

The Empirical Practice Movement

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Since its beginnings in the 1960s, the empirical practice movement in social work has addressed three major goals: advancement of the use of research methods in practice as a means of facilitating assessment, guiding intervention planning, and evaluating cases; promotion of the use of interventions whose effectiveness has been demonstrated through empirical research; and creation of knowledge through studies carried out by practitioner-researchers. Through an examination of the origins and development of the movement, I consider issues it has generated, as well as its successes and shortcomings in pursuing its goals.

Empirical practice has come to be known in social work as an approach that stresses a research orientation. Although empirical practice has affected all aspects of social work, this article will be limited to its impact on direct work with client systems.

Three facets of the approach can be distinguished. One is the use of research methods in practice to facilitate assessment, to guide intervention planning, and to evaluate results in work with individuals, families, and groups. The second is the application, whenever possible, of interventions of demonstrated effectiveness, that is, interventions whose efficacy has received research support. The third is knowledge building through disseminated studies carried out by practitioner-researchers.

As Stanley Witkin has observed, empirical practice (sometimes called “empirical clinical practice”) is “one of the most significant developments in social work” in recent years.¹ It is widely taught in schools of social work, is the subject of numerous texts and journal articles,

and has made its mark on research and practice. It has also provoked a continuing controversy.

Although there has been a good deal of literature on empirical practice, there has not been, to my knowledge, a comprehensive effort to trace the evolution of this form of practice and to examine critically its current status. Given the importance of the movement, and its age—about a quarter century—a review of this kind is in order.

A Historical Prelude

From the beginnings of the social work profession the scientific method was viewed as a model for the systematic study and treatment of individual cases. The use of science as a guiding light for practice was articulated by Mary Richmond in *Social Diagnosis*.² In Richmond's formulation, a social diagnosis is the product of a scientific process. Facts are gathered to serve as the basis for hypotheses, which are then tested by obtaining relevant evidence. Although the psychoanalytic movement that began in the 1920s introduced radically new theories and interventions for casework, it also adhered to the paradigm of study, diagnosis, and treatment that followed presumed scientific principles. Florence Hollis, a leading advocate of psychoanalytically oriented casework, expressed this continuity: "Casework is a scientific art. Certainly since the days of Mary Richmond we have been committed to objective examination of the facts of each case. We draw inferences from those facts, we diagnose, we view the individual against a frame of reference which is itself the product of informal research. We constantly alert ourselves to sources of error."³

The paradigms of Richmond and Hollis called for practice to be scientific in the sense of science as a rational, systematic, problem-solving activity. This kind of problem solving is not unique to science and indeed can be a model for practice in many fields not usually thought of as scientific, such as law and journalism. The paradigms of Richmond and Hollis lacked the specific directives and procedures found in contemporary empirical practice, such as the collection of baseline data, the use of research instruments, the measurement of case progress, and the employment of research-based interventions. Most of the wherewithal for this kind of practice had not been developed when these paradigms were conceived. Nevertheless, Richmond and Hollis made the point that scientific methods were applicable to social work practice and set the stage for further developments in their application. Moreover, the paradigms established a baseline that can help determine whether a sample of contemporary practice presumed to be "empirical" does indeed differ from practice that is scientific in the sense of being simply a systematic, problem-solving activity.

The Beginnings of Empirical Practice

The evolution of these earlier paradigms into contemporary empirical practice had its origins in developments in the early 1960s. The many currents in these developments can be perhaps best brought to life through people and places. The evolution of empirical practice has differed markedly from that of other major practice movements in social work in that it has been led by social work practitioner-researchers and researchers based in academic settings. To be sure, social work academics contributed to other movements, such as psychodynamic, family, and crises therapies, but these movements were also well embedded in the practice community, which provided much, and in some instances most, of the intellectual leadership.

Empirical practice was largely the creation of research-oriented academics who had entered doctoral programs in schools of social work in the 1950s and early 1960s. This was a period of rapid development of such programs and the beginning of a major shift in academic leadership from masters- to doctoral-trained faculty, especially in the direct practice area.

Many of these new empirically minded faculty had been trained in psychodynamic approaches, which, at the time, held a virtual monopoly in direct social work practice. While they may have questioned the weak scientific bases of these approaches, they had little choice but to accept them. Their doctoral training did not provide alternatives to these approaches but did stimulate skepticism about them. Moreover, their training reinforced their research-mindedness, exposed them to developments in the social sciences, and equipped them with tools to study practice. In short, their training had prepared them to search for new practice alternatives.

