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Evidence-Based Programming in the Context of Practice and Policy

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Abstract

Scholars, practice professionals, and policymakers should welcome the new era of evidence-based programming and policies, but these constituencies need to be realistic about the complexities, uncertainties, and limitations that lie beneath what could easily become a simplistic process. This paper discusses some of the requirements for the replication of evidence-based programs, suggesting that many of these underlying assumptions are often not met. One of these requirements is the evidence itself, and alternative evidentiary criteria are discussed. A main theme is that even if a well-documented program exists, implementing it in communities on a broader scale requires different processes that are less well studied. For example, some alternative approaches to summarizing actionable knowledge are offered, including characteristics of effective programs, consensus groups, and the Pathways Mapping Initiative. In addition, strategies are discussed that hold the promise of bringing scholars and community stakeholders together in a collaborative process that will build community capacity and create and implement effective programs and services on a broader scale. Finally, the research enterprise itself needs to be transformed to more effectively contribute to program and system community change. Recommendations for improving the process are offered.

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FROM THE EDITOR

The purpose of using developmental research to guide policy-making is to make sure that what we do for children and families is as effective as possible. We sometimes forget, however, that this job does not end with policymaking. Implementation of policy is equally important. No Child Left Behind (NCLB) is a good example. It is a reasonable piece of legislation. However, its implementation identified a host of problems not the least of which was the tremendous state variability in implementation (See, e. g., Porter & Polikoff, *Social Policy Report*, 2007). For this reason, those of us interested in the uses of research have to attend to policy implementation as well as policymaking. This is the main reason that SRCD has both Congressional Policy Fellows, who deal with policymaking, and Executive Branch Fellows, who focus on implementation.

Another important distinction is between policy and practice. Our tendency is to attend to policy, but practice is equally important; and the two require different approaches. This issue of *Social Policy Report (SPR)* by Robert McCall addresses the use of research to inform practice. Evidence-based practice is a popular idea today. However, as McCall points out, we have to be clear and careful about our definition of evidence. Some would argue, for example, that only results from Randomized Control Trials (RCT's) should be used to support practice or inform policy. McCall has an earlier *SPR* (2004) in which he addresses whether RCT's should be the gold standard for evidence in the definition of evidence-based. The point is that using the term "evidence-based" does not necessarily tell us which practices should be disseminated and replicated. In this article McCall also raises the equally important, but often overlooked, issue of practice-based evidence. One mistake that researchers have made, which has limited their influence and effectiveness in practical settings, is to assume that communication is unidirectional, with researchers being the experts with all the information to offer. In fact, policymakers and practitioners have just as much information to offer academia as we have to offer them. This is perhaps more true of the practitioner community than of policymakers because of the great diversity of approaches to practice.

We are also pleased to have three outstanding commentaries by Karen Blase and colleagues, Jean Grossman, and our own Mary Ann McCabe. All three as usual make important complementary points to the main article. We are especially pleased to have the statement from Dr. McCabe; she has worked in practice her full professional career and has worked hard in several ways to put it on SRCD's agenda. Now after 6 six years of directing SRCD's Office of Policy and Communications (OPC) she is moving on to a new phase in her career. Hence, it is fitting that she is able to address one of the issues about which she is passionate in this, her last, issue of *SPR*.

This is a somewhat unusual *SPR* for us in that it commands a different audience than we typically reach with *SPR*. Jack Shonkoff has an article in *Child Development* (2000) in which he discusses the different cultures of research, policy, and practice. This *SPR* continues that tradition. Brooke and I hope that with this issue, our next to last issue as editors, we increase our attention to practice, in its multifaceted nature, as well as policy.

Lonnie Sherrod, Ph.D., Editor
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Evidence-Based Programming in the Context of Practice and Policy

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Toward More Realistic Evidence-Based Programming and Policies¹

This is the era of evidence-based programming and policies, in which funders and policymakers are demanding that the services they fund and the policies they create are grounded in empirical research and evidence of their effectiveness. Such programs and policies, it is reasoned, hold the promise of providing needy citizens with more effective services that will contribute to more productive and satisfying lives.

The new value for research and evaluation represents a long-sought opportunity for scholars, not only to be heard but to influence practice and policy to an unprecedented degree. After decades of realizing that research had extremely modest influences on policymaking (e.g., Weiss, 1977, 1988; Chelimsky, 1991; Leviton & Borouch, 1983), suddenly it would appear that research has not only “arrived” but occasionally it has become the coin of the practice and policy realms.

But every new opportunity has its risks. For example, are scholars up to the task? Do researchers have sufficient evidence on service programs and policies? Will scholars agree on what is “evidence,” or will they get bogged down nitpicking the limits of existing research? Will applied professionals glibly declare each program to be “evidence-based” if there is some research, any research, justifying directly or indirectly some element of the program? Are service professionals prepared to create, implement, monitor, and evaluate evidence-based programs? Will policymakers demand that scholars simply declare programs “proven” or “not proven”; will the “proven” programs be faithfully implemented; and will these transplanted programs actually work? Will anyone pay to find out if they work in the new context, or will policymakers assume they are already “proven” to be effective and do not need further evaluation? The quality of implementation of a program is as important to achieving desired outcomes in society as the original demonstration of the program, but we do not have a well-developed science of implementation. In the long run, what do scholars, practice professionals, policymakers, and communities need to do to cultivate these seeds of change and bring them to full flower?

This paper supports the new emphasis on evidence as a basis of practice and policy, but all constituencies—research and evaluation, practice, and policymaking communities—need to be realistic about this approach. Thus, while this paper emphasizes the complexities, uncertainties, and limitations that lie beneath what can easily become a simplistic process, it also suggests approaches to deal with these issues and still produce usable guidelines on which practitioners and policymakers can act.

More specifically, this paper considers issues in how the process of creating and implementing evidence-based social and behavioral programming and policies, primarily for chil-

dren, youth, and families, might proceed. Much of what is presented below derives from the author’s and colleagues’ at the University of Pittsburgh Office of Child Development experience with policies, practices, and program evaluation in one locale (e.g., Groark & McCall, 2005, in press; McCall, Green, Strauss, & Groark, 1997; McCall, Groark, & Nelkin, 2004), and many proposals discussed here have not been studied or evaluated. The presentation accepts the existing research literature as a starting point of this process, so it focuses on how that literature is summarized, communicated, used, and implemented. A major point is that documenting with rigorous demonstration programs and evaluations that a given service program can be effective at achieving its goals is only the first step along the science-to-practice continuum; what happens after that is just as crucial to the success of bringing that program to scale and achieving effectiveness in other communities, and the evidence for the effectiveness of these implementation procedures is quite sparse.

The programming to be discussed tends to be community-based, publicly available services that are nearly universal within a geographical area or institution (e.g., school) or with broad eligibility criteria (often income), such as Head Start, drug and alcohol prevention programs, and family support services, rather than traditional psychotherapy, for example (see APA Presidential Task Force on Evidence-Based Practice, 2006; Weisz, Jensen-Doss, & Hawley, 2006). The policies most frequently refer to those that define, fund, and implement such services. For the most part, local (i.e., state, county, city) policies are emphasized in this paper, because most services are selected, funded, and implemented at these levels, and the evidence for and processes to implement local services can be different than for national programs (i.e., Head Start).

The purpose of this paper is to consider and assess how this process is often currently carried out, alternatives to the way evidence is summarized and brought to the process, factors in how communities attempt to use such knowledge to implement or create services, and how the general research enterprise could be broadened to be more helpful. The paper argues that the process could be improved if several of the procedures suggested were used and studied to a greater extent.

