

Intervention Research in Social Work: Recent Advances and Continuing Challenges

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The purpose of this article is to review substantive and methodological advances in interventive research. Three substantive advances are discussed: (a) the growing use of a risk factor perspective, (b) the emergence of practice-relevant microsocial theories, and (c) the increased acceptance of structured treatment protocols and manual. In addition, three methodological developments are discussed. They include new developments for dealing with attrition, for dealing with selection effects, and for decomposing complexities using text and numerical analyses. Arguing that intervention research holds the potential to unify research scholarship in social work, the conclusion discusses ongoing challenges associated with the implementation of new programs, variance in outcomes by method, reactivity to measurement, and construct validity in the context of culture.

Keywords: social work research; intervention research; methodology

In 1971, Scott Briar and Henry Miller wrote a book called *Problems and Issues in Social Casework*. In that book, they defined intervention as “an attempt to induce change selectively” (p. 173), and they argued that “the sources of knowledge [for intervention include] not only the experiences of caseworkers . . . but a broad range of studies bearing on the problems of induced change” (p. 174). In today’s parlance, these sources of knowledge include studies of

- risk and protective factors related to social and health problems
- studies of recovery from risk exposure (as in ethnographies of resilient children)
- studies of attempts to systematically induce change through individual, family, group, and community intervention, including significant changes in public policy.

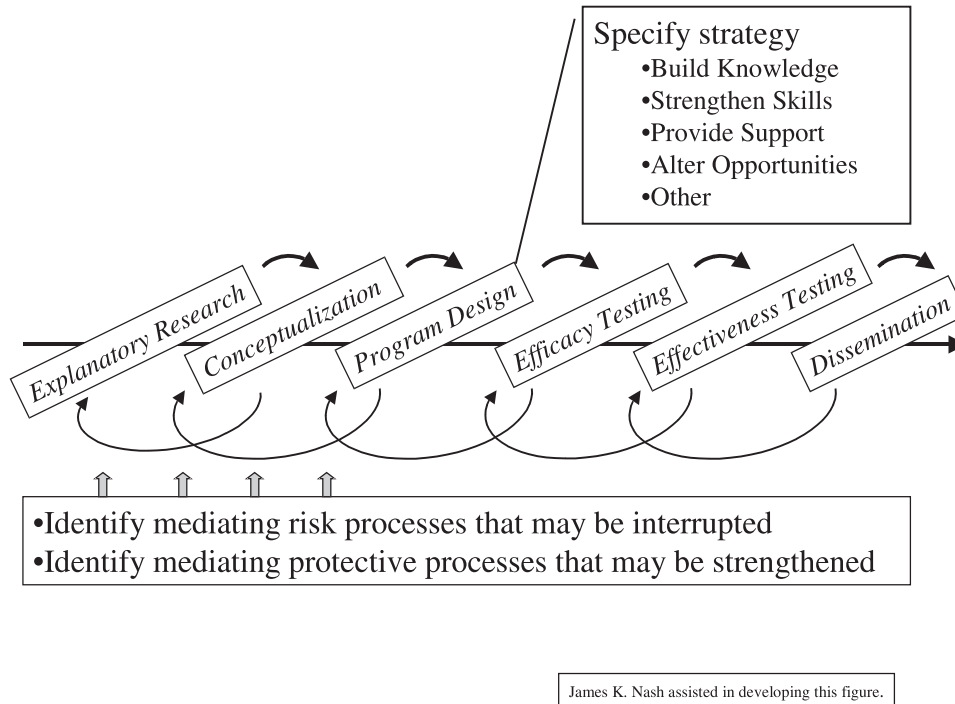
The profession of social work is distinguished from anthropology, psychology, sociology, and other disciplines by its focus on induced change (Rosen & Proctor, 1978;

Rosen, Proctor, & Staudt, 1999; Schilling, 1997). As social work researchers, we are not content to study phenomena as they exist. The essence of social work research is the study of intervention—the development and design of systematic change strategies (Rubin, 2000; Thyer, 2000).

The purpose of this article is to selectively review substantive and methodological advances in interventive research and to discuss challenges confronting social work scholars in extending knowledge regarding intervention. I will not review interventions per se. Given our diverse interests and areas of expertise, that would be a large undertaking . . . more, perhaps, than what can be accomplished in a single lecture (for a recent review of interventions related to children and adolescents, see Allen-Meares & Fraser, 2004). Instead, I focus on changes over the past decade that contribute to the design and development of interventive knowledge. In addition to explanatory research, intervention research is requisite for a profession because professions are committed to change. In their landmark book, *Intervention Research: Design and Development for Human Service*, Rothman and Thomas (1994) described a six-phase engineering perspective for the development of social and health services interventions. These phases or steps include (a) problem analysis and project planning, (b) information gathering and synthesis, (c) design of intervention, (d) early development and pilot testing, (e) evaluation and advanced development, and (f) dissemination.

