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Volume Author/Editor: Phillip B. Levine and David J. Zimmerman, editors

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Chapter Authors: Phillip B. Levine, David J. Zimmerman

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14 Conclusions

Phillip B. Levine and David J. Zimmerman

Our goal in this volume has been to identify the childhood interventions that are most successful at alleviating subsequent poverty. That goal is, in the words of one referee of this volume, “outlandishly ambitious.” Ambition, however, seems essential when 13 million children are living below the official poverty line. Each day that passes, money, time, and energy are devoted to reducing child poverty. “Bang for the buck” is likely to be a key factor in policy choice for poverty reduction. Dollars spent on low return interventions are dollars that could have done more to reduce poverty.

Our empirical approach, described in this volume, has been designed to guide an efficient allocation of resources in this endeavor. As a matter of practical necessity, these investment decisions need to be made using the best information currently available. The reality is that decisions need to be made now, not sometime in a utopian future when all of the relevant effects and interactions have been precisely pinned down. Indeed, we suspect that such a fully informed state of affairs will not be arriving anytime soon. That practical reality puts the burden on developing an approach that is no doubt prone to error, but that, hopefully, is reasonable and useful and, importantly, can be put into action now.

In this volume we have attempted to forge such an approach. We have assembled a comprehensive review of the existing base of knowledge on a wide range of interventions targeted at children, with the aim of raising their lifetime economic prospects. We have attempted to provide a rational sifting of the evidence, connecting the dots between interventions and

Phillip B. Levine is the Class of 1919 Professor and chair of department of economics at Wellesley College and a research associate of the National Bureau of Economic Research. David J. Zimmerman is a professor of economics and Orrin Sage Professor of Political Economy at Williams College and a research associate of the National Bureau of Economic Research.

outcomes—often forcing us to extrapolate the effects several years into the future. That indeed is an "ambitious" enterprise. We hope that we have conveyed throughout the volume that this enterprise has been undertaken with a spirit of deep humility. We began this project with our eyes wide open—with excitement about the potential benefits, but also painfully aware of the difficulties the analyses presented. While we may not have inventoried all of the potential pitfalls, we have tried to be as transparent in our choices as possible. That transparency is intended to allow future research to refine and improve on our efforts. Indeed, it is best to think of this first effort as a prototype upon which future efforts can extend and improve. Any refinements that advance our initial goal are zealously encouraged.

What, then, have we learned? First, and not surprisingly, some interventions show more promise than others. Several interventions convincingly show positive impacts on the human capital of children, while others do not. Early childhood education programs, mentoring programs, reductions in class sizes, curricular reforms, improved teacher training and increased teacher pay, increased college aid, and intensive vocational training can all be credibly linked to poverty reduction. On the other hand, parenting programs, school vouchers, after-school programs, dropout prevention programs, substance abuse programs, general jobs programs, and employment/training subsidies either show limited effectiveness in reducing poverty or lack a credible empirical basis from which to draw conclusions. Further, child care programs, child health programs, housing vouchers, or programs targeting teen pregnancy may demonstrate credible treatment effects, but have little impact on the human capital characteristics likely to raise a child's economic outcomes. As we have tried to convey, many of these interventions may make a great deal of sense as a matter of social policy. For example, teen pregnancy prevention programs can generate high benefits by way of reducing subsequent welfare expenditures. These programs, however, appear to have little impact in creating the human capital necessary to keep the program participants out of poverty.

The second conclusion (again, not surprising) is that there is considerable variation in the size of the returns generated by the competing investments. These returns, quantified as the impact on children's lifetime earnings resulting from a $1,000 investment in a given intervention, vary positively with the size of the intervention's effect on lifetime earnings and negatively with the cost of the intervention. Measuring the earnings impact per $1,000 investment across a heterogeneous mix of interventions is one of the signature contributions of this volume. Some interventions show great promise, whereas other investments may have significant impacts on a child's human capital but do not generate positive net benefits due to their cost. Based on our interpretation of the results presented in chapter 13, our view is that intensive early childhood education, college aid, and intensive vocational training appear to be the three interventions that have the largest effect, relative to their cost, in reducing subsequent poverty.

These results, of course, must be interpreted with caution. First and foremost, reducing poverty is only one of many goals in determining which public policies ought to be pursued. Clearly there are other important goals that must be taken into consideration. Our analysis has solely focused on poverty reduction.

Moreover, there are a variety of technical issues that need to be taken into consideration as well. For example, there are no standard errors around the benefit-cost ratios and simply sorting the interventions from high to low on this criterion would suggest a level of precision in the estimates that simply does not exist. Indeed, such a sorting would reflect the various strategic decisions that made this analysis possible. First, the set of interventions being considered is a result of the set of interventions that have been studied. Further, only those interventions that have been subjected to a rigorous evaluation are given consideration. There may be interventions that work, but that have not yet been attempted, have not been subjected to a credible evaluation, or by chance, have not demonstrated their efficacy in the existing evaluations. For some interventions we have the benefit of several well-structured evaluations. For other interventions we may know much less.

