Public/Private Ventures

P/PV

Summer Training and Education Program (STEP)

Report on Long-Term Impacts

Jean Baldwin Grossman
Cynthia L. Sipe

Winter 1992

Public/Private Ventures is a national, not-for-profit corporation that designs, manages, and evaluates social policy initiatives aimed at helping youth whose lack of preparation for the work force hampers their chances for productive lives. P/PV's work is supported by funds from both the public and private sectors.

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EXECUTIVE SUMMARY

For decades, youth who do not finish high school have been at a significant disadvantage in the labor market relative to high school graduates, but during the early 1980s, the relative well-being of dropouts grew even worse. High school dropouts represented a higher proportion of the non-employed and welfare recipients, and even those dropouts who could find jobs were paid substantially less than their graduate counterparts. And programs designed to reintegrate dropouts into the labor market have not worked well.

The STEP program, developed in the mid-1980s, differs from other employment and training programs in several respects. First, it attempts to nip the dropout problem in the bud. Participants are in-school youth (14- and 15-year-olds), who are still too young to drop out legally and who might be more receptive to intervention. Second, the program was designed to span 15 months, an unusual duration for an employment and training program, most of which do not have funding set up for a year-round intervention. The two summers represent STEP's most intensive periods, but minimal contact with the youth is attempted during the intervening school year. (More substantial support during the school year was desired, but not achieved during the demonstration.) Third, the model provides payment to youth for their summer class hours, as well as for their work hours. Finally, the program directly addresses the interactions between teenage parenting and high school graduation. The curriculum includes a component on responsible social and sexual behavior as an integral part of the summer classes.

To investigate STEP's viability, P/PV installed the program in five sites. The model and the research design were refined during the initial two summers, 1984 and 1985. Then, starting in the summer of 1986, the longitudinal evaluation of the program began. Over two years, STEP attracted over 3,000 JTPA-eligible, educationally disadvantaged youth. Half of these youth were randomly assigned to a treatment group and were offered the opportunity of participating in the 15-month program, while the remaining half were offered a one-summer job in the federally funded Summer Youth Employment and Training Program (SYETP).

The experiences of these youth, presented in this report, were monitored in interviews and the collection of school transcript data through Fall 1990. By that time, many of the youth were old enough (18 and 19 years old) to have graduated from high school, to be productively employed, or to have children and be on welfare.

Earlier reports have documented that STEP was found to be operationally viable, reasonably inexpensive, and able to improve its

participants' reading and math scores, and knowledge of life skills, relative to what they would have been in the absence of the program. This report, which presents postprogram impacts of STEP participation on the youth's educational achievements, sexual behavior and economic conditions, finds no evidence to suggest that the short-term STEP intervention had long-term impacts. The report, however, presents a clear picture of how the behaviors of these youth evolve.

PARTICIPANT CHARACTERISTICS AT INTAKE

The STEP demonstration targeted a young, economically and educationally disadvantaged group of youth. The program operators attracted over 3,000 such youth to participate in a 15-month program offering half-time work and half-time classes during two summers, as well as support during the intervening school year. While most youth came to the program after being enrolled in the eighth or ninth grade, their average achievement on the Metropolitan Achievement Test at the point of enrollment was at the sixth-grade level in reading and the seventh-grade level in math. Almost a third of the participants had been held back in school at least one grade and many exhibited poor school attendance. All the youth were economically disadvantaged, as defined by JTPA standards, and, in addition, almost half lived in female-headed households. Thus, the program enrolled youth with serious economic and educational problems.

Combined with the youth's poor school performance, approximately 40 to 45 percent of the 14- and 15-year-old participants reported that they had had sexual intercourse by the time they enrolled in the program. Of the sexually experienced, half were sexually active within the immediately preceding two months. Despite being sexually active, two-thirds did not use contraceptives and had poor knowledge of birth control methods and availability. Conventional wisdom suggested that the confluence of poor educational performance with early and high levels of unprotected sexual activity did not bode well for the educational prospects of these youth. Overall, the targeted youth appear to be a group that could be well served by the STEP program.

POSTPROGRAM IMPACTS

The evaluation design for this demonstration allows us to determine whether youth who were enrolled in STEP behaved differently than similar youth who received only a SYETP job. STEP's ultimate goal was to increase the proportion of youth who graduate from high school and thereby improve their employment prospects. While STEP treatments' basic reading, math and fertility-related knowledge increased during the program relative to the controls, analysis conducted for this report found that, once the program ended, program impacts rapidly decayed. Two to three years after the program, the participants' educational and economic outcomes

were the same as those of the controls: treatments were as likely as controls to drop out, to graduate, to get a GED certificate, to go to college and to obtain employment.[1]

Although STEP did not change the youth's educational and economic achievement, treatment youth continued to know more about contraceptives than did control youth. However, it is unclear whether this knowledge translated into changed sexual behavior. On one hand, Cohort II treatments, especially girls and those who had not already had sexual intercourse before enrollment, were more likely to have stated that they used contraceptives four and a half years after enrollment than controls. However, despite the greater use of contraceptives, the treatment girls were also more likely to have a child, even though the incidence of recent sexual intercourse and the frequency of sexual intercourse was the same for both groups. This seeming contradiction in impacts, and the fact that the results are not mirrored in Cohort III, raises questions about the validity of the contraceptive impact. Thus, at this point, the evidence leads us to conclude that STEP did not change adolescent sexual behavior.

Welfare use (WIC, AFDC, and Food Stamps) among those who were eligible for various forms of public assistance was also the same for treatments and controls.

In short, the evaluation revealed that STEP did not change youth's lives relative to a group of similar youth who received one or two summers of work experience only. Participants continued to be more familiar with birth control methods than control group youth, but there were no long-term effects on fertility-related behaviors or educational and economic experiences.

A PORTRAIT OF DISADVANTAGED YOUTH

The study of STEP-eligible youth, while yielding disappointing impact results, provides us with a clear profile of disadvantaged young people.

Cohort III youth were 17 or 18 years old at the time they were interviewed and should have been in 12th grade or graduated. Approximately a quarter of them graduated from high school, just over 20 percent had dropped out, and the remainder were still in school. These figures indicate that only about 60 percent of those who were expected to graduate in 1990 did so.

Among Cohort II youth, who were a year older and further along in school, approximately 55 percent had diplomas. Between 25 and 30

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[1] In addition, no subgroup of participants--defined by gender, race/ethnicity, entry grade level, or site--fared better than average.

percent of the youth had dropped out of school and five percent had GED certificates. The remaining 10 to 15 percent were still in high school.

While these figures confirm that youth in the STEP demonstration were an educationally at-risk group of youth, we find that many had not given up on education. About half of all the graduates were enrolled in college. Only 28 or 29 percent of the younger Cohort III were not involved in some type of educational program at ages 17 or 18. Of the group who were out of school at that point, most worked or cared for a child. Approximately 10 percent were not working, mostly because they could not find a job. Approximately half of the older Cohort II youth attended some kind of educational program. Of the youth who were no longer in school, either because they had graduated or dropped out, approximately two-thirds worked or cared for a child. Seventeen percent were neither working nor caring for a child; most were unable to find a job.

Teenage mothers are a group of particular policy interest. Although STEP was designed to reduce the number of girls who became teenage parents, it did not achieve its goal. Approximately 22 percent of the 17- or 18-year-old Cohort III girls had children (treatments and controls alike) and approximately 33 percent of the 18 or 19 year old girls had children. Almost 70 percent of the girls who could have received WIC (i.e., who had been pregnant) made use of the nutritional program, about 60 percent of mothers were receiving welfare, and just over a third of the sample lived in households that received food stamps. This high level of dependence on public assistance is to be expected, given the options these young mothers have.

The portrait that is painted by these findings has a few bright spots: the majority has not severed ties with education and the vast majority of those not in school are either working or caring for a child. However, the long-term consequences of dropout rates of 30 percent are serious.

STEP alone will not improve the picture in the long-run. This evaluation and its failure to show positive long-term impacts has led us to reflect extensively about its meaning. Full discussion of STEP's implications for youth programming in general and the appropriate place for traditional employment and training programs for youth can be found in this report's companion volume, Anatomy of a Demonstration: The Summer Training and Education Program (STEP) from Pilot Through Replication and Postprogram Impacts.

I. THE DEMONSTRATION: DEVELOPMENT, OPERATION AND IN-PROGRAM RESULTS

The Summer Training and Education Program (STEP), an intervention designed to help reduce dropout levels among poor and educationally disadvantaged youth, addresses two factors closely associated with dropping out: poor academic performance and adolescent parenthood. Through a mix of work experience, basic skills remediation, and life skills and opportunities (LSO) instruction, STEP seeks to improve reading and math skills, increase the graduation rate and reduce the incidence of teen parenthood among disadvantaged youth who are doing poorly in school. STEP attempts to achieve these important impacts without changing school operations and without massive expenditures. Thus, the program represents an effective type of intervention, one that can be activated before youth leave school, and that does not require institutional change or long-term, high-cost programming.

STEP targets 14- and 15-year-old youth who are at risk of dropping out of school. All participants are low-income and perform below grade level in either reading or math. The model offers youth two consecutive summers of remediation, life skills instruction and work experience. During the intervening school year, the program attempts to keep in contact with STEP youth through counselors/advocates and group meetings. During the summers, participants spend half their program hours attending remediation and life skills classes, and half their time in work experience.

Following a successful three-city pilot during the summer of 1984, the national demonstration of STEP began in Summer 1985 in five cities--Boston, Massachusetts; Fresno, California; Portland, Oregon; San Diego, California; and Seattle, Washington. June 1985 to May 1986 was designated a start-up period, during which operational problems were resolved and the intervention was further refined. The demonstration's operational period concluded in Summer 1988.

To evaluate the impacts of the program, half the eligible youth interested in participating in the demonstration were randomly assigned to the treatment group and offered STEP program services; the remaining half were assigned to the control group and offered a summer job through the federal SYETP. All youth were paid minimum wage for their participation, both in class and on the job. In San Diego and Seattle, control youth were offered a job during both summers of the program; in the other sites, control youth were offered a job for the first summer only.

The demonstration was designed and managed by Public/Private Ventures (P/PV) of Philadelphia, a private not-for-profit corporation that seeks effective approaches to job training and

education for disadvantaged youth. Funding for the demonstration's planning, oversight and research was provided by The Ford Foundation, the U.S. Department of Labor, The Robert Wood Johnson Foundation, the U.S. Department of Health and Human Services, W.T. Grant Foundation, The William and Flora Hewlett Foundation, The Edna McConnell Clark Foundation, Lilly Endowment, The Ahmanson Foundation, Aetna Life and Casualty Foundation, and the James C. Penney Foundation. At the sites, responsibility for day-to-day management and operation of the program was assumed by employment and training agencies, school districts and a number of other local institutions. Local program operation costs were covered primarily by Title II-B funds under the Job Training Partnership Act (JTPA); local school districts contributed in-kind resources (primarily classroom space) and modest levels of funding.

Three cohorts of disadvantaged youth participated in the demonstration in each of the five sites. The first cohort of youth, who entered STEP in Summer 1985, were not included in the post-program analysis because their participation took place during STEP's start-up year.[1] The second cohort entered the program in Summer 1986, participated in school-year support (SYS) activities during the 1986-87 school year, and returned for a second summer in 1987. The third cohort entered the program in Summer 1987, participated in school-year support during the 1987-88 school year and returned for a second summer in 1988.

Previous reports have described implementation of the STEP model during the first three summers of the demonstration and the in-program impacts of participation for all three cohorts. This report examines STEP's postprogram impacts.

Impact measurements were derived primarily from data collected in a Fall 1990 follow-up interview with youth from Cohorts II and III and from high school transcript data for those youth. For Cohort II youth, who completed STEP at the end of Summer 1987, the follow-up interview captured experiences up to three and a quarter years postprogram, when the youth were 18 or 19 years old. For Cohort III youth, who completed STEP at the end of Summer 1988, the follow-up interview captured experiences up to two and a quarter years postprogram; the youth were then 17 or 18 years old. For each youth, transcript data were available for each youth's high school experience from 9th grade to their grade during School Year 1989-90.

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[1] Major modifications to the remediation component of the program were made after implementation problems were noted following the first summer. Thus, the experiences of Cohort I are not included when describing the impact of the current STEP design.

The remainder of this chapter provides background on the problems associated with dropping out of high school and teen pregnancy, discusses how the STEP program was integrated into the existing youth programming framework, describes the model and its implementation in the demonstration sites, and reviews our earlier research findings.

BACKGROUND: THE STEP HYPOTHESES

Education is critical in breaking the cycle of poverty. High school graduates have access to income and career opportunities that greatly enhance their ability to become economically self-sufficient. High school dropouts, on the other hand, have a much more difficult time finding jobs and earning enough to keep themselves out of poverty. According to a 1983 U.S. Department of Commerce report, the average high school dropout can expect only two-thirds of the lifetime earnings of the average high school graduate. Despite such bleak economic prospects, almost half a million youth leave school each year without earning a diploma.

Increasing the educational attainment of youth who might otherwise have dropped out of school should increase their employment opportunities and improve their ability to be economically self-sufficient. Economic studies have well-documented that additional years or even months of schooling result in additional income (e.g., Griliches and Mason, 1972; Mincer, 1974; Willis, 1986). Whether high school graduation itself adds to an individual's earnings, as opposed to the additional year of education it represents, is still unclear. (See Hungerfrond and Solon, 1987 and the differing view of Card and Krueger, 1992.) However, keeping youth in school longer is unquestionably beneficial (Angrist and Krueger, 1990). Some studies have also demonstrated that better basic skill levels in reading and math are related to earnings (Jencks et al., 1979; Berlin and Sum, 1988; Blackburn and Neumark, 1991).[2]

The gaps between high school graduates and dropouts in earnings and employment are substantial and are growing steadily. Between the early 1960s and the early 1980s, disparity in the two groups' earnings increased from 31 percent to 59 percent among 18- to 24-year-old males (Berlin and Sum, 1988). During roughly the same period, the percentage of dropouts who were employed declined significantly--from 53 percent to 40 percent. The problem is especially acute among minority groups, particularly blacks and Hispanics. Many minority youth are raised in low-income households and are the most likely to exhibit educational deficiencies

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[2] The numerical significance of test scores in explaining earnings, however, seems small.

that will limit their long-term chances to become economically independent and productive citizens.

The issue of the high school dropout rate and its economic implications may become even more important in the future. Demographic projections indicate that growing proportions of the youth population will be from the groups with the highest dropout rates: the economically disadvantaged and minorities. Kaufman and Frase (1990) find that 12 percent of white and other, 14 percent of black and 33 percent of Hispanic youth between the ages of 16 and 24 have dropped out of school. The problem is much worse in the large cities. For example, in New York City, three out of four blacks and four out of five Hispanic youth do not graduate in the traditional four year high school program (New York State Department of Education, 1988). As the number of disadvantaged youth increases, the consequences of undereducation and low levels of literacy will significantly affect society as well as individuals.[3]

The STEP intervention aims to increase youth's educational levels and, ultimately, graduation rates by focusing on two major predictors of dropout behavior: poor academic performance and teenage parenting. First, it targets youth who have had negative academic experiences (e.g., getting poor grades or being held back a grade level) and attempts to improve their academic skills through a skills-based reading and math curriculum. Surveys of dropouts most frequently cite negative academic experiences as the primary reason for youth dropping out of school. For example, approximately 42 percent of dropouts surveyed by the High School and Beyond study reported receiving primarily Ds in their classes (Institute for Educational Leadership, 1986). Dropouts are typically young people who are unable to function on a par with fellow students.

Research on Job Corps participants and disadvantaged high school and college students has shown that well-designed remedial education programs are capable of producing academic gains (Taggart, 1981; Kapsis and Protash, 1983). In a study of summer school students in Atlanta (Heyns, 1978), students gained, on average, the equivalent of one month on the word knowledge portion of the Metropolitan Achievement Test (MAT) for each month of instruction. Thus, P/PV hypothesized that a specially tailored remedial education would mitigate the academic problems of potential dropouts.

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[3] The exact form of the problem under debate is typified by controversy between the Hudson Institute (Johnston and Packer, 1987) and the Economic Policy Institute (Mishel and Teixeira, 1991). However, both sides predict labor mismatches in the future.

A crucial determinant of academic performance is the extent to which learning slows, stops or decays over the summer months. Findings from several studies indicate that summer learning loss is particularly acute for educationally and economically disadvantaged youth, who consistently lose ground over the summer relative to their advantaged counterparts (Pelavin and David, 1977; David and Pelavin, 1978; Hammond and Frechtling, 1979). A 1978 report to the New York State Board of Regents claimed that "educationally disadvantaged students lose about three to four months of their year's growth over the summer months, compared with students scoring above grade level, who showed one month's growth during the summer" (cited in Quinlan et al., 1987). Hayes and Grether (1969) suggest that as much as 80 percent of the difference in school performance between advantaged and disadvantaged students may be due to differential summer learning. These findings indicate that summer learning is a critical factor in education (Heyns, 1987).

Although summer programs are offered by many schools, most do not offer remedial help. Also, many disadvantaged youth would rather spend their summer months working at a job than in a summer program that does not offer wages. Results of the Youth Incentive Employment Program demonstration indicate that providing dropout-prone youth with jobs alone does not help keep them in school (Gueron, 1984). And while work experience during high school pays off in future earnings and employment (Meyer and Wise, 1980), the effects are strongest for youth who graduate from high school (Berlin, 1983; Berlin and Sum, 1988; Taggart, 1981).

STEP provides youth with both work experience and remediation. Since youth are paid for hours spent at both activities, they are drawn into a program designed to stem dropout rates.

Adolescent parenthood is the second predictor of dropout behavior addressed by STEP. Although poor academic performance is the most commonly cited reason for leaving school, studies show that as many as three-quarters of female dropouts cite pregnancy and/or marriage as their reason for leaving school (Coombs and Cooley, 1968; Presser, 1978). A number of studies conclude that girls who become mothers "while they are in junior high school or high school complete on average fewer years of school, are less likely to earn a high school diploma, and are less likely to go on to college and graduate study than those who delay childbearing until their twenties" (Hayes, 1987). The consequences of adolescent parenting fall heavily on females, but research findings also indicate that teen fathers are more likely than other males to drop out of high school prior to graduation (Hayes, 1987).

The causal relationship between teen parenting and educational attainment is a complex one, particularly among females. Young

women who have the lowest level basic skills are many times more likely to become mothers (Dryfoos, 1990). There is evidence that many adolescent mothers drop out prior to pregnancy and that becoming pregnant may be precipitated by poor academic performance.[4] As Dryfoos (1985) points out, "if motherhood follows unsuccessful school experiences, it may be possible that upgrading the quality of education would have an impact on fertility rates."

A number of studies attest to the difficulty of a successful intervention once the first pregnancy has occurred--the most recent being a series of evaluations of Project Redirection (Quint and Riccio, 1985). Project Redirection researchers have concluded that in light of the complexity of the problems faced by young people once they become parents, strong support should be given to program efforts aimed at preventing the first pregnancy.

Preventing the first pregnancy is not easy. The Panel on Adolescent Pregnancy and Childbearing (Hayes, 1987) concluded after their two-year intensive review of research that: "Because there is so little evidence of the effectiveness of the other strategies for prevention, the panel believes that the major strategy for reducing early unintended pregnancy must be the encouragement of diligent contraceptive use by all sexually active teenagers." In addition, they felt that "life options" of teenagers must be enhanced.