A Brief Personal Interpreted History

Policymakers and funders have long desired “evidence” that service programs and policies achieve their intended goals, a value previously labeled “accountability”. President Lyndon Johnson’s Great Society was called the “experimenting society” by Campbell (1969), because many of the new programs and policies were experimentally evaluated, even with random assignment or quasi-experimental procedures. But such evaluations were often very large and expensive and aimed primarily at deciding simply whether the program “worked” or “did not work” and sometimes whether it was “cost-effective”. Unfortunately, the conclusion often was that the program “did not work”, and the result, in combination with a conservative political climate, was that social program research and evaluation temporarily fell into disfavor with policymakers

(e.g., Rossi & Freeman, 1993). Perhaps this outcome was a case of shooting the evaluation messenger along with, or instead of, the program, but it also likely derived from the fact that the information provided by such evaluations consisted largely of an effectiveness grade for a specific program and little else. The lessons learned from the “experimenting society” should be kept in mind during the current incarnation of evidence-based practices and policies.

Although large-scale social experimentation and evaluation passed out of favor, policymakers nevertheless continued to want to know whether their policies and services were accomplishing their intended purposes, and the need for accountability increased as problems appeared to increase while resources to deal with them diminished. In the author’s experience, two decades ago at the local level, “evaluation” of services often consisted simply of counts of the number of people served, their demographic characteristics and geographic locations, as well as the specific services they received; later policymakers began to demand more information on outcome effectiveness—did the participants benefit from the service or policy? For example, the National United Way urged its local affiliates to require grantees to provide some evidence of program effectiveness, and the Reinventing Government movement urged government agencies to demand accountability, not only from its grantees but also from themselves.

But as millions if not billions of dollars were being spent on human services and policies, policymakers apparently grew impatient with the accountability process. They often required each new program to conduct an evaluation, and the program and evaluation would be funded for only a few years and enough time for one cohort of participants. Moreover, the emphasis often cut straight to assessing outcome, skipping the monitoring and evaluation of the implementation of the service program and the fidelity of the service to its theoretical guiding principles (e.g., Fixsen, Naoom, Blase, Friedman, & Wallace, 2005). These themes persisted despite occasional demonstrations (when rare funding permitted) that service programs often needed two or three cohorts of participants for service professionals to learn and refine the implementation of the program before participants benefited (Fixsen et al., 2005)—a validation of Campbell’s (1987) admonition “to evaluate no program before it’s proud.” This represents another lesson learned.

Finally, policymakers wondered why they should try out so many new service programs and wait several years to find out if they worked when they could insist on replicating locally those service programs that already had been tried and found to be effective elsewhere. The strategy of replicating proven programs became the guiding principle of “evidence-based programming and policies”, and today it is often the primary strategy of local funders and policymakers. On the surface, this approach is logical

and reasonable; beneath the surface it is more complicated, uncertain, and potentially limited than it would appear.

Requirements for Replication of Evidence-Based Programs

In its simplest and most direct form, evidence-based practices often consist of identifying service programs that have been evaluated and found effective in one location and replicating those service programs in another. An example is the nurse-practitioner home visiting program of Olds and Kitzman (1993) aimed at high-risk mothers to minimize abuse and neglect and maximize development of their infants and toddlers, which has been widely implemented across the country and which has a relatively larger research base than many programs. But to replicate proven programs

as a common strategy requires that certain circumstances are present, and these requirements must be recognized and are often not fulfilled (e.g., Flay et al., 2005; Groark & McCall, 2005, in press; McCall et al., 2004).

Requirements to replicate programs are often not met.

Requirements for Replicating Proven Programs

1. *Some programs must exist that have been evaluated and demonstrated to be effective.* The most obvious requirement is that programs must already have been created, evaluated, and found to effectively prevent or treat the target problem or benefit the participants in the intended way. Some social problems have plagued society for years, and the government has funded a variety of attempts to prevent or rehabilitate such individuals. Teenage problem behavior (e.g., school failure, risky sexual activity, substance abuse, delinquency, and violence) is a domain in which numerous programs have been attempted and evaluated over the years, and this research literature, more than many, is up to the task of evidence-based programming (e.g., Weissberg & Kumpfer, 2003; Weisz, Jensen-Doss, & Hawley, 2006).

But this is not the case in every domain that government serves, and the reasons for this deficiency vary. For example, at least four decades of research show the potential of quality early childhood care and education programs to improve school readiness, reduce school failure and special educational services, improve employability, and lessen antisocial behavior and criminality (e.g., Haskins, 1989; McCall, Larsen, & Ingram, 2003; Ramey & Ramey, 1992; Reynolds, Wang, & Walberg, 2003). But it is only recently that research is being focused on the effectiveness of various early childhood curricula that promote emerging literacy, numeracy, appropriate social-emotional development, and pro-social behavior. Further, there is now some evidence on the essential elements of Head Start and Early Head Start programming that actually produces the benefits that the research demonstrates is possible, but this literature is less developed with respect to local early care and education services more generally. Here is a case in which research preceded policy in describing the potential of a human service but for decades was less prepared

to substantiate the effectiveness of specific curricula or program characteristics.

Conversely, social problems and policies frequently precede research. For example, when youth gangs emerged as a social issue in the early 1990's, there were few "proven programs" to keep youth out of gangs at the time policy-makers needed to act.

In some other domains we may never have sufficient evidence substantiating effective programs, especially for preventing low-frequency problems and problems that are difficult to solve with circumscribed interventions. Despite the great costs to victims and society of child abuse and mass adolescent killings in schools, it is difficult to conduct the necessary research to prevent such low-frequency phenomena in which risk factors are not sufficiently precise to identify potential problem individuals to study.

Finally, sometimes the government itself has failed to invest in evaluation, even for some of its largest programs. For many years, Congress voted to increase the number of children who could participate in Head Start, now the nation's largest direct service to young children, but it did not fund

evaluations or other research on the program's effectiveness. When the program came up for reauthorization nearly a decade ago, there was little information on its effectiveness (this has now changed; see Ludwig & Phillips, 2007; Puma, Bell, Cook, Heid, Lopez, Zill, et al., 2005).

Therefore, for a variety of reasons, the research and evaluation literature may not be able to deliver "proven" programs to policymakers to replicate for every, or even most, human service domains.

2. *A program needs to be packaged and described in detail sufficient for others to faithfully replicate it.* Educational curricula, for example, are articulated in great detail and packaged in a format and with a variety of supporting materials that allow schools and teachers to implement them easily and faithfully. This is likely the case because schools want packaged curricula and for-profit publishers compete to sell such curricula on the basis of their comprehensiveness, quality, and ease of implementation. But most other human services have not expected, planned for, or had funding to write down and package for dissemination a detailed description of the service program, training materials, implementation plan, and so forth. Even the research literature is lacking full descriptions of service programs, despite the facts that the research community values descriptive detail and that many of the programs evaluated were "demonstration" programs that presumably would be replicated if found successful. Consequently, in many cases the research literature is in the embarrassing position of having demonstrated that a certain "program" or "intervention" produced successful outcomes but having limited information describing the nature of that "program" or "intervention" for either research purposes or program replication (Fixsen et al. 2005).

3. *Local service providers must be willing and able*

to faithfully replicate the documented program. Even if the program is described completely, local service providers may not replicate it faithfully (McCall & B. L. Green, 2004). Even multi-sited national interventions, such as the Comprehensive Child Development Program, which prescribed the service regimen in great detail so that a uniform intervention could be implemented in numerous sites around the country, found that the actual program that was implemented differed substantially in character from one site to the next (Gilliam, Ripple, Zigler, & Leiter, 2000). Through training and experience, service providers develop their own beliefs and standards about services that work, and they are taught to tailor services to the individual needs and circumstances of their participants and service environment. Some program features may need to be modified to conform to local regulations, the local standards of best practice, available resources, and even the dictates of the agency's board of directors (Fixsen et al., 2005).

