

# Do Early Childhood Intervention Programs Really Work?

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## Executive Summary

When early intervention programs were originally developed in the sixties, their primary objective was to raise the intellectual achievement of disadvantaged children. There is little doubt that these programs can increase test scores in the near term. Unfortunately, there is a definite tendency for these gains to fade out over time. The evidence is mixed on the question of whether it is possible to make them permanent.

However, there is a growing body of evidence that early intervention programs can generate permanent changes in social behavior. Five major studies have shown that these programs reduce the incidence of social problems by large amounts when the children reach adolescence and adulthood. The effects seem to be larger for more severe problems. The most consistent and largest impacts were found on serious crime. Cost-benefit analyses were done for four of the five programs. Benefit exceeded costs in each case, and the ratio was as high as 7:1 in one study.

Four of the five studies used "experimental designs," in which individuals were randomly assigned to treatment or control groups. This methodology is the gold standard in evaluation research. However, all four of those studies were relatively small. As luck would have it, results from the long-term follow-up evaluation of a larger program that used an experimental design are due out in about a year. Therefore, it makes sense to wait for these results, before embarking on a major policy initiative. If the new study confirms the results of the other five, the case to move ahead will be very strong.

The most obvious approach would be to upgrade Head Start. Currently, Head Start is a watered-down version of the model programs that have proven so successful. With sufficient resources, it would be possible to turn Head Start into a true replication of

the models. Skeptics argue that while we can replicate the form of "hothouse" models, we can never replicate the substance or the success of such models on a large scale.

That may be true, but at this point it is merely conjecture. There won't be any concrete evidence unless we actually try to make Head Start as intensive as the model programs. Doing so would cost a substantial amount of money and thus entail some risk. But the evidence clearly suggests that, if it were successful, we could reduce the incidence of major social problems in America by large amounts. In other words, the benefits could potentially be enormous. So the risk may be well worth taking.

## I. Introduction

In the middle of the 20th century, the orthodoxy in psychology was that young children are very malleable. Then at a certain age, perhaps 6 or 7, their minds and personalities solidify, and they become much harder to change. At the same time, sociologists were trying hard to come up with solutions for various social ills, like poverty, school failure, and juvenile delinquency. Eventually, researchers put two and two together and began developing intensive early intervention programs for poor children and their families. The idea was to provide them with an enriched educational environment at a pre-school and perhaps also to teach their parents how to improve their environments at home.

When these programs were first developed, the primary criterion used to evaluate their effectiveness was whether they increased the children's IQ or achievement test scores. At first these initiatives seemed successful beyond the developers' hopes. They raised test scores by huge amounts, bringing poor children up to or even beyond the average after just a year or two. It quickly became apparent, however, that many of these gains faded out after the children left the programs and went on with the normal course of their lives. For a time, early intervention was considered by many to be a disappointment. There were exceptions, tantalizing hints that programs could permanently raise intellectual achievement if only they were intensive enough or done in the right way. Some recent findings offer hope. Nevertheless, there hasn't yet been a consistent enough pattern of long-term success to justify the cost of intensive early intervention on the basis of intellectual effects alone.

A funny thing happened, however, when the children grew up. The people who, 15 or 20 years earlier, had spent a year or two in high quality pre-schools behaved a lot differently than the ones who hadn't. Participants in these programs committed fewer crimes. Some studies also found that the programs reduced other problems such as welfare dependence, dropping out of school, out of wedlock childbearing, or drug abuse. In many cases, the sizes of these impacts were quite dramatic. The fact that these programs had such effects among high-risk populations suggests that if carried out on a national scale, they would have the potential to reduce the incidence of some of our nation's worst social problems by large amounts. Moreover, they can be extremely cost effective, generating benefits far in excess of their costs.

The key question is whether the evidence is strong enough yet to justify a major policy initiative. That is a judgment call. Our view is that we are getting close to that point, but perhaps we are not quite there yet. However, a long term follow-up of the Infant Health and Development Program, a large scale multi-site early intervention is just beginning. If that evaluation yields results consistent with the findings of the studies discussed below, it will probably be enough to justify a large scale investment in intensive early intervention programs.