With these fertility/education interactions in mind, STEP requires youth to participate in Life Skills and Opportunities (LSO) classes. In these classes, youth are taught about responsible social and sexual decision-making.

In sum, STEP was designed to address two urgent needs that cause youth to be at risk of dropping out of school: educational deficiencies and teen pregnancy. STEP services target those who stand to lose the most over the summer and can most benefit from remedial instruction. Since youth are paid to participate, the opportunity to earn income over the summer is also afforded. Through participation in STEP, at-risk youth receive the benefits of instruction in math, reading and life skills that they need while they earn an income and gain work experience through a summer job.

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[4] For an excellent review of this topic, see Cheryl D. Hayes (ed.), Risking the Future: Adolescent Sexuality, Pregnancy, and Childbearing, a summary of the research findings gathered over a two-year period for a Panel of Adolescent Pregnancy and Childbearing.

STEP'S INTEGRATION WITH THE JTPA SYSTEM

In addition to addressing the needs of at-risk youth, design of the STEP model responded to the policy debate about their education and employment. STEP provides integrated services at a reasonable cost, an early intervention and an innovative approach to helping youth who are not doing well in school.

STEP operates within an already existing program--SYETP, which serves large numbers of disadvantaged youth, primarily through summer work experience. SYETP, authorized and funded under the Job Training Partnership Act (JTPA),[5] has several purposes: to "enhance the basic educational skills of youth; to encourage school completion or enrollment in supplemental or alternative school programs; and to provide eligible youth with exposure to the world of work" (JTPA, Part B, Sec. 251). Because STEP capitalizes on the resources of SYETP, it can be implemented in a large number of jurisdictions for a relatively low incremental cost.

The idea of embedding a remediation component into the SYETP program was a popular public policy approach in the mid-80s. Following an assessment of employment and training programs for in-school youth, the National Research Council's Committee on Youth Employment Programs recommended that SYETP be modified to address basic skills deficiencies among the disadvantaged youth it serves. Specifically, the panel made the following recommendation:

> Attempts should be made to restructure some elements of the Summer Youth Employment Program to systematically test whether SYEP [sic] elements can be used to enhance basic education sufficiently to reduce school dropout rates. . . . Elements of SYEP could be structured so some skills training is added to the pure work experience in order to determine whether such training enhances the long-term employment effects of the program. (Betsey et al., 1985:27)

A year later, in 1986, Congress responded to growing concern about the basic skill levels of disadvantaged youth by passing amendments to JTPA that directly address the panel's suggestions. These amendments mandated that JTPA funds be spent on increasing literacy and reducing high school dropout rates. Although specific funding levels for remedial programs are not stipulated, the amendments require that service delivery areas (SDAs) assess

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[5] JTPA, enacted in 1982, was designed to provide training aimed at increasing the employment rate and earnings of participants. Congress viewed this training as a long-term investment in human capital.

the reading and math skills of all SYETP participants and that each SYETP program include a remedial education component, starting in 1987. STEP, which had operated since 1984 (at least in a pilot form), clearly addressed Congress's concerns.

THE STEP DEMONSTRATION MODEL

The model is described here as it was implemented during the demonstration. In the replication sites that have been established since the end of the demonstration, the target population and the components have been essentially the same, except that sites have been encouraged to expand their school year activities.

Target Population

The STEP demonstration model targeted disadvantaged youth who are "at risk" of dropping out of school and becoming adolescent parents. Eligibility was largely determined by SYETP, which paid participants' wages. To meet federal SYETP eligibility requirements, STEP youth had to be at least 14 years old and economically disadvantaged. However, STEP further required that youth be no more than 15 years old, and be educationally deficient but still attending school. Eligible youth were those who performed below their grade level in reading or math according to recent standardized test scores and/or a recent history of grade retention. However, a fourth-grade reading floor was imposed to ensure that youth would have the minimal decoding skills needed to benefit from the remediation curriculum.

STEP was directed at 14- and 15-year-olds for several reasons. First, although these youth are interested in obtaining summer jobs, opportunities for them to do so are limited. Second, most are making the difficult transition from junior to senior high school, a point at which problems often manifest themselves. The dropout rate begins to accelerate as youth enter high school and approach age 16--in most states, the age at which they can "legally" drop out of school. Third, students are thought to be more receptive to remedial efforts at the ages of 14 and 15 than at later ages. Finally, as adolescents, many of these youth are beginning to be sexually active and are therefore running the risk of becoming parents. Thus, their need for information on sexual attitudes and responsible behavior is heightened.

Program Components

With small variations in implementation across the five demonstration sites, the STEP demonstration model involved four key components: remediation, life skills, work experience and school-year support (SYS). During each of two summers, participants were offered approximately 90 hours of remedial instruction in basic reading and math skills, 18 hours of LSO instruction,

and at least 80 hours of work experience. During the intervening school year, SYS activities designed to encourage youth to remain in school were offered. These activities were aimed at helping STEP participants maintain the progress made in the first summer and encouraging them to return for the second summer.

The remediation component provided a minimum of 90 hours of skill-based group and individually paced instruction. The curriculum, Practical Academics, was designed to address two major interlocking problems--deficiencies in basic skills, and a lack of control over the learning process. The remediation plan required that students spend roughly 20 percent of their instructional time in computer-assisted instruction (CAI), which is intended to motivate youth and promote faster learning. In addition, at least 20 minutes each day was spent in silent sustained reading.

Practical Academics was originally built around a set of learning "modules." Each module used topics expected to be of interest to STEP's student population while addressing specific reading and math skills, including categorizing and sequencing written information, distinguishing fact from opinion, interpreting charts and graphs, and solving math word problems.[6]

The modules presented a process for teaching skills through modeled instruction and applied, individualized follow-up activities. Each skill was reinforced at increasing levels of difficulty as students worked up to a level that challenged them. Meanwhile, they receive prompt feedback on skill attainment and progress. CAI and silent sustained reading constituted the remainder of the remediation schedule. This individualized approach was adopted to maximize students' "time on task" by allowing them to work with materials on appropriate skill levels.

Although the modules were complete instructional units, they were designed to accommodate modification or enhancement to meet local needs. In addition to using the modules developed by P/PV, several sites followed P/PV guidelines to develop modules that were approved for local use.

The Life Skills and Opportunities (LSO) component of STEP stressed responsible social and sexual attitudes and behavior. A primary focus of the curriculum was the relationship between

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[6] The original thematic modules included: "Adjusting to the Working World" and "Getting and Handling the Job," which were job skills modules; "Pioneers in Their Field," "Early Explorers" and "World Cultures: Pros and Cons of Arranged Marriages," which were school-based modules; and "Substance Abuse," "Getting the Message from the Media," "Decisions" and "Self-Identity," which were life skills modules.

decisions related to sexual behavior and youth's personal development. For example, it helped youth fully explore the economic, health and social implications of early childbearing. In addition to enhancing students' understanding of the consequences of parenting, the LSO curriculum discussed the benefits of delaying sexual involvement and explained how to consistently and effectively use various methods of contraception. Job-equality issues and the consequences of AIDS and substance abuse were also covered.

During the first summer of their participation, students received approximately 18 hours of LSO instruction. Lectures, discussions, films, role-plays, field trips and outside speakers were used to stress to youth the importance of planning for their future, while considering both their goals and responsibilities. For example, youth learned the importance of deciding whether to have sex or abstain from sexual relations. If they did choose to have sexual intercourse, they learned how to have sex responsibly. Volume II of the curriculum, used in the second summer of the program, provided an additional 18 hours of instruction that reinforced information covered in the first summer and introduced new material on substance abuse, general health issues and career planning.

In the **work experience component** of STEP, youth were usually assigned to half-time jobs, either in their neighborhood or near their remediation site in order to minimize transportation problems. On average, STEP youth were younger than the majority of SYETP participants; thus, their inexperience and child labor law requirements limited the types of jobs available to them. Most STEP youth were placed in maintenance, recreation, clerical and child care aide positions. A few were assigned to special group projects, such as producing a newspaper or painting a mural. Sites attempted to give youth "better" jobs during the second summer. All were paid minimum wage for their participation in both work experience and remediation/life skills instruction.

The school year support (SYS) component spanned the period between participation in the first and second summers of STEP. It provided ancillary support to the school program, support that involved periodic group activities and individual contact between youth and a designated counselor/advocate. If possible, counselors/advocates aimed to help youth remain in school by contacting them a minimum of twice a month, by either providing or referring them to needed tutorial and/or counseling services and by monitoring their school attendance and performance. They also encouraged youth to return for a second summer in STEP.

Parental involvement in SYS was encouraged through periodic informational mailings and meetings held at the beginning of the school year. Parents were encouraged to assist in organizing and

conducting group activities, and all parents formally approved the participation of their child in STEP.

SYS was the least successfully implemented component of STEP during the demonstration. Advocates found it extremely difficult to keep in contact with the participants, and involvement in SYS was very uneven across sites. In general, involvement was much lighter than expected.

STEP IN THE DEMONSTRATION SITES

Site selection for P/PV's STEP demonstration was based primarily on the prospective lead agencies' management and administrative experience and capacity; the degree of cooperation secured from relevant institutions, notably the JTPA agency and the school district; the extent of the local resource commitment; their ability to serve adequate numbers of youth, of whom a large proportion would be minorities; and their willingness to undertake random assignment, collect the data and carry out other requirements of the research design. Following a careful review and screening process, P/PV selected five sites: Boston, Massachusetts; Fresno, California; Portland, Oregon; San Diego, California; and Seattle, Washington. (The sites' management structures are described in Appendix A.)

Collaboration between educational institutions and employment and training organizations is essential to implementing the STEP model. School systems, Private Industry Councils (PICs) and city agencies must work cooperatively toward achieving the simultaneous goals of improving participants prospects for high school graduation and preparing them to enter the labor market. In most of the five sites, the key agencies had worked together to some degree prior to STEP. However, the intensity and complexity of the STEP model represented a challenge for most sites.

While many of the demonstration's start-up problems were reduced or eliminated in time for the second summer (1986), additional problems were created by the introduction of the Practical Academics curriculum, which was not available in the start-up year. Since it was important that instructional emphasis remain on academic skills (i.e., reading and math) rather than on module content topics, teachers needed more training and guidance in using the learning modules, which were an unfamiliar instructional format. Integrating the CAI exercises, skill reinforcement activities and materials for silent sustained reading into skills instruction dictated by the modules also posed problems.

With each passing year of the demonstration, the delivery of the program components improved as both the institutions and the staff became more familiar with their roles. Despite growing familiarity with the many elements of the model, however, some problems persisted year after year. One such problem was the

recruitment and reenrollment of STEP youth. The need for early outreach and recruitment efforts echoed throughout the demonstration. Sites with expanding economies, in particular, had to develop creative strategies to lure youth into and back to the program. In the following sections, we review the implementation of each component.

Remediation

While P/PV was pleased with the general implementation of the STEP model during the build-up year, and with research findings for the first cohort of participants, we significantly redesigned and strengthened the remediation component following the first summer of operations.

During the first summer of the demonstration, the remediation curriculum framework (CORE) provided guidelines and detailed information for sites to use in developing local curricula. However, sites soon found that they had neither the time nor expertise to develop a new curriculum for STEP teachers at the local level. Thus, for Summer 1986, P/PV developed the scripts for topic-based learning modules that would be the foundation for the local remediation programs.

Practical Academics retained CORE's basic instructional approach of a skill-based, individually paced curriculum, but its learning modules put basic skills instruction in the context of student-centered issues. Modules focused on critical thinking skills and included applied learning activities. Sites selected supplementary skill reinforcement, CAI materials and books for silent sustained reading. To further strengthen the planning and implementation of the remediation component of STEP, each site was required to hire a lead teacher who was responsible for the overall management of remediation, beginning with curriculum development in the spring and continuing through the summer program. While sites were encouraged to modify and enhance the curriculum modules for local needs, the introduction of Practical Academics greatly reduced the responsibility of local developers.

Improvements made to the remediation component in 1986--lead teachers, silent sustained reading, improved teacher training, and increased technical assistance--were employed through the rest of the demonstration. Across sites, teachers assessed the instructional approach positively. Students' displayed high levels of attentiveness in remediation sessions and attended classroom activities an average of 85 percent of available time.

Life Skills and Opportunities

From the beginning of the demonstration, LSO was enthusiastically received by both teachers and students. The first volume, used for first-year participants, was highly regarded. The second-

summer volume, though also well-received, was found to require some revision, mostly in the sessions concerning careers, health and nutrition, and substance abuse. Revisions were made for the summers of 1987 and 1988.

Work Experience

The work experience component of STEP was fairly well-implemented during the demonstration. At first, it was difficult to convince employers to hire STEP youth, both because they were younger than employers were accustomed to hiring, and because the youth needed part-time jobs. However, once employers became familiar with the STEP program, they were no longer resistant to the idea of hiring younger youth, including STEP treatments, who were available only part time. Many worksites continued to hire STEP youth throughout the demonstration years.

School-Year Support

Like the remediation component, the school-year support (SYS) component was significantly altered following the first year of operation. During the 1985-86 school year, four of the five demonstration sites operated an SYS program. However, the programs provided few real services and attendance at group events was poor. In response to the disappointing results of the first year, P/PV made structural and management changes for the 1986-87 SYS program and intensified its oversight. Each site was required to hire an SYS coordinator, who was responsible for planning and managing SYS activities; hiring, training and supervising counselors/advocates; and planning group activities. Counselors/advocates were responsible for making individual contacts with youth, linking them to social services as needed, and monitoring the youth's school performance.

Further improvements were made following both the second and third summers. These improvements included having youth choose a "favorite teacher" mentor, publishing newsletters, providing tutoring, and developing individual year plans for each student. The improvements led to more uniformity in the level of contact with the youth across sites and more services for youth. But implementing SYS continued to be a challenge. For example, tutoring, a service identified by sites as critical, was difficult for many sites to provide because of a series of logistical problems, including transportation barriers, scheduling problems and the availability of tutors. Also the youth themselves often did not recognize their own need for tutoring. Participation in group activities continued to be disappointing as well, and rates of exposure to SYS activities remained lower than desired.

IN-PROGRAM AND EARLY POSTPROGRAM RESEARCH FINDINGS

Since STEP's inception, P/PV has been evaluating the program. The research components of the STEP demonstration addressed both the feasibility and effectiveness of providing services that build on the existing SYETP. During the four-year operational phase, we examined the feasibility of implementing a multiple-service, low-cost intervention. SDAs had expressed serious concerns regarding implementation of remedial components in their summer youth programs, since this was a new service area for them. We found that STEP was operationally feasible and reasonably inexpensive.

Analysis of in-program outcomes has shown that STEP successfully produced significant short-term benefits for participants in both educational and fertility-related outcomes.

In participants' first summer of STEP, the substantial learning losses that typically occur over the summer for this population were mitigated. Treatment youth outscored their control group counterparts in both reading and math by approximately half a grade equivalent at the end of the first summer. Cohort II treatment youth outscored control youth by six-tenths of a grade equivalent in reading and eight-tenths of a grade equivalent in math. Cohort III treatment youth outscored their control counterparts by five-tenths of a grade equivalent in reading and six-tenths of a grade equivalent in math. When the scores of the two cohorts are analyzed together, the first-summer impacts are a four-tenths of a grade equivalent advantage in reading for treatments and a five-tenths of a grade equivalent advantage in math.[7]

The first summer of STEP participation also had a substantial impact on youth's knowledge of contraceptive information. Knowledge of birth control methods and availability significantly increased for both cohorts in every site, racial/ethnic and gender subgroup. For Cohort II treatment youth, this improved knowledge may have translated into behavioral changes. Sexually active STEP participants were 53 percent more likely to report using contraception than their control counterparts at the conclusion of the summer program. However, increased contraceptive use was not found among Cohort III participants.

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[7] The grade equivalent impacts for the aggregate sample (Cohorts II and III combined) cannot be obtained by averaging the cohort-specific grade equivalent impacts. Grade equivalents are not linear measures and cannot be analyzed directly. We analyzed the scaled scores first, then converted the impact on scaled scores to a grade equivalent.

Approximately three-fourths of the treatments returned for a second summer of STEP. Because controls were not asked back for a second summer except in San Diego and Seattle, second-summer educational impacts (treatment/control differences) could not be calculated for the sample as a whole.[8]

In San Diego and Seattle, however, we could estimate second-summer impacts. We found in those two sites that Cohort II second-summer treatments outscored second-summer controls by seven-tenths of a grade equivalent in reading and by eight-tenths in math. In Cohort III, however, analysis of San Diego and Seattle second-summer youth yielded no significant impact in either reading or math.

Examining changes in test scores over the whole 15-month program, we found that Cohort II treatment youth in San Diego and Seattle outscored control youth by nine-tenths of a grade equivalent in reading and seven-tenths in math over the 15 months. Among Cohort III San Diego and Seattle youth, the 15-month impact was five-tenths of a grade equivalent in reading and four-tenths in math. While we must be cautious in generalizing from only two sites, these in-program results are encouraging.

Information from the three-month follow-up interview, available for all treatments and controls, showed that over the 15 months, STEP youth significantly increased their knowledge of birth control beyond what they would have gained as part of normal maturation and/or as the result of any sex education received in school. In addition, treatment youth retained most of what they learned in the first summer of STEP over the subsequent year.

Thus, STEP was shown to be operationally feasible and to have significant in-program impacts on math and reading ability, and fertility-related knowledge. These findings and the Congressional mandate to provide remediation to SYETP youth have led over a hundred SDAs to adopt the STEP program.

Whether the short-term impacts continue and ultimately affect critical factors like graduation rates, employment and pregnancy rates, is the question to be addressed in the remainder of this report. The research design used in the evaluation of STEP is described in Chapter II. The youth from Cohorts II and III who participated in follow-up interviews are profiled in Chapter III. Postprogram impacts are discussed in Chapter IV.

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[8] P/PV considered testing controls during their second summer, but the pilot experience showed that without providing controls with a job during the second summer, very few would show up to be tested.

II. RESEARCH DESIGN

The evaluation of STEP was designed to assess both the impacts of the program on participants and the feasibility of implementing the model in various settings and on a large scale. Research began with the pilot program in 1984 and will continue through 1993--five years after completion of the four-year operational phase of the demonstration.

To date, P/PV has published its findings with respect to the viability and replicability of STEP by describing the processes involved in planning, coordinating and operating the program--including problems encountered and their solutions. Earlier reports also present the in-program impacts of STEP on its participants' academic performance and development of responsible social and sexual attitudes and behavior.

This report addresses the postprogram impacts. STEP was designed to reduce the occurrence of adolescent parenthood, decrease the proportion of youth who drop out of high school, and increase high school graduation rates. Ultimately, P/PV hoped that these impacts would translate into improved early labor force experiences. Analyses conducted for this report address these longer-term outcomes.

IMPACT ANALYSIS

The impact analysis assesses the effect of STEP participation on educational, fertility-related and work-related outcomes. Program impacts are determined by comparison of outcomes for treatment youth with those for the randomly assigned control group who participated in SYETP work experience only. The ultimate concerns of STEP are high school graduation rates, teen parenting and youth employment; however, outcomes in related areas are also observed and analyzed--at multiple points in time.

Beginning with the school year following STEP participation, we examine whether treatment youth:

- Exhibit improved academic performance during the school year; and
- Experience a reduction in year-to-year school dropout rates.

From the postprogram interviews, we examine whether STEP treatment youth:

- Have a greater knowledge of effective contraceptive practices;

- Have a greater tendency toward reduced levels of sexual activity and/or improved contraceptive use;
- Experience fewer cases of adolescent pregnancy and parenthood;
- Have increased high school graduation rates; and
- Achieve increased employment and earnings in their early labor force experience.