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Figure 1: Intervention Research: Identifying Mediating Processes as the Basis for Program Design

In the problem analysis and information-gathering steps, explanatory research, which includes cross-sectional and longitudinal studies such as the Dunedin Health and Development Study in New Zealand (Moffitt, Caspi, Harrington, & Milne, 2002) and the Metropolitan Area Child Study in the United States (Hanish & Guerra, 2002), has an important place. It should identify populations with heightened risk, and it should specify factors associated with elevated levels of vulnerability. Ideally, these identified risk factors then become the bases for a change strategy.

However, historically, explanatory research provided little information about how to change risk and protective processes. Moreover, it sometimes identified conditions and processes that were not subject to control. Gender, for example, is correlated with aggressive behavior, however gender is not subject to change. Similarly, model-building research sometimes identified conditions that were difficult to change. Research on delinquency showed that association with delinquent peers was highly associated with offending (for review, see Shoemaker, 2000). However, altering peer relationships is a daunting challenge. At the program level, risk factors such as gender and peer relationships tend to have greater interventive value when research begins to articulate the mediating/moderating processes that lead from vulnerability-producing conditions to differential outcomes.

In that regard, an emerging tenet of intervention research is the identification of mediating (and moderating) factors that explain outcomes. This could involve, for example, understanding more fully the relationship between early peer relationships and association with delinquent peers. Or in the case of gender, it could involve understanding more fully the impact of relational or sexual victimization on cognitions—scripts, schemata, and attributional biases (e.g., Crick, Grotpeter, & Bigbee, 2002). From this perspective, explanatory research is most helpful when it identifies risk processes that may be interrupted and protective or adaptive processes that may be strengthened through intervention (Cicchetti & Hinshaw, 2002).

As shown in Figure 1, which presents a prevention science way of thinking about intervention research, explanatory research should lead to the conceptualization and design of programs in terms of mediating risk and protective processes. This, of course, is tricky. It involves translating mediating processes into actions that can be undertaken in practice. These actions include, but are not limited to, building knowledge, strengthening skills, and providing support or opportunities (e.g., the support and job training opportunities afforded by psychosocial clubhouses). Once designed, a program is tested in efficacy and effectiveness trials. Efficacy studies maximize efforts to deliver a service with fidelity; and in small, controlled

trials, they focus on outcomes. If findings are promising and funding is available, efficacy trials are followed by larger studies of the effectiveness of interventions when they are brought to scale. Finally, in this linearization of the intervention research process, dissemination, which is a vastly underestimated problem, follows effectiveness studies. However, of course, findings at any point in time can lead to a retracing of previous steps.

TO WHAT DEGREE ARE SOCIAL WORKERS INVOLVED IN DOING INTERVENTION RESEARCH?

Because the study of interventions occupies a central position in research in social work, we might ask: To what degree is the profession producing intervention research? In a recent review, 863 (47%) of 1,849 articles published from January 1993 to July 1997 by social workers in 13 practice-oriented journals contained original research (Rosen et al., 1999). However, only 126 (15%) of the 863 research articles focused on the effects of an intervention, and of those, replicable interventions were reported in only 53 (42% of 126) articles. Thus, about a dozen research reports a year described an evaluated intervention in such detail that a practitioner might be able to replicate it. Similarly, a study of research indexed between 1990-1999 in Psycinfo, Medline, Ageline, and SWAB databases identified evaluations of 130 social work intervention programs (Reid & Fortune, 2003). Across these programs, 107 met criteria for replicability (by virtue of the availability of practice protocols or treatment manuals) and at least one positive outcome relative to outcomes observed in a comparison or control group. Thus, in this review also, social work researchers produced about a dozen articles on intervention research annually. Because social work researchers publish in a wide array of journals, these studies probably underestimate the true amount of intervention research in the profession. However, taking that into account, it seems nonetheless a small number for a practice-oriented profession.

This probably reflects the fact that research on induced change is time-consuming and expensive. Intervention research is, perhaps, more difficult than other types of research because it requires skills that range

- from hobnobbing with practitioners . . . to estimating hierarchical linear models or, in the case of qualitative methods, to spending hours and hours in front of an Atlas/ti screen trying to think through convergent themes in text data

- from puzzling over client recruitment . . . to using multiple imputation programs to estimate values for missing data
- from providing clinical supervision . . . to assessing treatment fidelity.

At once, intervention researchers must be clinical, substantive, and methodological experts. They have to have good street sense. They have to be skilled in building partnerships with clinicians and with agencies. Similar to good practitioners, they must be able to encode social cues, interpret social information, and regulate their own behavior to be effective in a variety of settings.

ADVANCES IN INTERVENTION RESEARCH

Recent advances hold the potential to expand our capability to do intervention research. These include at least three substantive and three methodological developments. The three substantive advances involve:

- the growing use of a risk factor perspective
- the emergence of practice-relevant microsocial theories
- the increased acceptance of structured treatment protocols.

On the methodological side, many developments in qualitative and quantitative methods are noteworthy. I briefly address only three:

- methods for dealing with attrition
- quasi-experimental designs for dealing with selection effects
- advances in analytic methods for both text and numerical analysis.

I chose these in a decidedly unscientific way. They are based on my reading and experience. I do not present these as the most significant advances. They simply seem to me to be advances that will help in my own work and, perhaps, in work that you are doing. I apologize if I have missed your favorite "advance."

