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Context Matters in Child and Family Policy

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Abstract

The traditional model of translation from basic laboratory science to efficacy trials to effectiveness trials to community dissemination has flaws that arise from false assumptions that context changes little or matters little. One of the most important findings in developmental science is that context matters, but this fact is not sufficiently taken into account in many translation efforts. Studies reported in this special issue highlight both the potential of systematic interventions in parenting, peer relations, and social-cognitive skills training, and the problems that will be encountered in trying to bring these interventions to a community context. It is advocated that developmental scientists start from within the community context itself so that translation to policy is only a small step. It is also advocated that this research be conducted through rigorous community randomized controlled trials.

Kudos to the editors! In crafting this special issue, they have implemented a process that did not merely attract existing developmental science, it also pushed scholars to think in new ways and to stretch their science. The articles in this special issue represent the best of contemporary translation science in child development, and they set new standards for rigor and ecological validity in the field. At the same time, crucial questions must still be addressed before the translation to practice and policy is complete. I conclude that the field must adopt a very different approach from the traditional linear, basic-research-to-policy path; instead, we must recognize that contextual differences between the laboratory and the policy setting are so great that we need to embed our research within the community policy context itself.

On paper, the path followed in the translation science of child development is straight and logical. It has been adopted from the NIH Roadmap (Zerhouni, 2006), articulated in models of prevention of mental disorders (Coie et al., 1992; Mrazek & Haggerty, 1994), and is well-represented in several of the articles in this special issue. The goal of the field of translational developmental science is to use “basic science” to cause population-level impact on raising healthy children, that is, to improve the overall rates of healthy outcomes and to decrease the overall rates of problem outcomes for the population of children in society. This is the standard to which citizens and policy makers, who fund the science, can and should hold the field accountable. So, is there any movement in population measures across secular trends?

Unfortunately, the field has not yet demonstrated success in the goal of population impact. Secular trends are not encouraging. Collishaw, Maughan, Goodman, and Pickles (2004) have reported that rates of adolescent mental health problems, especially conduct disorder and depression, have actually *increased* across time in western societies. Twenge et al. (2010) has reported that indicators of young adult psychopathology reveal a large, one standard deviation *increase* in most problem scores across the past 70 years. A secondary

goal of translational science is to reduce the gaps between the rich and poor, between the haves and the have-nots, in important outcomes for children. Has the gap been reduced? Costello (in press) argues that the gap has not been reduced across time and newer technologies and interventions might actually inadvertently increase the gap.

Although numerous developmental-science-based interventions, including some reported in this volume, have been found to have favorable impact on the narrow group of children that have been targeted for these interventions, it is a long step to go from those findings to population impact, and few of these interventions have demonstrated a positive effect on the entire population of children in a community, as indicated by population-level effects. Herrera, Kauh, and McMaken (this volume) report that a well-known, evidence-based intervention, Big Brothers Big Sisters School-Based Mentoring, has no positive effects after one-and-a-half years when implemented on a large scale. What is wrong here? I conclude that we need a fundamental shift in how translation research is conducted in order for the goal of population impact to be achieved.

The translation model on which most research has been based begins with identification of a human behavioral problem through empirical documentation and epidemiology. The model then brings the phenomenon out of the community and into the laboratory so that “basic science” can reveal the “truths” of human behavior. These truths document human problems, the developmental processes that lead to them, and the way out of them. The third step is to translate these truths into intervention strategies that are evaluated experimentally through rigorous trials. The translation includes identifying populations, ages, and targeted social and intrapersonal processes in development that mediate ecologically important outcomes for children. This step also involves engineering of human development and the art and genius of intervention. The emphasis at this step is on rigorous evaluation of efficacy of the intervention to achieve its stated goals, at the admitted cost of ecological validity. The pristine conditions of an ideal efficacy trial (and the goal in these trials is indeed to be as pristine as possible) rarely match the community context of the problem being addressed. In fact, the goal of an efficacy trial is to test whether behavioral change *can* be engineered, not whether the population is actually changed. They are more a test of developmental theory than an application to practice and policy. Indeed, I argue that the contribution of these studies is more to developmental science than to policy, because the context differences between an efficacy trial and community-based intervention policy are so great that the translation is minimal. The fourth step of translation models moves closer to community impact through effectiveness trials, in which an intervention that has undergone efficacy trials is implemented and tested in real-world conditions. A trade-off is acknowledged that sacrifices scientific rigor for community embeddedness. Finally, the last step is a dissemination effort. Here, a randomized experiment is rarely attempted, and, instead, the focus is on whether the intervention can be implemented with fidelity, sometimes at scale. Models of translation also suggest that the path should be two-way, that is, when the outcomes of intervention require theories of human development to be modified or highlight areas where future truths should be unearthed by basic science.