This report presents all these postprogram impacts on outcomes for Cohorts II and III.

DESIGN STRATEGY

During the STEP demonstration, P/PV employed an experimental design involving random assignment of eligible youth to treatment and control groups. After determining youth's program eligibility and obtaining parental consent,[9] P/PV's site coordinators followed uniform guidelines to randomly assign half the eligible youth at each site to the STEP treatment group, which divided its time between classes and work experience. The remaining youth were assigned to the control group and spent all program hours in work experience provided by SYETP. Both treatment and control youth were compensated at minimum wage for all program hours, whether work experience or instructional time.

Random assignment is critical to the research design and the determination of program impacts. Because eligible youth were randomly divided between the treatment and control groups, both groups are statistically equivalent on observed and unobserved characteristics at baseline. Thus, the experience of the control group is indicative of the experience treatment group members would have had in the absence of STEP. Therefore, we can be fairly confident that differences in outcomes that arise between the groups are attributable to the program and not to other factors.

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[9] Before youth were accepted, written parental consent was obtained. Parents or guardians agreed to allow youth to participate in a research study involving random assignment and to participate in remediation and life skills components if assigned to the treatment group. Parental consent was also needed for schools to release youth's records. While youth were informed of their selection for STEP and a summer job as soon as the eligibility determination process was complete, the specific assignment to treatment or control status was withheld until completion of the pretest.

During the demonstration, a combined analysis sample of 3,226 treatment and control youth was selected. The sample was built up over two years, with between 300 and 330 youth enrolling each year in each site. A sample of this size enables us to be reasonably certain policy-relevant impacts will be detected, given reasonable assumptions regarding the explanatory power of the variables in the multivariate analysis and sample attrition over the course of the follow-up period.

DATA COLLECTION STRATEGY

The data on the individuals in the STEP sample have been and will continue to be assembled from a number of different sources and at various points throughout the demonstration. (See Table II.1.) Among the sources of data are participants' performance on standardized tests, responses to self-administered questionnaires, program records, school records, and questionnaires completed by remediation and life skills instructors. Beyond the demonstration operational phase, data are being collected through school records and telephone or in-person interviews conducted with both treatment and control youth.

Academic Testing Over the Summer

The reading and math subtests of the Intermediate Level Metropolitan Achievement Test (MAT) Survey Battery provide the primary measures of academic performance over the summer.[10] These subtests, consisting of 60 items in reading comprehension and 50 items in math, provide nationally normed performance measures. The intermediate-level test is most appropriate for students performing at the fifth- to seventh-grade level. The intermediate level was selected based on the finding that STEP youth in the pilot study generally attained scores in this range at baseline.

Once random assignment was completed, both treatment and control youth took the MAT to assess baseline reading and math abilities.[11] Youth were retested at the end of the first summer. To protect against memory effects, participants were administered one form of the MAT as a pretest and another as a posttest. The two forms--Forms JS and KS--are functionally equivalent; in other

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[10] The MAT was used to test the youth because it was an academic test that was nationally normed on school aged youth that was respected in the education community. The TABE, a test more commonly used in employment and training programs, was not used because it is normed to an adult population.

[11] Although random assignment occurred before testing, neither proctors nor students were informed of the assignments until after testing.

Table II.1

THE RELATIONSHIPS BETWEEN CALENDAR TIME, PROGRAM TIME, AND INTERVIEW TIME BY COHORT

	SY 1986-87				SY 1987-88				SY 1988-89				SY 1989-90				SY 1990-91			
	Summer	Fall	Winter	Spring	Summer	Fall	Winter	Spring	Summer	Fall	Winter	Spring	Summer	Fall	Winter	Spring	Summer	Fall	Winter	Spring
Cohort II																				
Months Post-Enrollment	0-3	4-6	7-9	10-12	13-15	16-18	19-21	22-24	25-27	28-30	31-33	34-36	37-39	40-42	43-45	46-48	49-51	52-54		
Program	(1st Summer)——SYS——(2nd Summer)																			
Proximate Grade	—9/10————					-10/11————				-11/12————				-12/Graduate————				—Graduate———		
Age	————14-16————					————15-17————				————16-18————				————17-19————				————18-20————		
Cohort III																				
Months Post-Enrollment					0-3	4-6	7-9	10-12	13-15	16-18	19-21	22-24	25-27	28-30	31-33	34-36	37-39	40-42	43-45	46-48
Program					(1st Summer)——SYS——(2nd Summer)															
Proximate Grade					—9/10————					-10/11————				-11/12————				-12/Graduate———		
Age					————14-16————					————15-17————				————16-18————				————17-19————		
Interviews						Wave I Interview (Cohort II only)				Wave II Interview								Wave III Interview		

words, the score attained is independent of the form being administered.

Although the initial research design called for testing both treatment and control youth in the second summer, the pilot study experience resulted in revision of this plan. That experience indicated that without the offer of employment during the second summer, few control youth returned for testing. The expense and effort of locating, recruiting and testing controls on a demonstration-wide basis could not be justified. In Cohort II and III, San Diego and Seattle decided to offer control youth a second summer of work experience; thus, both groups were tested in the second summer in these sites. Test score and questionnaire data were not collected during the second summer for controls in other sites.[12]

To minimize the possibility that control youth would have less motivation to perform at the posttest than treatment youth--and would thereby spuriously elevate the estimate of program impact--an incentive scheme was devised to provide both groups with an external reason for doing their best. Any youth taking the MAT received one chance in a drawing for one of 15 $50 prizes awarded at each site. An additional chance was received for each correct answer. Thus, a youth could maximize his/her chances of winning by correctly answering as many questions as possible; any youth responding at random, drawing designs on the answer sheet or simply not answering the questions would decrease his/her chances of winning. This incentive was available to both treatment and control youth at both the pretest and posttest. Assuming that these incentives had their predicted effects, any control group losses observed were the result of true losses in academic skills.

In addition to this regulation of students' motivation to perform well on the tests, care was taken to ensure that testing conditions were identical for treatment and control youth. Sites were required to use the same locations and proctors for treatment and control youth. In most cases, treatment and control youth were tested side by side in the same classrooms.

Questionnaires

Self-administered questionnaires completed by youth in conjunction with testing at the beginning and end of the first and second summers provide data on demographic characteristics and on measures of attitudes, knowledge and behavior that are relevant

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[12] Although questionnaire and test data were not collected for most control youth in the second summer, school records and interview data are being collected for all treatment and control youth during the longitudinal phase of the study.

to the program. Demographic data collected include household composition, languages spoken, grade enrollment, grade retention and absenteeism. Attitudes and knowledge measured include self-esteem, educational expectations, occupational/career aspirations, awareness of the consequences of adolescent pregnancy and birth control knowledge. Responses to various components of STEP are measured using the endline questionnaires.

School Records

Data from high school transcripts are essential in determining the long-term effects of program participation on school performance, retention and high school graduation. During the early years of the demonstration, P/PV obtained data from the school districts' MIS departments. Since that time, we found that these automated data were often incomplete and misleading, as is discussed more fully in Appendix E. Thus, starting in 1990, school districts in the participating demonstration sites have agreed to provide transcript data for the duration of the research phase of the project, including standardized test scores, grade levels, and dates of withdrawal, transfer and/or graduation for each youth in the STEP sample. In addition, the schools provided data on local policies and procedures that facilitate interpretation of student records. They also lend assistance in tracing and gaining access to the records of students who transfer to other school districts.

Data are collected for a youth's entire high school experience. To date, we have the transcripts for Cohort II and III members through school year 1989-90. The school districts provide a transcript on each youth, whether or not he or she is still enrolled. For students who transfer out of their school district, information up to the time of the transfer is provided. To the extent possible, P/PV tries to obtain transcripts for youth who have transferred out of the district. The original school sometimes provides P/PV with the name and address of the student's new school; sometimes this information is obtained by calling contact people, such as parents or other relatives. Through such tracking methods, we attempt to collect the necessary data and identify the school enrollment status for the highest percentage of youth possible. P/PV will continue to track all students in the STEP sample and collect school data through SY 1991-92.

Follow-up Interviews

After the operational phase of the demonstration, follow-up information on STEP youth from both the treatment and control groups in Cohorts II and III is collected through telephone or in-person interviews. To date, Cohort II youth have been contacted during three postprogram interviews (Wave 1, 2 and 3) and Cohort III youth during two postprogram interviews (Waves 2 and

3). The first postprogram wave of follow-up interviews was conducted with youth in Cohort II in Fall 1987. The second wave was conducted with Cohort II and III between October 1988 and January 1989. The third was conducted between October 1990 and January 1991. P/PV contracted with Abt Associates, Inc. to conduct interviews with STEP youth. In-person interviews were conducted with youth who could not be reached by telephone. The data collected through these interviews address issues of the long-term impact of STEP participation that cannot be determined from school records data. Interviewers gather retrospective information on sexual activity and contraceptive use, pregnancies, children fathered, pregnancy outcomes, employment and earnings history, current employment status and earnings. They also verify educational data, including current enrollment status, grade progression, educational expectations, high school graduation and post-high school education.

Summary of Data Collection Methods

Both in-program and postprogram data were collected for this study. The primary in-program data sources were the summer tests, questionnaires and program records. All youth were scheduled to attend testing sessions at the beginning and end of the first summer of STEP. Treatment youth who returned for a second summer and control youth who accepted second-summer jobs attended additional testing sessions at the beginning and end of the second summer.

Postprogram data include information from the follow-up interviews and high school transcripts. Cohorts II and III were interviewed in Fall 1988 and Fall 1990. A Fall 1987 interview is also available for Cohort II. The youth will be interviewed once again in Fall 1992. Transcripts were collected for all Cohort II and III youth through school year 1989-1990. Final transcripts will be collected in Fall 1992.

ANALYTICAL STRATEGIES

The estimates of program impact presented in this report rely heavily on multivariate analysis. Randomization ensures that the treatment and control groups are similar with respect to both observable and unobservable characteristics at the point of randomization. Since differences that arise in the future can therefore be attributed to the program, simple comparisons of mean outcome measures for the original treatment and control groups provide unbiased estimates of the program impacts. If, however, outcome measures are not available for all members of the original treatment and control groups because of non-response or missing data, simple comparisons can be misleading. Regression analysis provides more precise estimates of program impacts by controlling for differences in observable (baseline and demographic) characteristics between the two groups. By control-

ling for the variation due to baseline variables, the remaining unexplained differences in mean outcome measures can be attributed to the program with greater precision. Details of the analytic strategy are presented in Appendix B.

In our discussion of the results, we indicate whether an impact estimate is statistically different from zero by labeling statistically non-zero estimates as "significant." In this report, the term is reserved for estimates that are not equal to zero at a 0.10 or greater level of significance.[13] "Significant" thus refers to an impact's statistical significance, not necessarily the size or importance of the impact.

When discussing subgroup estimates, a second aspect is also of interest: whether the effect of STEP differs with respect to a particular characteristic, such as race or ethnicity. A test of whether the subgroup impacts are equal to one another was conducted for all subgroup analyses.

SUMMARY OF THE EVALUATION DESIGN

This report considers the impact STEP had on participants' lives in three broad areas: educational achievement, fertility-related behavior, and employment. Information on these outcomes and other important intervening factors has been collected from approximately 3,000 treatment and control group members on an ongoing basis since the time they were randomly given their treatment status. The data analyzed in this report were primarily collected from follow-up interviews (administered by phone or in person) or from school transcripts. For approximately half the sample, the interview was administered four and a half years after program enrollment. For the other half, the interview was administered three and a half years after enrollment. Multivariate analysis of these data provide us with the impact estimates.

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[13] In other words, an impact estimate is "significant" if there is only a one in 10 chance that treatment and controls would differ by the observed amount when in fact the true impact of STEP was zero.

III. PARTICIPANT CHARACTERISTICS AT BASELINE

This chapter describes the baseline characteristics of the youth who participated in the third wave of follow-up interviews, conducted in Fall 1990. As discussed in Chapter II, the research design for STEP includes follow-up surveys of both treatment and control youth in four data collection waves. The baseline data presented in this chapter were collected at the beginning of each cohort's first STEP summer: Summer 1986 for Cohort II and Summer 1987 for Cohort III.

The youth who participated in the third wave of follow-up interviewing are fairly similar to the original group of youth selected to be in the sample, as is shown in the tables of this chapter. More important from an analytical viewpoint, there is no differential attrition by treatment status, as is shown in Appendix C.[14] Therefore, the interview sample is representative of the original sample.

The data confirm that the STEP program successfully selected economically and educationally disadvantaged youth. The youth performed poorly in school, testing well below their age-appropriate grade levels, and came from disadvantaged homes. The data also show that a substantial number were already sexually experienced at age 14 or 15. The targeted youth thus fit the stereotype of students at risk of dropping out of school. The characteristics of the sample are discussed in more detail in the following sections.

COHORT II AND III INTERVIEW SAMPLES

Of the 1,635 youth randomized in 1986 for Cohort II, 77 percent (1,263 youth) were found and interviewed four and a half years later during the third wave of longitudinal surveys.[15] Of the 1,591 youth randomized in 1987 for Cohort III, 85 percent (1,347 youth) responded (three and a half years later) to the Wave 3 interview. As might be expected from the high rate of response, the third wave interview sample is very similar to the youth in the original sample. In all tables in the chapter, characteris-

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[14] Because the characteristics of youth who participated in the third wave of interviews were similar to those of the youth who completed tests and questionnaires at baseline and there was no differential attrition, it was very likely that the estimated impact coefficients would be consistent. Therefore, sample selection corrections were not used in the subsequent analyses.

[15] These numbers refer to surveys completed as of January 31, 1991. However, interviews continued with members of the Wave III sample through June 1991.

tics of both the original and the interview samples of Cohorts II and III are presented to allow the interested reader to compare the two. The discussion, however, focuses on the characteristics of the interview sample. Unless explicitly stated, the characteristics are statistically the same for treatment and control group members.

Demographic and Economic Characteristics

To be eligible for the STEP program, youth had to be 14 or 15 years old at the beginning of the program, meet JTPA standards of economically disadvantaged and be performing below grade level.[16] To ensure that the youth could benefit from the designed curriculum, however, youth had to have at least a fourth grade reading level. The tables in the chapter show that both Cohort II and III fit these criteria. Youth from both cohorts were either age 14 or 15 at enrollment, were mostly members of a minority group and were poor.

As shown in Table III.1, 14-year-olds made up a slightly higher percentage of the interview sample than 15-year-olds. This is appropriate because more 14-year-olds enrolled in the program--particularly in Boston, which specially targeted the younger youth.

While the program recruited approximately equal numbers of males and females, a higher proportion of female enrollees responded at the follow-up: 54 percent of the Cohort II interview sample and 52 percent of Cohort III interview sample were females. Appendix Tables C.2 and C.3, which present treatment and control group characteristics separately, show that female treatments in both cohorts were more likely than female controls to respond to the interview. This tendency is one of the few that differs statistically by treatment status. Within the Cohort II treatment group, 58 percent were female, compared with only 50 percent of the control group. Within the Cohort III treatment group, 56 percent were female compared with only 49 percent of the control group.

Blacks represented the largest racial/ethnic group in either cohort, making up approximately 45 percent of the sample (both cohorts). Asians and Hispanics were roughly equally represented, with both groups accounting for approximately 20 percent of the

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[16] In Fresno, Portland, and Seattle, youth had to test at least one grade level below current level in reading or math. In Portland, the youth could qualify if they tested at least one grade level below current level in language. In Boston, youth had to score between the 20th and 40th percentiles in reading or math and/or have a score on the Degree of Reading Power Test of 25 to 47.

Table III.1

BASELINE DEMOGRAPHIC AND ECONOMIC CHARACTERISTICS OF COHORTS II AND III

	Cohort II		Cohort III	
	Original Sample	Interview Sample	Original Sample	Interview Sample
Demographic Characteristics				
Age	#			
14 Years Old	54.1%	55.4%	58.0%	57.8%
≥ 15 Years Old	45.9	44.6	42.0	42.2
Gender	###		#	
Male	49.7%	46.3%	48.7%	47.7%
Female	50.3	53.7	51.3	52.3
Race/Ethnicity	###		###	
Asian	17.9%	19.8%	17.1%	18.2%
Black	46.2	44.3	50.1	48.2
Hispanic	18.9	19.2	18.7	20.2
Other	17.1	16.6	14.0	13.4
Percentage Having Difficulty With English Language	15.2% *	16.1%	13.7% *	14.4%
Economic Characteristics				
Percentage from Female-Headed Households	49.1% *	47.7%	49.4% ***	47.8%
Mean Household Size	5.1	5.1	5.0 *	5.0
Percentage Who Have Never Worked	69.0%	68.3%	66.6%	67.4%
Sample Size	1,514	1,186	1,454	1,251

Note: Within the original and interview samples, treatment and control means did not differ significantly at the 0.10 level except by gender, which differed significantly between treatment and control groups among the original samples for both cohorts. The sample size given in the table is the number of individuals who were pre-tested. The sample sizes of the other variables differ slightly from these figures.

*** Indicates that the means for the original sample and the interview sample are statistically different from each other at a 0.01 level of significance.

* Indicates that the means for the original sample and the interview sample are statistically different from each other at a 0.10 level of significance.

Indicates that the distribution of the original and interview samples differs with respect to this characteristic at a 0.01 level of significance.

# Indicates that the distribution of the original and interview samples differs with respect to this characteristic at a 0.10 level of significance.

overall sample. The remaining 15 percent were whites and other racial/ethnic groups. There are slightly fewer blacks in the interview sample than were in the original randomized sample because the response rate among blacks was lower than that among any other group.

The distributions of race/ethnicity differed somewhat by site, as illustrated in the tables in Appendix C, reflecting the differences in their local populations. Blacks constituted the largest racial/ethnic group represented in four sites, but Hispanics were the largest group served in Fresno (at 50 percent). In the San Diego and Seattle sites, Asians comprised approximately 30 percent of enrollees. Portland had the largest contingent of whites and other ethnic groups.

The ethnic diversity of the STEP sample produced a population that was multilingual. Approximately 15 percent of the youth had trouble either in reading and writing English or in understanding teachers who instruct in English. As shown in Appendix Tables C.2 and C.3, the percentage experiencing problems with English differed by site, just as the composition of ethnic groups differed by site. San Diego--with its large Asian population--had the greatest percentage of youth who had a problem either reading and writing English or understanding teachers who instruct in English. More than a quarter of San Diego youth had problems with English.

All STEP youth were considered economically disadvantaged by virtue of their eligibility for federally funded JTPA programs. However, just under half of these youth, 48 percent in both Cohort II and Cohort III, were further disadvantaged because they came from female-headed families--a background that typically signals economic hardship.

For two-thirds of youth, STEP provided the first paid work experience, an indication of the limited employment opportunities available to youth of this age group, especially those from disadvantaged socioeconomic backgrounds.

Educational Ability

Depending on their date of birth, 14- and 15-year-old youth who were making normal progress should have been enrolled in the eighth, ninth or 10th grades. As Table III.2 shows, approximately 60 percent of both cohorts were enrolled in the eighth grade. In all but Boston, approximately half the STEP youth were in eighth grade and half in ninth or 10th grade. In Boston, the STEP program greatly emphasized serving exiting eighth-graders. Ninety percent of the youth in Boston were enrolled in the eighth grade in the school year prior to their STEP participation. Overall, the mean grade of enrollment was 8.37.