Substantive Advances in Intervention Research

Risk factor perspective. First, through advances in developmental psychopathology and, more broadly, advances in prevention science, there is increasing acceptance of a risk factor perspective as a basis for scholarly inquiry and practice in social work. The development of a common language between practitioners and researchers

may increase the potential for collaboration. In addition, the risk factor perspective is atheoretical; that is, it posits no particular types of variables or causal structures. In addition, it is cross-disciplinary, understood in aging, child welfare, health, juvenile justice, mental health, substance abuse, and other settings.

To be sure, there are continuing challenges in using a risk factor approach. In particular, conceptualizing protection remains problematic. Often equated with assets and strengths, protection is widely misused and is inadequately distinguished from risk. However, here too progress is being made in drawing distinctions between protective (interactions) and promotive (main) salutogenic effects (see, e.g., Fraser, 2003; Sameroff & Gutman, 2004).

Theoretical developments. The second substantive advance has to do with theory: Theory has become more relevant and less balkanized. Broad theories such as ecological theory and learning theory, which Kazdin (1999) called untestable approaches, have given way to highly-specified microsocial theories that posit mediating mechanisms which are often subject to intervention. In my field of children's studies, these include coercion theory (e.g., Reid, Patterson, & Snyder, 2002) and attachment theory (e.g., Cassidy & Shaver, 1999). In addition, renewed interest in the work of Park, Burgess, Shaw, and McKay from the Chicago Area Projects (e.g., Park, 1916) has given rise to a modern version of social disorganization theory (Coulton, Korbin, & Su, 1999; Sampson, Morenoff, & Gannon-Rowley, 2002; Sampson, Raudenbush, & Earls, 1997), a perspective that is beginning to specify mediating mechanisms for community interventions. At once, theory has become less remote and more relevant to practice. This is because, in part, social work scholars, such as David Hawkins and Rico Catalano, refused to accept the encamped nature of sociological theories of delinquency and developed a practice relevant perspective—the social development model—that incorporates social control, social learning, and social structural perspectives with a microsocial change strategy (e.g., Huang, Kosterman, Catalano, Hawkins, & Abbott, 2001). That change strategy involves (a) altering the opportunities afforded by social structures, (b) strengthening the skills that people bring to bear in solving life's problems, and (c) balancing recognition, including environmental contingencies, so as to promote prosocial behavior.

At the same time, researchers developing theories have attended to sampling as never before. Theories have been tested in samples including adequate representation of

girls and of children from a variety of racial and ethnic backgrounds. Social information processing theory, a microsocial theory that focuses on the role of cognitive distortions in the etiology of aggressive behavior, has been tested now in samples of African American, Latino, and White children (Crick & Dodge, 1994; Fontaine, Burks, & Dodge, 2002; Graham & Hudley, 1994). It elevates the behavioral theory perspective of the 1970s to a cognitive behavioral theory perspective that is readily integrated with theories of arousal and emotional regulation. We used it as the basis for developing a treatment manual called *Making Choices: Social Problem Solving for Children* (Fraser, Nash, Galinsky, & Darwin, 2001).

Structured practice resources. Third, written treatment manuals and practice protocols are becoming more common. Whether through the accountability movements in education and social welfare or through evidence-based practice, the use of practice guides is obtaining growing acceptance among practitioners. Moreover, the development of treatment manuals has become an essential ingredient in applying for funding for intervention research. At the same time, some commercial presses are realizing that profits can be made in publishing treatment manuals. NASW Press and Guilford Press, for example, have practice resources book series. Through the Web sites of the Campbell Collaboration and many professional organizations, systematic reviews of the literature and practice guidelines for criminal justice, education, health, mental health, and social welfare services are being developed (see www.campbellcollaboration.org/Fralibrary.html). In addition, the development of electronic resources, such as the National Guideline Clearinghouse (www.guideline.gov/) and Resources for Social Workers (RSW; www.nyu.edu/socialwork/wwwrsw/), provides a new means for disseminating research knowledge.

Methodological Advances in Intervention Research

Turning now from substantive to methodological advances in intervention research: These involve new methods for dealing with attrition and selection, plus advances in methods for analyzing numerical and text data. The core idea of intervention research continues to follow a counterfactual model; That is, we ask: "What would have happened to the people who received an intervention in the absence of the intervention" (or counter to the fact that they received an intervention)? A treatment effect is the difference between what actually happened and what would have happened in the absence of intervention (Shadish, Cook, & Campbell, 2002). Because

people cannot simultaneously receive an intervention and not receive an intervention, we usually compare those people who received the intervention to a group of people who are similar in every way except for the fact that they did not receive a treatment. However, it is never as easy as that.