This propensity is based on sound therapeutic rationale, but it threatens the logic of replicating a program, since the program actually implemented may not, perhaps should not, be the clone

of the program demonstrated to be effective. Further, the adjustments made to fit the local participants, environment, and service providers' skills and beliefs are rarely studied, so the generalizability of a service program is often unknown, because program replications are rarely studied (or published) and policymakers may make the inference for economic reasons that a replicated program does not need to be evaluated because it is based on a program already "proven" to be effective.

Therefore, service professionals are unlikely to faithfully replicate a program, the changes they make may or may not be effective but this is not usually known, and funders and policymakers need to be encouraged to fund both monitoring program implementation to assess the fidelity of replication as well as evaluating the program's outcomes to assess whether the replication is as effective as the prototype.

4. *Replicating research-documented programs assumes they will produce benefits comparable to the original demonstration project.* Even if the original program is written down in detail and faithfully replicated, there is no guarantee that it will be as effective as its predecessor (e.g., L. W. Green, 2001). Indeed, it is widely understood in the human service field that "service programs do not travel well", that is, they are not as effective when replicated as the original demonstration (Fixsen et al., 2005).

There may be several reasons for this (e.g., Fixsen et al., 2005). First, as described above, the program may not be faithfully replicated and the adjustments to local circumstances may inadvertently limit (or increase) the program's effectiveness. Second, not all programs work for all participants, and a program that works for one type of participant in one part of the country may not be effective for other participants in other locations. Third, not all ele-

We know the program works,
but what is the program?

ments of the original demonstration that are crucial to its success may be replicated. For example, the commitment, enthusiasm, and skills of the creators of the original demonstration program as well as the particular provider-client interactions and relationships that they fostered may have been crucial to its success, but those personnel characteristics may not have been duplicated by the providers of the replication. We commonly recognize that the individual characteristics of a teacher contribute substantially to the effectiveness of a given curriculum, and this principle may be equally applicable to other human services.

The Evidence Needed to Declare a Program “Evidence-Based”

In the philosophy of science, nothing is ever “proven” with total certainty, because cause and effect is probabilistic, and in the behavioral sciences those probabilities are often modest. But policymakers tend to want to know whether a given service program works—“yes” or “no”, not “maybe”, “sometimes”, or “to some extent for some people”. How much evidence and what kind of evidence is needed to declare that a service program is worthy of being replicated on a larger scale?

Criteria for judging the evidence. There have been several attempts to set forth criteria for what constitutes evidence that a given program “works”. At the extremes, one set (e.g., Biglan, Mrazek, Carnine, & Flay, 2003) simply consisted of seven levels of the quality of evidence, another (Shaxson, 2005) listed five components of “robustness” (i.e., credibility, generalizability, reliability, objectivity, rootedness), and another (Flay et al., 2005) presented 47 standards for efficacy, effectiveness, and dissemination of programs. Between these in level of detail, Groark and McCall (2008) offered standards of sound program evaluation that could be used to judge the persuasiveness and comprehensiveness of the research literature on a service program. These schemes are very general, but others have provided guidelines for evidence pertinent to programs aimed at specific problems (e.g., Barkham et al., 2001; Elliott & Mihalic, 2004). Unfortunately, these different criteria do not always assign the same overall level of quality to a given program (Elliott & Mihalic, 2004).

Crucial elements of judging a literature on a service program include 1) the persuasiveness of the research that the program *can* produce the intended benefits under ideal and controlled conditions (i.e., “efficacy”), which often depends on the technical details of the research methodology; 2) the extent to which the program *does* produce benefits when implemented in the field under conditions and with target participant groups, staff, and budgets that are similar to those likely to exist when the program is replicated in a community (i.e., “effectiveness”); 3) estimation of effect size (i.e., odds ratios and cost-benefit analyses; see Scott, Mason, & Chapman, 1999) that demonstrate that the program provides sufficient benefits to enough of the people

participating to justify its cost; 4) reasonable judgments identifying (if not evidence supporting) the crucial elements of the program necessary for its effectiveness to guide which aspects are essential and which are more amenable to tailoring to local circumstances, and 5) feasibility (i.e., detailed descriptions of crucial services and procedures, personnel, and budgets necessary to implement the program).

Reaching consensus. Judging the persuasiveness of a research literature requires technical skill, even when one or more of the sets of criteria cited above are employed. Practitioners and policymakers who do not have training in research methodology might seek the advice of specialists; and those specialists should recognize that experts do not always agree with each other (Elliott & Mihalic, 2004), practitioners and policymakers are not interested in the level of detail that they are, and that the desired outcome is a recommendation of whether the evidence justifies replication under the practice, policy, and budget circumstances that exist in that locality.

Sometimes this review of evidence has already been conducted and published. For example, the Campbell Collaboration (C2) is a network of researchers, practitioners, policymakers, and consumers

that produces reviews of the effectiveness of interventions on targeted groups (Cooper, 1998), and the Centers for Disease Control’s (2006) *Guide to Community Preventive Services* reviews evidence on a variety of physical and behavioral health programs and makes recommendations. More specific to children, the What Works Clearinghouse (www.w-w-c.org) and the Division for Research on Exceptional Children (Odem, Brantlinger, Gersten, Horner, Thompson, & Harris, 2005) provide such reviews among others (see Winton, 2006, Appendix).

A similar strategy is the “consensus conference or committee,” often conducted under the auspices of the National Institutes of Health, the National Academy of Sciences, or some other neutral body, in which academic authorities on various aspects of an issue are asked to review the literature in their specific domains and then come together to forge a conclusion that represents the best consensus on the issue at hand. These conferences range from having a highly specific focus (e.g., do immunizations containing small amounts of mercury produce childhood autism?) to much broader concerns (e.g., what is the potential contribution of quality early childhood care and education? Bowman, Donovan, & Burns, 2001; Shonkoff & Phillips, 2000).

Bringing Evidence to Practice and Policy

The so-called “consensus” resulting from these processes is not likely to be a simple “proven” or “not proven”. The policy maker often needs to decide immediately, cannot wait for more research to be done, and must act on the “best available research” at the moment even if it is methodologically limited, fragmentary, and more uncertain than conclusive. Making recommendations for action on the

Programs are unlikely to be replicated faithfully.

basis of limited and uncertain evidence is not a common or comfortable task for most scholars, and the process may need non-research professionals and others to fill in the knowledge gaps and craft recommendations that fit practice, social, fiscal, and political circumstances, standards, and constraints. In short, evidence-based programming and policymaking may need to be more complex and comprehensive than simply replicating “proven programs”, and the processes involved in implementing evidence in local community programming are just as crucial to providing service benefits as having a “proven program” but with much less evidence to support them.

Those who wish to go beyond simple service program replication emphasize three themes, which are not mutually exclusive. The first represents more comprehensive and flexible methods of reviewing the information available and casting it in a form that is more readily usable in creative and flexible ways by practitioners and policymakers. The second theme emphasizes new approaches to translating research into practice and creating and implementing evidence-based services in communities. The third theme proposes broadening the research enterprise itself to be more directly relevant to the information needs of those who desire to create broad systems of services and policies for children and families.

Alternative Approaches to Summarizing Actionable Knowledge

The first theme is represented by two strategies that attempt to broaden and improve the utility of the information brought from existing research and practice beyond the effectiveness of specific, potentially replicable, service programs. These suggestions are based on the premise that simply replicating service programs one by one does not permit much flexibility, adaptability, or innovation nor does it create a uniform system of services in a particular locality.