Further, there are methodological difficulties that are inherent in testing certain interventions. Universal programs, for example, cannot be randomly assigned. There is also a fundamental problem in that we often face a trade-off between observing a current intervention along with some necessarily short-term outcome versus an old intervention along with a good estimate of the impact on lifetime earnings. For example, we might observe an educational intervention today along with its impacts on the test scores of young children versus an educational intervention that occurred several decades earlier but that allows us to observe the resulting impact on lifetime earnings. Here we must consider the difficulties of extrapolating the effects of the current program versus the possible external invalidity of the older study. All of these considerations impact the set of interventions ultimately being considered.

There are other complications. The production of human capital may be a multiplicative process. For a child to thrive interventions might need to be sustained and not be piecemeal. Integrated programs offered with some continuity may have effects, whereas isolated programs may not. Interventions that are short-lived or of insufficient intensity may deliver only modest and perhaps fleeting effects. We might not be surprised to see the effects of preschool "fade away" if investments in the child end with that program. We might not be surprised to see limited effects from a low-budget training program targeting highly disadvantaged young adults. The returns from one intervention may also hinge on earlier investments. For example, the returns to curricular reforms may be affected by whether the child attended a high quality preschool and is "school-ready." Fortunately, the range of interventions we have considered can shed light on some of these concerns. Some programs are comprehensive. Some persist for several years. We do not know, however, what an optimal mix of programs might be. We are, ultimately, constrained by the interventions that have been constructed and evaluated.

It is also worth noting that just because a program has high net benefits does not mean it can easily be expanded. Expanding a program entails certain risks. Some programs may be difficult to "scale-up." It may be difficult, for example, to reduce class sizes while at the same time holding teacher quality constant. A successful comprehensive program may hinge on the ability to recruit extraordinary program administrators. Furthermore, such administrators may be in short supply.

Beyond these difficulties, our rankings depend on our mapping outcomes that occur early in life to economic outcomes that sometimes occur much later. It is a mildly heroic task to extrapolate improvements in test scores at a young age to improvements in lifetime earnings. There is limited research helping us here and we have attempted to fill the gap by providing estimates of our own. We also provide some evidence suggesting our estimates are sensible. Again, however, there is uncertainty around these estimates and this uncertainty is not reflected in our rankings. If we have overstated the effect of an early improvement on later outcomes then programs that occur earlier might have exaggerated benefits. Similarly, underestimates would provide muted benefits.

Another concern is that the populations served by the various studies differ in how poor they are. Programs like QOP or Job Partnership Training Act (JPTA) are targeted at very disadvantaged populations. They face a challenging task in delivering returns. Other programs such as mentoring have targeted somewhat less disadvantaged populations. The hurdle they face is lower if the returns associated with an intervention vary inversely with the challenges facing the targeted population.

Given these and a myriad of other possible concerns we suggest that the results be interpreted with caution. While there are no doubt flaws in our approach, we do believe that we have used the existing evidence to the best of its ability. One might reasonably approach the results with the aim of deciding where to place their bets in fighting child poverty. Some interventions look like goods bets. Some are risky. Some simply do not look like winners and, given the current state of our knowledge, should not be supported.

We should also note that our approach has been entirely empirical. Our findings have not been guided by any particular model of human development. The connection between intervention and outcome is, in effect, a black box. One fruitful extension would be to synthesize our findings with different models of child development to see which models are most consistent with the empirical evidence. Ultimately, it would be helpful to understand the mechanisms at play. A clearer understanding of how interventions affect the dynamics of family behavior, for example, would be useful. Indeed, an understanding of the mechanisms at play at the house, neighborhood, school, or neural level, for example, would allow better predictions regarding promising future interventions.

Perhaps the strongest conclusion we can draw is that additional evidence is needed. In an ideal world we would know the full range of plausible interventions; the nature of their interactions; their optimal target group, duration, and timing; and a complete understanding of the mechanisms at play, along with information of their long-run impact on economic outcomes. We would then select those programs so as to equate the marginal benefit of poverty reduction to the marginal cost. This ideal certainly suggests the value of panel data on a wide range of experimentally evaluated interventions, along with a theoretical framework for interpreting the findings. We are a long way from that ideal. Indeed, what often confronts us is an abundance of unconvincing research. Careful, systematic, experimental studies are needed to separate the wheat from the chaff. Such a process is incremental and slow. To that end, we hope this volume provides some useful guidance.