound unappreciative of the genuine assistance that we received in most cases, but at the same time the story of the mishaps and misadventures that befell us in carrying out this seemingly simple task is a harrowing one. Let me, however, stick to my one example.

The case in point involves a boys' club, meeting on Saturday nights. Although all arrangements had apparently been made, the club leader—and, to be fair, I must say that he is probably a volunteer rather than a professional—seemed quite suspicious and wanted to review the questionnaire. He was particularly struck by the fact that, at the bottom of the page, the boys were asked to fill in their names. He announced that he knew a great deal about research—that as a matter of fact, he had taken several courses at Teachers College—and that he knew it to be very bad practice to ask people to sign their names to questionnaires. He was very resistant to the explanation that the questionnaire would be useless to us without the names since we were using the questionnaire to select particular cases for another purpose. Finally, he gave in. Or, so it seemed. But while the boys were filling in the questionnaire, he suddenly rose and announced, "I see that they are asking you to fill in your names. If I were you, I wouldn't do it."—and walked out of the room. Was this malice? or compulsiveness? or sheer stupidity? I do not know, but I do know that it wasted time and money for us and came near gumming up the works because it happened that this was a fairly crucial group.

Please do not misunderstand me. I am not saying that this behavior is typical of social workers. I am merely citing this case as an extreme example of what can happen with people who do not understand the nature of research. I should perhaps add that the expectation that if you were a reasonable person you would change the wording of questions, add or delete questions, and/or alter the stated rationale for administering a questionnaire is fairly common, and without any appreciation of the implications of such changes for any given project as a whole. The suggestion may even be sound. But it rarely occurs to inexperienced people that a great many man-hours of labor may have gone into the construction of a seemingly simple questionnaire, that the very suggestions for change may have been the subjects of prolonged discussion and experimentation, and that, in any case, any tampering with the questionnaire or procedure may render the results no longer comparable to the results of other administrations. There are times when you have to carry on with what you recognize to have been a mistake.

Let me turn now to another problem in the administration of research. I have in mind the problem of timing. This is particularly likely to be a critical problem for research in group-work and educational settings. Few inexperienced people realize how much time is consumed in the preliminary spade-work for getting a project going, the planning, the preparation of instruments, and the making of arrangements. There is always the danger that by the time you are ready to start the main phase of a study, the appropriate moment may already have passed; and this may entail months of delay while you are waiting for the next appropriate moment to begin. Or, to get in under the deadline, you may find yourself tempted or compelled to make hasty and often unsatisfactory compromises with what you know to be the best research procedure.

While on the subject of time, I should mention another and very serious hazard. This is the danger of not leaving enough time in the overall plan for the analysis of the data and the writing of the report. I can readily enumerate for you half-a-

dozen very elaborate studies which might have had great significance if somebody could only find enough time to analyze the data. The basic trouble with all of these studies was that they were over-ambitious. They collected far more data than they knew what to do with. This is a very easy pitfall to fall into. It is so easy to think up items of information that you feel you simply must have in order to do an adequate job. The trouble is that if you collect all of it, you are likely to find yourself with nothing. The criterion should be, not simply whether you *should* have certain information, but also whether you have effective means of gathering it and whether you will be able to handle it. And bear in mind the following, seemingly paradoxical, but fundamentally sound maxim. The bulk of your data should have been analyzed before you start collecting them: You should have prepared the skeletons of your tables and you should have anticipated what you will do with the findings should they come out one way or another.

I should, perhaps, add that, from a strictly technical point of view, the most admirable research is not necessarily the most ambitious, nor even the most daring. It is rather the most incisive research that is most admirable—the kind of research which sets a highly specific question to which it can provide a specific answer.

Father Time also has his hand in another and quite different problem which frequently arises. This is the problem of turnover in your human subjects. Suppose, for example, that your study design calls for a test before a particular experimental program is introduced and re-tests at various points after its introduction. You think you have, say, 100 subjects, but then some are absent for the pre-test, but maintain perfect attendance thereafter; others drop out during various phases of the program while new cases come in; still others maintain perfect attendance except that they miss the final test; and so on. You may actually have 100 papers filled in at each testing, but your effective number of subjects is the number that has been present throughout—and this may turn out to be far less than 100.

The problem of dropouts also introduces another complication. Insofar as the reasons for dropouts are related to the factors you are studying—and the relationships can be quite subtle—this changes the character of your sample. Suppose, for instance, that you are testing the effectiveness of a particular therapeutic device or of a group work program for improving intergroup relations; and suppose, further that some cases drop out before you can assess their progress under the new treatment. Then, even though you may be in a position to compare the initial and final status for only those cases who remain throughout the study and even though you may have similar data for a control group, you may still get an inflated picture of the effectiveness of the experimental factor. For the dropouts may have induced the kind of selection which leaves you with only those cases which can profit from the program—or, maybe, you have for some uncontrolled reason lost your best cases.