Characteristics of effective programs. Rather than focus solely on delivering one or more packaged service programs for replication, scholars and practitioners also should provide policymakers with characteristics of successful programs. Some broad-based consensus groups have done this with varying levels of deliberateness and specificity. Specific characteristics would provide guidelines to practitioners on what elements should compose a service program, and policymakers can evaluate the merits of service program proposals for funding against these criteria (e.g., Groark & McCall, 2005; McCall et al., 2004; Nation et al., 2003). Particularly useful, but not commonly available, is a strong theory of change and evidence of how or why a program works that could guide replications and variations of a service.

Characteristics of successful programs might be of two kinds, general and specific. When characteristics of successful programs have been listed for human services pertaining to a variety of different prevention and treatment goals and domains, many of the same characteristics appear on each list. For example, Table 1 presents one integration of several lists of characteristics of successful programs in early childhood care and education (McCall,

Larson, & Ingram, 2003), family support (Layzar, Bernstein, & Price, 2001; Schorr, 2003) and adolescent problem behavior prevention services (Kumpfer & Alvarado, 2003; Nation, Crusto, Wandersman, Kumpfer, Seybolt, Morrissey-Kane, & Davino, 2003). These general characteristics in turn can be supplemented by characteristics that are more specific to the particular type of program and the problems or skills that are its focus.

This approach is not intended to replace reviews of specific program effectiveness, but to supplement them, and it has certain additional advantages. For example, characteristics of successful programs permit flexibility, adaptation, and creativity at the local level. Further, they likely are more stable across changes in evolving research literatures than specific program evaluations.

On the other hand, characteristics of successful programs are rarely studied directly with deliberate experimental manipulation or non-randomized comparisons; even outcomes across studies with versus without a specific characteristic are rarely systematically compared. Instead, these characteristics are often simply those that are commonly represented in the programs that have been shown to be effective. Therefore, they are likely to be confounded with other factors, all may not be necessary for program effectiveness, and little direct evidence likely will exist to document that any single characteristic is necessary or even helps produce successful outcomes. But these characteristics do represent conventional wisdom or “best practices” that have been associated with successful outcomes, and they potentially represent a partial set of criteria for policymakers to use in crafting Requests for Proposals and selecting those to be funded and for practitioners to use as a foundation for program development and as standards of implementation.

Communities of practice. A central concern of practitioners is that the research literature often does not tell them what really matters in practice (e.g., Buysee & Wesley, 2006, Foreword). Consequently, practitioners sometimes form “communities of practice” (Lave & Wenger, 1991), which are composed of people who have a common practice interest who share their perceptions and observations and learn from each other’s everyday experience (Wesley & Buysee, 2006).

The members may or may not communicate information from the research literature, depending on the backgrounds of the people involved. But the simple dissemination of research information is not likely to be sufficient to prescribe what should be done in practice. This is why many definitions of evidence-based practice and policies emphasize that it consists of research evidence *integrated with professional wisdom* (e.g., Buysee & Wesley, 2006), *clinical expertise*, and *professional and client values* (e.g., Sackett, Straus, Richardson, Rosenberg, & Haynes, 2000). Thus, communities of practice should involve both practitioners and researchers, and as such they have the potential to blend research and practice, adapt evidence-based programs to fit specific circumstances, and fill gaps in the research literature with “best practices” based on professional experience.

Pathways Mapping Initiative (PMI). The Pathways

Mapping Initiative (PMI) of the Project on Effective Interventions at Harvard University represents an approach developed by Lizbeth B. Schorr and colleagues (Schorr, 2003) that consists of a systematic attempt to broaden the knowledge brought to the practice and policy tables beyond traditional research evaluations of specific service programs and to integrate it with professional experience and values. Proponents of PMI regard the traditional knowledge currently available to communities as inadequate because it comes in small, isolated, and disjointed pieces; often arrives too late; is derived from a severely limited range of interventions that have been and can be elegantly evaluated; and usually fails to identify what really made the intervention work.

The PMI process attempts to broaden the knowledge base about “what works” by applying reasonable judgments and plausible interpretations to a preponderance of evidence culled from accumulated professional experience as well as program evaluation findings, both coupled with strong theory. Further, PMI’s information is developed, organized, and presented in a way that is intended to help communities themselves to think coherently and systematically across programs and across systemic and disciplinary boundaries to determine 1) the combination of actions

needed to produce a desired outcome, 2) the key ingredients that likely make those actions effective, and 3) the community contexts that will influence effectiveness.

The Mental Mapping Process that PMI uses is similar to the typical “consensus conference” described above, but it extends beyond the research evidence by convening groups of highly knowledgeable, experienced, and diverse individuals including researchers, practitioners, policymakers, and other potential stakeholders. The process asks these individuals, “considering the evidence from research, theory, and experiences you have been exposed to over the years, what could a community most effectively do if it were committed to achieving the specific outcome under consideration (e.g., higher rates of school readiness, improved family economic success)?” Further, the goal is not simply to identify effective programs but to identify the elements of a community-wide system, despite limited relevant research, that will contribute to improved outcomes.

Such a process also concentrates on filling the gaps in knowledge as well as identifying current interventions, services, and supports to forge new connections among them. For example, when participants were asked to focus on improving school readiness for the highest risk children, they did not recommend new programs or replications of

Table 1.
Some General Characteristics of Effective Behavioral Child and Family Intervention Programs

Program Characteristics

Theory based. Program has a conceptual rationale and evidence that the interventions should produce the desired outcomes.

Family focused. Services are aimed directly at all relevant family members and attempt to improve family relationships and dynamics to create a mutually supportive family environment and establish social support for family members from the community.

Appropriate timing. Services are initiated when people need and are receptive to them, typically early in the development of the target behavior or problem.

Sufficient dosage. Services have sufficient duration (i.e., months from start to finish), frequency (i.e., service contacts/hours per month), and density (i.e., percent time engaged in activities known to contribute to positive outcomes); higher dosages may be needed for participants at greater risk.

Accessible. Services are readily and conveniently obtained (e.g., transportation, child care provided), appointments easily arranged, services promptly delivered.

Matched to participants. Services are sensitive and matched to the needs, culture, developmental level, and circumstances of the participants.

Personnel Characteristics

Well educated and trained personnel. Personnel are both generally well-educated and well-trained specifically for their responsibilities.

Good supervision. Front-line personnel are continuously monitored and supervised in a thoroughly supportive (perhaps “reflective”) fashion by competent administrative personnel.

Structural support. Personnel are structurally supported by providing them enough time, small caseloads or few children, space, equipment, consultants, specialized services, and supportive colleagues.

Relationships. Personnel develop relationships with participants that are warm, trusting, empathic, and sincere, and personnel are perceived by participants to be competent, genuine, and concerned.

Participant Characteristics

Engaged. Participants are engaged in the program, attend and participate in services and program activities, persist in the program until graduation, and support family members and friends in their participation.

Social-behavioral risk. Social-behavioral services are most effective with participants at greatest social-behavioral and economic risk, rather than at medical risk.

Based upon reviews of family-based services, early childhood care and education programs, and adolescent problem prevention programs (for more details, see Kumpfer & Alvarado, 2003; Layzar, Goodson, Bernstein, & Price, 2001; McCall, Larson, & Ingram, 2003; Nation, Crusto, Wandersman, Kumpfer, Seybolt, Morrissey-Kane, & Davino, 2003; Schorr, 2003). Reprinted from Groark & McCall (2005) with permission.

specific programs but emphasized the connections that must be built between existing programs and institutions to make it easier to bring needed help to disconnected families. To guard against being swayed by the enthusiasm of one or two individuals or current “fads” in best practices, PMI takes care to ensure a rich mix of backgrounds and outlooks among participants and to develop strong consensus across these diverse perspectives. The first PMI Pathways addressed school readiness, successful transition to young adulthood, and the prevention of child abuse and neglect (www.PathwaysToOutcomes.org).