An important nexus of this development was the newly developed doctoral program at the Columbia University School of Social Work. Among the students in this program in the late 1950s and early 1960s were a number of future academics who played key or supporting roles in the development of empirical practice. Included in this group were Scott Briar, Irwin Epstein, Harvey Gochros, Henry Miller, Edward Mullen, Ben Orcutt, the author, Arthur Schwartz, Richard Stuart, and Tony Tripodi.⁴ A research-oriented faculty stimulated students to think in an empirical and critical way about social work practice. Particularly influential was James Bieri, an empirically oriented psychologist whose project on clinical judgment was used as a base for the dissertations of several of the students mentioned above. A strong emphasis on social science and research methodology was provided by such faculty members as Robert Bush, Richard Cloward, Samuel Finestone, Alfred Kahn, William McGill, Lloyd Ohlin, and Herman Stein; Lucille Austin and Hollis brought research perspectives

to their teaching of the prevailing psychodynamic approach to practice.

The inadequacy of existing casework methods and the need for a "new wave," one that would have a stronger base in research, was a recurring topic of discussion among the students. Yet neither faculty nor student research posed any direct challenges to the psychodynamic model or developed alternatives to it. The program prepared students for a different approach to intervention but fell short of helping them develop it.

An alternative that would fit well into the new practice empiricism was, however, on the horizon. In the mid-1960s, faculty and doctoral students at the University of Michigan School of Social Work, under the leadership of Edwin Thomas, began to experiment with the new behavioral methods that had begun to emerge a decade earlier in clinical psychology and psychiatry. In a series of dramatic and controversial presentations at the 1967 Annual Program Meeting of the Council of Social Work Education, this group unveiled their "socio-behavioral" approach to social work.⁵

The socio-behavioral and other behavioral models presented attractive alternatives to those with a research orientation to social work intervention. The models were the product of experimental research, drew on the well-tested tenets of learning theory, made use of measurable treatment and outcome variables, and, perhaps of greatest significance, contained a rigorous research methodology for guiding and evaluating intervention in the single case. They were clearly models in which researchers could feel at home.

Behavioral approaches to social work soon began to spread from Michigan to other schools. Part of this dissemination process occurred through doctoral students and faculty who were trained in the approach and who established instructional or research activities at other schools. Thus, Sheldon Rose, a faculty member who had worked with Thomas, took a position at the University of Wisconsin—Madison School of Social Work, where he set up a research program in behavioral group work. Doctoral students who were trained at Michigan in the early years of its behavioral program and the schools of social work with which they became affiliated included William Butterfield, Washington University; Eileen Gambrill, University of Wisconsin—Madison and University of California, Berkeley; and Clayton Shorkey, University of Texas at Austin.

However, the Columbia students referred to above were also playing important roles in this dissemination process. Stuart became one of the leaders of the behavioral movement at Michigan and was joined there by Tripodi and Epstein. Impressed with the work of Stuart and others at Michigan, Schwartz, another of the Columbia group, set up a behavioral program at the School of Social Service Administration

at the University of Chicago. He in turn recruited Elsie Pinkston, a clinical psychologist, who had received her training in behavior modification at the University of Kansas, a leading center of this form of practice. Pinkston expanded the Chicago program considerably and in the process influenced the work of the author (another of the Columbia graduates) and of his colleague Laura Epstein, who at the time were developing the empirically oriented task-centered practice model.⁶

Two other Columbia graduates, Briar and Miller, took positions at the University of California, Berkeley. Both had learned something about behavioral methods during their doctoral studies at Columbia and continued their study of them at Berkeley. They collaborated on a book that stressed use of a more empirical orientation to social work practice in general and greater use of behavioral methods in particular.⁷ In effect, the book presented one of the first formulations of contemporary empirical practice. Briar subsequently became dean of the School of Social Work at the University of Washington. Under his leadership, faculty with behavioral orientations, including Rona Levy, a Michigan Ph.D., and Steven Schinke, a student of Rose's at Wisconsin, were recruited, and an educational program stressing the integration of practice and research was established.

As it continued to spread, the behavioral approach became the leading form of empirical practice. But at the same time, a more general conception of empirical practice began to emerge. This conception was built around the methods of assessment, case monitoring, and outcome evaluation that were used in the single-system studies of behavioral approaches. It was assumed that these methods could be applied to any form of direct social work practice. They could not only guide practice in individual cases but also could be used in more rigorous forms by practitioner-researchers to test the effectiveness of practice models. Knowledge from such research, as well as from other kinds of research-based knowledge, should be used as a base for selecting methods of intervention.

The empirical practice movement emerged as a complex combination of a practice model and a research agenda. In important ways, it defined how practice should be done, and its definition included such components as describing goals in measurable terms and using research-based interventions. It also prescribed an approach to doing practice research whereby social workers functioning as both researchers and practitioners would play a central role. The empirical practice movement became a distinct approach to practice, one that absorbed the measurement technology of behavior modification without adopting all of its technology for effecting change.

Although it was fostered by developments in psychology, the empirical practice movement was by no means a straightforward borrowing,

as has been the case with many practice innovations in social work. In social work, empirical practice developed in its own fashion, for several reasons.