There are other problems of sampling. Statistical inferences can be made legitimately only to the universe of which the sample is a *random* sample. (If we were to consider stratified samples and other complications this statement would have to be revised somewhat, but it would still remain basically true.) Two conditions must be satisfied before we can speak of a random sample: (a) Every case in the universe must have an equal probability of being drawn in the sample and (b) the chances of drawing any one case must be independent of the chances of drawing any other case. Now, when the sampling process is limited in any way these two conditions no longer hold for the total universe of, say, human beings; but they may hold for some more narrowly defined universe. Thus, if our sample contains only 16 year old boys living in Boston, this is the maximum extent of our universe: 16 year old boys living in Boston.

The trouble arises from the fact that we do not always know the precise definition of the universe from which our sample is a random sample—i.e., the universe about which we may legitimately make inferences. Thus, to return to the problem I have already discussed, the problem of dropouts: the universe must be defined in such a way as to exclude the *kind of cases which drop out under the circumstances of our study.* Notice that I say the *kind of cases* rather than the *actual cases which did drop out.* If we exclude only the latter, we have included in our definition of the universe cases which *would have dropped out* if we had included them in our sample—and these cases are not covered by our evidence. This is the very nub of the problem: unless we know what kind of cases drop out rather than the specific cases which do drop out, we do not know in what way our universe is being restricted. A similar problem inheres in every instance where some uncontrolled restriction is imposed on the sampling process. This does not mean that we must cease to operate under such circumstances, but rather that we must proceed with great caution and at our own risk.

I have, of course, not exhausted the problems of sampling, but I must go on to other problems. Let me take a somewhat related one, the problem of agency records. Whenever our studies cut across several agencies, we run into the problem created by the fact that agency records are frequently neither up-to-date, nor complete, nor comparable. As a result, we lose a tremendous amount of data* which would otherwise be readily available. At the very least, these deficiencies create problems for us in defining our universe. At the worst, they make it difficult or impossible to execute what may be highly desirable study designs. The growing interest in social welfare research should have at least one salutary by-product: an increasing pressure toward the standardization of agency records.

But I must again proceed—this time to the problem of diagnosis and measurement. It is a sad fact that we are woefully lacking in satisfactory instruments for these purposes. I cannot detail at this point the progress that has been and is being made in the clarification of concepts and the development of instruments, but in large measure we must still proceed with more or less makeshift devices. Yet, here too is an area in which time and our own fumblings will bring the remedy.

In some ways, more serious than the problem of a lack of adequate measuring instruments is the problem created by our lack of clarity about our goals, about the specific things we are aiming at and want to accomplish in the various fields of social welfare. Yet, it is obviously in terms of our social welfare goals that we must make our decisions about what we want to measure. Interestingly enough, this difficulty is not quite as serious as it sounds. For even if the outcomes that we attempt to measure are not the most pertinent ones or the ones we would have selected (had we been wiser), our research can still advance the cause of knowledge in our field; it can still help to establish functional relationships between specified procedures and specified outcomes.

I am sorely tempted to linger in this area of diagnosis and measurement and consider the problems created for us by the fact that so few of our subjects are as cooperative and obliging, as trusting and unsuspicious, as literate and articulate as are college sophomores. Or, by the fact that we can, after all, take only a small portion of our time with our clients for the specific purposes of diagnosis and measurement. But the great truth allegedly discovered by Henry Luce prevents me from doing so, and, like time, I too must march on.

In the paper I read to you last year, I reviewed some general principles for doing socially significant research. I shall not now attempt to go over them again. I do want, however, to call your attention to a common difficulty in the implementation of one of the principles I then discussed. The principle stated that, "To be socially useful, research must deal with problems that have present social consequences or are likely to demand solution within the near future." I spelled out some of the implications of this principle, but I did not explicitly touch on the difficulty I now have in mind. This is the difficulty of determining the most important problem on which research should be done.

Within a given agency, it may seem that this difficulty is a slight one indeed. Any agency should be able to detect its greatest trouble spots, and the ways of eliminating these trouble spots are likely to constitute the problems most urgently calling for solution. I suspect, however, that the salient trouble spots are likely to be connected with some administrative aspect of the agency, an administrative aspect which as likely as not is unique to the agency. It is when we consider the field as a whole, however, rather than when we confine ourselves to any particular agency that the difficulty arises.