The most distinctive features of the PMI process are:

Bridge disciplines and go beyond the traditional evaluation literature to identify the specific actions (i.e., services, supports, and other interventions) in many diverse arenas that contribute to achieving specified outcomes;

Identify easy-to-use indicators of progress toward specified outcomes;

Describe the essential elements needed for success with precision and detail, even in the absence of absolute certainty, to increase the likelihood that promising efforts can be taken to scale or transported successfully to new environments;

Identify the elements of community contexts and system infrastructures that support and sustain effective change over time to clarify the limitations of individual programs and illuminate synergy in the work ethics across domains.

As yet, the PMI Pathways process or products have not been empirically evaluated.

Improving Community Capacity to Create and Implement Evidence-Based Practices

Holding consensus groups and PMI workshops that summarize the research evidence and characteristics of successful programs blended with multi-disciplinary knowledge of best practices are examples of summarizing “what works.” But is this sufficient? Not likely. First, there is a dissemination gap—what scholars know to be important and effective (or not) is often not adequately communicated or followed by practice professionals (Ringwalt et al., 2002; Wandersman & Florin, 2003). For example, the DARE programs to prevent drug abuse had been adopted in 80% of the elementary schools in the United States despite their limited effectiveness (e.g., Enett, Tobler, Ringwalt, & Fewling, 1994; General Accounting Office, 2003). Second, while dissemination of information to practice professionals and policymakers could be improved, it is naïve to believe evidence of effectiveness alone will lead to successful programming (e.g., Bero, Grilli, Grimshaw, Harvey, Oxman, & Thomson, 1998; L. A. Green & Seifert, 2005). Other factors, such as local social context, cost, and political issues will play a role, and so will the capacity of agencies and even the larger community to use the information to create and implement new and successful programs.

Therefore, having in hand a well-documented model service program with demonstrated effectiveness is certainly helpful, perhaps even necessary, but probably not sufficient. The process of getting communities to want the program, modifying the demonstrated program to fit local circumstances if necessary, and having the program implemented with reasonable enthusiasm and fidelity by agencies and staff who did not create the original program is equally necessary to “bring the program to scale.” But there is only limited research on the effectiveness of such processes (e.g., Fixsen et al., 2005).

The gap between science and practice resides not only in communication failure, but also the inability of some communities to implement quality programming and make systemic changes. Chinman et al. (2005) identify four

broad factors that contribute to the gap between science and practice and that require community capacities. First, implementing high-quality service programs is a complex process, requiring a significant amount of knowledge and skills that are far beyond following a service practice recipe. Second, systems factors pertaining to coordina-

tion among different agencies and community readiness to adopt and maintain new strategies must be accommodated. Third, having sufficient financial, technical, and personnel resources is a typical challenge. And fourth, local clientele and circumstances may pose unusual difficulties, and there may be little guidance from the research literature.

The typical approaches to building community capacity focus on technology transfer (Backer, David, & Soucy, 1995), which assumes that community capacity is limited by a lack of information that can be remedied by training and technical assistance. Chinman et al. (2005) reviewed the variety of training programs offered in substance abuse prevention, for example, and concluded that, while helpful, they have serious limitations, often pertaining to the appropriateness of the content and barriers to incorporating the information into practice. Similarly, having an intermediate set of professionals provide direct, hands-on technical assistance has a good rationale (Fixsen et al., 2005); but the literature (Chinman et al., 2005) suggests that even no-cost technical assistance may not always be welcomed, some minimum level of community capacity is required to fully utilize technical assistance, and community organizations are better able to implement some types of assistance (e.g., planning, implementation, organizational maintenance) than others (evaluation and data analysis). Further, while studies of technical assistance have demonstrated improved community processes, few studies have examined whether participants have improved outcomes.

The development of community capacity appears to be broader and deeper than these strategies can readily accomplish. The need for community capacity building goes beyond the ability to replicate evidenced-based *programs*; what is needed is a collaborative community *process* (L.

Consensus groups and communities of practice fill in the gaps in evidence.

W. Green, 2001) that will create not just programs but integrated systems, rooted in theory and evidence, and appropriately nested in the financial, political, personnel, clientele, needs, and assets of a particular community. The literature (Chinman et al., 2005) suggests at least four requirements of such a community capacity building process. First, the community must have genuine member *involvement and commitment* to the process of improvement. Second, organizations and the wider community must possess *skills* in a variety of domains (e.g., managing an effective community organization, planning and implementing quality programming, matching program to local circumstances and resources). Third, existing and new *resources* must be identified, acquired, and managed. Finally, there must be a sense of collective efficacy or *power* to be able to guide and manage the skills and resources toward effective outcomes.

One approach to building community capacity is to involve research, practice, funding, policymaking, and opinion leaders in a collaborative planning process, for example one led by an independent moderator that blends elements of strategic planning, needs assessment, logic models, a theory of change (e.g., Chen & Rossi, 1983, 1987), and monitoring and evaluation strategies. Several outlines of the steps (Fixsen et al., 2005) and materials to guide the process are available. For example, a Strengths, Weakness, Opportunities, Threats (SWOT) analysis (Kearns, 1992) provides a structure for general strategic planning. The Kellogg Foundation (1998, 2000) offers a handbook for conducting a generic logic model, Benson (1997) presents an asset-based needs assessment of adolescents, and Catalano and Hawkins (1996) describe risk and protective factors and suggestions for program development for preventing antisocial behavior. Further, the Getting to Outcomes (GTO) process (Wandersman, Imm, Chinman, & Kaftarian, 1999, 2000) has been packaged into a manual published by RAND Corporation (Chinman, Imm, & Wandersman, 2004; available at <http://www.RAND.org/publications/TR/TR101>), the Search Institute (Fisher, Imm, Chinman, & Wandersman, 2006) has blended Getting to Outcomes with adolescent asset and needs assessment, and the Getting to Outcomes approach is available in an interactive web-based technology system called *iGTO* (<http://www.kithost.net/igto/>).

The GTO, for example, represents a structured logic model process that systematically guides an organization or community, perhaps using an independent moderator, through a planning process designed to build community capacity, generate enthusiasm and commitment (i.e., “buy in”) by community stakeholders, and create and implement effective service programs and systems. The GTO process includes ten generic steps, although the examples often pertain to specific goals (e.g., substance abuse prevention, sexual and domestic violence prevention; e.g., Wandersman, 2003):

1. What are the needs and resources available?
2. What are the goals, target population, and desired outcomes?
3. How does the program incorporate scientific evidence and best practice?
4. How does the program fit or complement other programs already in existence?
5. What skills and capacities are needed to implement a quality program?
6. What are the elements of program implementation?
7. How will the quality of program implementation be assessed?
8. How well does the program produce its intended outcomes?
9. How will continuous quality improvement strategies be incorporated?
10. If successful, how will the program be sustained?

Implementing a program in the field often benefits from a program development process, such as a logic model.

Step 3 is the primary juncture for injecting theory of change and research evidence. For example, what is the rationale (theory) and evidence that one versus another action (services, legislation) is likely to produce the desired outcomes, why should those services produce those benefits, and under what circumstances will they be successful?

A thorough planning process, such as SWOT and GTO, perhaps using a collaboration of scholars, evaluators, service professionals, funders, and policymakers, is a good beginning, but the same collaborators are also likely necessary to structure the implementation and oversee the operation of a new service program (Fixsen et al., 2005; Groark & McCall, 2008), and some communities and agencies may need assistance in conducting those processes. Although widely advocated in some circles to achieve the goals mentioned above, little research is available on the effectiveness of these approaches (see Fixsen et al., 2005).