First, there was interest in developing an empirical approach to practice that could be seen as distinct from its behavioral parents. Behavior modification was given a cool reception by the social work practice community, which viewed it as too circumscribed to serve as a general practice approach. Its emphasis on behavior change was interpreted as mere "symptom removal" that did not address underlying problems. There were concerns about its "mechanistic" and "manipulative" character, its making excessive use of external rewards, and its downplaying of the practitioner-client relationship. Although the developers of empirical practice were attracted to behavior modification, they were searching for a model with a wider range of application and were anxious to develop an approach that would be acceptable to a broader range of practitioners. One answer seemed to be a form of practice that could make full use of behavioral methods without being restricted to them.

Second, evidence that an empirical practice could be something other than behavior modification was beginning to emerge from research on short-term treatment, on the "core conditions" in nondirective counseling, and on cognitive therapy.⁸ For example, short-term treatment models of the day, despite their origins in ego psychology, called for the targeting of specific problems and goals and for structured interventions—features that fit well with empirical practice. It was becoming possible, as Joel Fischer subsequently demonstrated, to put together behavioral and research-based methods from other approaches in a form of eclectic empirical practice.⁹

A third set of forces that shaped the empirical practice movement emanated from the "effectiveness controversy" and from growing emphasis on accountability in the human services.¹⁰ The effectiveness of existing casework practice had been thrown into question by a series of evaluative studies that began to appear in the early 1960s. One of the most important and influential of these studies, *Girls at Vocational High*, had been the subject of a column in the *New York Herald Tribune*, by the well-known science writer Earl Ubell, under the ominous title "Social Casework Fails the Test."¹¹ Essentially, these studies, which had included applications of casework in the schools, corrections, public welfare agencies, and services to the aging, had failed to find differences in outcome between treated and control groups. By the mid-1970s this body of research had been summarized and agonized over in several conferences and publications.¹²

Concerns over the effectiveness of casework merged with concerns over social work's accountability. The social programs of the 1960s were increasingly becoming the objects of evaluation. Results were

often disappointing. There were growing demands that social workers and other human service professionals become accountable in the sense of achieving what they had promised to deliver. The perceived lack of accountability had prompted harsh attacks on social work at the highest levels of government. Thus, President Richard Nixon's chief advisor on domestic affairs, John Erlichman, referred to social workers as "parasites sucking the fiscal blood."¹³

In a climate of growing dismay over efficacy and accountability problems, the need to demonstrate the practice effectiveness of treatment became not only a rallying cry but also an article of faith for the rising generation of academics involved in both research and practice. One could no longer assume that "casework worked." Moreover, an increasing number of people were looking over the caseworker's shoulder and asking hard questions. There was clear need to build truly effective practice models and to demonstrate their efficacy. Empirical practice appeared to be a means of achieving that goal. In particular, the single-system design seemed able to demonstrate effectiveness with less investment of time and resources than group designs, with greater opportunities for replication, and perhaps with greater precision.¹⁴ As a result, empirical practice was given a strong push toward becoming a vehicle for practice development and evaluation.

Critical Reactions

As the empirical practice movement began to gain strength in the late 1970s, critical reactions began to appear. One kind of criticism, voiced by Martha Heineman, was directed at the presumed "logical empiricist" epistemology underlying not only empirical practice but most mainstream research in social work as well.¹⁵ Although ill-founded in the opinion of the empiricists, Heineman's harsh criticisms of what she saw as the epistemological bases of the empirical practice movement served the useful function of provoking responses that in effect clarified the actual epistemology of the movement.¹⁶

More specifically, Heineman used empirical practice as an example of how mainstream researchers made arbitrary claims for the superiority of their own brand of methodology. Moreover, in making scientific requirements an important consideration in building and implementing practice models, empiricists, she asserted, were heading in the wrong direction. In her opinion, "The requirements of science should be adapted to existing forms of practice."¹⁷

Another kind of criticism was directed more specifically at the single-system design as a medium of practitioner research. According to Jill Kagle, the assumptions and methodology of this design were too restrictive given the turbulence and complexity of the world of agency practice.¹⁸ Roy Ruckdeschel and Buford Farris also viewed the design

as "having a very narrow range of applicability for social work practice," for reasons similar to Kagle's.¹⁹ They proposed qualitative methodology as an alternative.

A common theme in all of these criticisms was the failure of an empirical practice orientation and its methodology to come to grips with the complexities of social work practice. To the empiricists the criticisms were off the mark. They would acknowledge that their approach might not capture the ins and outs of *existing* modes of practice. They would argue that much of this practice had really not demonstrated its value and perhaps was not worth studying. Rather, they were advocating a different kind of practice, one that would be both more effective and more amenable to study because of its research orientation. The extent to which this kind of practice could be established and whether it would prove superior to existing forms was in their view the central issue. I shall return to this issue and take up other critical reactions, within the context of the development of the different components of the empirical practice movement.