## II. Early Intervention and Intellectual Development

The ability of early childhood intervention programs to raise children's cognitive test scores is one of the most well documented findings in social program research (Barnett, 1995). Unfortunately, these gains tend to fade out over time. The seminal report of the Consortium for Longitudinal Studies (Lazar & Darlington, 1982) found no lasting effects on IQ scores. It did find some evidence of impacts on achievement test

scores (particularly in math) through the end of fifth grade, but not beyond that. Barnett (1995) analyzed results from 36 early intervention programs. He found no consistent pattern of effects on either IQ or achievement tests beyond the end of elementary school.

This is not to say that there is definitive evidence that early intervention cannot generate permanent gains. A few studies have shown such effects. The Abecedarian Project is probably the foremost example of these. The Abecedarian Project provided extremely intensive educational enrichment to disadvantaged children, beginning just six weeks after birth and lasting until kindergarten (Campbell et al., 2002). A follow-up study of the individuals was done when they reached 21. Those who had participated in the program still had significantly higher scores on IQ tests, math tests, and reading tests than individuals who had been assigned to a control group.

The Abecedarian Project differed from most other early intervention programs in two ways. First, it started working with the children when they were extremely young, as early as six weeks old. Second, it placed more emphasis on intellectual growth than its counterparts, which focused on socioemotional development. So it is possible that early enrichment programs can have permanent effects, if they are designed in just the right way. However, there is some skepticism over the Abecedarian results. Large developmental differences between the treatment and control groups were found as early as six months. Perhaps the program caused these differences. But they may also be an indication that the treatment group was ahead of the control group from the very beginning (Spitz, 1992).

The Abecedarian Project did use a pure "experimental design." Children were randomly assigned to treatment and control groups. This is, by far, the best way to

determine the true effects of a program. It isn't foolproof, however. Random assignment is best because it ensures that, on average, there will be no systematic differences between treatment and control groups. This is just an average, however. It is possible that smarter kids wound up in the treatment group just by chance. Unfortunately, there is no way to know for sure. We need to look to other studies to confirm the results.

The Milwaukee Project provided intellectual enrichment to the children of mothers who were classified as mentally retarded or near retarded. It also offered parent education and vocational training to the mothers themselves. By the time the children reached 14, those in the treatment group scored substantially higher on IQ tests than those from the randomly assigned control group (Garber, 1988). However, there is some skepticism about these findings, as well. The initial gains in IQ were enormous. Yet, they were not accompanied by any significant differences between the treatment and control groups on achievement tests. One possible explanation is that the gains were achieved by teaching to the test. The fact that one of the original program developers was convicted of misuse of federal funds added to the skepticism. Still Garber, who ultimately took over full control of the program and shepherded it through the follow-up studies, is beyond reproach. And teaching to the test should not have had any effects on the results at age 14. So the findings cannot be completely discounted.

The Chicago Child-Parent Centers (CPCs) provide educational enrichment and family support to children between the ages of 3 and 9 in conjunction with the city's public schools. The CPCs are a fairly large scale working program, not a pilot. Reynolds (1998) has followed a sample of 1,150 children since they were enrolled in twenty CPC preschools and kindergartens in the mid-eighties. They have also followed the progress

of a comparison group of similar children. However, children were not randomly assigned to the treatment and control groups.

Reynolds found that the CPCs did generate long-term gains in academic achievement, but only for children who participated for at least three years. Those who participated the longest gained the most. By eighth grade, children who had been in the treatment group for all six years scored about half a standard deviation higher on both reading and math tests than children in the comparison group. These long-term participants had gained about one full grade level in achievement, making up roughly half the gap to the national average.

Since the children were not randomly assigned to the treatment and comparison groups, we need to be cautious about the results. The "quasi-experimental" design of the study makes it much more likely that the two groups were systematically different from the very beginning, in ways that could have influenced the findings. However there is a striking feature of the pattern of results that lends credence to them. There was a very consistent relationship between the size of gains and the duration of participation. Each extra year of participation after the first tended to raise performance on each one of five different measures of achievement. The correlation isn't perfect, but we wouldn't expect it to be, given random error. The positive trend was quite strong, particularly when all five measures are considered. It indicates that the program was conferring tangible benefits which accrued slowly, but consistently over time. Such a consistent "dose-response" relationship can be a strong sign that the program really was having positive effects on the children. Of course, it is possible that children who stayed in the program

longer had parents who were more concerned about their education. So the lack of randomization still leaves us with some doubts about the validity of the results.