Creating comparability in two groups is difficult. When sample sizes are large enough, random assignment is useful. However, when sample sizes are small, groups may not be equivalent even if randomly assigned. In addition, sometimes random assignment cannot be used. Instead we match participants or recruit comparison groups of people. This introduces selection biases, the possibility that an observed treatment effect is due not to the intervention but to differences between the people who received the intervention and those who did not. Moreover, there are a variety of other threats to making counterfactual arguments. These include, for example, differential attrition and maturation, and the effects of measurement or simply receiving special attention. Finding a treatment effect is contingent on the design of an intervention (e.g., does the intervention target the right risk or protective factors, does it actually change them?), the fidelity of its implementation, the precision of measurement, and a host of other concerns that make intervention research and, indeed, doing experimental studies, look similar to a career-ending enterprise (for a critique of randomized studies, see Heckman & Smith, 1995). Because there are so many ways to fail to observe a treatment effect or to falsely observe a significant treatment effect, one must always ask: “Is it plausible that an observed treatment effect is due to the intervention?” The plausibility that an intervention produced an outcome is seldom crystal clear, however advances in research methods make it possible to make more persuasive arguments.

These are new methods for dealing with:

- attrition biases—the issues of “missingness” and multiple imputation of missing values
- selection biases—nonexperimental designs for when random assignment fails or is not possible
- analytic advances—for decomposing complexities.

Attrition bias. Attrition lowers statistical power, and if it is related to treatment, it can compromise the equivalence of treatment and control groups. There are two types of attrition. *Measurement attrition* is the loss of data from participants in a study. *Treatment attrition* is the loss of participants in an intervention, regardless of whether they continue to participate in data collection. Partici-

pants who drop out of an intervention can sometimes be enticed to remain in a data collection. From an intent-to-treat analytic perspective, this is critical. Even when treatment attrition cannot be prevented, redoubled effort is needed to prevent measurement attrition.

Measurement attrition is often called the problem of “missing data.” It can range from missing information on a single item from a single participant to the loss of all information from a participant who refuses to continue in a study. When measurement attrition is large, participants with missing information are usually removed from the data set. Then analyses are conducted to determine whether attrition is biased, differentially affecting either an intervention or a control condition. This often involves estimating rates of attrition by intervention group and describing different patterns of attrition over time, at different sites, and across participants with different characteristics. One hopes that attrition is random . . . however it usually is not (Shadish et al., 2002).

In the same study, different variables may have different rates of attrition or patterns of missingness. When missingness is random or MAR (missing at random; i.e., the probability “of missingness may depend on the observed values Y_{obs} but not on the missing data Y_{mis} ”), a variety of techniques may be used for estimating the missing information (Schafer, 2001, p. 359). Parenthetically, MAR is distinguished from MCAR (missing completely at random), which means that the missing value is unrelated to any patterns in the data (Allison, 2001). MAR allows missingness to be related to patterns in explanatory measures and in the observed outcome measure(s). Missingness cannot, however, be related to the actual missing values. This is difficult to test by examining the observed data, and it is an assumption that is sometimes difficult to support. For example, if placement data on a child in a child welfare study is missing because the child has run away, the value of Y_{mis} is clearly related to the reason for missingness. Essentially MAR permits imputation without having to model the missing-data mechanism (Schafer, 2001).

Often we wish to impute a value for missing information so that it can be included in the analysis (Rubin, 1996). The most common imputation methods are substitution of a mean score for a missing value, “hot decking” or replacement of missing values with values from a pool of active or similar cases, and the estimation of a missing value using a linear set of predictors (Schafer, 1997). All have serious limitations, for example, mean substitution corrupts marginal distributions, and linear prediction distorts correlations. Both lead to low standard errors and spurious significance.

In addition to expectation-maximization (EM) and direct maximum likelihood (also called raw and full information MLE), new methods that use Markov chain, Monte Carlo simulation approaches to impute missing values have recently become available (Maxin, 1999). In Monte Carlo simulation-based “multiple imputation” programs, entire data sets with imputed missing values are estimated multiple times. For each imputed data set, parameters are estimated, and then they are averaged across data sets to produce a final estimate. With EM and direct maximum likelihood methods, these multiple imputation methods have shown great promise when missing data can be predicted by patterns in the observed data (Schafer, 1999). This advance in research methodology applies, of course, to all kinds of studies (for a review, see Allison, 2001). However, its potential contribution to intervention research looms large because group sizes are often limited and the power to detect a treatment effect is particularly vulnerable to attrition.

Selection bias. The second methodological advance to which I would like to draw your attention has to do with selection effects. Selection bias occurs when the average person in an intervention condition differs from an average person in a control or comparison condition. Selection bias may be present when random assignment fails or when quasi-experiments without random assignment are employed. Even when random assignment is employed, selection biases can be introduced

- when participants assigned to a treatment expect to change (the so-called randomization bias when participants change behavior due to assignment to an experimental intervention)
- when participants assigned to a control group seek an alternative service to meet needs that will not be met because they will not receive the treatment (the so-called treatment substitution bias when participants gain access to a close substitute for the intervention)
- when there is treatment attrition, that is, when participants drop out because of something related to the intervention (Heckman & Smith, 1995).

In addition, the findings from studies with random assignment can be compromised by other well-known factors. These include Hawthorne effects (changes due the effects of attention received in a intervention condition but not necessarily due to the actual substance of the intervention), John Henry effects (changes occurring when participants regard assignment to the control condition as an adversity over which they try to prevail), and contamination or spillover effects (changes occurring when par-

ticipants in a control group are inadvertently exposed to elements of the treatment condition).