The Growth of the Movement

Developments in the empirical practice movement will be examined in relation to each of its core components—research methods in practice, demonstrably effective interventions, and knowledge building. Although to separate these interrelated facets may be somewhat artificial, there are advantages in doing this, since each has evolved in a somewhat different manner and each presents its own distinctive set of issues.

Research Methods in Practice

The main idea driving the empirical practice movement was the use of research methods to guide and evaluate service in individual cases. Although this approach was meant to be used by social work practitioners, wholesale adoption did not seem in the offing. The most likely route to dissemination seemed to be through social work educational programs. In the early 1970s, content on empirical practice, with an emphasis on single-system designs, was introduced into research and practice courses, mostly the former. Efforts were made to teach empirical practice from both research and practice perspectives, through team teaching, integrated research and practice courses, and other devices. As part of their course work, students were usually required to apply empirical practice methods to one of their own cases in their field placements or employing agencies.²⁰

From the faculty's perspective, the courses showed promise of breaking through the legendary antipathy of social work students toward

research. Through the single-system design, research could be connected directly to what students were most interested in: practice methods and their own cases. From the students' standpoint, research appeared to have some relevance to their main goal of acquiring practice knowledge and skill. Moreover, there was evidence that the integrated courses, in particular, had positive effects on the students' attitude toward research, learning research methods, and perceived readiness for practice.²¹

As this educational movement gained momentum, its adherents convinced the Board of the Council of Social Work Education (CSWE) to require social work education programs, both graduate and undergraduate, to prepare students to evaluate their own practice. The CSWE's action (in 1984) provided further stimulus to the movement. By the late 1980s, a comprehensive survey revealed that the research offerings of one-third of the graduate schools of social work emphasized single-system designs and self-practice evaluation, with additional schools having substantial content of this kind in their curricula.²² Integrated formats for teaching practice and research were reported by almost 40 percent of the schools.

How successful was this educational strategy for dissemination of empirical practice? Since the early 1980s, a number of studies have attempted to provide some answers to this question.²³ In these studies, samples containing graduates of particular programs or practitioners in general have been asked to respond to questionnaires about their use of specific components of empirical practice. By and large, the studies have reported substantial use of such components as specifying target problems and goals, describing goals in measurable terms, and monitoring client change over time. Only small minorities of graduates have reported much use of more time-consuming or intrusive operations, such as standardized questionnaires or graphs, to measure change. Also, only a small minority of graduates, roughly 10 percent, reported conducting single-system or group studies relating to their practice, with some evidence that their studies favored group rather than single-system designs.²⁴ The lack of time and agency support as well as interference with practice have been cited by practitioners as major reasons for not using research methods in their cases.

As has been suggested by Briar, the results can be interpreted as evidence that the educational programs in empirical practice are having some impact.²⁵ A number of empirical practice components are being widely used, and there are at least some practitioners involved in research. Further, the amount of exposure to empirical practice content appears to be correlated with the extent to which these components are used.²⁶

Although the studies suggest that a beginning has been made in the dissemination of empirical practice, their results must be interpre-

ted with some reservations. As Cheryl Richey, Betty Blythe, and Sharon Berlin have pointed out, "Semantic differences in descriptions and definitions of component evaluation activities may result in the underreporting and overreporting of such activities."²⁷ For example, Cynthia Penka and Stuart Kirk found in their study of members of the National Association of Social Workers, two-thirds of whom had little or no exposure to single-system designs in their graduate education, that subjects reported using such components as "operationalizing target problems" or "monitoring client change" with over three-fourths of their clients on the average.²⁸ Are many of the practitioners in these studies interpreting such components in ways that are congruent with a broad conception of empirical practice? Is it possible that "monitoring client change" simply means asking clients about their progress in the course of a clinical interview? Are some of the practitioners using the kind of scientific orientation advocated by Richmond and Hollis, one that is not unique to empirical practice? To what extent are their responses influenced by a wish to appear "scientific," especially on a questionnaire sent to them by researchers? There is certainly need for more penetrating studies of practice to give more satisfactory answers to some of the questions raised—for example, studies of actual samples of practice. Finally, studies show little use of more time-consuming but potentially useful tools, such as standardized instruments, despite the availability of comprehensive packages developed by academic researchers.²⁹

Questions concerning the extent of the use of research methods in practice are related to another issue: the feasibility and appropriateness of some types of single-system designs in service contexts. In examining this issue it is useful to distinguish between two broad categories of single-system designs: the more complex, rigorous, and intrusive designs such as withdrawal, reversal, or multiple baseline designs, on the one hand, and, on the other, the simpler, less rigorous, and less intrusive designs, such as the AB design (collection of baseline data followed by intervention and monitoring of progress). A critical difference between the two types is that the former offers far better experimental control than the latter. For example, with a withdrawal or reversal design, it is possible to assert with a reasonable degree of confidence that changes associated with an experimental intervention are in fact the result of the intervention and are not extraneous factors. Such an assertion is far more difficult to make with an AB design.

