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# Still Waiting for the Right Questions

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This supplement to the *American Journal of Preventive Medicine* addresses an ambitious and important challenge—the task of evaluating community-based interventions designed to promote social environments that facilitate positive health outcomes. Three types of interventions were selected for systematic analysis: early childhood development, affordable housing for low-income families, and culturally competent health care. This commentary focuses on the first of the three.

At the ground level, research on the effectiveness of early childhood interventions has progressed steadily over the past few decades. These gains are reflected in increasingly sophisticated theories of change, growing methodologic rigor, and the assessment of a broader array of mediating and outcome variables. As one steps back and examines the empiric knowledge base from a broader perspective, however, it is difficult to avoid the nagging conclusion that the wheels of progress have been spinning more than rolling. In this context, the extensive literature review compiled by the Task Force on Community Preventive Services (the Task Force) illustrates the extent to which early childhood intervention research persists in asking questions that have been answered and avoids some of the complex challenges that demand creative investigation.

Two of these challenges are particularly worthy of serious attention. The first is focused on the formulation of research questions. The second is embedded in the limited availability of measures to assess the social and emotional dimensions of human development that contribute to success in school and later in life. Together these two challenges must be addressed if our knowledge, policies, and practices are to make a real difference in the lives of young children and their families.

Concerns about the questions that have guided research efforts are simple and straightforward. Why, after almost 4 decades of carefully evaluated demonstration projects and multiple literature reviews, do we continue to feel a need to repeatedly address the generic question of whether early childhood interven-

tions can produce positive developmental outcomes? The consistency of the research findings—short-term gains on cognitive measures, subsequent differences in academic achievement scores, and significant differences in grade retention and special education placement—make the persistence of this question even more curious.

Concurrently, despite repeated calls for a more differentiated approach to the question of program effectiveness, we have made relatively modest progress in our understanding of how impacts vary by program model and for different types of children and families. Even more troubling, we have made virtually no progress in our ability to understand what it takes to change the developmental trajectories of the most vulnerable young children who live in the most threatening environments.

Anderson et al.<sup>1</sup> underscore the importance of this challenge, noting that successful interventions are “particularly important for children in communities disadvantaged by high rates of poverty, violence, substance abuse, and physical and social disorder.” However, the typical, modestly funded early childhood program is rarely a match for the serious health and developmental threats these children and families face; they are usually the toughest group to engage in services, and they comprise a disproportionate share of sample attrition in most program evaluations. Thus, intervention research that persists in reporting aggregate effectiveness has limited value in advancing our capacity to affect the health and development of those young children who are at greatest risk for poor outcomes.

The second challenge underscored by the papers in this supplement to the Journal relates to the disproportionate focus of early childhood effectiveness studies on cognitive and academic outcomes and the relative paucity of data on social and emotional impacts. In view of the growing recognition of the importance of these latter dimensions, their continuing status as “second class” variables is both perplexing and inexcusable. Limited availability of valid and reliable measures provides an explanation but not an excuse. The fact that a literature review published in 2003<sup>1</sup> concludes that there are insufficient data on social and emotional outcomes to warrant any overarching conclusions about the impacts of early childhood interventions is absolutely unacceptable.

The science of early childhood intervention tells us what is possible. The translation of that knowledge into

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policies and programs demonstrates what we are willing to do with what we know. The politics of resource allocation would be guided more constructively by empirical research if we moved beyond the basic question of *whether* early childhood interventions work and began to seriously address the more compelling challenge of *how* to achieve a maximum return on our early childhood investments. For the intervention research community, this underscores the importance of a more explicit focus on understanding what does and what does not improve the odds of more favorable outcomes for children with different needs, particularly for those who are the most difficult to reach.

The evaluation of early childhood interventions and the translation of research findings into policies and practices have focused far too much attention on winning the hearts and the minds of opinion leaders and policymakers. The time is long overdue for us to invest greater energy in developing a more differentiated knowledge base that can be used to nurture the hearts and the minds of those young children who most need our help.

### Reference

1. Anderson L, Shinn C, Fullilove M, et al. The effectiveness of early childhood development programs: a systemic review. *Am J Prev Med* 2003;24(suppl 3):32-46.

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