



**INTRODUCTION**

**IMPLEMENTATION  
RESEARCH IN EARLY CARE  
AND EDUCATION**

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The past 50 years have brought exceptional gains in federal, state, and local funding for early care and education in the United States. In turn, the field is working hard to make good on the evidence-based promise that quality early childhood education (ECE) can create better child and adult outcomes, particularly for underserved children. In the long run, however, if the field cannot answer implementation scale-up questions related to the specifics of how and when ECE is effective, continued support and increased investment for ECE is potentially at risk.

As the number of publicly funded ECE programs increases, policymakers will need empirical evidence to justify the taxpayer investment. Such justification will require a stronger understanding of the essential components of an ECE program's design, as well as solid evidence on which components, or constellations of components, are most effective in achieving strong outcomes for specific subgroups of children. Expectations for child outcomes must be based on the realities of the program components, the target populations, and the financial and human resources that support program implementation. We need more robust quantitative and qualitative data to ensure stronger outcomes for all young children and significantly narrow the opportunity and achievement gap for minoritized children and those living in poverty. Believing in magic will not produce strong outcomes (Brooks-Gunn, 2003). Overpromising and underdelivering will have catastrophic results for the children and families who might benefit most from ECE initiatives.

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Our standard strategy for assessing program effectiveness has been the randomized controlled trial (RCT). Such trials randomly assign some children to a group that receives a defined treatment and others to a group that does not. Assuming that all things are equal, post-treatment differences between the two groups can be attributed to the treatment's impact. This methodology, which we believe allows us to make causal inferences, provided the early evidence of ECE programs' potential in the landmark Perry Preschool and Abecedarian studies (Heckman, Moon, Pinto, Savelyev, & Yavitz, 2010; Schweinhart et al., 2005; Campbell, Pungello, Miller-Johnson, Burchinal, & Ramey, 2001; Ramey et al., 2000), and it is still considered to be the gold standard.

But this volume asserts that RCTs could be greatly enhanced by the findings from rigorous empirical data that provide contextual information about the participants, the settings, and the overall conditions under which the treatment is conducted. Throughout this volume, this type of analysis is referred to broadly as implementation research. However, our intent is not to provide a single definition of implementation research. Rather, we hope to initiate a conversation that is centered on what else needs to be explored about how ECE programs are operationalized and what shape the research might take. We hope the range of perspectives about implementation research that our chapter authors bring will serve to enrich the discussion.

As with research design, ECE programs do not follow a single model, and mean comparisons between control and treatment groups may not capture important nuances of variation in program delivery, educator skill, dosage, and so forth. For example, an RCT of the Tennessee Voluntary Prekindergarten (TN-VPK) program conducted by researchers from Vanderbilt University (Lipsey, Farran, & Hofer, 2015; Lipsey, Farran, & Durkin, 2018) highlighted that fully understanding program implementation and evaluation is a complex task.

The TN-VPK, a full-day prekindergarten program for 4-year-old children who will enter kindergarten the following school year, was evaluated using an RCT. At the end of preschool, TN-VPK attendees had significantly higher achievement scores than children who did not attend the program. But this advantage disappeared by the end of kindergarten. While the largest effects were seen among English learners regardless of their mothers' education status, by second grade, the average score of the TN-VPK treatment group was *lower* on most measures than the average score of the control group (Lipsey, Farran, & Durkin, 2018; Lipsey, Farran, & Hofer, 2015). Understandably, these surprising and disturbing findings elicited a range of interpretations, from claims of methodological error to suggestions that they were evidence of the ineffectiveness of all ECE programs.

Yet the critical question to be answered was "Why do these data look like this?" An RCT may not, by itself, answer crucial questions. Do these data reflect variability in the fidelity of implementation of the program's essential components? Are all children experiencing the program under the same conditions? Are specific subgroups of children demonstrating different responses to the intervention?

It is time to acknowledge that researchers, policymakers, and practitioners may not sufficiently understand how various components of ECE programs work or what their differential contribution is to a range of positive and negative outcomes for young children.

What does it really mean when we report the mean differences between control and treatment groups? Earlier evaluations, such as the Perry Preschool Project and the Abecedarian study were conducted when few ECE offerings were available. The treatment group received the intervention, and the control group received nothing. Given the significant growth in the number and type of ECE programs over the past 50 years, as Jeanne Brooks-Gunn and Sarah Lazzeroni note in this volume, there is no longer a "clean" control group. Families have many more ECE options, and children not assigned to a treatment group may be in an alternate type of ECE program, presenting a significant challenge in understanding to what type of group the treatment is being compared.

Is there a protocol that can guide our understanding of what is really happening in ECE programs? How should we report on the implementation and eventual evaluation of ECE initiatives such as center- and school-based programs, home visiting, family child care programs, and state-funded preschool?