When differences between intervention and control or comparison groups are observed, the effects of selection can be entangled with the effects of treatment. This confounds arguments about the effect of an intervention. Indeed, it has led some experts to conclude that “there is a sizable divergence between the theoretical capabilities of evaluations based on random assignment and the practical results of such evaluations” (Heckman & Smith, 1995, p. 107).

Historically, it was argued that random assignment is the best—if not the only—way to control for the effects of selection; however, recent advances give researchers new tools to deal with selection when random assignment is not possible or is compromised. All these new approaches have to do with modeling selection bias and statistically adjusting group differences. If potential predictors of selection are measured, a “propensity score analysis” can be used to estimate the effect of selection.

A propensity score is the predicted probability of being in the treatment or comparison group. Typically, logistic regression is used to develop a set of explanatory measures and interactions that maximally distinguish the treatment from the comparison group at pretest. Often an alpha of entry is set at $p < .10$ or even $p < .25$. Another way to think about this is that the propensity score is the predicted probability of group membership (treatment or control) based on any measured information that is not caused by the intervention (for an application to attrition, see Sosin, 2002). In practice, this makes it possible to match intervention and control or comparison group participants on the estimated logit (of the predicted probability of group membership), a single number that represents a relatively large number of covariates (Morgan, 2001).

A variety of techniques are employed to caliper matches (Glazerman, Levy, & Myers, 2002). These include “nearest neighbor,” “kernel,” and “forced” matching with and without replacement sampling. As in cluster analysis, nearest neighbor involves selecting the best single match. However, sometimes a kernel approach is used—all matches within a certain bandwidth are identified for each treatment subject and then a single score for matched comparison-group participants is estimated by placing higher weight on persons closer to the treatment participant. Typically, a resampling strategy is used, that is., matches are replaced in the pool of possible matches (Heckman, Ichimura, & Todd, 1997). Sometimes too a forced match strategy is used. It involves finding the nearest neighbor to any treatment participant who

cannot be caliper matched. Each, of course, has strengths and limitations. However, discussion of these is beyond the scope of this article.

The propensity score can be used in a routine covariance analysis, although there is some evidence that a matching and stratifying approach is more efficient (Heckman et al., 1997; Morgan, 2001). Treatment effects are often estimated within quintiles of the propensity score, that is, differences on outcome measures between participants and their matches are estimated within five strata. Perhaps the best-known research in this area is that of James J. Heckman, an economist who in 2000 won the Nobel Prize for his work on these nonexperimental methods. Heckman sought to deal with selection biases in studies of labor market participation and the effectiveness of the national Job Training Partnership Act. The models developed by Heckman and others involve using a variety of alternative strategies for matching and for making statistical adjustments between program participants and nonparticipants. Typically, they employ at least two equations: one to estimate selection bias and one to estimate a treatment effect. His difference-in-differences approach to matching appears to significantly reduce selection bias in quasi-experimental designs, when matching is done locally (e.g., within local labor markets such as cities) (Heckman et al., 1997; Smith & Todd, 2000).

These methods are only beginning to penetrate social work, and depending on the estimation model, they produce a range of outcomes—some of which approximate treatment effects when random assignment is used (e.g., see Sosin, 2002). To be sure, the evidence is uneven, with about one half of the studies attempting to replicate experimental findings by using nonexperimental designs producing promising findings (Glazer et al., 2002). Thus caution is warranted. The conditions under which a propensity score plus matching strategy seem more likely to replicate experimental findings are not yet clear. However, they appear to include (a) when the comparison group is drawn from within the agency or local community and (b) when preintervention measures are used in a selection bias equation (Glazer et al., 2002). Even though these emerging methods tend to require larger samples, their potential is great, because, at the agency level, it is random assignment and not pre- and postmeasurement that often compromises intervention research.

Analytic advances in decomposing complexities. Finally, with regard to the third advance in methodology, broad advances in analysis give intervention researchers a

variety of new tools for decomposing complexities. On the quantitative side, programs are now readily available for structural equation modeling—a family of statistical methods that reduce random measurement error in multi-item measures and that permit defining the effects of interventions as altered developmental trajectories relative to trajectories in a control or comparison group (Curren & Muthen, 1999). On the qualitative side, a variety of programs for text analysis have been developed. Beyond simple name and code programs, they provide a means to code volumes of narrative and then to link constructs anchored in text to related constructs that are also anchored in text.

Just reviewing statistical methods in a bare bones way, advances in variable-centered and person-centered analyses appear to be leading to a general latent structure perspective (Muthen, 2002). Even in well-controlled trials in which a routine service is augmented by an experimental service, assessing the effect of the added service is complicated by patterns in variation that may not be fully exposed in a main effects decomposition. Some of these new methods are useful for detecting differences in the presence of significant sample variation. Mixture modeling, for example, is a person-centered analytic technique that extends cluster analysis (Everitt, Landau, & Morven, 2001). Named “mixture modeling” because it integrates continuous with categorical latent variables, it allows different individuals in a sample to belong to different groups without their membership being observed (Jones, Nagin, & Roeder, 2001). This “latent class membership” is inferred from the data, and then these classes can be related to outcomes using continuous-time or discrete-time modeling frameworks. In a longitudinal study of 926 boys from low-income neighborhoods in Montreal, Brame, Nagin, and Tremblay (2001) used a mixture approach to model developmental trajectories for early childhood (teacher-reported) and adolescent (self-reported) physical aggression. A three-group model of low, medium, and high aggression best fit the childhood data. A four-group model of no aggression, increasing aggression, decreasing aggression, and high aggression fit the adolescent data. The joint distributions of childhood and adolescent developmental trajectories provide strong support for two conclusions. First, the majority of aggressive children do not become aggressive adolescents. Second, physically aggressive adolescents have almost always been physically aggressive children. There was little support for a “late-onset” perspective, wherein a child who is not aggressive becomes aggressive as an adolescent (Brame, Nagin, & Tremblay, 2001).