Even if early intervention programs cannot, by themselves, permanently raise cognitive test scores, it doesn't mean that attempts to enrich the intellectual environment of young children have no value. Perhaps gains tend to fade out after the programs end because the children return to disadvantaged environments and enter poor schools (Lee & Loeb, 1995). Pairing preschool programs with effective school reform might be the key. The fact that extended participation (through third grade) substantially increased the effects of the Chicago CPCs bolsters that hypothesis. So even if early intervention isn't a sufficient condition for long-term intellectual gains, it could still play an important role.

In sum, there is reason for both doubt and hope that early intervention programs can generate permanent gains in cognitive test scores. However, there is strong evidence that they can generate permanent changes in social behavior that are probably even more beneficial, both to participants and to society as a whole.

### III. Early Intervention and Social Behavior

The Perry Preschool was, in many senses of the word, the mother of all social programs. Initiated in 1962, it was one of the first rigorously planned social experiments. The evaluation of it was one of the first to use a pure experimental design, in which children were randomly assigned to treatment and control groups. It was a model for Head Start and, in a looser sense, for much social policy and experimentation that evolved over the ensuing decades. The consistency and duration of follow-up studies of the long term effects of the program are unparalleled.

The program itself was the archetypal early intervention program for preschoolers. Three and four year old children participated for one or two school years. The school applied classic principles of child development to create a nurturing environment which would foster social, emotional, and cognitive growth. Student-teacher ratios were very low, with four teachers in each class of 20-25 children. Teachers made weekly 90 minute home visits to every family. All teachers were certified in both early childhood and special education.

The most recent follow-up was done when the participants were 27 years old, more than two decades after they completed the program (Schweinhart et al., 1993). By that age, the program had a number of large effects, several of which were truly dramatic. It quadrupled the proportion of individuals who were earning more than \$2,000 per month, from 7% to 29%. It almost tripled home ownership, raising it from 13% to 36%. It reduced the proportion who had ever received welfare by a quarter, from 80% to 59%. It quintupled the proportion of women who were married, from 8% to 40%. (It had no effect on the percentage of men who were married, but since men tend to marry later than women, it would not be surprising if such an impact showed up later.) It decreased the proportion of women who had ever born a child out-of-wedlock by almost a third, from 83% to 57%. It cut the total number of arrests in half. And in what may be the single most extraordinary gain ever achieved by a social program, it reduced the rate of hard core criminality (defined as an individual having five or more arrests) by four-fifths, from 35% to 7%.

All told, the program was extremely cost effective. It generated \$7.16 in benefits for society as a whole for each dollar spent (assuming a 3% discount rate). And this

does not even take into account any psychological benefits which accrued to the participants and their families, benefits which common sense would tell us were huge.

A number of other early intervention programs have been the subjects of long-term follow-up studies that examined effects on social behavior. Each one of them produced substantial benefits of some kind. In general, the programs had their largest and most consistent impacts on the most serious social problems, such as crime and delinquency, and among the most disadvantaged groups.

For example, the Elmira Nurse Home Visiting Program (Olds et al., 1998) provided pre-natal and post-natal home visits to expectant mothers between 1978 and 1980. During the visits, the nurses taught the mothers parenting skills, encouraged healthy behavior during and after pregnancy, promoted personal development of the mothers, provided information about social services available to them, and tried to involve relatives and friends in the pregnancy and care of the child. Fifteen years later, the children from the treatment group had half the number of arrests and just a third of the convictions and probation violations of those in the randomly assigned control group. Among a subsample of low income, unmarried women, the program reduced arrests by almost 60% and convictions and parole violations by more than 80%. Total benefits exceeded costs for this group by a ratio of more than 5:1 at a 4% discount rate (Karoly et al. 1998). Benefits also exceeded costs for the less disadvantaged group, but only by a little.

There were no significant effects on school suspensions or sexual behavior, and impacts on alcohol use were mixed. In other words, children from both groups exhibited some problem behavior, but children from the treatment group were much less likely to

cross the line where police and the courts had to become involved. It will be interesting to see if future follow-ups will show more differences between the groups, along the lines of the Perry Preschool results. This could happen if most of the kids who were engaging in non-criminal problem behavior grow up to lead generally productive lives, while many of the ones who were already getting in trouble with the law by age 15 have a hard time breaking the pattern.