Table III.2

BASELINE EDUCATIONAL CHARACTERISTICS OF COHORTS II AND III

	Cohort II		Cohort III	
	Original Sample	Interview Sample	Original Sample	Interview Sample
Educational Ability				
Mean MAT Reading Score	720	721	719	719
(Grade Equivalent)	(6.0)	(6.0)	(5.9)	(5.9)
Mean MAT Math Score ***	699	701	708 ***	710
(Grade Equivalent)	(6.7)	(6.8)	(7.0)	(7.0)
Grade in School				
8th Grade & Below	60.1%	60.6%	59.9%	60.0%
9th Grade	34.5	34.1	34.9	34.8
10th Grade & Above	5.4	5.2	5.2	5.2
Grade Retention History				
Pecentage Ever Repeating a Grade	33.2%	33.3%	34.8% **	33.5%
Absenteeism (Year Prior to Enrollment) ##			###	
Absent ≤ 10 Days	75.4%	76.8%	72.9%	74.7%
Absent ≥ 11 Days	24.6	23.2	27.1	25.3
Educational Expectations				
Percentage Expecting to Pursue Post-High School Education	71.3%	71.6%	76.6%	76.4%
Sample Size	1,514	1,186	1,454	1,251

Note: Within the original and interview samples, treatment and control means did not differ significantly at the 0.10 level except for absenteeism. For this characteristic, treatments and controls differed significantly from each other among the Cohort III interview sample. The sample size given in the table is the number of individuals who were pre-tested. The sample sizes of the other variables differ slightly from these figures.

*** Indicates that the means for the original and the interview samples are statistically different from each other at a 0.01 level of significance.

** Indicates that the means for the original and the interview sample are statistically different from each other at a 0.05 level of significance.

Indicates that the distribution of the original and interview samples differs with respect to this characteristic at a 0.01 level of significance.

Indicates that the distribution of the original and interview samples differs with respect to this characteristic at a 0.05 level of significance.

STEP youth exhibited serious deficiencies in basic skills. While the mean grade completed was 8.37, the average test scores on the MAT administered at baseline indicate that youth were performing substantially below their grade levels in both reading and math. Both cohorts of youth read at only a sixth-grade level and were at or near the seventh-grade level in mathematics.[17]

As further evidence of the sample's educational deficiencies, we found that one-third of the youth had been held back in at least one grade before enrolling in STEP. In addition, approximately one-quarter of the youth reported absences in excess of 10 days during the school year prior to STEP enrollment. Both being held back in school and high rates of absenteeism are strong predictors of dropping out.[18]

Although the STEP youth came to the program performing poorly in school, the youth as a group held high expectations for pursuing post-high school education. Over 70 percent of the youth expected to pursue some form of education after completing high school.

Fertility-Related Characteristics

As briefly discussed earlier, the classroom component of STEP includes both academic instruction and instruction in life skills. This section describes the STEP sample in terms of their attitudinal, behavioral and knowledge characteristics on issues central to the life skills component, issues that primarily concern sexuality. To determine knowledge, attitudes and behaviors of youth with respect to these fertility-related issues, a series of questions are asked at baseline.[19]

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[17] The average math score of the interview sample is slightly greater than that of the original sample of youth. The difference is small but statistically significant.

[18] Nonrespondents to the Wave 3 follow-up interview were somewhat more likely than respondents to have repeated a grade and more likely to have missed more than 10 days of school the preceding year.

[19] In consideration of the sensitive nature of many of the questions, questionnaires were processed in a way that ensured the confidentiality of responses. Identification numbers were used on questionnaires rather than names and completed forms were sealed in envelopes by respondents. These measures were taken primarily to ensure confidentiality, but also served to improve the rates of response on typically high non-response items. However, as expected, the response rates on these questions was still lower than those on other types of questions. The sample sizes on these items, noted in Table III.3, were smaller than those noted previously in this chapter.

On entering STEP at age 14 or 15, a substantial proportion of youth reported being sexually experienced. As Table III.3 shows, 42 percent of Cohort II and 44 percent of Cohort III youth reported having had sexual intercourse before joining the program.[20] (On average, youth who were sexually experienced said they began sexual activity at the age of 12.) Of the 463 Cohort II youth and 523 Cohort III youth reporting sexual experience, 49 percent of Cohort II and 50 percent of Cohort III were sexually active in the two months prior to program start-up. Despite the fact that many of the youth had had intercourse, few used contraceptives. Two-thirds of the recently sexually active youth said they had not used any form of contraception during their last intercourse.

This lack of sexual responsibility occurred even though 69 percent of Cohort II and 73 percent of Cohort III youth had previously taken a sex education course. Among Cohort II youth, 14 girls and 11 boys reported having been pregnant or responsible for a pregnancy prior to enrolling in STEP. Twelve of these youth had a child prior to enrolling in STEP. Among Cohort III youth, 27 girls and 15 boys reported having been pregnant or responsible for a pregnancy prior to enrolling in STEP. Thirteen Cohort III youth had a child before enrolling in the program.[21]

Not surprisingly, given the limited use of contraception, the youth displayed limited knowledge on issues related to birth control. On a series of eight items testing their knowledge of the availability of various contraceptive methods, youth correctly answered an average of only three. On nine general birth control knowledge items, an average of four were correctly answered. The youth appeared to retain little from their previous sex education courses.

SUMMARY OF PARTICIPANT CHARACTERISTICS

Baseline data indicate that youth enrolled in the program were a group whose need for an intervention like STEP was evident. With below-grade-level test scores (youth averaged a sixth-grade level in reading and seventh-grade level in math), frequent grade retention histories and sporadic school attendance patterns, these youth were likely to drop out of school unless they could be helped academically. Worsening their likelihood of completing

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[20] The percentage of youth who were sexually experienced was significantly lower among the Wave 3 respondents than the nonrespondents.

[21] More control group members had a child at baseline (10) than treatment group members (3). This difference is statistically significant at a 0.05 level.

Table III.3

BASELINE FERTILITY-RELATED CHARACTERISTICS OF COHORT II AND III
(Sample sizes given in parentheses)

	Cohort II			Cohort III		
	Original Sample		Interview Sample	Original Sample		Interview Sample
Percentage With Prior Sexual Experience	44.7% (1,409)	***	41.9% (1,106)	45.9% (1,389)	***	43.7% (1,196)
Percentage Sexually Active in Past Two Months (of those with sexual experience)	51.3% (669)		48.5% (489)	51.9% (618)		50.3% (507)
Pecentage Not Using Contraception When Last Sexually Active	63.8% (670)		64.6% (491)	65.3% (686)		65.0% (566)
Number of Females Ever Pregnant	18		14	33		27
Number of Males Ever Fathered a Child or Reponsible for a Pregnancy	19		11	19		15
Number Married	8		4	7		5
Number with Children	17		12	14		13
Percentage Who Took A Sex Education Course Previously	69.3% (1,425)		68.6% (1,115)	72.6% (1,392)		72.7% (1,200)
Mean Number Correct on Birth Control Availability Questions (8 Items)	3.09 (1,517)		3.03 (1,189)	3.36 (1,431)	**	3.32 (1,231)
Mean Number Correct on General Birth Control Knowledge Questions (9 Items)	4.08 (1,488)		4.06 (1,164)	4.32 (1,428)		4.29 (1,230)

Note: Within the original and interview samples, treatment and control means did not differ significantly for most of the characteristics listed above. The percentage with prior sexual experience and the percentage who took a sex education course previously differed at the 0.10 level for the original sample of Cohort III. The number of respondents with children differed between treatments and controls among the interview sample of Cohort III.

*** Indicates that the means for the original sample and the interview sample are statistically different from each other a 0.01 level of significance.

** Indicates that the means for the original sample and the interview sample are statistically different from each other a 0.05 level of significance.

high school was an early and high incidence of sexual activity and their limited knowledge of birth control.

The youth in the two cohorts had very similar baseline characteristics. In both cohorts, a slight majority were female, they were racially and ethnically diverse, and they possessed substandard basic skills. Their school participation characteristics were similar, and comparable proportions of the two cohorts were sexually experienced. The two cohorts' contraceptive knowledge and behavior were also similar.

Thus, youth in both cohorts were drawn from the pool of youth who were educationally and economically disadvantaged.

IV. POSTPROGRAM IMPACTS

This chapter presents STEP's postprogram impacts on measures of education, reproductive behavior, early employment and welfare use for Cohorts II and III. Data on these outcomes were obtained primarily from the latest follow-up interview data, collected between October 1990 and January 1991. The analysis of educational impacts was supplemented with data from students' high school transcripts. The employment analysis was supplemented with Cohort II interview data collected in the previous interviewing wave, between October 1988 and January 1989.

For Cohort II youth, who enrolled in STEP in June 1986, the 1990-91 follow-up interview provides information on educational, fertility- and employment-related behavior approximately four and a half years (54 months) after STEP enrollment--three and a quarter years (39 months) after participants' putative program exit. For Cohort III youth, who enrolled in June 1987, the 1990-91 interview provides information on behavior approximately three and a half years (42 months) after enrollment, or two and a quarter years (27 months) postprogram.

The majority of the Cohort III youth who were observed 39 months postprogram were between 17 and 18 years old. Approximately half of them would have graduated if they had completed one grade each year after entering STEP; the other half would have been in 12th grade. Cohort II youth were one year older, so were 18 or 19 years old and nearly all were scheduled to have graduated.

This chapter presents analyses conducted on educational outcomes, fertility-related outcomes, welfare receipt and employment. There is no evidence from these analyses that STEP had any significant long-term impacts on participants. These results are quite consistent across race/ethnicity, gender, site, grade level and cohort subgroups.

Because STEP had no impact on most of the outcomes we considered, the results of many of the analyses are simply summarized here, highlighting the principal outcomes and those that have puzzling patterns of results. Full presentations of the results can be found in Appendix H.[22]

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[22] For instance, the analyses of STEP's impact on grade progression, test scores, employment, earnings, and hours are presented in Appendix H.

EDUCATIONAL IMPACTS

STEP had little or no positive impact on youth's educational experience:

- Treatment and control youth were equally likely to drop out by the time of the interview;
- Those remaining in school performed quite similarly on standardized tests; and
- Treatment youth had completed approximately the same number of grades as controls prior to the interview, and had earned similar numbers of graduation credits.

Fifty-four months after enrollment, Cohort II treatments were actually somewhat less likely to have graduated. On the other hand, 42 months after enrollment, Cohort III treatments and controls appeared equally likely to have graduated. It is possible that Cohort II treatments are progressing through school more slowly--not dropping out, but not yet graduating.

In an effort to explain the lack of impact, P/PV conducted a supplementary study in which a sample of STEP youth were asked whether STEP had led them to switch to a more difficult academic track or take harder courses. Their answers gave no evidence of activities that could be expected to slow academic progress.

Data Sources

For some outcomes--namely grade progression, graduation and dropping out--we examined two sources of data: high school transcripts and the follow-up interview.

The data from the school transcript span the youth's entire high school experience and are useful in providing a longitudinal look at STEP's educational impact. Because the school data were collected in Fall 1990, a maximum of six terms of post-enrollment high school data could be collected for Cohort III and up to eight terms for Cohort II. Thus, all measures presented for Terms 1 through 6 were based on data for both cohorts, while the measures for Terms 7 and 8 were based on only Cohort II.[23]

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[23] In Boston, we were able to obtain transcripts only at the end of each school year, rather than at the end of each term as in the four other sites. This does not cause a problem because we examine cumulative behavior. However, the sample size for the fall terms are smaller than the spring terms by the number of Boston youth included in the spring terms.

A disadvantage of transcript data is that transcripts are often missing for youth who transfer out of a district.[24] In addition, the quality of the school-recorded data is variable by outcome. (A full discussion of these issues is presented in Appendix F.) We conclude that school-based data are useful in corroborating, supporting and enhancing our understanding of STEP impacts, but are likely to under- or overestimate dropout and/or graduation rates.

Interview data enable us to examine the youth's behavior six months later than do the transcript data. In addition, the follow-up interview provides data for all sample members, whether or not they had moved from their original school district. Because the interview data were collected more recently, they were used for the multivariate analysis. However, because the information was self-reported, one must suspect that some respondents presented themselves in a more positive light than was actually merited.

Dropping Out

Two terms after enrollment, more of the treatment group members had dropped out than had control group members. But, eight terms after enrollment, the difference in the cumulative dropout rate for both groups is no longer significant (1.6 percentage points).

Table IV.1 shows that, as one would expect, the dropout rate grew over time. The first column presents the mean cumulative dropout rate for the treatment group at each point in time; the second column presents the difference between the treatment group's level and the control group's level. Thus, the table shows that after Term 2--i.e., at the end of the school year between the two STEP summers--9.6 percent of the treatment group had dropped out of school. The comparable control group rate was 2.2 percentage points lower. While the treatment group's dropout rate remained slightly larger over the period, we cannot reject the hypothesis that the dropout rates of the treatment and control groups were essentially the same after the first year.

The higher dropout rate reported by schools for treatments between their two STEP summers is puzzling and could not be

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[24] Appendix Table G.9 presents the number of transcripts available each term. One would expect the sample to fall over time as youth drop out and graduate, thus, Table G.9 also presents the number of transcripts that are inexplicably missing. By Term 6 (the last term for which we have information for both cohorts), 5 percent of the transcripts that should have been present were missing. However, throughout the eight terms, there was no systematic treatment and control difference in the rates of missing data.

Table IV.1

CUMULATIVE DROPOUT RATES AND STEP TREATMENT/CONTROL DIFFERENCES

Terms After STEP Enrollment	Treatment Group Mean	Control Group Mean	T/C Difference (in percentage points)
In-Program			
Term 1	6.6%	4.4%	2.2**
Term 2	9.6	7.6	2.2**
Postprogram			
Term 3	13.0	11.4	1.6
Term 4	18.0	17.0	1.0
Term 5	20.6	21.1	-0.5
Term 6	27.9	26.2	1.7
Term 7	28.1	26.4	1.7
Term 8	31.4	29.8	1.6

Note: The sample size for each term is shown in Appendix Table G.9. Data for terms 7 and 8 based on Cohort II youth only.

** Indicates that the impact is statistically different from zero at a 0.05 level of significance.

corroborated by other school data on grade-level enrollment, which showed no discernible difference between treatments and controls. (See Appendix Table H.1.)

Self-reported dropout rates are lower than school-based ones. Because data from the schools and the interview are not available for the same time period, however, it is difficult to know precisely how the two measures relate to one another.[25]

Multivariate analysis of the interview data, shown in Table IV.2, supports the finding that STEP had little effect in reducing cumulative dropout rates for the sample as a whole 42 months post-enrollment for Cohort III or 54 months post-enrollment for Cohort II. We examined impacts by subgroups to see whether STEP may have been more effective in reducing dropout behavior among youth who entered the program early, i.e., following eighth grade. The opposite was found. Cohort III youth who entered STEP after ninth grade dropped out at a lower rate than their control counterparts. However, this was not true for Cohort II. As presented in Appendix H, corroborating subgroup evidence was not found for other related outcomes, such as highest grade completed or graduation credits earned. Thus, we do not conclude that STEP was more effective for those entering the program following ninth grade.

Table IV.2 also shows that white and "other" treatments were significantly more likely to drop out of school than their control counterparts. The treatment/control difference reached statistical significance for Cohort II, and while not statistically significant, the pattern was mirrored among Cohort III youth. Again, corroborative evidence from other data on highest grade completed or graduation credits earned was not found, thus we must conclude the pattern is merely a statistical peculiarity, not a true subgroup impact.

Next, we examined reasons for dropping out. These include: 1) school-related reasons, such as disliking school or being unable to keep up; 2) pregnancy/parenting-related reasons; and 3) need for income and other reasons, such as being expelled, being institutionalized, self or family illness or entering the military. Overall, 25 to 35 percent of dropouts reported school-related reasons as primary in their dropout decision; 20 to 25

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[25] As a point of reference, the last school-based dropout rate for Cohort III treatments, Term 6, was 28.0 percent. According to interview data, 22.6 percent of Cohort III treatments had dropped out by the time of the interview (approximately six months later than the last term for which we have transcript data). For Cohort II treatments, the Term 8 school-based rate was 31.4 percent, while Cohort II treatments' reports in their follow-up interviews show that 25.1 percent had dropped out.

Table IV.2

ESTIMATED IMPACTS OF STEP ON HIGH SCHOOL DROPOUT RATES
(42 AND 54 MONTHS AFTER ENROLLMENT)

Group	Treatment Impact, Cohort III 42 Months Post-Enrollment (in percentage points)	Treatment Impact, Cohort II 54 Months Post-Enrollment (in percentage points)
Overall	- 1.0	4.1
Gender		
Male	1.0	5.1
Female	- 2.9	3.2
Race/Ethnicity		#
Asian	- 8.7	- 0.5
Black	- 2.3	- 1.5
Hispanic	- 2.1	8.0
White and Other	6.6	15.6**
Site		
Boston	- 4.0	0.1
Fresno	- 2.7	7.9
Portland	0.4	0.9
San Diego	0.4	0.8
Seattle	1.6	10.1*
Grade Completed at Enrollment	##	
8th	2.4	2.7
9th	- 5.7	6.8
Sample Size	1,335	1,253

Note: The mean dropout rate among Cohort III controls was 22.6%. The mean dropout rate among Cohort II controls was 25.1%.

** Indicates that the impact is statistically different from zero at a 0.05 level of significance.
* Indicates that the impact is statistically different from zero at a 0.10 level of significance.

Indicates that the impact differs with respect to this characteristic at a 0.05 level of significance.
# Indicates that the impact differs with respect to this characteristic at a 0.10 level of significance.

percent cited marriage, pregnancy or parenthood as the primary reasons; and 45 to 55 percent cited the need for income and/or other reasons. In other words, STEP addressed reasons that about half the dropouts reported as primary.

Nevertheless, more Cohort II treatments than controls said they dropped out for the very causes that STEP hoped to affect. On the other hand, Cohort III treatments and controls were equally likely to cite school- or parenting-related issues as a very common reason for dropping out. Among Cohort III self-reported dropouts, 35.8 percent of controls and 36.2 percent of treatments said they simply did not like school. In Cohort II, 25.2 percent of controls and 30.3 percent of treatments said they did not like school. Pregnancy, marriage or parenthood constituted the most important reason for 22.3 percent of Cohort III control youth and 21.3 percent of the treatments. Among Cohort II, 20.0 percent of the controls and 26.6 percent of treatments reported pregnancy, marriage or parenthood as their reason for dropping out. The remaining youth cited job-related reasons, or such reasons as entering the military, being expelled, illness or family responsibilities.

Graduation

The impact of STEP on graduation probability is mixed. Table IV.3 shows that for Cohort II youth, a greater percentage of graduation-eligible controls graduated from high school. Cohort II's treatment group graduation rate was 4.6 percentage points lower than the control group's the first term, and the difference grew to 14.6 percentage points by the end of the second potential graduation year. Cohort III's treatment group rate of 50 percent was approximately the same as that of comparable controls.

Graduation-eligible youth were defined as youth who could have graduated at a particular point in time. Cohort II youth, who started STEP after having completed ninth grade (as of SY 1985-86), could have graduated at the end of SY 1988-89, assuming they progressed one grade each year after they had enrolled in STEP. Similarly, Cohort II youth who enrolled in STEP after eighth grade and Cohort III youth who enrolled after ninth grade were in the class of 1990.