Revising the Research Enterprise and Integrating It with the Community

Capacities should not only be built in the community; the capacity of scholarship to improve the process, services, and community systems also needs improvement.

In many quarters of behavioral science, basic research rooted in theory and aimed at describing general cause-and-effect principles preferably with randomized research designs that emphasize internal validity has dominated the academic value system; in contrast, applied research has sometimes been ignored, if not denigrated (Groark & McCall, 2005; McCall, 1996; McCall & B. L. Green, 2004). Perhaps as a partial result, those seeking to improve community services and systems lament that social and behavioral sciences have not sufficiently studied many of the most crucial questions needed to achieve these broader practical outcomes (e.g., Auspos & Kubisch, 2004; Chinman et al., 2005; Wandersman & Florin, 2003). These different

voices converge on the principle that to improve services, service systems, and policies the research community must accommodate to the needs of practice and policy by broadening its methodological value system and studying more directly topics that are necessary for broad system and policy changes.

The need for a science of practice and implementation. While NIH's emphasis on "translational research"—translating basic research knowledge into practice—is a worthy pursuit, practice fields need a science that studies how service professionals actually practice, how programs are successfully implemented, and the characteristics of service delivery that contribute to participant benefits. Fixsen et al. (2005), in their exhaustive review of implementation research, argue that an evidence-based program will not produce benefits unless it is properly implemented, and we know empirically very little about implementation, leaving practice fields with "the paradox of non-evidence-based implementation of evidence-based programs" (Drake, Gorman, & Turrey, 2002). The inattention to implementation, both scientifically and professionally, may explain why services often do not work, why they do not "travel well" from originating to replicating agencies, and why simply replicating proven programs may not always be a successful science-to-practice strategy. Effective implementation is a complex enterprise, needs serious expertise, requires cumulative knowledge over repeated implementations, and often takes time and several cohorts of participants to perfect. This is why successful implementation of evidence-based programs may be accomplished best by "purveyors"—groups who specialize in the implementation of specific service programs who collaboratively work with agencies to establish that service (Fixsen et al., 2005). At the same time, funding agencies and scholars need to support and study practice and implementation per se to complement research on service programs.

A science of community change. Some scholars argue for an even broader science of community change (e.g., Auspos & Kubisch, 2004; Chinman et al., 2005; Wandersman, 2003; Wandersman & Florin, 2003). They cite four major reasons for advocating a different paradigm for developing and using evidence to influence policy, practice, and community change.

First, *the traditional and valued scientific paradigms are not likely to be applicable.* Because a single entire community may be the object of change, sampling and the classic randomized control trial are not suitable. Anthropology's methods of studying a single culture may be more relevant. Further, many focal intermediate and long-term outcomes (e.g., community support, collaboration among organizations, motivation of clients to participate in programs) are not easily quantified, and demonstrating improvements in an entire community or organizational system represents a much more difficult research task than

showing a specific measurable benefit in a highly-targeted group of 30 individuals. Also, the length of time needed to achieve community change is much longer than the patience and resources of traditional social and behavioral research funders. As a result, community change must be studied with different methods, it requires more time, and it likely will provide answers with less certainty than many traditional scholars and funders have come to expect.

Second, *community-change initiatives place a strong emphasis on community involvement, community choice, and community building.* These community-building activities are not under the researcher's control, must be tailored to local circumstances and adapted over time, and often do not turn out to be the ideal strategy based dispassionately and uncompromisingly on existing research. Consequently, the community (the object of study) must be an active participant in the design of its own program (the independent variable) and evaluation (data collection) to motivate participants, to match the program to the unique characteristics of that community, and to implement the program in the context of the finances, power, and politics of that environment. The GTO and similar approaches described above are aimed at this theme.

Third, *a uniform intervention is not likely to be appropriate when so many facets of a community's system require change.* Communities are often highly decentralized, contain numerous stakeholders and power groups, and have multiple and discrete funding streams each with their own complex sets of rules and regulations, and policies are influenced or set by a diverse array of philanthropists, administrators, and legislators at the local, state, and federal levels. And while they must work within the influence of these constraints, community-building professionals operate largely at the ground level and consist of a diverse set of organizations and groups that are typically small, unaffiliated, and work independently rather than as part of a larger network. Therefore, no single mechanism is likely to easily produce change throughout this diverse field. Thus, the resulting literature will be a collection of case studies from which more general themes must be inferred.

Finally, *actual change in communities in specific services and in community systems is more likely governed by political, ideological, or fiscal priorities than by research findings.* Such changes occur via many different avenues and at the hands of many different people, and the process is a continuing one of lessons learned, feedback, revision, and evolution conducted over a period of time in which many of the major players, including political administrations, may change.

Community science. Some scholars (e.g., Chinman et al., 2005; L. W. Green, 2001; Wandersman, 2003; Wandersman & Florin, 2003) recognize the value of the predominant evaluation model and rigorous research methodology,

Current translational research does not go far enough toward studying implementation and community change.

Commentary

A Case for Replicating Proven Programs

Jean B. Grossman

McCall addresses a very important issue: “How can society best learn from and utilize the findings from research to improve the efficiency of its social programs?” Much of the paper is devoted to examining how researchers and practitioners can work and learn together to foster better alignment between practice and theory. While in one sense this can be thought of as “evidence-based” programming, most policymakers or funders use the term differently. When they refer to “evidence-based programming,” they often mean the replication of proven program models.

McCall addresses replication in the beginning of his paper. He rightly notes that there are unfortunately few such proven programs, and thus argues for more research-based innovation in program practice. The problem is that we cannot assume these innovative new practices are effective—they are promising perhaps, but not proven.

There is definitely a role for social entrepreneurs fostering innovation, but there is also an important role for replication. As Racine states, the nonprofit sector “cannot thrive solely on the celebration of differences, especially not when communities have problems and challenges in common.”¹ There is a widespread belief that that every program must be tailored to individual locales because the people and issues are surely different from community to community. However, people’s taste differ and yet, Starbucks is more or less the same all over the country. The tendency to want to change and adapt programs has undermined many “replication” efforts. Decades of evaluation research has shown that it is actually very difficult to improve the lives of individuals above and beyond what would happen to them without programmatic intervention. Most social programs are built on strong theory, but the devil is in their operational detail as to whether they actual improve lives. Given the paucity of truly proven programs, it is critical that we find ways to effectively reproduce the ones that work.

Let me then expand on an area that McCall only touched on, namely, what it takes to replicate proven programs. All too often new sites modify successful models to make them “better fit” their local context, without identifying the core elements that made the model effective in the first place. This doesn’t mean that replicated programs have to look *exactly* like the “mother” site; in fact, some variation is probably necessary to meet local needs. But it’s important to ascertain which program elements can be changed and which ones are absolutely vital to producing positive results.

Programs that want to expand should spend the time and money necessary to thoughtfully and carefully replicate. The Edna McConnell Clark Foundation, BridgeSpan and Public/Private Ventures have been helping organizations that have strong evidence of success expand for more than a decade, doing so in a very methodical manner. They have learned that replication cannot be done quickly or on the cheap (Racine, 2003, 2004; Roob and Barbach, 2009; Summerville, 2009). A program’s critical elements must be identified. Intentional training, support and communication is needed for several years. Funding strong infrastructure (e.g., the hire of a strong director and having a data system), while not the direct services most donors want to support, is imperative to successful replication and ultimately the delivery of strong services. Finally, continuous monitoring and evaluation is necessary to ensure the program, which will and should change over time, continues to run as it is intended.