This translation model has been an apparent success according to the scientists, as demonstrated by the many articles in this special issue. But it has fallen short of leading to population-level change in raising healthy children for the citizenry, as Shonkoff and Bales (this volume) note. I argue that three errors are made in the assumptions in the translation model that reduce its impact on population outcomes. The first false assumption is that findings readily generalize across contexts, particularly from the laboratory context to the community context. They do not. The lack of generalization should not come as a surprise, given that one of the most important findings from basic science is that context matters, and many studies of children occur in university community and IRB-research contexts that

differ from the contexts of policy and community-level intervention. A second false assumption is that the conclusions of laboratory studies are free from context factors that would constrain the conclusions made by the scientist. Every behavior is embedded in a context. Finally, it is falsely assumed that the rigors that are required for randomized experiments must be sacrificed in research in a community context. This last assumption leads to relatively little high-quality research on community processes and population outcomes for children.

I argue that a paradigm shift is necessary in realizing that all behavior is embedded in a context. Behavior is to context as figure is to ground. No one context is more or less “real” than any other context, but some contexts are more ecologically important than others. “Pulling” a phenomenon out of its natural context into the laboratory context for purer scrutiny runs the risk of losing the phenomenon altogether because it gets divorced from the contextual circumstances that define the phenomenon. The common framing of translation models assumes that there must be a trade-off between scientific rigor and real-world conditions. “Basic” science brings a phenomenon into the laboratory in order to achieve the greatest rigor possible in the least ecologically-valid conditions possible. The farther one moves down the translation path, the less pristinely rigorous is the science being conducted, but the presumed gain is an increased focus on the community context. Translation science supposedly moves from the rigor and purity of basic science to the messy reality of the community context.

The problem with this model is that it just doesn’t work. When a phenomenon is taken out of its context, it becomes a different phenomenon. Translation often fails. A related problem is that the carefully constructed, basic-science-based, intervention programs and efficacy studies that establish the potential of these interventions have rarely, if ever, led to population-level changes when implemented at larger scale in the context of a community.

I will illustrate reasons for this gap in translation below, describe ways in which the studies in this volume set a new standard for rigor for research within community settings (as well as ways in which some studies still fall short), and then offer a different model for future inquiry, in which rigorous science is conducted within the contexts that have the most meaning.

Some of the Successes of Translation Science... and the Challenges that Remain

Context Matters in How Children Develop

The studies in this special issue highlight the important but complicated roles that various contexts play in children’s development. Fiese, Winter, and Botti (this volume) peek into family homes to observe that the family mealtime context is related to a child’s asthma symptom severity. The findings are sensible. Garber et al. (this volume) find that a parent’s changing level of depressive symptoms predict a child’s level of depressive symptoms, a family context effect that is likely mediated by specific parenting behaviors.