The graduation rates shown in Table IV.3 are based on school data and are likely to underestimate the percentage of youth who actually graduate because some youth move out of their original school districts to parochial school or other school districts. Although we attempted to collect school records for all students, including those who had moved, we were less successful with those who had left the district. In addition, the interview data cover six more months than the school data. Thus, we also examined

Table IV.3

CUMULATIVE GRADUATION RATES AND STEP TREATMENT/CONTROL DIFFERENCES

Possible Graduation Term	Percentage of the Treatment Group Who Had Graduated	Percentage of the Control Group Who Had Graduated	T/C Difference (in percentage points)
First Term			
Cohort II	38.7%	43.3%	-4.6*
Cohort III	50.0	48.9	1.1
Second Term			
(Cohort II only)	40.4%	50.2%	-9.8**
Third Term			
(Cohort II only)	43.7%	58.3%	-14.6***

Note: The sample size on which the differences are based are 1713 youth for the first term (1217 from Cohort II and 496 from Cohort III); 479 youth for the second term and 510 youth for the third term. (Boston youth were not available for the second-term averages.)

*** Indicates that the impact is statistically different from zero at a 0.01 level of significance.

** Indicates that the impact is statistically different from zero at a 0.05 level of significance.

* Indicates that the impact is statistically different from zero at a 0.10 level of significance.

self-reported graduation status, recognizing that the self-reported rate may overstate the actual rate.[26]

The analysis of the self-reported graduation status is shown in Table IV.4. By the Fall 1990 interview, all of Cohort II and approximately half of Cohort III could have graduated. The self-reported graduation rate among Cohort III treatments who could have graduated was 62.4 percent; among Cohort II treatments, the rate was 63.9 percent.

Table IV.4 shows that the 42-month Cohort III impact was essentially zero and that there were no differential impacts by subgroup (in part because the sample size was small). As we found from the transcript data, however, the Cohort II treatments were less likely to graduate. Cohort II treatments were 5.8 percentage points less likely to graduate than comparable controls. When subgroup impacts were examined, it was found that the estimated impacts among Hispanic and "white and other" youth were numerically the most negative. However, we cannot reject the hypothesis that the race/ethnicity subgroup impacts were actually all equal.

As discussed in the beginning of this chapter, Cohort II treatments' lower propensity to graduate than controls is perplexing. They are not dropping out, but neither are they graduating. An additional interview conducted with a sample of treatments, asking about how STEP may have changed their academic course, provided no good answers.

Non-High School Education

New educational doors may open to youth once they leave high school. For dropouts, GED programs, technical schools and other training programs are the main alternatives. Table IV.5 shows that 15.8 percent of dropouts among Cohort II treatments and 21.6 percent of those in Cohort III were pursuing a GED certificate at the time of the interview. An additional 10.9 percent of Cohort II treatment dropouts and 9.0 percent of Cohort III treatment dropouts had already obtained a GED certificate and were pursuing no other educational activity. Approximately 11 percent of Cohort II and III dropout treatments pursued some other form of education. Multivariate analysis of GED receipt found no difference between the treatments and controls. (See Appendix H.)

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[26] Another reason school-based and self-reported graduation rates may differ is that the youth may report having received a diploma when in fact they received an alternative credential, such as a certificate of attendance. See NCES's 1988 Dropout Report (Frase, 1989) for a full discussion of alternative credentials.

Table IV.4

ESTIMATED IMPACTS OF STEP ON GRADUATION RATES
(42 AND 54 MONTHS AFTER ENROLLMENT)

Group	Treatment Impacts, Cohort III 42 Months Post-Enrollment (in percentage points)	Treatment Impacts, Cohort II 54 Months Post-Enrollment (in percentage points)
Overall	2.4	-5.8*
Gender		
Male	7.1	-4.2
Female	-1.9	-8.0
Race/Ethnicity		
Asian	25.3*	-4.4
Black	2.6	-0.5
Hispanic	-1.6	-13.2*
White and Other	-12.0	-12.2
Site		
Boston	0.8	-1.7
Fresno	2.4	-7.3
Portland	-6.8	-7.1
San Diego	10.7	-5.7
Seattle	4.1	-6.7
Grade Completed at Enrollment		
8th	n.a.	-4.2
9th	n.a.	-8.0
Sample Size	539	1,119

Note: On average, 62.4% of Cohort III controls who could have graduated did so by their Wave III interview, while 63.9% of Cohort II controls did so.

* Indicates that the impact is statistically different from zero at a 0.10 level of significance.

Table IV.5

EDUCATIONAL ACTIVITES OF DROPOUTS AT THE TIME
OF THE WAVE III INTERVIEW

	Cohort III Dropouts		Cohort II Dropouts	
Activity	Treatments	Controls	Treatments	Controls
Technical School	2.4%	3.1%	1.8%	4.2%
Pursuing a GED	21.6	19.6	15.8	16.3
College	4.2	5.5	3.6	3.2
Training Program	4.8	4.9	5.4	2.1
Nothing	67.1	66.9	73.3	74.2
Has a GED	9.0	5.5	10.9	14.2
Doesn't have a GED	58.1	61.3	62.4	60.0
Sample Size	167	163	221	190

Note: Columns may not total 100% due to rounding.

Approximately half of the high school graduates were pursuing a college education. Table IV.6 indicates where graduates were pursuing additional education, if they were doing so. The distribution of activities did not differ significantly between treatments and controls for either Cohort II or III. In addition, the multivariate analysis (in Appendix H) finds no impact of STEP on college attendance overall or for any subgroup.

LIFE SKILLS OUTCOMES

The source of data on postprogram life skills outcomes is the follow-up interview, which collected extensive fertility-related information--including knowledge about various contraceptive methods, as well as information on sexual activity, contraceptive use, pregnancies and pregnancy outcomes. Our analyses of these data yield mixed impact results. Given the lack of clear patterns and non-compatible findings, we conclude that STEP probably did not affect sexual behavior. Because so little is known about teenage sexuality, the analyses of fertility-related outcomes are presented here in full.

Knowledge of Contraception

The strongest and most consistent in-program impact of participation in STEP was an increase in knowledge about contraception. As treatment youth entered the program, neither they nor the control youth had a great deal of accurate knowledge about contraceptive methods and their availability. By the end of the first summer, however, treatment youth increased their knowledge significantly while control youth's level of knowledge changed very little. A subset of the original knowledge questions were asked at the follow-up interview to determine whether treatment youth's advantage in knowledge of contraceptive methods had been retained.

The results presented in Appendix Table H.7 indicate that the in-program impact on contraceptive knowledge endured. As one would expect, the knowledge level among control youth increased as they matured, but treatment youth still exhibited a higher average level. Although the magnitude of the impact differs by gender, race/ethnicity and site, the impact is significant for nearly every subgroup at both 42 and 54 months after enrollment in the program.

Sexual Activity

A more important indicator of LSO's effectiveness is whether participation in the program translates into more responsible behavior. We found that participation in LSO courses did not affect the likelihood that youth would initiate sexual activity, nor did it affect their level of sexual activity once initiated.

Table IV.6

EDUCATIONAL ACTIVITES OF GRADUATES AT THE TIME OF THE WAVE III INTERVIEW

Activity	Cohort III Graduates		Cohort II Graduates	
	Treatments	Controls	Treatments	Controls
Technical School	0.6%	2.2%	1.5%	2.8%
College	54.8	49.2	48.1	49.7
Training Program	1.1	1.7	3.9	2.8
Not Engaged in Education	43.6	46.9	46.3	44.8
Sample Size	179	179	333	364

Note: Columns may not total 100% due to rounding.

Table IV.7 presents the impact of STEP on the probability of initiating sexual activity by 42 months and by 54 months post-enrollment. About 40 percent of STEP youth reported prior sexual experience at baseline. As the youth mature, the portion of the sample who report having had sexual intercourse at least once increases. By the time of the follow-up interview, over 70 percent reported being sexually experienced (70.7% of mostly 17- or 18-year-old Cohort III control youth and 77.1% of mostly 18- and 19-year-old Cohort II control youth).

Youth who reported ever having had sexual intercourse were asked about their recent sexual activity. As shown in Table IV.8, sexually experienced treatment youth in Cohort III were more likely than controls to report sexual activity within the previous three months. This was true for both males and females, for Hispanic and white youth and for youth in Portland. However, when sexually experienced Cohort III youth were asked about sexual intercourse during the previous four weeks, there was no difference between treatments and controls--approximately 63 percent in both groups indicated that they had been sexually active during that time. Most indicated that they engaged in sexual activity about once a week during that period. Thus, the sexual activity of Cohort III treatments and controls is most likely similar. The same conclusion is made for Cohort II youth 54 months post-enrollment. Neither outcome measure differed by treatment status.

Contraceptive Use

STEP's approach to teaching responsible sexual behavior focused on proper and consistent use of contraception, as well as on abstinence. Thus, although STEP did not affect rates of sexual activity, an increased use of contraception would be a positive outcome. However, findings in this area are inconclusive.

Tables IV.9 through IV.11 present STEP's impact on reported contraceptive use at most recent intercourse for sexually experienced youth, giving data for youth who were already sexually experienced when they entered the program and those who initiated sexual activity since STEP enrollment. Examining these two groups of youth separately is important because the probability of influencing youth's behavior may be greater if the influence is exerted prior to the initiation of that behavior.

The results presented in these tables are mixed. Among Cohort II youth, treatment youth were significantly more likely than control youth to report use of contraception. STEP had an overall impact, but girls, Asians and youth in San Diego were most likely to be affected by participation in the program. As expected, the impacts were much stronger among youth who initiated sexual activity after enrollment in STEP than among those who were already sexually active when they entered the program--

Table IV.7

ESTIMATED IMPACT OF STEP ON RATES AT WHICH YOUTH INITIATE SEXUAL ACTIVITY
(42 AND 54 MONTHS AFTER ENROLLMENT)

Group	Treatment Impact, Cohort III 42 months post-enrollment (in percentage points)	Treatment Impact, Cohort II 54 months post-enrollment (in percentage points)
Overall	0.7	-3.1
Gender		
Male	-3.4	-3.8
Female	3.1	-2.6
Race/Ethnicity		
Asian	-4.1	2.2
Black	2.0	-5.7
Hispanic	-4.5	-6.5
White and Other	14.5	-4.0
Site	#	
Boston	4.7	-7.0
Fresno	9.7	-2.5
Portland	5.1	-7.1
San Diego	-14.7**	-0.8
Seattle	3.1	1.2
Sample Size	1,153	1,080

Note: Among Cohort III youth, 70.7% of the control group were sexually experienced at the time of the Wave III interview. Among Cohort II youth, 77.1% of the control group were sexually experienced at the time of the Wave III interview.

** Indicates that the impact is statistically different from zero at a 0.05 level of significance.

# Indicates that the impact differs with respect to this characteristic at a 0.10 level of significance.

Table IV.8

ESTIMATED IMPACT OF STEP ON RATES AT WHICH YOUTH ENGAGE IN SEXUAL ACTIVITY WITHIN THE PAST THREE MONTHS (42 AND 54 MONTHS AFTER ENROLLMENT)

Group	Treatment Impact, Cohort III 42 months post-enrollment (in percentage points)	Treatment Impact, Cohort II 54 months post-enrollment (in percentage points)
Overall	8.3***	2.5
Gender		
Male	8.5*	7.0
Female	8.0*	-1.0
Race/Ethnicity		
Asian	-1.3	-2.4
Black	5.6	5.2
Hispanic	15.6**	-5.1
White and Other	15.4*	6.3
Site		
Boston	5.7	5.4
Fresno	6.4	-0.4
Portland	11.8*	-1.1
San Diego	10.9	9.2
Seattle	6.6	-0.9
Sample Size	788	816

Note: Among STEP youth who were sexually experienced at the time of the Wave III interview, 78.9% of Cohort III control group members had engaged in sex within the past three months and 81.0% of Cohort II control group members had engaged in sex within the past three months.

*** Indicates that the impact is statistically different from zero at a 0.01 level of significance.
** Indicates that the impact is statistically different from zero at a 0.05 level of significance.
* Indicates that the impact is statistically different from zero at a 0.10 level of significance.

Table IV.9

ESTIMATED IMPACTS OF STEP ON RATES OF CONTRACEPTIVE USE AMONG SEXUALLY EXPERIENCED YOUTH (42 AND 54 MONTHS AFTER ENROLLMENT)

Group	Treatment Impact, Cohort III 42 months post-enrollment (in percentage points)	Treatment Impact, Cohort II 54 months post-enrollment (in percentage points)
Overall	3.2	8.1**
Gender		
Male	-1.1	6.4
Female	7.8	9.7*
Race/Ethnicity	#	
Asian	-0.7	30.1**
Black	-5.5	8.6
Hispanic	21.0**	6.6
White and Other	10.1	-2.2
Site		
Boston	-7.5	4.1
Fresno	13.1	1.7
Portland	8.3	10.0
San Diego	3.2	28.7***
Seattle	-5.6	-1.1
Sample Size	645	665

Note: Among STEP youth who were sexually experienced at the time of the Wave III interview, 62.1% of the Cohort III control group had used contraception the last time they enagaged in sex, and 63.9% of the the Cohort II control group had used contraception the last time they engaged in sex.

*** Indicates that the impact is statistically different from zero at a 0.01 level of significance.
** Indicates that the impact is statistically different from zero at a 0.05 level of significance.
* Indicates that the impact is statistically different from zero at a 0.10 level of significance.

# Indicates that the impact differs with respect to this characteristic at a 0.10 level of significance.

Table IV.10

ESTIMATED IMPACT OF STEP ON RATES OF CONTRACEPTIVE USE AMONG YOUTH WHO WERE SEXUALLY EXPERIENCED AT BASELINE (42 AND 54 MONTHS BEFORE ENROLLMENT)

Group	Treatment Impact, Cohort III 42 months post-enrollment (in percentage points)	Treatment Impact, Cohort II 54 months post-enrollment (in percentage points)
Overall	-0.2	1.5
Gender		
Male	-5.4	0.3
Female	9.5	3.6
Race/Ethnicity	##	
Asian	19.3	-22.5
Black	-11.2	2.5
Hispanic	33.8**	1.3
White and Other	1.1	1.6
Site	##	
Boston	-20.0	9.8
Fresno	31.5**	-2.3
Portland	-11.0	18.6
San Diego	-14.6	4.5
Seattle	-2.1	-24.8*
Sample Size	372	337

Note: Among STEP youth who were sexually experienced at the time of the Wave III interview, 62.1% of the Cohort III control group had used contraception the last time they engaged in sex, and 63.9% of the Cohort II control group had used contraception the last time they engaged in sex.

** Indicates that the impact is statistically different from zero at a 0.05 level of significance.
* Indicates that the impact is statistically different from zero at a 0.10 level of significance.

Indicates that the impact differs with respect to this characteristic at a 0.05 level of significance.

Table IV.11

ESTIMATED IMPACT OF STEP ON RATES OF CONTRACEPTIVE USE AMONG YOUTH WHO WERE SEXUALLY INEXPERIENCED AT BASELINE (42 AND 54 MONTHS AFTER ENROLLMENT)

Group	Treatment Impact, Cohort III 42 months post-enrollment (in percentage points)	Treatment Impact, Cohort II 54 months post-enrollment (in percentage points)
Overall	8.6	15.9***
Gender		
Male	9.8	16.0
Female	8.0	15.9**
Race/Ethnicity		
Asian	-21.8	39.2**
Black	7.7	16.1*
Hispanic	13.0	14.2
White and Other	28.9	-1.2
Site		##
Boston	18.9	4.3
Fresno	-3.2	3.9
Portland	22.3	1.4
San Diego	16.5	55.7***
Seattle	-19.8	26.8
Sample Size	273	328

Note: Among STEP youth who were sexually experienced at the time of the Wave III interview, 62.1% of the Cohort III control group had used contraception the last time they engaged in sex, and 63.9% of the Cohort II control group had used contraception the last time they engaged in sex.

*** Indicates that the impact is statistically different from zero at a 0.01 level of significance.
** Indicates that the impact is statistically different from zero at a 0.05 level of significance.
* Indicates that the impact is statistically different from zero at a 0.10 level of significance.

Indicates that the impact differs with respect to this characteristic at a 0.05 level of significance.

supporting the importance of influencing behavior before it becomes problematic. In contrast, STEP had no apparent impact among Cohort III youth; only Hispanic treatment youth in the cohort were more likely than their control counterparts to use contraception.

In addition to asking about use of contraception at most recent intercourse, youth were also asked about the frequency of contraceptive use during the preceding four weeks. Frequency of contraceptive use was similar for treatment and control youth in both cohorts; about 23 percent of all sexually active youth indicated that they had not used contraception at all during that period, while more than half (ranging from 52% to 60%) indicated that they had used contraception every time they had sexual intercourse. The dissimilarity of responses to the two questions--about contraceptive use at last intercourse and during the previous month--undermines the credibility of the positive impact derived from answers to the first question.

Youth who indicated that they had not used any contraceptive method when they had intercourse were asked to give the main reason. The most frequent responses were that they forgot, that they didn't think about it, or that they don't like it. The only exception to this was among treatment youth in Cohort II, for whom the most frequent response (26% of youth) was that they were pregnant or wanted to become pregnant.

The most frequently used contraceptive method among Cohort II youth was the pill, with 61 percent of control youth and 53 percent of treatment youth who said they practiced contraception indicating that they used the pill at their most recent intercourse. Nearly everyone else, 36 percent of control youth and 44 percent of treatment youth, indicated that they had used a condom. Cohort III youth who practiced contraception (who were, on average, a year younger) were most likely to report having used a condom (54% of control youth and 50% of treatment youth) at their most recent intercourse, while about 44 percent of both treatments and controls indicated that they used the pill.

STEP's impact on contraceptive behavior is difficult to determine. On one hand, Hispanic youth, girls and youth who were sexually inexperienced at baseline appear to have been more likely than their control counterparts to have practiced contraception at their most recent intercourse. On the other hand, these impact estimates were not statistically significant in both cohorts, and when youth were asked about their frequency and method of contraception during a longer time frame (the previous four weeks), the treatment/control differences disappeared. In addition, as we will see, treatment girls were no less likely to have been pregnant. Thus, the validity of estimated positive impacts on contraceptive use is open to question.

Fertility Outcomes

Table IV.12 presents data on pregnancies and births among female treatment and control youth. The percentages of treatment and control females who have been pregnant were similar (as were the treatment/control percentages of males reporting that they had caused a pregnancy). The pregnancy rates for STEP girls are somewhat higher than the national rates reported for non-white 18- and 19-year-olds (26.1%) reported in 1988 (Simons et al., 1991).

By 42 months post-enrollment, nearly one-third of Cohort III females (who were mostly 17 and 18 at the time of the interview) had had at least one pregnancy. Among Cohort II females, who were a year older when they were interviewed, approximately 45 percent had had a pregnancy. About half the females in both groups who had been pregnant had had multiple pregnancies (as indicated by a rate of 1.5 pregnancies per female). Multivariate analysis, shown in Appendix Table H.8, found no impact of STEP on the probability of having a pregnancy for the sample as a whole or for any subgroup.

Just over half of all pregnancies reported by the females resulted in a live birth, which is in line with national averages for females in this age range (Simons et al., 1991). The percentages are similar for treatment and control youth in Cohort III, but in Cohort II, treatment youth pregnancies were more likely to result in a birth. Again, the multivariate analysis (shown in Appendix Table H.9) confirms the overall result. Treatment females are more likely to have a child by 54 months post-enrollment. The statistical analysis indicates that black Cohort III treatment females were less likely than controls to have a child, but a similar result was not found for Cohort II.