Society does need to discover or create new programs in areas where there are no proven ones. However, the bias in the nonprofit world is for everything to be home grown and local. Given how hard it is to engineer change in the natural courses of people’s lives, we should invest more in successfully spreading proven programs than we do now.

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¹ Racine, David (2003) “Dissolving Dualities: The Case for Commonsense Replication.” *Nonprofit and Voluntary Sector Quarterly*, 32 (2), 307-314 p.314.

Commentary

The Silo Problem Mary Ann McCabe

This *Social Policy Report* by Robert McCall, who is an expert at bridging research with practice and policy, raises important and timely issues. His report summarizes the history and current state of the field of *evidence-based practice and programming*. There is widespread and growing support for using research evidence to support investments and to optimize outcomes - across fields as diverse as education, health care, and social services. However, we continue to witness obstacles to effective translation, dissemination, diffusion, adoption and implementation. It is important to maintain a broad definition of "evidence-based practice", similar to that promoted by the Institute of Medicine (2001) which includes the integration of the best available research information with practitioner wisdom and experience, consumer/client culture and values, and available resources. It is equally important to note that we continue to observe an emphasis of evidence-based practice over practice-based evidence or research-tested programs that attend as much as possible to real world settings and constraints.

Clearly, sharing evidence about programs proven to be effective in the research setting, or dissemination, is a necessary first step. In this regard, McCall discusses considerations for judging "evidence", or actionable knowledge, and methods for arriving at consensus. However, he highlights the additional obstacles to community implementation of programs. For example, he identifies the fundamental assumptions underlying replication of research-based programs and practice that are often not met. Among these is identification of the critical components in programs that make them effective which are the most vital aspect to replication outside the research setting. Replication is challenged by the all too frequent "silos" of academia and the community.

Communities are complex systems, as are the organizations and people within them. Therefore, the process for communities to adopting new programs and implement them with fidelity to the model program in research is neither simple nor straightforward. McCall describes approaches to building community capacity in this regard. He also points out that the actual implementation of programs, and program outcomes, need continued study in the community. Again, this requires ongoing collaboration between researchers and those overseeing program implementation - whether practitioners, administrators or policymakers.

This *Social Policy Report* emphasizes the importance of overcoming the "silo problem". It highlights the essential role of collaboration for evidence-based practice and programming—collaboration across researchers, practitioners, intermediaries (also known as knowledge brokers or purveyors), federal, state and local policymakers, funders and opinion leaders. McCall proposes innovative methods for enhancing collaboration among these groups to drive greater success.

However, additional "silos" also need to be overcome and therefore warrant mention. On the continuum of basic research to applied research to translation for practice, there are multiple opportunities for the failure of dissemination, diffusion, transfer or uptake. And yet there are critical pieces of information in these varied types of research for effective practice and community programs. Further, each area of programming for children and families that seeks to employ evidence-based practice—early education, education, mental health, health, health promotion, substance abuse, child welfare, and so on—does not typically learn from another field about implementing research-based programs, or effective knowledge transfer processes. The academic and professional literature, and even social networks, in each field are often quite distinct. Similarly, service delivery systems are also typically very separate, so that lessons learned about implementing research-based programs are not likely to be shared—even within the same community. Finally, funders very often operate independently of each other in their methods for encouraging translation and implementation of programs in communities, such that successes and failures cannot be shared very easily.

The field of developmental science can and should play a leading role in bringing research evidence to practice and policy in order to improve the lives of children and families. We welcome and applaud the interest of policymakers and funders in turning to research evidence to inform their investments, and hope they will continue to recognize the many types of evidence available to inform different aspects of programs and practice. However, it will be imperative for them to invest in studies of program implementation and outcomes. Scientists will be wise to increasingly collaborate with practitioners, program administrators, and policymakers, in order to conduct research that is well informed by real world practice and policies. In addition, they might begin research with an eye on the translation and dissemination process, including key partners for bringing research knowledge to communities. This *Social Policy Report* reminds us that if we keep doing what we have always done, we will get what we have always gotten. The challenge now is to do even better.

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Commentary

“Evidence-Based Programming in the Context of Practice and Policy” Karen A. Blase, Melissa Van Dyke, and Dean Fixsen

“All organizations are designed, intentionally or unwittingly, to achieve precisely the results they get.”
R. Spencer Darling

New paradigms do not fare well in existing systems, and evidence-based service delivery in the real world requires a paradigm shift. We appreciate the author’s thoughtful articulation of the complexities related to implementing evidence-based practices and programs in the real world. Research, practice, capacity building, and policy are critical to moving evidence-based services from boutique operations for a relative few, to widely accessible programs and practices that benefit consumers, communities, and society. An important, overarching theme is that the practice and science of implementation must be utilized and improved in order to make better use of intervention research results. More rigorous research on the “what”—the intervention—will not tell us “how” to implement with fidelity and good outcomes over time and across practitioners in complex settings.

Understanding the contributions and limitations of rigorous intervention research relative to implementation is critical. Information about scientific rigor, effect size, and target population can help communities explore whether a given program or practice meets a need in their community. Scientific rigor is important. Choosing well is important. Implementation is hard work, and we need to be convinced that the effort will be ‘worth it’ in terms of the desired outcomes. However, choosing well will not help the community implement well (Fixsen et al., 2005).

Implementation involves installing the infrastructure and processes needed to initiate and sustain effective services over time and across practitioners. This means communities need to acquire implementation knowledge regarding “how” the practice will come to life in their community. How will organizations need to change to support the new way of work? How should practitioners be selected, trained, and coached? How will fidelity be measured and outcomes monitored? How will the broader system of services, supports, policies, and funding streams need to change to support a new way of work? And, who will do all this work? Some of the considerations and challenges related to the “how” include understanding that:

Changes will need to occur at the practice, organization, and systems levels. Targeting change at the practitioner level alone will not result in sustainable practice improvement.

Policies need to change to enable practices. We need policies to enable practice, and we need practice to inform policy (see www.scalingup.org).

There are significant funding barriers to supporting the implementation of evidence-based practices. Funding and support for the infrastructure to create and sustain practitioner competence is a ‘forever’ cost and is essential to producing consistent outcomes across generations of practitioners and managers. If sustainable training, coaching, mentoring, and data systems are not in place, then the practitioner competence will not be changed, improved, or sustained and benefits will not be realized.

Communities, agencies and funders will need to be disabused of the notion that we choose evidence-based programs and practices because we know they work, and therefore we will not have to fund process (fidelity) and outcome evaluation. Evidence-based programs and practice work in the real world *because* practical program evaluation is used to inform decision-making. Eliminating this piece of the infrastructure is a bit like taking a boat for spin, determining that the rudder and compass work and then tossing them overboard to save time and money (Biglan & Ogden, 2008).

Communities need knowledgeable partners to make full, effective, and sustainable use of evidence-based programs. Purveyors and intermediaries are critical to developing efficient and effective processes to create the necessary conditions for success, and they represent new roles, functions, and structures (Olds, Hill & O’Brien, 2003). While scholars may contribute to understanding these emerging entities and functions, it is not likely that they will participate in directly providing such implementation services and expertise. Universities simply are not designed to support or reward such functions, and the skill sets required do not directly map onto the skill sets of researchers and scholars.

There is growing recognition that there are important implementation research agendas to be pursued related to effective implementation and organizational and systems change. As the author notes, traditional randomized trials are not our sole process for creating new knowledge and, in the case of implementation research, are not likely to be practical, affordable, nor are they likely to tell us what we need to know about the multitude of complex factors that interact across stages of implementation (Panzano, et al., 2004). We have policy, methodological, and theoretical work to do to improve the funding for and the science and practice of implementation.