Guerra, Williams, and Sadeck (this volume) find that a negative school context (e.g., nonsupport for social relationships, norms for high aggression) predicts a child’s bullying behaviors. Farrell et al. (this volume) also find that middle school classroom norms for aggression and a child’s network of associations with deviant peers are contexts associated with a child’s level of physical aggression but that the home context of parental support for nonaggression protects a child from these harmful school context effects. One context offsets another context. Similarly, Watamura et al. (this volume) examine the NICHD Study of Early Child Care and Youth Development (SECCYD) to find that growing up in a context

of poor quality home environment during the preschool years predicts a child's poor social-emotional adjustment, but that a context of high-quality out-of-home child care can partially offset these adverse effects. Richert's (this volume) review suggests that yet another context in the early years, a child's exposure to screen media, has strong impact on the child's social-cognitive development. Schofield et al. (this volume) use a multigenerational data set to conclude that the relation between the context of low socioeconomic status and adolescent emotional instability is probably reciprocal: low status influences adverse adolescent outcomes but also is selected by individuals with those patterns.

One of developmental science's most important contributions has been to articulate how context makes a difference and renders findings from one context invalid in other contexts; ironically, we still need to learn that intervention research conducted in the context of efficacy trials, with volunteer participants, limited samples, and ideal intervention fidelity, does not readily generalize to intervention as implemented in community settings. Of course, when similarities remain, translation holds, but we know relatively little about the parameters that define when the translation will or will not hold. Perhaps this question can engage the next generation of research in translation science.

Context Matters in Translating Descriptive Research to Policy Formulation

Several studies in this special issue attempt to bring findings about children's context effects to bear on issues in intervention and policy reform. These studies suggest that interventions and policies might improve children's healthy development, but not before additional questions are answered, lest policy change is misinformed. For example, Morrissey et al. (this volume) address the important problem of child obesity. Mining the rich data base of the NICHD Study of Early Child Care and Youth Development, they find that each time a mother changes employment her child's body mass index increases about one tenth of a standard deviation, or one pound every five months, beyond normal growth. This is a startling finding that sparks additional questions before the intervention becomes obvious. They found no mediators of this relation, and so it remains plausible that a third variable accounts for an otherwise coincidental relation with no causal import. But if maternal employment change is a causal factor, surely maternal employment per se is not responsible. There must be a maternal behavior that is associated with changes in the maternal employment context that leads a child's eating and sleeping patterns to be disrupted, perhaps lack of parental supervision. What is the policy implication? No one would rationally suggest that when mothers lose a job they should not look for another one. And we do not need these findings to suggest that mothers should be allowed to keep their jobs. The findings at this point would seem to have greater importance for an understanding of how obesity develops in children (perhaps with a contribution from maternal work context) than for obvious policy. The major point, again, is that children's context matters.

Monahan, Lee, and Steinberg (this volume) examine the context of adolescent part-time work. By controlling for selection effects through propensity score matching, they find fairly convincing evidence that working more than 20 hours a week while attending high school is associated with increases in substance use and delinquency. The policy formulation is more direct in this case: perhaps restricting adolescent employment to 20 hours a week or less would improve adolescent outcomes. This policy reform should not be implemented at scale on the sole basis of the correlational evidence, but the evidence suggests a hypothesis that can be tested by manipulating policy. It makes great sense to implement a policy change to test this idea, and to do so in some schools but not others, selected by random assignment. Furthermore, both of these studies are contextualized in a 1990s, western-society economy, and it is not clear how broader economic context changes would alter the individual-level patterns described by the studies.

Context Matters in Randomized Controlled Trials in Raising Healthy Children

This special issue will be most cited for its numerous well-conducted randomized controlled trials that test hypotheses that had been raised by the findings of descriptive developmental science. These studies make two important contributions. First, of course, they test the potential of systematic intervention on children's healthy development. Based on developmental science, the interventions are mostly directed toward one of three processes that seem pivotal in shaping children's healthy development: parenting, peer context, and intrapersonal processes of social cognition. Second, they provide a test of developmental theory itself by the most rigorous method available: random assignment. In general, the studies in this volume lead to the conclusion that there is promise in some of these interventions, but impact on the population of children has not yet been demonstrated. The impact on developmental theory seems more substantial.