This volume is intended to initiate a conversation among applied researchers who wish to use their methodological skills to help policymakers and practitioners design questions and get answers that can enhance the quality of life for all young children and their families. We also hope that policymakers will find some important questions to ask and answer as they begin to bring ECE programs to scale at the federal, state, and local levels.

## HOW THE VOLUME IS ORGANIZED

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This volume is divided into three main sections that are intended to provide an overview of what we know about the effectiveness of ECE interventions, what remains to be understood, and what the path forward might entail.

In the first section, we describe the current state of understanding around the effectiveness of ECE interventions from birth to 8 years. Much has been learned about providing high-quality experiences for young children. The pioneering work of Brooks-Gunn, Margaret Burchinal, Linda Espinosa, Dale Farran, and Robert Pianta has advanced our knowledge of what it takes to offer high-quality experiences that promote stronger outcomes for young children. They have made us aware that we need to take a deeper look into the essential components of early childhood interventions and to meaningfully explore what works (or not), for whom, and under what conditions. We are extraordinarily fortunate to begin this volume with chapters in which these researchers describe the current state of knowledge in ECE. We see from their research that issues of equity in access to and quality of ECE programs continue to hover over the ECE landscape.

Burchinal and Farran tackle the thorny issue of the relationship between our current indicators of program quality and child outcomes, while Brooks-Gunn clarifies what child outcomes we should reasonably expect from ECE programs. Building on the foundation of our current knowledge, Iheoma Iruka identifies the potential root causes of documented disparities and proposes potentially mitigating practices and policies.

Section 2 covers what still needs to be understood in terms of content, practice, and outcomes. Farran explores what factors might strengthen outcomes for young children and develops the notion of constellations of classroom practices and specific content that show potential to enhance children's learning and development. Pianta and Bridget Hamre discuss research on effective elements of professional development and describe the need to scale effective professional development systems. They offer a set of research questions related to scaling professional development systems that highlight such issues as purpose, supports, intensity, duration, and effectiveness. In her chapter, Espinosa reviews effective program models, instructional practices, and the educator competencies needed to provide high-quality ECE for dual language learners.

The section ends with Jason Sachs’s powerful voice from the field as he reflects on building and scaling up the Boston Public Schools’ prekindergarten to second grade program. Sachs describes the intentional use of research to guide change and the realities encountered while conducting implementation research.

Section 3 explores how implementation research can help us understand ECE program effectiveness and makes a case for why we need new research approaches. JoAnn Hsueh, Tamara Halle, and Michelle Maier frame the measurement landscape needed to tell the full story of how ECE programs are actually implemented. They assert that strong implementation research is the key to how demonstrated, positive child outcomes from small-scale model ECE programs can be achieved in large-scale adaptations across populations and settings.

Maier and Hsueh provide definitions of implementation research, looking both inward at the program itself and outward at the significant organizational and contextual factors. Next, Halle’s chapter outlines distinctions among implementation science, improvement science, and program evaluation.

Sharon Ryan outlines the importance of qualitative perspectives in research design and asserts that research should continue to explore the impact of inequities that exist across the ECE workforce in terms of compensation, work environments, and benefits, especially as these relate to teacher well-being, turnover, and retention.

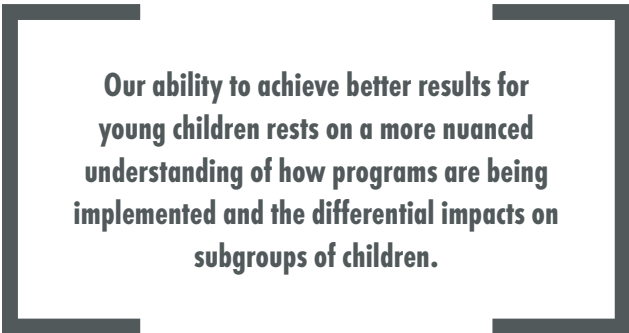
The section ends with Milagros Nores’s discussion of the need to address equity issues in research design, measures, and methodology, as well as the role of implementation research in understanding what might reduce or increase inequity.

Finally, in an afterword, Sara Vecchiotti reflects on the volume as a whole and its implications for those conducting policy-relevant implementation research.

## THE PROMISE OF IMPLEMENTATION RESEARCH

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Our ability to achieve better results for young children rests on a more nuanced understanding of how programs are being implemented and the differential impacts on subgroups of children. Implementation research is an intriguing tool that can add significant contextual information to our understanding of the effectiveness of ECE programs. Implementation research might also help reveal how issues such as race, gender, class, and linguistic diversity interact with ECE program delivery and, ultimately, with outcomes for young children. These historically intractable issues may be central to understanding the relationship between populations most in need of services and specific program components or constellations of program components.



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For over 100 years and under changing organizational structures, the Foundation for Child Development has supported research on the well-being of young children. We hope this volume carries on the Foundation's tradition of working to fill gaps in research and making research more relevant and useful to policymakers and practitioners. Our ultimate goal is always to ensure that every child reaches their full potential.

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