In the midst of all the complexities and challenges it is important to note that science-based programs and practices are being effectively used across the nation and internationally. While they are not yet fully scaled up, ac-

cess is increasing. Scholars, practice professionals, community members, policy makers, purveyors, and intermediaries need to join in the effort to understand and support best practices for intervention *and* for implementation.

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but emphasize that such an approach is not sufficient for influencing the nature and quality of every-day community-based services, interventions, and systems. A new, or at least substantially revised, discipline of "community science" is needed that will more effectively contribute to bridging research and scholarship on the one hand and community practice and policy on the other.

Community science would have several characteristics:

- A systematic process would plan, implement, and evaluate interventions (e.g., GTO), both their implementation and outcomes, rather than simply replicating packaged interventions;
- Researchers would collaborate with and support practitioners, patients, clients, and community leaders who would have the major responsibility for creating, implementing, and monitoring the interventions as well as being accountable for all the decisions they make;
- An emphasis would be placed on having the technological tools to disseminate science to practitioners and to provide comprehensive technical assistance to practitioners through an intermediate unit to help tailor knowledge and the change process to local circumstances.

The focus of community science would be to add value (i.e., capacity), rather than simply research findings, to the community, and it would study the community change process to guide its improvement. For example, proponents identify certain issues that community science should address that are currently neglected:

- Studies of policies and procedures that facilitate or hinder the adoption and implementation of effective interventions, the transition of demonstration programs to permanent and publicly available services, and the technology of effective information dissemination;
- Studies of the effects of age, gender, ethnicity, and social-cultural factors that affect access to, use of, or acceptance of preventive interventions;
- Studies of the costs associated with the delivery of preventive interventions as well as methods of financing them; and
- Studies of why model demonstration programs created by researchers tend to be less effective when they

become publicly funded and implemented in the community by experienced service professionals in the context of real-world constraints.

Aspen Institute's Roundtable on Community Change. The Aspen Institute's Roundtable (Auspos & Kubisch, 2004) is another voice for comprehensive scholarly and community change. It seeks to develop a more coherent infrastructure for distilling and disseminating conclusions and lessons from recent and current initiatives, applying knowledge from related disciplines and fields, identifying new knowledge needs, and designing next-generation interventions and evaluations. It calls for the creation of a new learning cycle, that, in ideal form, moves away from the focus on single initiatives, integrates information from a full range of sources, organizes the information and assesses its value, takes the most promising elements and turns them into hypotheses, designs interventions that can test those hypotheses in a high-quality way, and evaluates the results. The Roundtable seeks to be a catalyst for change that recognizes and attempts to cope with the realities and requirements of both the nature of the research enterprise and the task of changing community systems.

Conclusions

After decades of largely being ignored and only rarely influential, applied scholars who work to improve human welfare directly are suddenly being looked to by service practitioners, funders, and policymakers who hope that research and scholarship can lead to more effective and financially efficient services and systems of support for high-risk citizens. This new attitude should be viewed by both groups as a rich opportunity, but it also constitutes a mandate to build a bridge over the chasm that has traditionally separated these communities. To do so, all parties must realize that the bridge is complex and must not be oversimplified. Further, to cross the bridge will require new attitudes, methods, and research paradigms as well as a greater understanding of the complexity, cost, and length of time required to create the knowledge, services, and policies that will produce better outcomes and a better society.

Implications

Acknowledging the limited research on many aspects of the process of implementing evidence-based programs and bringing them to scale in communities, the preceding discussion has several implications for consideration by researchers and practice and policy professionals.

Researchers might consider the following:

- Avoid emphasizing outcomes, especially in the first cohort of participants, until the process of implementation is well established—documenting effective program procedures should be the first outcome.
- Describe successful programs in sufficient detail so they can be replicated by others, including by paraprofessionals.
- Attempt studies of the implementation of programs operated by staff similar to those who would operate the program if it were brought to scale, and describe variations in the program to fit local participants, budgets, and political circumstances.
- When evaluating program implementation and outcome, consider a broader range of research designs, evidence of replication and generality, external as well as internal validity, various measures of effect size, and feasibility of replication and bringing the program to scale.
- Collaborate with practice and policy professionals in formulating reviews of evidence that are not only scholarly but frame implications for practice and policy and fill-in the evidentiary blanks with best practices and best attainable solutions.
- To the extent possible, attempt to study the essential characteristics of a program necessary to produce positive outcomes.
- Begin to develop a science of community change that studies implementation by community professionals and how communities change their systems of human services.

Practitioners and policymakers should consider the following integrated national and local approach:

1. A coordinated set of interdisciplinary research, practice, and policy consensus groups, each focused on a major service/policy need, should be created at the national level. They should be composed of scholars, practice professionals, policymakers, funders, and service clients with a strong, task-oriented, neutral leader who is sensitive to the values and unique contributions of each constituency. The charge for each group is to conduct a needs assessment (e.g., extent of the problem or need, prognosis and consequences of inaction, resources and personnel available relative to the need, and current approaches and effectiveness), a review of the evidence of promising approaches, and recommended guidelines for effective services or interventions and appropriate measures. The latter should be composed by the entire interdisciplinary group using a structured process consisting of the most promising elements of consensus groups, GTO and logic models, and Pathways, for example. The product should incorporate the research evidence, identify limits and gaps in the research literature, fill those gaps with best-evidence-available

practices and consensus wisdom, identify characteristics of effective programs, and provide guidelines for one or more service options for potential implementation at federal, state, and/or local levels. The specificity of the guidelines will vary with the research basis and the documented or estimated need for variations to fit different client groups and local social and political circumstances. The guidelines should also contain principles for implementation and program process and outcome evaluation that blend the need for common design features and measures with local variations as well as a template for an application for funding to be made by communities and agencies to government or private funders that helps to structure locally proposed services to fit the guidelines. These consensus groups should be revisited periodically because social needs, research, and practice principles change over time.

2. Government and private funders should use the guidelines and application template in combination with making technical assistance available to communities and agencies to conduct local planning and program development activities. The latter should involve the same interdisciplinary mix of people and the same general logic model process as the consensus groups described above, except the guidelines would provide an evidence-based outline as the basis for possible services and activities to be adapted to local needs. Further, technical assistance should be made available if needed by independent and experienced “purveyors” to lead the local planning group, design the local evaluation, and meet the criteria of the funding application.

3. Applications for funding could be judged in part on the extent to which the local proposals fit the guidelines on the one hand and propose reasonable and appropriate local variations on the other.

4. Once funding is awarded, additional technical assistance, perhaps by having experienced purveyors collaborate with local agencies, should be made available as needed to assist agencies and communities to implement their proposed project (e.g., see Fixsen et al., 2005; McCall, Green, Strauss, & Groark, 1997; Groark & McCall, 2008) and especially to conduct the evaluation.

5. Scholars and practice professionals, especially those engaged in the above processes, must vigorously encourage the development of the research base for such an enterprise, including feeding the results of the evaluations of these programs back into the ongoing consensus groups, identifying and studying gaps in the service research, encouraging a broad range of methodologies, developing a science that studies implementation and practice per se, broadening the academic value system to more fully embrace quality work in this domain, and studying this process in the manner advocated by the community science proponents (e.g., Auspos & Kubish, 2004; Chinman et al., 2005; Wandersman, 2003; Wandersman & Florin, 2003).

Footnote

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Content

The *Report* provides a forum for scholarly reviews and discussions of developmental research and its implications for policies affecting children. The Society recognizes that few policy issues are noncontroversial, that authors may well have a "point of view," but the *Report* is not intended to be a vehicle for authors to advocate particular positions on issues. Presentations should be balanced, accurate, and inclusive. The publication nonetheless includes the disclaimer that the views expressed do not necessarily reflect those of the Society or the editors.

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