Main effects of parenting interventions—Several studies evaluate the potential of systematic intervention with parents to exert a positive effect on child development. The most straightforward hypothesis from descriptive developmental science is that intervention to improve the quality of developmentally-appropriate parenting skills will improve child outcomes. Thomas and Zimmer-Gembeck (this volume) test the hypothesis that Parent-Child Interaction Therapy (PCIT), which is directed toward improving the ways that parents communicate and relate to their children, could indeed improve those parenting behaviors for families who had been referred by child protective service agencies due to suspected maltreatment. The results of their randomized controlled trial are encouraging. Lowell et al. (this volume) hypothesizes that some urban mothers and young children (aged 6-36 months) are at risk because of disruptions by family and community context factors on parenting behaviors and that change in parenting could alter child development. They test this hypothesis by randomly assigning families to a novel home-based, psychotherapeutic intervention that alters parent-child interaction. Indeed, they find that assignment to this intervention improves child language and conduct problems. This is indeed promising.

Main effects of peer context interventions—Karna et al. (this volume) implement a school-based intervention to alter peers' attitudes about social relationships, and their randomized controlled trial yields positive effects on subsequent bullying behavior. This again is a promising finding that affirms the developmental science.

Main effects of children's social-cognitive skills training—Several studies evaluate the third main hypothesis emanating from developmental findings, that intervention aimed to improve children's processes in self-regulation and social cognition could yield positive effects on other child outcomes because these intrapersonal processes organize and mediate development. Raver et al. (this volume) randomly assigns Head Start children to receive the Chicago School Readiness Project, which targets self-regulation. They find positive main effects on indicators of academic readiness.

Positive Effects, but Only for Some Sub-Groups

Unfortunately, many studies reveal that intervention has a positive effect only on subgroups of participants, even though current developmental theory would suggest that the effects should be more pervasive. These findings suggest either that the intervention effects are not robust or (more likely) that developmental theory itself must be enriched to accommodate group-specific processes.

Efficacious, but only for those who choose to participate—Stormshak et al. (this volume) took parenting intervention a step further by approaching families of middle school children to participate in the brief Family Check-Up, which has been found in hugely-

important past studies to improve parenting skills with younger children. Families that are randomly assigned to the intervention condition *and* elect to engage in the intervention are contrasted with a matched group of families within the control condition and are found to have better child outcomes, including lower rates of antisocial behavior and substance use. This finding supports the efficacy of the parent intervention, but it is constrained by an important qualification: preventive intervention is efficacious only for the subgroup that elects to be receptive to invitations to participate. In this study, only 42 percent of those families that are invited to receive intervention actually elect to receive it. So if the goal is to reduce community-wide rates of child problems, this intervention approach has not yet proven effective. These two findings, efficacy for those who elect to receive it but low uptake rates, are tremendously important. It may be sufficient for developmental theory to show that receiving parenting intervention leads to child outcomes, but public policy requires that population impact is achieved through high uptake rates. Future interventions might simultaneously target *both* the penetration of a program to reach all families *and* the impact of that reach on family outcomes. This is the biggest limitation on the field of efficacy trials: they reach only a limited segment of the population and do not necessarily generalize to the full population or to the context of full implementation.

Efficacious, but mostly for the highest-risk subgroup—Another important question is whether preventive interventions are equally efficacious for children of varying levels of risk. Developmental theory is equivocal in leading to hypotheses about whether the highest-risk group of children is more amenable to intervention than is the moderate-risk group. It is plausible that the underlying causes of problem outcomes are more deeply rooted in genetic and enduring environmental factors for the highest-risk group than for a moderate-risk group, suggesting that this group would be less susceptible to intervention effects. In contrast, Belsky and Pluess' (2009) susceptibility hypothesis asserts that children who are at highest risk for bad outcomes due to environmental causes may also be the most susceptible to positive effects of environmental intervention. The randomized controlled trial by the Conduct Problems Prevention Research Group (this volume) reveals that it is the highest-risk group of early starters in conduct problems who benefit most from this multi-faceted ten-year intervention. Similarly, Reynolds et al. (this volume) finds that higher-risk families benefit more in the long run than do lower-risk families from the Child-Parent Center intervention.