Table IV.13 presents similar data for males. The overall percentage who reported having caused a pregnancy (about 20%) was considerably lower than the pregnancy rate among females. As was true among females, however, the percentages of treatment and control males responsible for a pregnancy were similar. The results of the multivariate analysis, shown in Appendix Table H.10, confirm that STEP had no impact on the probability that a male would cause a pregnancy.

Among males who have caused a pregnancy, about half claim responsibility for at least two pregnancies (as indicated by the pregnancy rate per male in Table IV.13). Slightly more than 40 percent of the pregnancies reported by males ended in a birth. (Cohort III control youth were an exception, reporting that 61 percent of the pregnancies they caused resulted in a birth.) Participation in STEP had no impact on the likelihood that males would have a child, as indicated in Appendix Table H.11.

Table IV.12

FERTILITY DATA FOR FEMALES
42 AND 54 MONTHS AFTER ENROLLMENT IN STEP

	Cohort III 42 months post-enrollment		Cohort II 54 months post-enrollment	
	Treatments	Controls	Treatments	Controls
Ever Pregnant (percent)	120 (31%)	112 (35%)	175 (48%)	134 (43%)
Number of Pregnancies (rate per pregnant female)	175 (1.5)	161 (1.4)	267 (1.5)	195 (1.5)
Births (% of pregnancies)	90 (51%)	90 (56%)	164 (61%)	109 (56%)
Low Birth Weights (% of births)	10 (11%)	9 (10%)	14 (9%)	11 (10%)
Children Living With Mother (% of children)	88 (98%)	83 (92%)	149 (91%)	100 (92%)
Sample Size	381	323	367	311

Table IV.13

FERTILITY DATA FOR MALES
42 AND 54 MONTHS AFTER ENROLLMENT IN STEP

	Cohort III 42 months post-enrollment		Cohort II 54 months post-enrollment	
	Treatments	Controls	Treatments	Controls
Caused a Pregnancy (percent)	59 (20%)	58 (17%)	56 (21%)	85 (27%)
Number of Pregnancies (rate per father)	96 (1.6)	72 (1.2)	83 (1.5)	122 (1.4)
Births (% of pregnancies)	37 (39%)	44 (61%)	34 (41%)	54 (44%)
Children Living With Father (% of children)	7 (19%)	17 (39%)	10 (29%)	17 (31%)
Sample Size	301	337	268	314

Substance Use

The final set of LSO-related outcomes examined concerned the use of alcohol and drugs among STEP youth. A secondary focus of LSO (beyond fertility-related issues) was health and substance abuse. Thus, in the follow-up interview, youth were asked about their use of cigarettes, alcohol, marijuana, cocaine and other illegal drugs. About 20 percent of Cohort III youth and about 23 percent of Cohort II youth reported that they smoke cigarettes (not shown in the table). These percentages were similar for treatment and control youth.

As indicated in Table IV.14, more than 40 percent of Cohort III youth and over 45 percent of Cohort II youth reported that they had used alcohol within the previous year. The percentages were similar among treatment and controls at 42 months post-enrollment (Cohort III), but by 54 months post-enrollment, significantly more control youth than treatment youth reported alcohol use. This impact is significant for male, black and Hispanic youth.

Approximately 15 percent of the youth reported that they had used marijuana within the past year and between one and two percent reported that they had used cocaine. Undoubtedly, the reported level of drug use is erroneously low, but it is in line with other published data on drug use. (The Office of Educational Research and Improvement, 1991, reports that 19.7 percent of the class of 1988 seniors report some use of illegal drugs.) For both marijuana and cocaine, the percentages of treatment and control youth who reported use were similar.

The alcohol consumption impact may indicate more than just a reduction in treatments' drinking. The data youth provided on their drug use are clearly laden with errors that make it difficult to observe impacts. Information on alcohol consumption is less likely to be falsified because society does not consider underage drinking to be as serious a crime as drug use. Thus, youth are much more likely to disclose true alcohol use than their true drug use. Thus, if drug and alcohol use are correlated and STEP truly induced a reduction in alcohol use, drug use may be down too. It must be noted, however, that because the alcohol reduction is not seen in both cohorts, we must be somewhat skeptical about the validity of the Cohort II impact. Perhaps the older group has aged out of experimenting with alcohol.

EMPLOYMENT

The analyses of economic behavior indicate that STEP has not altered employment patterns for either in-school or out-of-school youth as a whole. Non-summer employment rates grew from approximately 25 percent during the first school year after STEP to almost 50 percent three school years after STEP's conclusion.

Table IV.14

SELF-REPORTED USE OF ALCOHOL, MARIJUANA AND COCAINE AMONG COHORT II AND III YOUTH (42 AND 54 MONTHS AFTER ENROLLMENT)

	Cohort III 42 months N=679, 656			Cohort II 54 months N=633, 618		
	Alcohol	Marijuana	Cocaine	Alcohol	Marijuana	Cocaine
Overall				**		
Treatments	41.9%	17.1%	1.5%	46.5%	14.4%	1.9%
Controls	43.9	16.2	1.4	52.0	17.2	2.3
Male				*		
Treatments	51.0%	21.7%	1.7%	57.6%	19.4%	2.6%
Controls	49.3	19.2	1.5	64.4	22.5	2.2
Female						
Treatments	34.7%	13.5%	1.3%	38.4%	10.7%	1.4%
Controls	38.3	13.1	1.2	39.4	11.7	2.3
Asian		*			**	
Treatments	16.0%	4.6%	--	19.7%	1.6%	1.6%
Controls	18.4	0.9	--	25.8	7.0	1.6
Black				*		
Treatments	40.8%	19.9%	--	47.3%	14.3%	1.1%
Controls	47.0	18.5	--	54.7	18.6	1.1
Hispanic				*		
Treatments	51.4%	13.6%	2.9%	56.9%	19.2%	3.8%
Controls	51.9	18.0	4.7	68.5	22.5	4.5
White and Other	*					
Treatments	66.7%	30.2%	2.1%	63.4%	23.8%	2.0%
Controls	54.2	25.3	3.6	59.4	20.0	3.8

** Indicates that the impact is statistically different from zero at a 0.05 level of significance.

* Indicates that the impact is statistically different from zero at a 0.10 level of significance.

Summer employment rates increased from 50 percent to almost 60 percent during the last summer of follow-up.

From the follow-up interviews conducted with the STEP youth, we have been able to track youth's employment since the end of their 15 months in STEP. Table IV.15 presents the mean treatment group employment rates and treatment/control group differences over three-month intervals. (Recall that some youth in each cohort began to graduate 36 months after their STEP enrollment, and that the next wave of graduates would appear after month 48.)

Table IV.15 shows that the employment pattern for treatment youth is basically the same as that of controls over the period, with one exception: treatment youth were less likely to be employed than control youth during the fall immediately after STEP's 15 months (months 16 to 18). This exception seems inconsequential. As would be expected, employment rates grew slowly over time, with employment rates in the summer (months 25 to 27, 37 to 39, and 49 to 51) always larger than school-year employment rates.

To investigate subgroup impacts, employment, earnings, wages and hours were examined for two periods in time. The first period was the quarter spanning months 37 to 39, the latest quarter for which employment information was observed for both Cohort II and III youth. The other period was the quarter spanning months 49 to 51, the latest quarter for which employment data were available for Cohort II. Since both these three-month periods were summers, employment activity can be observed among in-school as well as out-of-school youth. However, since the employment behavior of in-school and out-of-school youth is likely to be quite different, we analyzed them separately.[27]

STEP participation did not affect wages or hours for any group. When it did affect a particular group, it did so by increasing the probability of employment for members of that group. The data collected so far indicate that STEP may have a differential impact on employment rates of various racial/ethnic groups. During the fourth summer after STEP enrollment, black treatment youth were more likely to be employed than their control counterparts, while Hispanic treatments were less likely to be employed. During the fifth summer after STEP enrollment, however, Asian treatments were more likely than controls to be employed, while white and other treatments were less likely. Unfortunately, the

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[27] The multivariate analyses conducted controlled for the differences in the youth at baseline. The standard factors, such as reading and math scores, self-reported attendance, number of siblings, living in a female-headed home, and post-high school expectations, are described in Appendix B. In addition, the economic analyses control for the amount of work experience a youth has had prior to the period under consideration.

Table IV.15

AVERAGE EMPLOYMENT RATES AND STEP IMPACTS
FOR COHORTS II AND III

Post-Enrollment Months	Treatment Mean	Control Mean	T/C Difference
16-18	19.9%	23.7%	-3.8**
19-21	23.9	25.0	-1.1
22-24	26.8	28.1	-1.3
25-27 (Summer)	50.8	49.8	1.0
28-30	36.0	35.1	0.9
31-33	35.4	35.5	-0.1
34-36	35.8	36.9	-1.1
37-39 (Summer)	50.4	50.6	-0.2
40-42	45.7	41.4	4.3
43-45	46.5	44.6	1.9
46-48	47.4	46.4	1.0
49-51 (Summer)	57.0	58.6	-1.6

Note: Data for months 43-51 is based on Cohort II youth only.

** Indicates that the impact is statistically different from zero at the 0.05 level.

inconsistency of these patterns over time makes it difficult to understand STEP's true impact. The employment impact tables are presented in Appendix H.

Public Assistance Receipt

STEP's effect on the use of public assistance, particularly by young mothers, is a matter of particular policy concern. Therefore, we examined impacts with regard to receipt of Aid to Families with Dependent Children (AFDC), WIC (a federal program to improve the nutritional intake of pregnant and nursing mothers), and food stamps. A priori, it is difficult to predict how STEP participation would change young mothers' use of AFDC. Since most of the babies born to STEP youth by the date of the interview were quite young, averaging less than a year old, it would be unreasonable to expect that the mothers could be economically self-sufficient if not married. Thus, even if STEP participants were more likely to have been employed or to have earned more, AFDC use was unlikely to differ greatly between treatments and controls at this point in time. As hypothesized, no significant difference in welfare use exists between STEP treatment- or control-group mothers. There were also no subgroup differentials. These results are presented in Appendix Table H.12.

The use of WIC among all STEP girls who have ever been pregnant is displayed in Table IV.16. We found that, overall, treatment and control girls were equally likely to receive WIC. The impact did, however, vary by subgroup. The following subgroups of treatment girls seem more likely to participate in WIC than their control counterparts: Hispanic girls in both Cohorts, Cohort II Seattle treatment girls, San Diego girls and Cohort II girls enrolling after 9th grade who became pregnant. Apparently less likely to use WIC were "white and other" treatment girls in both Cohorts. The differential impact by race/ethnicity was significant only for Cohort III (42 months), whereas the differential impact by site and by grade at program entry was significant only for Cohort II (54 months). Given these mixed results, it is difficult to conclude that STEP affects WIC receipt.

The last welfare outcome considered is food stamp receipt. Unlike WIC and AFDC, which are primarily programs for women, both men and women can qualify for food stamps based on their income. However, because incomes differed by gender, the rate of receipt differed for men and women. Among women, 35 to 40 percent received food stamps, compared with 25 to 35 percent of men. Overall, there was no difference in the probability that a treatment or control group youth was living in a household that received food stamps. (See Appendix Table H.13.)

Table IV.16

ESTIMATED IMPACT OF STEP ON WOMEN EVER HAVING BEEN PREGNANT HAVING RECEIVED WIC (42 AND 54 MONTHS AFTER ENROLLMENT)

Group	Treatment Impact, Cohort III 42 Months Post-Enrollment (in percentage points)	Treatment Impact, Cohort II 54 Months Post-Enrollment (in percentage points)
Overall	-5.4	8.5
Race/Ethnicity	#	
Asian	10.7	-11.4
Black	-9.2	7.3
Hispanic	38.4*	23.6*
White and Other	-36.9**	-8.6
Site		#
Boston	-27.1	12.5
Fresno	25.8	3.2
Portland	-6.3	16.5
San Diego	2.7	30.2**
Seattle	-24.4	-33.5*
Grade Completed at Enrollment		##
8th	-7.8	-0.3
9th	-3.0	20.1**
Sample Size	233	309

Note: On average, 69.0% of Cohort III control youth had received WIC at the time of the Wave 3 interview, while 67.2% of Cohort II control youth had received WIC by this time.

** Indicates that the impact is statistically different from zero at a 0.05 level of significance.
* Indicates that the impact is statistically different from zero at a 0.10 level of significance.

Indicates that the impact differs with respect to this characteristic at a 0.05 level of significance.
# Indicates that the impact differs with respect to this characteristic at a 0.10 level of significance.

V. STEP TREATMENTS AND CONTROLS; A POSTPROGRAM PORTRAIT OF DISADVANTAGED YOUTH IN 1990

For four years, from 1985 through 1988, three groups of 800 low-income, dropout-prone 14- and 15-year-olds participated in a two-summer work/learning program whose effects on them were compared with the experience of similar youth who did not participate. We found the program to be carefully and well-implemented in the five demonstration sites, and to produce strong in-program results. However, longer-term impacts on the almost 1,300 treatment youth discussed in this report did not show up.

Two-plus years after the STEP experience, both treatment and control youth were failing in high proportions to graduate from high school, enter employment and avoid early parenting. And as they got older, that portrait of failure had become even clearer. In the absence of truly comparable national data on other urban disadvantaged teenagers, it is impossible to say whether or not this portrait is characteristic of similar youth nationwide. But it is clear that these youth, eligible for SYETP and STEP by reason of low income and poor educational achievement in the five cities, continue to show levels of attainment that raise serious concern.

In this chapter, we review the Fall 1990 status of STEP treatment and control youth in terms of the education and life skills outcomes reported in the last chapter, present the employment outcomes for the entire group, and discuss the status of an important subgroup, the females who became mothers after their participation in STEP.

EDUCATION AND FERTILITY STATUS

STEP did not increase graduation rates above what they would have been in the absence of the program. Statistical analysis of graduation for both cohorts concludes that STEP had no positive impact for the sample as a whole, nor did it increase graduation rates among any of the subgroups we examined. The promising in-program improvements in the youth's math and reading skills reviewed in Chapter One did not, in general, translate into improved educational outcomes in the postprogram period. The lack of results holds true for all subgroups and sites.

Three and a half years after their enrollment, we find that while half of the Cohort III youth were expected to have graduated high school, only a quarter had a high school diploma (26 percent of the controls and 27 percent of the treatments). Four and a half years post-enrollment, when all of Cohort II should have graduated, only 57 percent of the controls and 53 percent of the treatments had diplomas.

Three and a half years post-enrollment, approximately 22 percent of Cohort III--both treatments and controls--had dropped out of school. Four and a half years post-enrollment, 25 percent of the Cohort II controls and 30 percent of the treatments had dropped out. Statistical analysis of these data indicate that STEP affected neither cohort's dropout behavior.

While these dropout rates are much higher than the national average of 12.1 percent (Kaufman et al., 1991), they are similar to those reported in many inner-city school systems. For example, the New York City school district reported that four years after its Class of 1988 entered high school, just over 20 percent had dropped out but another 25 percent had enrolled for a fifth year.

Not all non-graduates were dropouts. Approximately half of those who had not graduated "on time" were still enrolled in school. As can be seen from Table V.1, Cohort II treatments were more likely than controls still to be in school. Taking more than four years to graduate high school is not uncommon. Among the High School and Beyond 1980 sophomore cohort, 83.4 percent graduated "on time," but an additional 8.2 percent of the original cohort had earned either a high school diploma or the equivalent two years later. In the STEP sample, 5 percent of Cohort II and 2.5 percent of Cohort III had earned a GED certificate by the time of the interview. There was no significant treatment/control difference. In addition, approximately 7.5 percent of Cohort III treatments and 11 percent of Cohort II treatments were enrolled in some form of alternative education.

Fifteen percent of the younger Cohort III and between 25 and 30 percent of Cohort II were enrolled in college, though detailed analysis of the data found that STEP had no significant impact on youth's college decisions for the sample as a whole or within any of the subgroups that were considered. The proportion of STEP youth going on to college is surprisingly high, given the nature of the population targeted. Taken as a percentage of individuals who had received a GED (5 percent) or a diploma (approximately 55 percent), we find that approximately 49 percent of Cohort II's college-eligible youth continued on to college. Using Census data, Carter and Wilson (1989) report that 57.5 percent of all 18- to 24-year-olds attended college in 1988, but that for minorities, the rates are much lower: 30.3 percent among low-income black graduates nationwide and 35.3 percent among Hispanic graduates. The comparable rate among STEP Cohort II Hispanic youth was qualitatively the same as the national average, 37 percent. However, STEP black youth--both treatments and controls--were a much more college-oriented group than average: 45 percent of Cohort II black graduates went on to college.

There are three possible reasons for this difference. First and most likely, STEP's requirement of a minimum fourth-grade reading

Table V.1

ECONOMIC AND EDUCATIONAL ACTIVITY OF COHORT II AND III
(42 AND 54 MONTHS AFTER ENROLLMENT)

	Cohort III 42 Months Post-Enrollment		Cohort II 54 Months Post-Enrollment	
Activity	Treatments	Controls	Treatments	Controls
In High School	49.4%	48.4%	13.0%	11.5%
Working	19.6	19.0	4.9	4.2
Not Working	29.8	29.4	8.2	7.4
In Alternate Education	7.5%	7.8%	11.0%	10.1%
Working	0.3	0.8	1.7	0.6
Not Working	7.2	7.1	9.3	9.4
College	15.4%	14.6%	26.4%	29.9%
Working	8.6	7.5	14.6	15.2
Not Working	6.7	7.1	11.8	14.7
Not In School	27.8%	29.1%	49.6%	48.6%
Working Mother	0.7	1.2	2.4	1.8
Working Nonmother	11.7	13.0	21.0	23.8
Non-Working	15.4	14.9	26.2	23.0
Mother	3.8	4.7	8.6	5.6
Married Females	0.4	0.3	0.3	0.5
Non-Married Females	4.2	4.1	7.2	6.4
Males	6.9	5.9	10.0	10.5
Sample Size	684	663	637	626

Note: Distribution of status between treatment and control youth in the two cohorts were statistically similar. Numbers have been rounded.

level for enrollment may have disproportionately disqualified black students, enrolling more of those likely to be college-bound. Second, the dropout rate for blacks in our five demonstration sites may be lower than it is in the nation as a whole, but no reliable studies of black dropout rates in urban school districts are available to support this speculation. Third, many black youth may have already dropped out of school before they were old enough to enroll in STEP.

Only 29 percent of controls and 28 percent of treatments of Cohort III were not involved in some type of educational program three and a half years post-enrollment. Of the group who were out of school at that point, most worked or cared for a child. Among Cohort II youth, who were a year older, nearly half were not involved in some type of educational program.

Of the youth who were not attending school, approximately half in both cohorts were working at the time of the interview. National and STEP youth employment rates are comparable. Nationally, 60 percent of all 17- to 18-year-old out-of-school youth were employed some time during 1988 (Simons et al., 1991), but the percentage of individuals employed some time during a year is typically greater than the percentage employed at a point in time.

The proportion who did not work was higher among treatments than controls primarily because more Cohort II treatments were mothers. This puzzling difference occurred largely because pregnant treatments were more likely than controls to deliver their children, though they were no more likely to become pregnant.

At the same time, the in-program effects of the LSO curriculum--increasing knowledge of contraceptive use and availability--do appear to linger, though the evidence is mixed as to whether the increased knowledge translated into reports of increased contraceptive use. The pregnancy rate among the females was the same for treatments and controls. However, the rate of live births was higher for Cohort II treatments than controls; fewer of their pregnancies were prematurely terminated (naturally or intentionally).