One facet of this issue has to do with efforts to extend single-system designs to "nonbehavioral" forms of practice. There has never been any inherent reason why such designs could not be used outside of behavioral approaches, and, in fact, they have been used in other contexts since the beginning of the empirical practice movement.³⁰ However, these designs came to social work in forms that had been

shaped by their use in behavior modification, and some of these forms, such as withdrawal or reversal designs, would be difficult to apply with most social work interventions. Conceptual work as well as practice applications by Neil Broxmeyer, Ruth Dean, Helen Reinherz, Kathleen Millstein, Joseph Regan, Judith Nelsen, and others have shown clearly how the simpler AB design can be adapted to most intervention models used by social workers.³¹

Application of complex designs has proven more difficult. These designs are hard to implement because of the characteristics of much traditional social work intervention, including the length of time needed to achieve effects, the use of indirect strategies to bring about change, the irreversibility of most effects, the nonlinear nature of many of the changes expected to occur, and the simultaneous pursuit of multiple goals.³² As has been noted, the cross-client multiple baseline design is perhaps the most feasible.³³ Still, this design requires an intervention whose effects can be detected within a reasonably brief period; otherwise, clients not treated first would need to wait in baseline conditions for excessive lengths of time. Perhaps the most practical use of the design would be to test the efficacy of specific techniques that might be expected to show effects quickly. Thus, David Kolko and Michael Milan made use of a cross-client multiple baseline design in their study of the effects of paradoxical intervention in the treatment of adolescent truancy.³⁴ In general, there is a need to develop a variety of multiple baseline and other complex designs that might be adapted to the characteristics of nonbehavioral social work.

Another facet of the issue of the complexity of single-system designs concerns their use within the context of ordinary service programs, regardless of the type of intervention tested. In Thomas's view, incompatibilities between research and service goals constrain the use of single-system designs in ordinary practice situations.³⁵ For example, the requirements of research might well pose difficulties for achieving service objectives, or attaining service objectives might be accomplished only at the expense of threats to research validity. Thomas made it clear, however, that research intrusiveness would be a problem largely with the more complex designs. Simpler designs might be more compatible with service but would produce less rigorous research. In a rebuttal, Gambrill and Richard Barth argued that simpler, service-compatible designs could also yield useful data and that, in general, conflicts between research and service were less likely to be a problem in empirical practice models.³⁶

The controversy puts age-old strains between research and service into the context of the single-system design. One way of resolving it would be, as Thomas has suggested, to give research priority in service experiments, to test either new and potentially more effective models of practice or practice components.³⁷ Although experiments would

presumably be of benefit to the clients, they would be clearly research undertakings. Another kind of resolution, not necessarily incompatible with the first, would be, as Gambrill and Barth propose, to use simpler designs as a means of producing knowledge.³⁸ The second solution can be justified on several grounds, including the need to determine whether supposedly effective methods can be successfully applied to new kinds of case situations and the need to produce provisional knowledge about what might work in areas where very little is known.

A final aspect of the complexity-of-design issue relates to the current trend to stress simpler over complex designs in training students and practitioners in the use of research methods in practice. Evidence of this trend can be seen in various reassessments of single-system designs that have appeared in recent years.³⁹ As Betty Blythe and Antoinette Rodgers have put it, "Over time, we came to realize that many designs, such as the withdrawal and multiple baseline designs, were inappropriate for practice. . . and began to emphasize simpler designs."⁴⁰ As these authors suggest, there was recognition that more complex designs were difficult to implement in ordinary practice situations; moreover, they were not being used to any extent in any case. Ways of simplifying even the simple designs have been suggested as a means of making them more attractive and feasible—for example, by using retrospective rather than prospective baselines.

Although this retrenchment of expectations was inevitable, it runs the risk of forsaking the better-controlled designs as a means of providing rigorous tests of interventions in ordinary service programs. After all, it was these designs that caused us to view single-system experimentation as an answer to vexing problems in testing service effectiveness. It would be unrealistic to propose use of such designs on a substantial scale in service programs, but it would be a step backward to abandon them entirely. Their selected use is an important component in a social agency's capacity to develop and test programs.

Although simpler designs are still seen as appropriate for use by practitioners, current emphasis is on practitioners' use of various tools, such as various kinds of measuring devices, to evaluate client change.⁴¹ Outside of educational contexts, use of such tools seldom leads to studies of any kind, and little is added by calling such efforts "single-system designs." It would be simpler and less confusing to refer to them as the "use of evaluative methods in practice."