Efficacious, but only for one gender—Some interventions yield positive effects on child development but only for one gender group, and the pattern is inconsistent. The Child-Parent Center intervention reported above (Reynolds et al., this volume) has more positive long-term effects on boys than girls. The Early Head Start intervention reported by Ayoub et al. (this volume) has positive effects on language development at age 24 months for girls but not boys. The New Hope anti-poverty program (McLoyd et al., this volume) to increase parental employment has positive effects on future orientation and employment for boys but not girls. As with other moderators, it is not clear whether these effects indicate non-robust intervention effects, or reflect unknown processes about how a gender-relevant context alters the impact of intervention on development.

How Intervention Effects Inform Developmental Theory

Some of the most exciting findings reported in this volume address the psychosocial mechanisms through which intervention exerts effects. Two processes are most heavily targeted: parenting and a child's social-cognitive skills. Descriptive findings from developmental science have pointed toward the crucial role played by children's intrapersonal processes, including an array of self-regulation, social-cognitive, and social information-processing patterns, in mediating behavioral outcomes. These processes have

been the target of many intervention programs, as systematically reviewed by Durlak et al. (this volume). Their wonderful meta-analysis reveals that these processes can be improved through social and emotional learning interventions, and that these improvements, in turn, improve children's behavioral and academic outcomes.

Several studies reported here test the mediating hypothesis directly. Raver et al. (this volume) finds that intervention improves preschool children's self-regulation, attention, and executive function skills, and this improvement, in turn, partially accounts for the positive impact of intervention on children's academic readiness. Velez et al. (this volume) find that among families experiencing divorce, random assignment to intervention improves parent-child relationship quality, which, in turn, improves children's coping efficacy.

Thus, the general pattern across studies is that intervention targeted toward parenting behaviors or a child's social-cognitive processes can be successful in changing those processes and that these changes mediate better behavioral outcomes for children. These findings provide some of the strongest evidence yet to support developmental models of how context influences parenting and children's social-cognitive processes, which mediate important child outcomes.

Gap between Successful Efficacy Effects and Lack of Population-Level Change

One of the most successful translations of descriptive research in child development has been the creation of parent-training programs. Since the seminal work of Patterson, Reid, and Dishion (1992), efficacy trials have tested the success of interventions that target parenting practices, especially to enhance the use of contingent praise and to decrease coercive harsh discipline, in order to have an indirect effect on reducing child conduct problems. The study by Brotman et al. (this volume) follows this tradition and extends the literature by arguing the need for universal access to parent-training programs, especially for urban Black and Latino families. It seems that their ultimate goal is population-level change. They show that a new program called *ParentCorps* can creatively adapt concepts from social learning theory into a 13-session parent-group and child-group program that is culturally accessible for urban Black and Latino families and universally offered to all families in pre-kindergarten programs in eight public schools. The authors use a rigorous randomized controlled trial to find significant positive impact of random assignment to the program on both parenting practices and child behavior problems. So would this program be likely to have impact on the population of children at these schools?

The results indicate that such an effect is doubtful. Of the population of 554 children enrolled in Pre-K in these eight schools, only 171 (31% of the population) eventually participated in the study. About a quarter were ineligible because the parent did not speak English. Others declined to participate, and still others attrited before the study began. So, even though the authors' careful analyses indicate that treated families perform better than control families, as a program that was designed to improve parenting practices and child outcomes *for the population*, the net impact is probably very low. The authors did not perform an analysis of whether the population of students in the four schools that had been randomly assigned to the intervention program have better overall scores than the population in the control schools. This population-level analysis would provide a better test of the actual impact that a policy to implement this program would likely have on community outcomes.

This account of the Brotman study is not an indictment of the study, the science, or the authors. In fact, this study is highlighted because it represents outstanding science within the translation tradition. I am truly admiring of Laurie Brotman. Rather, it is a general problem that intervention programs that are developed in order to have population impact may not

have that impact, and evaluators rarely even test whether the impact holds at the population level.