The Syracuse University Family Development Research Program provided home visits and high quality child care to low-income, mainly African-American families (Lally et al., 1988). The visits began during the mothers' pregnancies, and the child care lasted until the children turned 5. When the children were between 13 and 16 years old, the proportion of the treatment group who had probation records was just over a quarter of that of the randomly assigned control group. Moreover, the offenses of the control group were more severe and more costly. Over 40% of the control group who had records were chronic offenders, but none of the treatment group were. The court and probation costs of the control group offenses were nine times greater than those of the treatment group. We should put somewhat less weight on this particular study, because they failed to find a substantial portion of the original groups for the follow-up study. However, the fact that the results are consistent with those of the other programs lends credence to the overall conclusion that high quality early interventions can have big impacts on crime.

As noted above, the Abecedarian Project placed more emphasis on intellectual enrichment than other programs. That may be why it had its largest effect on an educational outcomes. By age 21, the proportion in college was  $2^{1/2}$  times greater

among the randomly assigned treatment group than the control group. Yet, the project did have significant impacts on other kinds of social behavior as well. The proportion who had had a child as a teenager was 40% lower among the treatment group. And the percentage who reported marijuana use in the previous month was more than double among the controls. Overall, benefits exceeded costs by more than 2:1 at a 3% discount rate (Masse & Barnett, 2003).

The treatment group also had a third less felony convictions, and a third fewer had been incarcerated. However the differences with respect to crime were not statistically significant. If this were the only study showing such effects, we should disregard those last two findings completely. But the fact that these results are consistent with those of several other studies means that we can at least keep them in the backs of our minds. Although the differences could very well have occurred by chance, it is more likely than not that the program did reduce felonies and incarcerations. It is reasonable to conclude that the Abecedarian results add a small bit of weight to the growing mass of evidence that early childhood intervention can prevent crime. At the very least, they provide no evidence against that hypothesis.

The Chicago Child-Parent Centers also had large effects on crime (Reynolds et al., 2001). By the time they were 18 years old, individuals who participated in the preschool portion of the program had a third fewer arrests of any kind and 40% fewer arrests for violent offenses than individuals in the comparison group. There was an additional 25% reduction in arrests for violent offenses for those individuals who continued on in the program through elementary school. Intervention at an early age was key, however. There was no reduction in arrest rates for those who entered the program

later, in elementary school. Overall, benefits exceeded costs by a ratio of almost 4:1, at a 3% discount rate (Reynolds et al, 2000).

Recall, however, that we need to be cautious about these results, because the Chicago CPC evaluators were not able to randomly assign children to the program. Still it is important to note that the findings are consistent with those from random assignment studies in two ways. First, early intervention did reduce crime by large amounts. And second, the effects were larger for the more extreme kinds of crime.

#### IV. Conclusion: Almost There

The evidence for long-term effects of early intervention programs on intellectual achievement is mixed. In contrast, five studies of the long-term effects of early intervention on social behavior showed large impacts. Is this strong enough evidence to justify a major policy initiative?

On the one hand, the results of the five studies are, for the most part, very consistent with each other, which is an important indication that they are accurate. They all show large impacts, which makes it all the more likely that the effects are real. Four of the five studies used experimental designs. The fifth one, the Chicago CPCs, demonstrated a dose-response relationship, which is a good sign. Four of the five found the largest impacts on the most extreme social problems, like violent crime. In the fifth, the Abecedarian, effects on crime were large. The fact that educational effects were even larger is not that surprising given that that particular program emphasized intellectual enrichment more than the others.

On the other hand, the four experimental studies were all small. The one large scale study was not able to use random assignment. The Abecedarian Project's effects on crime were not statistically significant. And the attrition rate in the Syracuse program follow-up study was high.

On the whole, the positives substantially outweigh the negatives. The studies demonstrate a consistent pattern of large effects on severe social problems. They suggest that if we, as a society, made a major investment in early intervention programs, there is a high probability that it would pay off many fold by reducing crime and other social problems in the future.

It is tempting to move ahead right away. However, there is reason to hold off for the moment. As luck would have it, a major long-term follow-up evaluation of a large scale early intervention program, the Infant Health and Development Program (IHDP) is just beginning. Results from this all important study will probably be available in March 2004.

The IHDP is the largest early intervention program ever evaluated using a pure experimental design. Implemented at eight different sites across the country, the program targeted low birthweight, premature babies. The children were stratified into two categories, low birthweight (between 2001 and 2500 grams) and very low birthweight (below 2001 grams). Then they were randomly assigned to treatment and control groups. The intervention consisted of two components, high quality child care at child development centers and home visits. Both treatments began at age one and ended at age three (Hill, Waldfogel, & Brooks-Gunn, 2002). The participants are now 17 years old, as the follow-up evaluation is beginning.