Among the more promising developments in the use of research tools in practice are computer-based applications, including *computer-assisted practice* and *expert systems*. Computer-assisted practice involves the use of computers to perform various tasks to facilitate assessment, case monitoring and evaluation, record keeping, and the like.⁴² A system developed by Paula Nurius and Walter Hudson permits clients to input their responses to scales directly into the computer, will score

the scales and graph the scores over the progress of the case, and will aggregate the results of single-case studies.⁴³ Although the use of computer-assisted practice is not yet widespread, there is research to suggest that its assessment functions are beginning to be used by a significant number of practitioners.⁴⁴

"An expert system is a computer program designed to give advice on decisions."⁴⁵ In social work applications, the advice the program gives is derived from detailed interviews with expert practitioners concerning a particular type of problem. Applying principles derived from work on artificial intelligence, the researcher uses the interview data to create a computer model of the expert's decision-making processes. In using an expert system, the practitioner answers questions about a case that are asked by the program, which then gives recommendations for action—for example, concerning a child placement decision. Expert systems in social work are still in an early stage of development and are not yet ready for large-scale implementation. Ultimately, they should be of considerable assistance in the decision making of practitioners, especially those lacking in training or experience.

Demonstrably Effective Interventions

Two approaches have been used to incorporate research-based interventions into practice. One has consisted of providing frameworks and guidelines to help practitioners identify and select empirically tested interventions. This approach may be combined with content on the use of research methods in practice or on the selection of interventions generally.⁴⁶

The other approach has been to present practice texts that draw heavily on empirically verified practice methods with citations of relevant research literature. These texts may also incorporate reviews of research, studies of practice, content on the use of research tools in practice, or guidelines for selecting practice methods.⁴⁷

As one might expect, behavioral methods are much in evidence in these texts but do not constitute a monopoly. Interventions not only cover a range of discrete techniques, such as the use of exposure in the treatment of phobias or cognitive restructuring in the treatment of depression, but also include a variety of more general practice principles and approaches derived from clinical and social science research. One finds ways of preparing clients for treatment, principles for matching clients and practitioners, considerations in use of time limits, and approaches to goal setting, to mention just a few examples. Finally, in keeping with an empirical orientation, interventions tend to take the form of well-explicated procedures applied in a structured fashion—that is, in a step-by-step manner—to specific problems.

There is evidence that these interventions are being taught in practice courses and are being used increasingly by trained practitioners.⁴⁸ However, evidence on the extent of their use is sketchy, at best.

A key issue has to do with the validity of the practice content (methods, principles, approaches, and so forth) selected. Can one assume that research provides a better basis for selecting practice content than other sources of knowledge, such as "practice wisdom"? In answering this question in the negative, Witkin argues that lack of objective criteria for determining empirical validation and limits on generalizing findings nullify the presumed superiority of research-based content.⁴⁹ Such an argument makes the naive, or perhaps disingenuous, assumption that empirical practice needs to deal in certainties in order to be viable. Selecting from a welter of studies uneven in quality and with often discordant results obviously demands judgment with no guarantee of consensus. There are always risks in generalizing from study populations to one's own clients. What is assumed is that these interpretative processes, messy though they may be, will provide, as a rule, better guidance for practice than is possible from other sources of knowledge.

Consider, for example, some interventions whose effectiveness has been demonstrated through research studies: the family treatment approaches designed to prevent relapses in adult schizophrenics,⁵⁰ parent-training methods to help parents control anger and abusive behavior toward their children,⁵¹ and exposure therapy for phobic disorders.⁵² Practitioners with cases similar to those included in the studies might apply the interventions with some confidence that they will be effective. Their confidence in these methods would be greater than if they were to use methods lacking empirical support, because for the tested methods there is better evidence for their efficacy. The evidence is better, I would argue, because it is the product of a systematic process that reduces error (through use of control groups, reliable measures, and so forth).⁵³ The same cannot be said for the evidence offered by practice wisdom.

When more than one method has research support, the empirically oriented practitioner may be able to pick and choose, but even here the choices are limited, since seldom are more than a few methods equally well supported. Even when there is equal support, there may be empirically based reasons for selecting one. For example, for a wide range of problems, planned short-term treatment has been found to be as effective as treatment of longer duration.⁵⁴ For such problems, short-term treatment could be selected on grounds of its greater efficiency.

The last issue I shall raise concerns the sufficiency of empirical methods as a practice base. Sharon Berlin and Jeanne Marsh have put it well: "Despite the importance of empirical knowledge, it is insuffi-

cient for guiding practice.”⁵⁵ Any approach to practice that is comprehensive enough to be used by students in practice courses and by practitioners in their work must, of necessity, include content lacking empirical verification, because there is no research base for many of the decisions social workers need to make. In addition, research knowledge varies considerably in degree of hardness, raising the recurring question of the amount and kind of evidence needed to constitute “research support.”