The problems that minimize population impact are many. First, most prevention programs are voluntary, and getting agreement to participate is a challenge. Scientists often skirt this issue by first requiring consent and then randomly assigning participants to treatment. This approach is consistent with ethics boards that frown upon studying non-consenting families. It also allows for rigorous comparisons in the tradition of “intent-to-treat” evaluation design. But this practice comes at a huge cost of lessening ecological validity because the sample being studied might not represent the population about which policy makers care: the entire community. The problem is more than a research nuisance: there is no empirical support that if a voluntary parenting program were to be offered to the entire community, the participation rate would be any higher. A second problem is that the exclusion criteria for participation in interventions often keep out families that are in high need. In many cases, families must speak English, or they must agree *not* to move out of the school district or neighborhood (or if they do, they get excluded from data analysis as missing values).

Scientists should contemplate creative evaluation designs that include the entire population, not only those individuals who consent to provide identified, individual-level data. Entire communities can be randomly assigned to an intervention policy, although this design often requires high funding levels. Administrative files can be used to generate outcome data for populations of children in a way that does not require individual informed consent, such as when the average academic test score for a school is used as the outcome variable in intervention research. Another possibility, currently being used in a study of child maltreatment prevention (Dodge, unpublished), is to randomly assign children to intervention or not based on an arbitrary publicly-available variable, such as even versus odd date of birth, and then to evaluate outcomes for these two groups at the aggregate level using administrative data. In this case, we are using child maltreatment report rates, emergency department visit rates, and pediatric visit rates, which are coded separately for even versus odd birth date groups but remain anonymous at the individual level. Such a design holds the intervention accountable for achieving impact on the entire population to which the intervention would be directed if it were to become public policy.

The challenges of attempting population impact are illustrated well in contemplating the scaling up of one of the most successful early intervention movements of all time: home-visiting for mothers of newborns. The premier program in this group, the Nurse Family Partnership (NFP) by Olds et al. (1998), has been successful not only in its efficacy trials but also as a model of how social science can shape public policy (Haskins, Paxson, & Brooks-Gunn, 2009). Home-visiting programs for new mothers have been targeted for unprecedented funding levels in the Obama health care legislation that passed in 2010 and have been supported by at least 40 states (Johnson, 2009) on the grounds that they will prevent child maltreatment, and so issues of scaling up are current and real. The NFP program is directed toward low-income, high-risk, primiparous mothers because of the epidemiological research indicating that this group is at high risk. The intervention is based in attachment theory, well known to developmental scientists (Olds et al., 2009). It has been shown in a small randomized trial in Elmira, New York, to prevent officially-reported child abuse among the subgroup that is low-income, unmarried, teenage, and not in a domestically violent relationship. Randomized trials in Memphis and Denver failed to replicate the significant impact on child abuse but did yield positive effects on other aspects of parenting.

So what is the likely impact on the community rate of child maltreatment if home-visiting programs such as the NFP are funded at scale in a community? We do not yet know because no rigorous study of at-scale implementation has been published, but several factors suggest

that the effect might be minimal. First, the NFP is offered only to a small group of women: first-time, low-income mothers who can be identified before the end of the second trimester. This restriction excludes the 62 percent of all births that are later-born and even more who are not low-income or cannot be reached mid-pregnancy. Although the targeted women are a high-risk group, other high-risk groups are excluded, such as those that cannot be identified until after the second trimester, infants in foster care, low-birth-weight infants, and infants born to mothers who had experienced maltreatment as children. Second, the empirical evidence for NFP is based on randomized trials that were conducted with women who voluntarily consent to participate in a research study that they know will last at least 27 months. Studies of volunteer programs for mothers are known to have difficulty in recruiting and sustaining mothers who have been involved in child welfare services, are substance-using, or are mobile (Daro, McCurdy, Falconnier, & Stojanovic, 2003). It is plausible that implementation with a larger community of women in a non-research context would attract more women, but they might respond differently to the intervention than did the mothers in the randomized trials. At the same time, full penetration is unlikely. In any case, it is not clear that the samples of women in the randomized trials represent the population of women who would receive the program in a community setting.