Earlier evaluations demonstrated that the program had large effects on cognitive test scores for the whole sample at age 3. The effects faded out over time. By age 8, there were no differences between the treatment group and control groups among the very low birthweight children. There were still medium-sized, statistically significant effects for the low birthweight group. Hill, Brooks-Gunn, and Waldfogel (2002) have also found a substantial dose-response relationship. When they compared a subset of the treatment group that participated consistently, they found large advantages on test scores over a comparison group among the controls, even at age 8. While we need to be cautious about these latter results, because of the possibility of statistical bias, the dose-response finding gives us reason to be optimistic that there will be permanent effects on intellectual development. What we are most interested in, however, is whether the program produced a long-term effect on social behavior.

The current evaluation will determine whether the program substantially reduced the incidence of serious social problems, such as crime, by age 17. If it did, that would make it the sixth study out of six to show a large effect of early intervention on social behavior. There would still be issues that could be raised with respect to any one of these studies. But taken collectively, this body of evidence would be very strong, strong enough to justify a major national policy initiative.

Of course this begs the question of what form the policy initiative should take. We already have a national early childhood intervention program in Head Start. Unfortunately, we do not have much evidence on the long-term effectiveness of Head Start. McKay et al. (1985) surveyed 1800 studies of Head Start programs. They found that, as is typical of early childhood programs, Head Start does raise intellectual

achievement temporarily. But these gains fade out during elementary school. Garces, Thomas, and Currie (2002) found some long-term benefits, but they were inconsistent. Head Start increased high school graduation rates, but only among whites. It decreased arrest rates, but only among blacks. Oden, Schweinhart, and Weikart (2000), found no long-term effect of a regular Head Start program on crime. But they also found that individuals who attended a Head Start program modeled after the Perry Preschool had 62% fewer criminal convictions by age 22 than the alumni of the regular Head Start center.

None of these studies used an experimental design, so we have to be cautious. But the results suggest that if we could upgrade Head Start to the standards of the model programs, we could reduce the incidence of crime, and perhaps other social problems, by large amounts.

It is commonly argued that it is impossible to create large scale national programs that meet the standards of "hothouse" models. That may be true, but it is merely conjecture. Head Start has not met the standards of model programs like the ones discussed above. But that doesn't mean it can't. There are tangible differences between Head Start and the model programs. As it stands, Head Start is essentially a watered down version of the models. Student-teacher ratios are much higher in Head Start. Teacher qualifications are lower. There is less teacher training. Some Head Start programs use model curricula; others do not. Even the ones that do often fail to implement the curricula as they were designed. We will never know for sure whether it is possible to repeat the success of the models on a large scale unless we attempt to turn Head Start centers into faithful replications of them.

To accomplish this, we would have to provide Head Start centers with at least three things: 1) more money; 2) a list of proven models to choose from; and 3) sufficient training and oversight to implement the models faithfully. To reform the whole Head Start program quickly, in one fell swoop, might not be feasible. It would be expensive. It would require mandating that individual centers adopt particular curricula. And, most importantly, the developers of the models are simply not equipped to replicate their programs on the kind of scale that would be required to reform the whole program in a short time.

A more realistic approach might be to set a goal of turning a certain percentage of Head Start centers into faithful replications of proven models each year. Increases in funding could then be offered as an incentive for centers to voluntarily adopt a proven model. And program developers could increase their replication capacity at a reasonable rate.

Also, research should be done comparing the effectiveness of centers that adopted the models to ones that didn't. (Ideally, more centers would volunteer to implement the reforms than we could handle at first. If that were the case, we could randomly assign centers to treatment and control groups.) The aim of this research would be to determine whether or not it is possible to replicate the success of model programs on a larger scale. The key to gaining a definitive answer to this question would be to ensure that the centers that implemented the models did so faithfully and were provided with sufficient funds to replicate the intensity of those interventions.

A detailed analysis of the cost of such reforms is beyond the scope of this paper. However, since the model programs are highly cost effective, the upgrade of Head Start

probably would be too, if it delivered comparable results. Attempting such an upgrade would involve risk. It is possible that skeptics who question our nation's ability to run successful programs on a large scale would prove to be correct. However, results from over forty years of research seem to indicate that early intervention programs can dramatically reduce the incidence of some of our nation's worst social problems. In other words, the benefits could potentially be enormous. So it may be a risk well worth taking.

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