Nevertheless, enough research has accumulated in enough areas of practice, I submit, to justify an approach that emphasizes the application of empirically supported knowledge, when such knowledge is available. All else being equal, the practitioner who consistently makes use of such knowledge should be more effective in the long run than one who does not.

Knowledge Building

At the beginning of the empirical practice movement it was hoped that agency-based practitioner-researchers, by using the single-system design, would be major producers of disseminated practice knowledge. This hope has not been realized. Agency practitioners have published only a handful of single-system studies over the past 2 decades.

Although the idea of practitioner-researchers as contributors to social work knowledge has remained dormant in the social agency, it has come to life in academic settings. Practitioner-researchers in such settings are likely to be faculty members (or doctoral students) with practice experience, who teach or supervise practice. They may also be currently involved in some form of direct practice activity. Much of their research takes the form of developing and testing innovative service approaches, which may in itself involve direct contact with clients or the supervision of practitioners who provide services. Usually, these approaches are tested in agency settings.

The published work of such practitioner-researchers began to appear in the early 1970s. Some of it has taken the form of single-system design studies. The number of single-system studies by practitioner-researchers based in schools of social work appears to be quite small when one examines only social work journals.⁵⁶ It enlarges considerably when one includes journals in psychology and other disciplines.⁵⁷ Moreover, a number of social work books have contained such studies.⁵⁸ Yet even when all sources are considered, the rate of production has been modest—probably not more than about three studies published a year. There has been little evidence of the use of replication series, which David Barlow and others have advocated as a means of establishing generality of results.⁵⁹ Finally, the impact of the studies on social work practice has been limited since the majority of them have been published in journals outside the field of social work.

A more influential form of research has consisted of group experimental tests of programs designed and directed by academic practitioner-researchers themselves. This style of group experimental research has represented a major departure from earlier experiments in social work, exemplified by *Girls at Vocational High*, in which researchers were cast primarily in the role of evaluators with little involvement in the design and operation of the service programs.⁶⁰ Indeed, the majority of service experiments reported in the social work literature through 1970 were of that type, whereas the majority that have appeared since then have followed the practitioner-researcher model.⁶¹ A particular strength of this model is that it enables researchers to design and shape their own interventions and then test them. Whether single-system or group designs have been used, the programs tested have had, for the most part, the characteristics of empirical practice—for example, structured, well-explicated interventions addressed to specific problems.

A major advance in such experimental undertakings has resulted from the efforts of Thomas and Jack Rothman. Thomas's earlier work on developmental research and Rothman's on social research and development have been combined into a new conception of *intervention research*.⁶² Their formulation provides systematic methods of using research in the design, development, testing, and dissemination of practice programs.

In the single-system and group experiments conducted during the past 2 decades, the empirical practice movement has scored some clear gains. In contrast to the experiments conducted during the 1960s, most of these studies have had positive outcomes.⁶³ Although the effects have been often limited—modest changes in circumscribed problems—and experimenter bias is a particular concern in the practitioner-researcher model, the studies have begun to produce the kinds of demonstrably effective interventions that empirical practice so badly needs.

Conclusions

The empirical practice movement has advanced social work's long and laborious pursuit of the goal of creating a scientifically based profession. However, the gains achieved have been partial, uneven, and not always clear.

Teaching students how to use research methods in their cases has become an established part of social work educational programs, and there is evidence of carryover into agency practice. Still, agency applications appear limited, and our picture of what they are actually like remains fuzzy. Contrary to early hopes, significant numbers of agency-based practitioner-researchers have not appeared. However, academic

researchers involved in practice have developed and tested an array of effective intervention models. Controlled single-system designs had been seen as a major breakthrough in effectiveness testing. So far, they have not added substantially to the knowledge base of social work, although they still have the potential to do so. As counterpoint, group experimental designs, which social work researchers were ready to write off in the early 1970s, have taken on new life. Significant advances have been made in the incorporation of research-based interventions into education and practice, but such interventions provide only a portion of the knowledge that is needed, and the extent of their use has not been adequately described.

As the historical review suggested, the empirical practice movement was instigated by research-minded academics who were involved in direct practice and committed to its improvement. Academics have continued to dominate the movement. If it is to realize in any substantial way its aim of making practice more effective, the movement must become better rooted in agency soil. It badly needs agency-based advocates, implementers, and, of course, practitioner-researchers. For this to happen, more extensive collaboration between our academic and agency establishments must occur, but the forms it should take are by no means clear.