Algorithms for mixture models continue to be developed. Some mixture models permit the creation of “latent profiles” where variables used to create groups are continuous. Solutions in mixture modeling are sensitive to the start point. Because of this, programs such as M_{plus} allow multiple start points to assess model fit. In SAS, mixture models are estimated using a program called TRAJ. TRAJ can be thought of as a semiparametric, group-based modeling procedure. It can use count, scale, or dichotomous data. It has Poisson model adjustments for modeling data involving rare occurrences. The Bayesian information criterion (BIC) is used to compare models. A lower BIC is generally used to identify preferred models (see Jones et al., 2001).

More important, mixture modeling can be combined with models that incorporate measures over time or time to the observation of an event, for example, change in placement status, recovery from an illness, or return to addiction. The approach is intended to augment hierarchical linear modeling and latent growth curves, which model trajectories using continuous multivariate density functions (Jones et al., 2001). Mixture techniques use a multinomial modeling approach to identify unobserved subpopulations with different parameter values (Muthen & Masyn, 2001). To be sure, the mixture technique is only recently available in SAS and M_{plus} , and it is likely to be much refined in the coming years. Even at this point, however, it plus advances in text analysis and in a host of statistical techniques—especially structural equation and hierarchical linear modeling—hold potential to uncover subgroupings of participants who benefit or fail to benefit from an intervention.

CHALLENGES IN INTERVENTION RESEARCH

In spite of substantive and methodological advances, we continue to confront a variety of problems that confound counterfactual arguments and lead some to conclude that the whole enterprise is not worth it. I am not in that camp. Intervention research fails mostly because of simple things. In the same way that the historic Cambridge-Somerville Project was challenged to deliver and measure a multielement intervention (McCord & McCord, 1959), we are today challenged to do the same . . . and we sometimes fail in the same ways. So I would like to discuss briefly the enduring issues of implementation, method variance, reactivity, and construct validity.

Treatment Implementation: Faithful Delivery and Adherence

In intervention research, implementation refers to carrying out a series of actions or activities that are expected to produce a positive response. In the era when researchers from academia evaluated programs but did not participate in their design, it was ignored as an element of the research process. Indeed, researchers often viewed themselves as evaluation experts whose university auspices conferred objectivity on studies. The evaluation of new programs and poorly designed programs led in the 1970s to widespread skepticism about the effectiveness of social and health services. Due in part to the growing participation of social work researchers in the design, development, and evaluation of innovative programs, this is not the case today. In nearly every field of practice, we can point to risk-focused, well-implemented, carefully evaluated programs that have positive findings (Allen-Meares & Fraser, 2004).

Implementation has to do with the fidelity of a program delivered by practitioners. As it is usually conceptualized, it depends on (a) the clarity of program elements, (b) the training and clinical supervision of staff, and (c) the management of organizational contingencies. However, implementation also has to do with the adherence to a program by participants. If one thinks about implementation as adherence and delivery, a host of factors contribute to what might be called the unobserved change model of an intervention. For example, whether a parent learns a particular skill in a parenting training program that is fully specified and employs the latest thinking depends on

- what the practitioner says when she or he first meets the parents
- how she or he responds to the first few things the parent says
- whether the parent regards her or him as having good intentions
- whether the parent has more pressing problems (e.g., a toothache)
- whether the parent’s children are being adequately supervised
- whether the time of the training is convenient
- whether transportation is available, and so on.

Implementation is more than the provision of a program. It has to do with the receipt of a program and adherence to program protocols. It is related to a personalized ap-

proach to recruitment and retention, culturally-sensitive staffing, the timing and targeting of program efforts to key transitions (e.g., the start of academic grading in the fourth grade or graduation from elementary school), and the matching of participants' interests to program content (Prinz et al., 2001). In efficacy studies (where one tries to deliver a program under the best possible circumstances), we often are able to provide supplemental training and supervision for practitioners; reimbursements to participants for their time; free transportation, meals, and child care during program meetings; and assistance in solving concrete problems prior to the provision of other elements of an intervention. In effectiveness studies (where a program is delivered under less-than-ideal conditions), these enticements—no matter how necessary we might feel they are—may not be available. Implementation is often the casualty.