I think you will be interested in an empirical approach to this difficulty with which the Research Center for Human Relations of New York University and CCI have been experimenting. This is an approach which we call "action analysis."

We know that in any given field of social welfare there is a valuable backlog of experience and creative critical thinking among the practitioners with reference to the problems of the field—experience and thinking which for one reason or another do not become available in the general literature and which, in any case, are not likely to be focussed in terms of the formulation of research problems. Specialized conferences and symposia constitute one device for drawing on this backlog, but the goal is usually one of arriving at a consensus. In the process, the refinements of individual thinking and experience are likely to be obscured and the views of only a few experts are likely to be sampled.

Action analysis, on the other hand, seeks to draw on the backlog in another way and for another purpose. It is not aimed at generating agreement, but rather focuses on the differences with regard to details of method and attempts

* I am referring here to data which may be used in the selection and statistical control of samples. This is the simplest use of regular agency data for research purposes. Other research uses of regular agency data involve many problems which can only be assessed in the context of specific research objectives.

to (a) crystallize these differences into sets of alternative hypotheses, (b) obtain a preliminary assessment of each of the alternatives by each of a fairly large number of selected practitioners, and (c) get some perspective on how "live" the issues are that bear on each set of alternatives so that experimentation can be directed at the most crucial issues.

In general, the procedure for an action analysis may be described as follows: The first step is to examine whatever pertinent literature we can find and to order our own thinking and experiences. With this as a background, the second step is to interview a small number of leading practitioners and thinkers in the field. The third step is to plot a semi-standardized interview for use with a much larger number of selected practitioners. The fourth step is to pre-test the planned interview schedule on a few cases and to make whatever revisions are indicated. The fifth step, of course, is to carry through the main body of the interviews and the final step of the action analysis proper is to analyze the returns. Need I add that, in a broader sense, the final step is to do the indicated experiments?

Experience with action analyses has convinced us that this type of study is not only useful in giving direction to research, but is actually quite valuable as an end in itself. Our first sally into the field of action analysis was concerned with programs directed at relieving intergroup tensions and resulted in Goodwin Watson's book, "Action for Unity." Actually, this study was not as successful as we had hoped in giving direction to research, but it did provide a valuable guide to practice in the field of intergroup relations. The relative failure in providing guidance to research may be attributed to the fact that we had not yet developed our schemes for analyzing the crucial variables and for framing the interviews around alternative sets of hypotheses. Building on the "Action for Unity" experience, however, later ventures in action analyses (in the fields of interracial housing and group work practice with racially mixed recreation groups) give promise of being far more successful in terms of the primary objective of giving direction to research. We are now in the second stage of an action analysis within the fields of Jewish education and group work.

Finally, I would like to return to one of the main problems in getting research done, the problem of cooperation between research people and practitioners. It is not likely, in the near future at least, that a large proportion of the significant research in the fields of social welfare will be carried on by practitioners and/or researchers working independently of one another. Indeed, from the viewpoint of social welfare workers who feel inadequate with regard to research skills, taking the initiative in setting up such relationships may be an effective way of coping with this inadequacy. There are a number of agencies which maintain research departments. The latter, I am sure, are in most cases eager to enter into collaborations which fit into their own research programs. At the very least, they are very likely to give you criticism and suggestions with regard to your research designs. There is also another fruitful direction for collaboration. There are a great many social scientists in the universities who are eager to contribute their research skills to socially significant research.

Collaboration, however, brings in a host of problems of collaborative relationships on a level quite different from those that I have touched upon earlier in my presentation. I want especially to call your attention to an excellent study by William Schwartz, entitled "Action Research in a Group Work Setting: A Record of a Cooperative Experience." The study is available as a master's thesis at the New York School of Social Work. It develops twenty principles affecting collaborative relationships and deals with such matters as: formulating the research objectives, criteria for the variables, experimental techniques in a group work setting, recording, role of the group leader, role of the research worker, and the total relationship. Most of the principles, if not all, are pertinent to research in other social welfare fields as well as group work.

Unfortunately, I cannot take the time to review Schwartz's twenty principles even though I am not in complete agreement with a few of them. But taken together, they add up to what I and my colleagues have elsewhere described as *participant* action research. In a nutshell, this means that ideally there should be in most cases, from the very first stages of the research program, complete integration between the research operation and the field operation involved in the research. Any other procedure is apt to be self-defeating and sterile.

In closing, I would like to add that I have been asked to deal with some of the difficulties in doing social welfare research. I have not attempted to describe the rewards and gratifications. I hope, therefore, that the one-sided presentation I have made is not so black as to discourage what I know is your growing interest in this field.