THE TEENAGE MOTHERS: EDUCATION, WORK AND WELFARE

Among Cohort III females (17 and 18 years old), approximately 22 percent of both treatments and controls had a child. Among Cohort II females (18 and 19 years old), approximately 33 percent had children. As shown in the last chapter, female treatments' STEP participation has had no visible impact on fertility behavior. However, because a teenage mother's ability to continue her education and/or support herself is clearly limited, this subgroup of youth is of special policy interest. Therefore, we will describe in more detail their activities with respect to educa-

tion, employment and use of public assistance at the time of the interview.

Eighteen percent of both treatment and control Cohort III mothers had graduated from high school; among Cohort II mothers, 35 percent of treatments and 43 percent of the controls were graduates. A large percentage of the mothers were high school dropouts--45 percent of the Cohort III treatment mothers and 52 percent of the Cohort II treatment mothers. Similar percentages of control group mothers were dropouts (49% and 44% for Cohorts III and II, respectively).

Table V.2 shows, however, that about half the mothers were still pursuing an education. Among Cohort III mothers, approximately 30 percent were still in high school and 20 to 25 percent were enrolled in some form of non-high school education. These are smaller percentages than were found in the general Cohort III sample--of which 49 percent were still in high school and approximately 20 percent were pursuing non-high school education. Among the older Cohort II mothers, approximately 7 percent were still in high school compared with 12 percent of the overall Cohort II sample. Other educational opportunities were being pursued by an additional 35 percent of the mothers compared with 39 percent among the general Cohort II sample. Table V.2 shows that there were no significant differences between the activities of treatment and control mothers at 42 or 54 months.

We also found there was no significant difference between treatments and controls in the probability that a young mother would be employed. At the time of the interview, only 12.2 percent of Cohort III and 19.2 percent of Cohort II treatment mothers worked, compared with 17.5 percent of Cohort III and 25.0 percent of Cohort II control mothers. As expected, most of the mothers (treatments and controls) received public assistance in some form. Sixty-four percent (64%) of the younger (Cohort III) mothers and 75 percent (75%) of the older (Cohort II) mothers had received WIC or AFDC payments at some time since becoming pregnant. Seventy percent (70%) of the pregnant girls in both cohorts had used WIC and, at the time of the interview, 56 percent of the Cohort III mothers and 68 percent of Cohort II mothers were receiving AFDC. The explanation for the older mothers' higher rate may be that more Cohort III mothers were covered under their mothers' cases, while Cohort II mothers had cases of their own.

The proportion of younger (Cohort III) mothers who lived in a household that received food stamps, on the other hand, was slightly greater than the proportion of Cohort II mothers who did (36% versus 31%). Overall, these figures confirm that young mothers are quite likely to need public assistance.

Table V.2

THE EDUCATIONAL ACTIVITIES OF STEP TREATMENT AND CONTROL WOMEN WITH CHILDREN

Enrolled In:	Cohort III 42 Months		Cohort II 54 Months	
	Treatments	Controls	Treatments	Controls
High School	31%	29%	7%	7%
Vocational or Technical School	0	1	2	7
GED Classes	15	10	15	13
College	4	4	12	10
Training Program	7	8	6	5
No Schooling	43	49	58	59
Sample Size	74	80	130	88

Note: Columns may not total 100% due to rounding.

CONCLUSION

The purpose of the STEP program was to help youth who were at great risk of dropping out of high school by providing them with two successful summers of work/education experience and with support during the intervening school year. It was believed that by improving their reading and math ability, and providing them with information that would help them choose not to become teenage parents, these youth would stay in school longer and ultimately be more self-sufficient than they would have been otherwise.

P/PV's earlier reports on STEP's in-program impacts indicated that the youth's academic skills did improve relative to controls over their first summer in the program. And to the extent that the goal of a youth program should be to improve some aspect of the developmental process, STEP performs reasonably well. Extended summer vacations, as are almost universal in the United States, are unstructured vacuums for disadvantaged youth and, STEP filled this void productively. The STEP experience productively engaged youth who might otherwise have floundered during their summer vacation.

However, this report indicates that three or four years post-enrollment, youth's short-term gains from STEP do not translate into positive changes in their lives. Treatment youth were no less likely to drop out of high school and no more likely to graduate, and they were employed in numbers similar to those of control youth. As well as being disappointing and in certain areas puzzling, these findings raise important questions for the whole youth development field, particularly when considered in conjunction with findings from analyses of other youth employment and training programs. These questions concern the following:

- The usefulness of continuing to expect longer-term impacts from short-term initiatives, as we traditionally do in designing research demonstrations;

- The wisdom of continuing to design limited impact, short-term interventions at all, in the face of the apparently overwhelming influence of the core school and life experiences of disadvantaged youth;

- The possibility of creating, from the several short-term interventions that <u>have</u> proved effective, a package and continuum of such interventions to meet the needs of various subgroups of youth for whom they have been shown effective; and

- The necessity of considering the larger issues--poverty, neighborhood and family influences, school experiences,

employment and postsecondary education opportunities--in designing interventions, or groups of interventions, that will have a chance to achieve longer-term impacts.

The traditional evaluation report would conclude with a brief discussion of the implications of the STEP findings for future youth programming. However, the questions raised here are too wide-ranging and complex for brief discussion. That discussion can be found in a companion piece, Anatomy of a Demonstration: The Summer Training and Education Program (STEP) from Pilot Through Replication and Postprogram Impacts (P/PV, 1992), which fully explores the implications of the demonstration.

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APPENDIX A

SITE DESCRIPTIONS

Boston

Oversight of STEP in Boston was the responsibility of the Mayor's Office of Jobs and Community Services. That office contracted with Action for Boston Community Development (ABCD), a community-based organization, to operate both SYETP and STEP.

Remediation and life skills classes were held at four local universities. Boston University, the University of Massachusetts/Boston, and Northeastern University participated throughout the demonstration. Harvard University joined as a fourth site beginning in Summer 1987. Treatment youth were assigned to attend classes at the university nearest to their home. They attended remediation classes Monday through Friday mornings, participated in life skills classes two afternoons per week, and worked on campus three afternoons per week. LSO instruction was provided by ABCD counselors, who also monitored work experience.

Control youth worked at regular SYETP jobs, most of which were located in their home neighborhood.

Fresno

The Fresno Private Industry Council (PIC) was the lead agency in Fresno, responsible for operational and fiscal management of STEP. The PIC contracted with the Economic Opportunity Commission for outreach, recruitment, intake, economic eligibility determination and worksite development for STEP youth. The remediation and life skills components were the responsibility of the Fresno Unified School District.

Remediation and life skills classes were held at one middle school five mornings a week. Treatment youth worked each afternoon. Control youth worked seven and a half hours per day. Both groups were assigned to jobs at schools, parks, government offices and private businesses throughout the city.

Portland

As in Fresno, the PIC in Portland was responsible for all aspects of STEP. Staff hired by the PIC to operate STEP were responsible for recruitment, eligibility determination, intake, worksite development, job placement, and monitoring and managing of the various components of STEP. Although teachers and aides were hired jointly by the PIC and Portland Public Schools (PPS), they were employed by PPS. A PPS building administrator was responsi-

ble for remediation and life skills activities, but day-to-day supervision was the responsibility of the PIC's STEP coordinator.

STEP treatment youth attended remediation and life skills classes each morning at an area high school. These youth worked each afternoon, while control youth worked full days. Approximately 25 percent of the jobs were custodial positions with PPS, but youth also worked at hospitals, parks, museums, and governmental offices.

San Diego

The San Diego Regional Employment and Training Consortium was the lead agency with primary responsibility for STEP in San Diego. The remediation and life skills components were subcontracted to the San Diego City Schools. The work experience component was subcontracted to the San Diego Regional Youth Employment Program.

Remediation and life skills classes were provided at first at five schools. The large number of remediation sites made logistics quite difficult so the number was reduced to two in 1987. Classes were held every morning, and treatment youth worked three hours each afternoon. Most San Diego treatment and control youth were placed in jobs near their home, primarily in custodial, recreation, child care, or clerical positions.

Control youth generally worked four days per week. Unlike the other sites, with the exception of Seattle, San Diego offered control group youth a job for a second summer.

Seattle

Implementation of STEP in Seattle was the responsibility of the city's Department of Human Resources (DHR). DHR had responsibility for all aspects of STEP, but received financial support from the Seattle King County PIC, Seattle Public Schools (SPS), the Washington State Commission for Vocational Education, and United Way of King County.

A centrally located middle school served as the site for remediation and life skills classes. At first, remediation and life skills classes in Seattle were offered both in the morning and the afternoons. However, logistical and management problems were encountered with the dual schedule. Thus, starting in 1987 classes were offered only in the morning.

Most first summer youth were assigned to group projects in grounds maintenance, neighborhood clean-up and theatrical programs. Jobs for returning youth included individuals assignments in clerical, maintenance and child care positions.

As in San Diego, control youth were offered an opportunity to return for a second summer of work experience through SPS's Work Training Program. All control youth worked seven hours each day.

APPENDIX B

ANALYTIC STRATEGIES

Estimation of the impact of participation in STEP relies heavily on multivariate analysis. This appendix presents the specific type of estimation models used.

A similar specification is applied to most of the regression models used to analyze the impact of STEP participation. The explanatory variables include: basic demographic characteristics, such as gender, age and race/ethnicity; the demonstration site; variables that describe the youth's home environment, such as family size and birth order; and educational characteristics, such as absenteeism and educational expectations.

Education and economic analysis models contain additional education-related characteristics, including youth's grade level at program entry, whether the youth have ever been retained in a grade, their enjoyment of reading and math, and their baseline scaled scores in reading and math MATs.

Models used to estimate the impact of STEP on contraceptive knowledge and use, consequences of adolescent pregnancy, delay of sexual initiation, contraceptive use, and pregnancies and children fathered include baseline measures of these fertility-related outcome variables in addition to background and demographic variables.

In general, the multivariate models used to estimate the impact of STEP on various outcome measures take the following form:

(1) $Y_2 = a + b_1Y_1 + b_2X + b_3T + e_1$

where:

- Y_2 = the postprogram (endline) value of the variable of interest
- Y_1 = the preprogram (baseline) value of the variable of interest
- X = a vector of explanatory variables
- T = whether the youth received STEP treatment
- a, b_i = coefficients
- e_1 = a stochastic disturbance term with a mean of zero and a constant variance

This specification made it possible to control for preexisting differences among youth while the impact of STEP was estimated (b_3).[1] If the treatment and control groups had differed with respect to unobservable characteristics, such as motivation, estimates from these regressions would have been biased. However, before conducting analyses, the nature of sample attrition was explored. No statistically significant treatment/control difference was found.

The overall impact of STEP is estimated by the dichotomous variable T, indicating control group status. In addition to estimating the overall effect of the program using equation (1), a series of subgroup-treatment interaction variables are used to estimate the effect of the program in specific sites and to estimate the effect of STEP on racial/ethnic and gender subgroups. Algebraically, equation (1) is modified as follows:

$$(2) \quad Y_2 = a + b_1Y_1 + b_2X + b_3T + c_1TS_1 + \ldots + c_4TS_4 + e_2$$

$$(3) \quad Y_2 = a + b_1Y_1 + b_2X + b_3T + c_1TR_1 + c_2TR_2 + c_3TR_3 + e_3$$

$$(4) \quad Y_2 = a + b_1Y_1 + b_2X + b_3T + c_1TM + e_3$$

$$(5) \quad Y_2 = a + b_1Y_1 + b_2X + b_3T + c_1TGR8 + e_3$$

where:

- S_i = site dummy variables[2]
- R_i = racial/ethnic dummy variables[3]
- M = a dummy variable that equals 1 for males
- GR8 = a dummy variable that equals 1 for individuals who enrolled just after 8th grade

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[1] This model is a more robust specification than one that analyzes change scores. An analysis of change scores assumes that the amount of change and pretest level of the outcome measure are perfectly related. If that assumption is violated, an analysis of change scores is a misspecification of the model and the resulting estimates of the coefficients are incorrect. The model estimated for the analysis reported here controls for pretest level if this assumption is violated and is equivalent to the change score model if this assumption holds.

[2] One site--Seattle--is omitted from the equation.

[3] One racial/ethnic group category--whites and others--is omitted.

By using one of these equations, the estimated impact on any individual can be calculated. A subgroup impact presented in the report, such as that for Hispanics, is the average of all individual subgroup members' impacts, assuming subgroup members differ from the rest of the sample only by being a member of that subgroup, i.e., their other characteristics are, on average, the same as for the overall sample.[4]

The use of OLS is not warranted when the dependent variable is dichotomous, such as whether a participant uses contraception during the program period or becomes a school dropout.[5] In such cases, a nonlinear maximum likelihood estimation technique, logit, is used to estimate treatment effects.

The key finding of the analysis is whether STEP has an effect on various outcome measures. In the discussion of the results, we indicate whether an impact estimate is statistically different from zero by labeling statistically non-zero estimates as "significant." In this report, the term is reserved for estimates that are not equal to zero at a 0.10 or greater level of significance using a two-tailed t-test.

When discussing subgroup estimates, a second finding is also of interest: whether the effect of STEP differs with respect to a particular characteristic, such as race or ethnicity. An F-test of whether the subgroup impacts differ from one another was conducted for all subgroup analyses.

In summary, a variety of analytic strategies are used to evaluate the impact of participation in STEP. The fundamental approach used a dummy variable (indicating treatment or control group status) in an OLS regression. Other analyses (logit) are used where the assumptions of the OLS model are likely to be violated.

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[4] This estimate is calculated as an appropriately valued linear combination of treatment and treatment-interaction coefficients. For example, the estimated impact on subgroup R_1 is: $b_3 + c_1 + c_2\ 0 + c_3\ 0$.

[5] See Amemiya (1981) for details about the problems involved in estimation with dichotomous variables.

APPENDIX C

RESPONSE RATES AND SAMPLE ATTRITION ANALYSIS FOR WAVE III

Approximately 15 percent of Cohort III and 23 percent of Cohort II were not interviewed in Wave Three. Accurate estimates of program impacts depend on the difference in patterns of attrition that occur between the treatment group and the control group. Thus, multivariate analysis to determine the nature of attrition from the randomized sample to the interview sample was conducted. Table C.1 presents the results of the logit analysis. It indicates that there was no systematic attrition based on treatment status. Because the characteristics of youth who participated in the third wave of interviews were similar to those of the youth who completed tests and questionnaires at baseline and there was no differential attrition, it was quite unlikely that the estimated impact coefficients would be inconsistent. Therefore, sample selection corrections were not used in the subsequent analyses.

Table C.1

ESTIMATED COEFFICIENTS OF THE RESPONSE LOGITS FOR COHORTS II AND III

Variable	Estimated Coefficient	
	Cohort II	Cohort III
Treatment Status	0.0190	0.1436
Male	-0.5728***	-0.2248
Asian	0.5860***	0.7978***
Black	-0.1460	0.0763
Hispanic	0.1139	0.5574**
14 Years Old or Younger	-0.2287*	-0.0946
Boston	0.1263	0.0511
Fresno	0.1205	0.0915
Portland	0.1347	0.3089
San Diego	0.3738*	0.4263*
Sample Size	1633	1590

Note: Individuals who responded to the Wave III follow-up interview were coded 1; those who did not were coded zero.

*** Indicates that the value is statistically different from zero at the 0.01 level.
** Indicates that the value is statistically different from zero at the 0.05 level.
* Indicates that the value is statistically different from zero at the 0.10 level.

Table C.2

BASELINE DEMOGRAPHIC AND ECONOMIC CHARACTERISTICS OF COHORT II YOUTH BY TREATMENT STATUS AND SITE

	All Sites		Boston		Fresno		Portland		San Diego		Seattle	
	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control
Demographic Characteristics												
Age												
≤ 14 Years Old	56.8%	54.0%	64.2%	57.5%	56.2%	61.5%	60.2%	51.5%	50.4%	54.3%	53.9%	44.9%
≥ 15 Years Old	43.2	46.0	35.8	42.5	43.8	38.5	39.8	48.5	49.6	45.7	46.1	55.1
Gender ###			##				##					
Male	42.4	50.3	39.7	52.0	44.6	50.0	44.1	56.9	42.2	44.2	41.4	48.3
Female	57.6	49.7	60.3	48.0	55.4	50.0	55.9	43.1	57.8	55.8	58.6	51.7
Race/Ethnicity												
Asian	19.2	20.4	7.9	11.0	13.1	16.4	12.7	13.8	33.3	33.3	27.3	28.0
Black	44.6	44.1	55.6	59.1	29.2	22.1	48.3	49.2	40.7	34.1	50.0	55.9
Hispanic	20.4	18.1	22.2	12.6	50.8	50.0	3.4	2.3	20.7	23.3	3.1	2.5
Other	15.9	17.4	14.3	17.3	6.9	11.5	35.6	34.6	5.2	9.3	19.5	13.6
Percentage Having Difficulty With English Language	15.2	17.1	14.6 **	6.0	15.7	20.7	8.1	11.5	25.4	24.6	11.3	22.9
Sample Size	637	626	126	127	130	122	118	130	135	129	128	118

Note: The sample size is the number of youth interviewed in Wave 3.

** Indicates that treatments and controls differ with respect to this characteristic at a 0.05 level of confidence.

Indicates that distribution of treatments and controls differs with respect to this characteristic at a 0.01 level of confidence.

Indicates that distribution of treatments and controls differs with respect to this characteristic at a 0.05 level of confidence.

Table C.3

BASELINE DEMOGRAPHIC AND ECONOMIC CHARACTERISTICS OF COHORT III YOUTH BY TREATMENT STATUS AND SITE

	All Sites		Boston		Fresno		Portland		San Diego		Seattle	
	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control
Demographic Characteristics												
Age												
≤ 14 Years Old	58.9%	56.6%	61.5%	59.7%	46.5%	51.1%	65.3%	56.1%	63.5%	65.3%	55.3%	49.1%
≥ 15 Years Old	41.1	43.4	38.5	40.3	53.5	48.9	34.7	43.9	36.5	34.7	44.7	50.9
Gender	##						##		##			
Male	44.3	51.3	40.6	48.1	48.1	45.3	46.7	60.4	41.9	54.2	44.7	47.3
Female	55.7	48.7	59.4	51.9	51.9	54.7	53.3	39.6	58.1	45.8	55.3	52.7
Race/Ethnicity												
Asian	19.2	17.2	7.7	7.0	17.1	13.7	11.3	10.8	33.8	27.8	27.2	27.7
Black	46.2	50.2	57.3	62.8	27.9	30.9	47.3	51.8	38.5	47.2	61.4	61.6
Hispanic	20.5	19.9	18.9	21.7	51.9	50.4	4.7	2.2	25.0	19.4	1.8	2.7
Other	14.2	12.7	16.1	8.5	3.1	5.0	36.7	35.3	2.7	5.6	9.6	8.0
Percentage Having Difficulty With English Language	15.3	13.3	10.7	10.9	11.5	10.4	9.5 *	3.8	29.9	25.4	14.8	16.2
Sample Size	684	663	143	129	129	139	150	139	148	144	114	112

Note: The sample size is the number of youth interviewed in Wave 3.

* Indicates that treatments and controls differ with respect to this characteristic at a 0.10 level of confidence.

Indicates that distribution of treatments and controls differs with respect to this characteristic at a 0.05 level of confidence.