It is not possible to estimate accurately the effect of a program if it was not delivered, however it is possible to estimate the effect of a partially implemented program. When a program is not implemented fully, there are a number of alternatives. These range from qualitative study of success and failure cases, to intent-to-treat analyses (where outcomes are estimated for participants who were assigned to treatment regardless of whether they completed or adhered to treatment), and to analyses by the type and amount of content received. The problem with the latter is that a selection bias cannot be eliminated. Participants who received treatment are typically different from those who did not receive treatment. Consequently, estimates of outcomes by types of services are confounded by the differing characteristics of participants who received and did not receive various elements of an intervention. Beyond the scope of this article, methods are rapidly being developed for treating random assignment as an instrumental variable when exposure to treatment has significant variation. Providing bounds on estimates of treatment effects as opposed to point estimates are based on making alternative assumptions about the effect of random assignment and the distribution of treatment compliance (see, e.g., West & Sagarin, 2000).

Method Variance: Contradictory Findings Contribute to Fuzzy Plausibility

Perhaps, the most significant change in research enterprise over the past 10 years is the profession's growing methodological pluralism. In addition to the growing acceptance and rigor of qualitative methods, it is now expected that a well-designed study will use a multimethod, multisource measurement strategy. In

doing research with children and families, it is quite common to use agency-level information, reports from teachers, reports from parents, and self-reports from children. Sometimes called *critical multiplism*, this perspective is based on the idea that the truth is never fully knowable, however that it likely lies somewhere in the approximation of treatment effects based on different sources and methods of estimation (Kirk & Reid, 2002).

Here is the problem. This often leads to a situation where some measures produce significant findings and other measures do not (see, e.g., Fraser, Pecora, & Haapala, 1991; Grossman et al., 1997; Henggeler, Clingempeel, Brondino, & Pickrel, 2002). Method variance refers to variation in scores because of the "reporting agent, context, strategy, scaling protocol, and length of recall required of raters" (Eddy, Dishion, & Stoolmiller, 1998, p. 61). Eddy et al. (1998) found that "a full 30% of the variance (across parent, child, and worker ratings of parenting) was due to method" (p. 61). If they are stable, method variances will not bias treatment effect estimates within methods. However, they may be responsible for different findings across methods. If they are unstable (influenced, say, by changing neighborhood conditions or the cycle of a disease or disorder), they may produce quite different findings within methods over time. Method variance often produces findings that are equivocal and not given to the authority orientations of many practitioners (for a discussion of authority-based practice, see Gambrill, 1999). In short, the plurality of our methods leads to complicated findings and fuzzy plausibility.

Controlling Reactivity: Sensitivity to Change Versus Reactivity in "Natural Raters"

A related challenge has to do with reactivity to measurement and intervention. Parent and teacher ratings, similar to the ratings from neighbors, relatives, friends, and others who are informed about the behaviors of clients, are used often by intervention researchers to increase sensitivity to change. However, the very familiarity that makes such ratings attractive also makes them problematic. Scores from "natural raters" are at once slow to react to recent changes in behavior and quite subject to suggestion that occurs through repeated measurement or knowledge that a client is involved in an innovative service (Stoolmiller, Eddy, & Reid, 2000). The first can produce low sensitivity to change on scales—such as scales that invite raters to score child attributes (e.g., never true of child, sometimes true of child, almost always true of child). This tends to underestimate a true treatment effect.

The second creates the expectation of change and tends to inflate treatment effects. This can be controlled by using a Solomon four-group design or reduced by using measures that are not sensitive to expectancy effects. These include physiological measures (e.g., saliva tests), measures by raters who are blind to treatment condition, moment-by-moment behavioral observation, and official reports such as arrests, births, and hospitalizations. However each of these solutions poses complications that elevate the difficulty and price of intervention research.

Culture and Construct Validity: Creating Culturally Anchored Programs and Methods

As the fourth challenge, I want to say a few words about culture and construct validity. Research is always embedded in culture. Although there is no widely accepted definition of culture, most agree that it includes values, norms, expectations, and behaviors that are at once transmitted from parents to children and influenced by the changing characteristics of the context. Culture is a concept differentially applied to disability, ethnicity, gender, geographic location (e.g., rural culture), nationality, race, religious affiliation, and sexual orientation (Hughes & Seidman, 2002). Culture influences our conceptualization of social and health problems and the responses of research participants to the research processes that we develop. It is impossible to develop culturally-free methods; our challenge is to develop culturally anchored methods.

Culturally rooted concepts and categories are the core elements of constructs. The physical sciences do not have this problem. The construct of weight, for example, is measured in widely accepted categories of pounds or kilograms. In social work, categories for important constructs such as attachment, depression, discrimination, and motivation do not exist. They must be developed. Moreover, many of our constructs have social, political, gender, and religious nuances. Conducting culturally-sensitive research requires more than diverse samples; it requires sensitive measurement.

The term *scale equivalence* refers to the alternative meanings given to items and response categories across peoples with different cultural backgrounds (Hughes & Seidman, 2002). Lack of structural or scale equivalence is evidenced when the number of constructs, their relationships, and item loadings vary across social and demographic groupings of people. Differences are often rooted in social experience and manifest in the lack of linguistic equivalence and cross-cultural relevance of constructs.

The measures one selects may not have linguistic equivalence (Do words carry the same meanings and nuances?), cultural relevance (Does the construct have the same relevance, e.g., racial socialization may have more relevance for African American than for White children?), and thus scale comparability.