Third, when implemented publicly at scale, it is plausible that a program that targets only high-risk families may suffer from stigmatization of its participants, yielding a perverse self-fulfilling prophecy of risk (Daro & Dodge, in press). Fourth, the proportion of women who start and then actually complete a home-visiting program has been found to be only one third (Daro et al., 2003). Fifth, yet another challenge is that the fidelity of intervention programs often declines when the program is implemented in the community, reducing its effectiveness (Durlak et al., this volume).

Finally, when a program is implemented at scale, the community context changes in unanticipated ways. For example, because one stated goal of home-visiting programs is to refer mothers to community services such as substance-abuse and mental health treatment (Olds, Sadler, & Kitzman, 2007), the strain on the community when a program is implemented at scale might become overwhelming. When implemented in small numbers, such as in a randomized trial, the nurses gain a competitive advantage by accessing whatever community resources are available. Implementing this program at scale without anticipating service changes that may be necessary at the community level is likely to yield unanticipated effects, such as long waiting lists for precious services.

The point here is that the context in which a dissemination effort is conducted is likely to differ from the context in which the efficacy trial on which it is based was conducted. The act of implementing a program at scale can change the context. Efficacious programs sometimes become ineffective when implemented at scale because the conditions under which they were tested simply cannot be sustained when implemented at scale.

This is the outcome that occurred when an intervention to reduce classroom size for early elementary school students was taken to scale as statewide public policy. “Basic” research had indicated that young children achieve at higher levels when they are taught in smaller (16-18) classes than larger (24-28) classes. Furthermore, an efficacy trial had supported the positive impact of small class size. The Tennessee Student/Teacher Achievement Ratio (STAR) Project was a randomized experiment with 11,600 young children who were assigned to either regular-size or smaller-size classrooms. Results indicated that children assigned to smaller classes performed better on academic scores in elementary school (Folger, 1992) and had higher high school graduation and college enrollment rates (Pate-Bain et al., 1997). In 1996, the State of California began to implement a statewide at-scale implementation of smaller class size. Unfortunately, Bohrnstedt and Stecher (1999) found

that in the first year of statewide implementation, the certification status and experience level of the average teacher in California schools decreased markedly, because there were not enough qualified teachers to fill all the new classrooms. The problem with poorly-credentialed teachers was even worse in schools serving low-income neighborhoods because the higher-quality teachers had a market advantage to dictate where they wanted to teach. As a consequence, the positive benefits of smaller class size were offset by negative effects of being taught by a less-credentialed teacher, and achievement gaps across ethnic and socioeconomic groups grew rather than diminished. The lesson learned is that “translating” basic science and the results of efficacy trials to community scale may yield unanticipated outcomes because of false assumptions that the context will remain the same.

Making the Case for Dissemination and Policy Efforts

Finally, even if population-level impact of an intervention can be demonstrated, there is the question of whether the public will adopt the intervention as public policy. Shonkoff and Bales (this volume) tells the story of successes and failures in trying to have an impact on public policy through different ways of framing issues in early childhood development. This is a very important task, and Shonkoff and Bales are doing as credible a job as has been attempted. But even this task can and should be subjected to rigorous scientific evaluation. Framing of issues can be assigned randomly to different communities in order to evaluate impact on outcomes such as public endorsement and funding levels. Doing so would surely lead to important findings and probably to some surprises.

Conclusion

The studies reported in this special issue raise the bar for research in translation science in child development. They tell us that the path of translation of a “basic science” finding or an efficacy-trial finding to community impact is a precarious one. They also tell us that successful translation is not likely to come through replication of the very same intervention program applied at scale without consideration of changes that must account for the differences in context. Finally, they tell us that community population impact is not likely going to occur by following manuals from efficacy-trial based programs. Rather, successful population-level impact is more likely to occur by creating community contexts that support important principles in child development, such as consistent positive parenting, supportive peer influences, and child social-cognitive skills. This last idea remains as a hypothesis for the next generation of research in this field.

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