Notes

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3. Florence Hollis, "Contemporary Issues for Caseworkers," in *Ego-oriented Casework*, ed. Howard J. Parad and Roger R. Miller (New York: Family Service Association of America, 1963), p. 13.
4. A number of other students in the program at this time subsequently made important indirect contributions to the empirical practice movement through their research and scholarship, although they have not been identified with that movement. They included David Fanshel, Trudy Festinger, Irving Piliavin, Aaron Rosenblatt, and Francis Turner.
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6. William J. Reid and Laura Epstein, eds., *Task-centered Casework* (New York: Columbia University Press, 1972).
7. Scott Briar and Henry Miller, *Problems and Issues in Social Casework* (New York: Columbia University Press, 1971).
8. For example, see Michael J. Mahoney, *Cognition and Behavior Modification* (Cambridge, Mass.: Ballinger, 1974) (cognitive therapy); William J. Reid and Ann W. Shyne, *Brief and Extended Casework* (New York: Columbia University Press, 1969) (short-term treatment); and Charles B. Truax and Kevin Mitchell, "Research on Certain Therapist Interpersonal Skills in Relation to Process and Outcome," in *Handbook of Psychotherapy and Behavior Change*, ed. Allen E. Bergin and Sol Garfield (New York: Wiley, 1971), pp. 299–344 (core conditions).
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10. William C. Sze and June G. Hopps, eds., *Evaluation and Accountability in Human Service Programs* (Cambridge, Mass.: Schenkman, 1978).

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13. *Wall Street Journal* (October 19, 1972), p. 20.

14. Michael W. Howe, "Casework Self-Evaluation: A Single-Subject Approach," *Social Service Review* 48 (1974): 1-23.

15. Martha B. Heineman, "The Obsolete Scientific Imperative in Social Work Research," *Social Service Review* 55 (1981): 371-97.

16. See, e.g., Sharon B. Berlin, "Dichotomous and Complex Thinking," *Social Service Review* 64 (1990): 46-59; John S. Brekke, "Scientific Imperatives in Social Work Research: Pluralism Is Not Skepticism," *Social Service Review* 60 (1986): 538-54; Mark Fraser, Mary Jane Taylor, Robert Jackson, and Jamal O'Jack, "Social Work and Science: Many Ways of Knowing?" *Social Work Research and Abstracts* 27, no. 4 (1991): 5-15; Walter H. Hudson, "Scientific Imperatives in Social Work Research and Practice," *Social Service Review* 56 (June 1982): 246-58; John R. Schuerman, "Debate with Authors: The Obsolete Scientific Imperative in Social Work Research," *Social Service Review* 56 (1982): 144-46.

17. Heineman (n. 15 above), p. 375.

18. Jill Doner Kagle, "Using Single-Subject Measures in Practice Decisions: Systematic Documentation or Distortion?" *Arete* 7, no. 2 (1982): 1-9.

19. Roy A. Ruckdeschel and Buford E. Farris, "Assessing Practice: A Critical Look at the Single-Case Design," *Social Casework* 62, no. 7 (1981): 419.

20. Scott Briar, "Incorporating Research into Education for Clinical Practice in Social Work: Toward a Clinical Science in Social Work," in *Sourcebook on Research Utilization* (Washington, D.C.: Council on Social Work Education, 1979), pp. 132-40; Deborah H. Siegel, "Can Research and Practice Be Integrated in Social Work Education?" *Journal of Education for Social Work* 19, no. 3 (1983): 12-19.

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27. Richey, Blythe, and Berlin (n. 23 above), p. 18.

28. Penka and Kirk (n. 23 above).

29. See, e.g., Kevin Corcoran and Joel Fischer, *Measures for Clinical Practice: A Source Book* (New York: Free Press, 1987); and Walter W. Hudson, *The Clinical Measurement Package: A Field Manual* (Homewood, Ill.: Dorsey, 1982).

30. Briar, "Incorporating Research" (n. 20 above).
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32. Kagle (n. 18 above).
33. William J. Reid and A. D. Smith, *Research in Social Work* (New York: Columbia University Press, 1989); and Nelsen, "Single-Case Research and Traditional Practice" (n. 31 above).
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37. Thomas (n. 35 above).
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45. Edward J. Mullen and John R. Schuerman, "Expert Systems and the Development of Knowledge in Social Welfare," in Videka-Sherman and Reid, eds. (n. 25 above), pp. 67-83.
46. See, e.g., Sharon B. Berlin and Jeanne C. Marsh, *Informing Practice Decisions* (New York: Macmillan, 1993); Edward J. Mullen, "Personal Practice Models," in *Handbook of Clinical Social Work*, ed. Aaron Rosenblatt and Diana Waldfogel (San Francisco: Jossey-Bass, 1983); Tony Tripodi and Irwin Epstein, *Research Techniques for Clinical Social Workers* (New York: Columbia University Press, 1980); and John S. Wodarski, *The Role of Research in Clinical Practice: A Practical Approach for the Human Services* (Baltimore, Md.: University Park Press, 1981).
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