In devising culturally-sensitive methods, we must identify the constructs that are relevant to the social processes affecting the people for whom an intervention is to be developed. This is not to ignore analysis, for analyses within racial/ethnic groupings are as important as analyses across racial/ethnic groupings. However, by starting with measurement, one implicitly starts with conceptualization . . . the specification of risk processes, constructs, and relationships among constructs.

Recent studies raise profound questions about the structure of constructs and the risk mechanisms that operate within and across peoples. Constructs such as relational aggression, familism, personalismo, racial socialization, and ethnic identity appear to be related to differing developmental outcomes for children from different social and demographic groupings (Crick et al., 2002; Laursen & Williams, 2002; Miller, 1999; Miller & MacIntosh, 1999; O'Brien-Caughy, Randolph, & O'Campo, 2002; Vega & Gil, 1998; Vega, Gil, & Wagner, 1998). Given this work, interventions with girls in elementary school would surely have to consider relational aggression. Cutting-edge interventions for children from many Latino families would surely have to consider such concepts as familism and personalismo. Similarly, a cutting-edge intervention for children from African American families would surely consider the construct of racial socialization.

Culturally anchored research then is based not just on sample size and diversity but also on the conceptualization of social and health problems and the specification of measurement models that may have common and culturally-specific elements. At the design and development stage of intervention research, we must determine whether differences warrant separate activities within an intervention, or whether differences are of such a magnitude that an entirely separate intervention based on new constructs is warranted. We must ask:

To what degree do risk factors such as relational aggression or, perhaps protective factors such as familism, warrant the development of culturally-specific activities and interventions?

We can know only if more of us begin to develop and test programs.

CONCLUSION

In conclusion, the substantive and methodological advances of the past decade give us new information and tools for developing interventions. On the substantive side, advances include the growth of an epidemiologically based risk factor perspective, theories that more clearly specify mediating social processes, and structured resources for practice. Methodologically, advances have been made in dealing with missing information because of attrition, quasi-experimental designs for dealing with the effects of selection, and decomposing the effects of interventions with new analytic tools for text and numerical data. At the same time, we are confronted with continuing challenges—ensuring that programs are carefully designed and implemented, interpreting findings in the face of method variance, creating measures that reduce reactivity and yet are sensitive to change, and developing culturally anchored programs and methods.

In the next decade, we will confront new challenges as well. These include challenges in measurement that arise from findings from the human genome project, from brain imaging research, and from advances in psychopharmacology. The more we learn about nature and genetic effects, the more it appears that nurture and environmental conditions affect the expression of disorders in persons with high genetic liability. We have a growing understanding of the role of neurochemicals in calibrating social behavior, living with disorders, and recovering from trauma. The development of measurement models that include psychosocial and biological measures is likely to produce new threads of research where health scientists and social work researchers collaborate to devise and test interventions.

In addition, because continuing to move forward is intimately linked to social work education, a related issue lies in creating structures that will strengthen the capacity of the profession to undertake intervention research. In part, the fragile connections between MSW and doctoral training in social work are implicated. Few, if any, MSW programs place emphasis on research. Unlike psychology, public health, and sociology where research training is sequenced from masters-level through doctoral courses, research training in social work occurs principally at the doctoral level. Thus relatively more content must be covered in social work doctoral programs. Moreover, courses in research methods and data analysis are merely a foundation for intervention research. Intervention research itself involves

- mastery of substantive and interventive knowledge in a social or health problem area

- skills in translating knowledge into treatment manuals
- ability to build partnerships and collaborate with agencies in implementing programs, training staff, and providing ongoing supervision
- expertise in developing measures of both program processes and outcomes.

There is not sufficient time in most doctoral programs to provide the range of coursework necessary. We need to create a continuum of research training in social work. Stronger predoctoral research training is needed, and we must develop a richer system of postdoctoral training opportunities.

The greatest challenge of all, however, is simply to do more intervention research. Twelve intervention studies per year is too few. Substantive advances allow us as never before to be precise about the risk and protective factors we wish to target. Methodological advances allow us to estimate treatment effects without random measurement error and in the face of significant missing data. New methods, being developed by Heckman and others, hold promise for strengthening the plausibility of arguments based on more practice-friendly matching designs.

Given these advances, I invite each of you to conduct one intervention study over the next 10 years. Assuming that 300 studies are done over the next decade, we will more than double our current productivity. If you are more oriented toward qualitative research, I ask you to study more than phenomena as they exist. Instead, devise a change strategy, implement it, and describe the processes leading to outcomes. If you are more oriented toward quantitative or mixed methods research, I ask the same thing: Let your descriptive and model-building studies lead you into intervention research. Write a treatment manual based on your findings and develop a program using a design and development approach.

Regardless of your methodological persuasion, it may take several years for you to do this, and so intervention design can be but one of several themes within your scholarly portfolio. However, it is an important theme. My challenge to you is simple: In the context of our methodological pluralism, let us be drawn together by our commitment to develop strategies for induced change, for the central most activity in social work is intervention and research on intervention is foundational to the profession.

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