Table C.4

BASELINE EDUCATIONAL CHARACTERISTICS OF COHORT II YOUTH
IN THE INTERVIEW SAMPLE BY TREATMENT STATUS AND SITE

	All Sites		Boston		Fresno		Portland		San Diego		Seattle	
	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control
Educational Ability												
Mean MAT Reading Score (Grade Equivalent)	719 (5.9)	723 (6.1)	703 (5.1)	709 (5.4)	724 (6.2)	729 (6.5)	719 (5.9)	726 (6.3)	729 (6.5)	732 (6.6)	722 (6.1)	717 (5.8)
Mean MAT Math Score (Grade Equivalent)	700 (6.7)	703 (6.8)	674 (6.1)	682 (6.3)	698 (6.7)	* 714 (7.2)	698 (6.7)	695 (6.6)	735 (7.9)	733 (7.8)	695 (6.6)	689 (6.5)
Mean Grade in School	8.3	8.3	7.9	7.9	8.4	8.3	8.4	8.4	8.5	8.5	8.5	8.4
Grade Retention History												
Percentage Ever Repeating a Grade	32.9	33.8	43.5	47.0	39.4	37.1	27.3	28.3	22.3	23.1	30.7	33.6
Absenteeism (SY 85-86)												
Absent ≤ 10 Days	76.5	77.2	73.4	80.3	67.7	68.1	78.4	77.7	84.6	80.7	78.9	79.2
Absent ≥ 11 Days	23.5	22.8	26.6	19.7	32.3	31.9	21.6	22.3	15.4	19.3	21.1	20.8
Educational Expectations												
Percentage Expecting to Pursue Post-High School Education	70.2	73.1	54.5	* 66.4	75.0	75.4	72.6	76.7	75.4	71.8	73.9	75.5
Sample Size	637	626	126	127	130	122	118	130	135	129	128	118

* Indicates that treatments and controls differ with respect to this characteristic at a 0.10 level of confidence.

Table C.5

BASELINE EDUCATIONAL CHARACTERISTICS OF COHORT III YOUTH
IN THE INTERVIEW SAMPLE BY TREATMENT STATUS AND SITE

	All Sites		Boston		Fresno		Portland		San Diego		Seattle	
	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control
Educational Ability												
Mean MAT Reading Score (Grade Equivalent)	720 (6.0)	719 (5.9)	722 (6.1)	724 (6.2)	724 (6.2)	726 (6.3)	743 (7.2)	739 (7.0)	707 (5.3)	704 (5.2)	698 (4.8)	698 (4.8)
Mean MAT Math Score (Grade Equivalent)	711 (7.1)	710 (7.0)	701 (6.8)	703 (6.8)	728 * (7.6)	744 (8.3)	719 (7.3)	709 (7.0)	729 (7.7)	718 (7.3)	670 (6.0)	669 (6.0)
Mean Grade in School	8.3	8.4	7.9	7.9	8.6	8.7	8.4	8.4	8.3	8.4	8.5	8.4
Grade Retention History												
Percentage Ever Repeating a Grade	33.2	33.9	48.8	46.8	28.9	27.2	27.4	28.6	27.8	31.6	33.0	38.5
Absenteeism (SY 86-87)												
Absent ≤ 10 Days	77.0	72.3	80.3	81.1	72.6	61.9	76.3	72.2	79.1	75.7	76.1	71.0
Absent ≥ 11 Days	23.0 *	27.7	19.7	18.9	27.4 *	38.1	23.7	27.8	20.9	24.3	23.9	29.0
Educational Expectations												
Percentage Expecting to Pursue Post-High School Education	74.5	78.3	67.6	64.4	79.6	84.0	71.2	78.2	77.3	78.2	76.1	83.0
Sample Size	684	663	143	129	129	139	150	139	148	144	114	112

* Indicates that treatments and controls differ with respect to this characteristic at a 0.10 level of confidence.

APPENDIX D

DROPOUT RATE DEFINITION

The following definition of a dropout was used to calculate dropout rates for the STEP sample: a student who leaves school (for any reason other than death) before graduation without transferring to another school or institution. Although the definition seems straightforward, the categorization of certain groups of students is problematic. The dropout status assigned to these groups are given below:

INCLUDED AS DROPOUTS

- Students who enter the military;
- Students who enter youth or job corps;
- Students who are suspended and are not known to have enrolled at another school;
- Students who leave school between terms and are not known to enroll at another school;
- Students from Special Ed and other special, ungraded or alternative programs who drop out;
- Students who leave due to hardship (transportation, child care, etc.);
- Students who leave school and enter a program not qualifying as a secondary school, even if they may confer a GED; and
- Students who are asked to leave because they are over a particular age (e.g., students over 21 years old in Seattle).

NOT INCLUDED AS DROPOUTS

- Students who die;
- Students known (by a transcript request or other information--from the school or the follow-up interviews) to have transferred to a public, non-public or other state-approved program, such as home-based instruction; and
- Students known to be transferred to another public institution, such as a prison, juvenile institution, or mental institution.

APPENDIX E

REASONS FOR USING SCHOOL TRANSCRIPT DATA

Data on educational outcomes came from youth themselves and/or the schools. As discussed in the methodological chapter, information from the youth was collected through an interview. School data could have been obtained centrally from the school districts' MIS systems or from the transcripts themselves, which must be obtained (for the most part) from the various school buildings.[1] Except for a time lag, the information from the two systems should have been identical. Thus, for simplicity, we originally decided to collect MIS data from the school districts.

Over time, however, we grew increasingly suspicious that the centrally stored data for this particular population of struggling students had significant problems that undermined their validity. Thus, in 1989, we funded a study to compare the quality of data from the two data sources.

We found that except for fully automated sites (where the central MIS was one and the same as the building-level records) and test score information, school district MIS data were often inaccurate. Some problems are inherent in school data no matter what the source and were expected. For example, attendance data are notoriously inaccurate. Also, credit data from alternative schools are often not available. Thus, as part of the sample moved to alternative schools, we were able to obtain only credit information on that portion of the sample that was able to remain in traditional school settings. This was likely to erode the representativeness of the data.

In addition to expected problems, however, we found that school building records were more accurate than the MIS data on several key variables. The most important of these was termination status. In this code resides information on dropout status and graduation. It appears that the transmission of data from individual schools to their central school district MIS is not uniform. Consider the case of a student who had not quite finished their requirements to graduate, but was going to finish in the summer. Some schools report to the district that the student did graduate while other schools report that the student did not. The problem should be resolved in the next semester, but in many cases, the correct information is never submitted--either the youth is never recorded as graduated or is marked as graduated even if they never returned to complete their course

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[1] In some sites, "the transcript" was what was on the automated system. In these sites, each school building has access to the system and inputs records themselves.

work. School transcripts also tended to contain more accurate information on students' dropout or transfer status. MIS errors appear to result from both data entry mistakes at the district level and the school administrators not passing on accurate information.

For these reasons, we made several changes in earlier reports. First, we collected school data from the transcripts themselves, not from the district-level MISs. Second, we decided not to analyze attendance or promotion, as both were severely flawed. We believe that while collecting transcript data is more expensive, it is the only way to get reasonably accurate data.

APPENDIX F

LIMITATIONS OF SCHOOL DATA

No one measure can provide a complete picture of school performance, and no one measure is without limitations. Thus, to investigate the effect of STEP participation on school performance, we examined its impact on several school outcomes and looked for a confluence of impact results that would give us greater confidence in our conclusions. A brief summary of the issues encountered in analyzing each outcome is presented here.

STANDARDIZED TEST SCORES

Within a given school district, analysis of test score data was straightforward. However, sites administered different standardized tests, and the scores they obtained could not be directly compared.

To conduct a cross-site analysis, test scores were converted to "normal curve equivalents" (NCEs). NCEs are similar to test score percentiles in that they range from 0 to 100; unlike percentiles, however, they are a linear measure that can be subjected to regression analysis. The transformation of the scaled scores to normal curve equivalents enabled us to pool test data across sites and analyze test data for the entire sample. Analyzing a larger sample size also increased our ability to detect small program impacts.

To verify the accuracy of the impacts estimated from the pooled analysis of test-score NCEs, site-by-site analyses of scaled scores were conducted. These analyses were relatively uncomplicated, but the sample within each site was relatively small, averaging approximately 150 treatments and 150 controls. Thus, it was difficult to observe statistically significant effects.

In addition, San Diego and Portland presented site-specific data collection problems. In San Diego, students are tested only every other year; thus, only sample members who were in the seventh, ninth or 11th grades were tested. Although this selection process did not bias the results presented in this report, it reduced the number of sample members for whom there were data.

In Portland, not all sample members took a standardized test, since Portland students are only tested annually between grades three and eight. Between the ninth and 12th grades, only students who have not yet met a minimum level of competency in reading or math are tested. Thus, in Portland, we could address the question of how STEP affected the reading and math skills of those who have not already reached the minimum level of competency, or are in the eighth grade or below. However, the impact on

test scores for the average Portland sample member could not be estimated. Thus, the aggregation of the test scores across sites (using NCEs) excludes Portland.

CREDITS

As a third measure of STEP's impact on school-year progress, data were collected on the number of high school credits earned by each student. Because students who were in eighth grade or below had not yet earned high school credits, they were excluded from this analysis. In addition, credit data were often not available from alternative schools. Thus, as part of the sample moves to alternative schools, we only obtain credit information on that portion of the sample that was able to remain in traditional school settings. This is likely to erode the representativeness of the data.

HIGHEST GRADE COMPLETED

The fourth measure of school-year performance is grade progression, which is influenced by a school district's philosophy of grade promotion. Promotion in one school district may not signify the same academic advancement as promotion in another. Therefore, grade promotion measures a student's progress through a school system, not necessarily his/her skills acquisition or academic achievement.

The more lenient a district's promotional philosophy, the smaller the estimated STEP impact is likely to be. At the extreme, suppose all students were promoted no matter what academic level they attained by the end of the year. There would be no difference in the proportion of treatments and controls who passed; therefore, the estimate would indicate that STEP had no impact on grade progression. Such an outcome, however, would not necessarily imply that STEP did not affect academic achievement.

Thus, caution must be used when comparing grade progression impacts within and across sites. This problem was minimized by using regression analysis and checking pooled results by analyzing promotion at each site separately. Nevertheless, the assessment of STEP's effect on educational progress must be based on analyses of both accumulated credits and grade promotion.

DROPOUT BEHAVIOR

Accurate statistics on dropout rates are very difficult to obtain from school data. Schools often cannot identify students that have dropped out because they did not or, if under 16 years old, legally could not formally withdraw. Thus, most school districts remove students from the active enrollment list based on non-attendance. However, the manner in which school districts record such withdrawals varies considerably. In addition, since schools

often qualify for funds based on the number of students they serve, the accuracy of dropout and enrollment data is suspect.

To examine dropout behavior while recognizing these problems, we examine both school-reported dropout status and self-reported dropout status. Appendix D presents the definition of dropout that was used for the school-reported data. Withdrawal because of expulsion was not counted as dropping out; only voluntary withdrawal, including withdrawal for such reasons as enlistment in the armed forces, was deemed dropping out. The measures used to define dropouts are likely to underestimate the number of dropouts for two reasons. First, a school system will often report inaccurately that a student is still enrolled. Second, youth who transfer schools do not also enroll in school even when their old school believes they have transferred.

Self-reported dropout status is probably a more accurate measure. It captures individuals still enrolled in school even if their original school district is ignorant of their location, as well as individuals who completed their high school education via a GED program.

GRADUATION

School-reported graduation rates are suspect. Schools lose track of many of the youth as they transfer schools. Thus, youth could graduate without the schools knowing about it.

In summary, the data obtained from school transcripts must be used cautiously. While any STEP impacts should still be evident in these data, since both treatments and control data are afflicted with these problems, the impacts will have to be larger to be statistically significant.

APPENDIX G

TECHNICAL DATA TABLES

TABLE G.1

ESTIMATED LOGIT COEFFICIENTS FOR
THE DROPOUT AND GRADUATION MODELS

	Probability of Dropping Out		Probability of Graduating	
	Coh III	Coh II	Coh III	Coh II
Constant	3.73 (1.37)	6.06 (1.29)	-11.81 (3.60)	-7.95 (1.42)
Treatment	-0.06 (0.14)	0.22 (0.14)	0.10 (0.21)	-0.25 (0.14)
Scaled Reading Score (in tens of points)	-.042 (.018)	-.047 (.016)	0.016 (.025)	0.040 (.015)
Scaled Math Score (in tens of points)	-.016 (.013)	-.014 (.013)	0.044 (.019)	0.006 (.013)
Reading Enjoyment Scale	0.05 (0.07)	-0.13 (0.07)	-0.13 (0.12)	0.06 (0.07)
Math Enjoyment Scale	-0.13 (0.07)	-0.07 (0.07)	-0.01 (0.11)	0.09 (0.06)
Repeated a Grade	0.44 (0.17)	0.52 (0.16)	-0.83 (0.32)	-0.40 (0.16)
Was Absent 10 or More Days of School	0.78 (0.16)	0.94 (0.16)	-0.96 (0.24)	-1.00 (0.17)
Expects to Pursue a Post-High-School Education	-0.52 (0.18)	-0.41 (0.16)	0.98 (0.29)	0.42 (0.16)
Family Size	0.01 (0.04)	0.01 (0.04)	-0.002 (0.05)	-0.02 (0.04)
Birth Order	-.008 (.049)	-.005 (0.05)	0.01 (0.07)	-.003 (0.05)
Has Problems with English	-1.01 (0.35)	-0.45 (0.27)	1.04 (0.46)	0.44 (0.26)
Lives In a Female Headed Household	0.14 (0.19)	-0.51 (0.21)	-0.48 (0.28)	-0.04 (0.20)
Did Not Respond to Household Question	0.31 (0.70)	0.04 (0.28)	-5.23 (6.35)	-0.10 (0.28)
Male	0.14 (0.15)	-0.05 (0.15)	-0.81 (0.22)	-0.30 (0.14)

TABLE G.1 (continued)

ESTIMATED LOGIT COEFFICIENTS FOR
THE DROPOUT AND GRADUATION MODELS

	Probability of Dropping Out		Probability of Graduating	
	Coh III	Coh II	Coh III	Coh II
Asian	-1.71 (0.41)	-1.11 (0.32)	1.00 (0.49)	1.40 (0.30)
Black	-0.61 (.21)	-0.12 (0.20)	0.56 (0.33)	0.40 (0.20)
Hispanic	0.08 (0.26)	0.44 (0.24)	-0.31 (0.41)	-0.09 (0.24)
Baseline Grade	-.003 (.116)	-0.23 (0.10)	0.85 (0.36)	0.52 (0.12)
Boston	-0.27 (.25)	-0.69 (0.23)	0.65 (0.50)	0.33 (0.23)
Fresno	-0.10 (0.27)	.012 (0.24)	0.47 (0.35)	0.13 (0.24)
Portland	-0.29 (0.25)	-0.36 (0.23)	0.48 (0.34)	0.32 (0.22)
San Diego	-0.02 (0.25)	0.04 (0.24)	0.09 (0.33)	0.03 (0.23)
Sample Size	1335	1253	539	1119
Fit Measure[1]	.135	.154	.191	.151

[1] This is the [-2 (log likelihood function with the intercept only) minus -2 (log likelihood function with the intercept and the covariates)] divided by -2 (log likelihood function with the intercept only).

TABLE G.2

ESTIMATED LOGIT COEFFICIENTS FOR
FOR THE DROPOUT AND GRADUATION MODELS
WITH TREATMENT INTERACTIONS

	Probability of Dropping Out		Probability of Graduating	
	Coh III	Coh II	Coh III	Coh II
Constant	3.52 (1.38)	5.82 (1.31)	-11.98 (3.62)	-7.92 (1.43)
Treatment*Asian	-0.49 (0.67)	-0.27 (0.47)	1.08 (0.65)	-0.19 (0.41)
Treatment*Black	-0.13 (0.20)	-0.08 (0.20)	0.11 (0.28)	-0.02 (0.20)
Treatment*Hispanic	-0.12 (0.28)	0.43 (0.28)	-0.07 (0.44)	-0.57 (0.30)
Treatment*Other	0.38 (0.34)	0.83 (0.33)	-0.51 (0.58)	-0.53 (0.32)
Scaled Reading Score (in tens of points)	-.043 (.018)	-.048 (.016)	0.017 (.025)	0.040 (.015)
Scaled Math Score (in tens of points)	-.015 (.013)	-.013 (.014)	0.042 (.019)	0.006 (.013)
Reading Enjoyment Scale	0.05 (0.08)	-0.13 (0.07)	0.13 (0.12)	0.06 (0.07)
Math Enjoyment Scale	-0.14 (0.07)	-0.07 (0.07)	0.01 (0.11)	0.10 (0.07)
Repeated a Grade	0.42 (0.17)	0.51 (0.16)	-0.82 (0.32)	-0.40 (0.16)
Was Absent 10 or More Days of School	0.78 (0.16)	0.95 (0.17)	-0.95 (0.24)	-1.02 (0.17)
Expects to Pursue a Post-High-School Education	-0.51 (0.18)	-0.43 (0.16)	0.96 (0.29)	0.43 (0.16)
Family Size	0.01 (0.04)	0.01 (0.04)	0.001 (0.06)	-0.02 (0.04)
Birth Order	-.009 (.050)	-.008 (0.05)	0.01 (0.07)	-.003 (0.04)

TABLE G.2 (continued)

ESTIMATED LOGIT COEFFICIENTS FOR
FOR THE DROPOUT AND GRADUATION MODELS
WITH TREATMENT INTERACTIONS

	Probability of Dropping Out		Probability of Graduating	
	Coh III	Coh II	Coh III	Coh II
Has Problems with English	-1.00 (0.35)	-0.46 (0.28)	-1.04 (0.47)	0.43 (0.26)
Lives In a Female Headed Household	0.14 (0.19)	-0.50 (0.21)	-0.48 (0.28)	-0.05 (0.20)
Did Not Respond to Household Question	0.32 (0.70)	0.09 (0.28)	-5.13 (6.13)	-0.12 (0.28)
Male	0.14 (0.15)	-0.05 (0.15)	-0.79 (0.22)	-0.30 (0.14)
Asian	-1.28 (0.54)	-0.66 (0.45)	0.31 (0.60)	1.24 (0.40)
Black	-0.34 (0.30)	0.35 (0.29)	0.28 (0.45)	0.14 (0.27)
Hispanic	0.35 (0.35)	0.63 (0.34)	-0.49 (0.55)	-0.05 (0.33)
Baseline Grade	-.009 (.116)	-0.23 (0.11)	0.90 (0.36)	0.53 (0.12)

TABLE G.2 (continued)

ESTIMATED LOGIT COEFFICIENTS FOR
FOR THE DROPOUT AND GRADUATION MODELS
WITH TREATMENT INTERACTIONS

	Probability of Dropping Out		Probability of Graduating	
	Coh III	Coh II	Coh III	Coh II
Boston	-0.28	-0.68	0.67	0.34
	(0.25)	(0.24)	(0.51)	(0.23)
Fresno	-0.11	0.18	0.47	0.11
	(0.27)	(0.24)	(0.35)	(0.24)
Portland	-0.30	-0.35	0.48	0.32
	(0.25)	(0.23)	(0.34)	(0.22)
San Diego	-0.02	0.07	0.08	0.01
	(0.25)	(0.24)	(0.33)	(0.23)
Sample Size	1335	1253	539	1119
Fit Measure[2]	.137	.159	.197	.153

[2] This is the [-2 (log likelihood function with the intercept only) minus -2 (log likelihood function with the intercept and the covariates)] divided by -2 (log likelihood function with the intercept only).

TABLE G.3

ESTIMATED LOGIT COEFFICIENTS FOR
THE PROBABILITY OF HAVING A CHILD
FOR FEMALES

	Probability of Being a Mother	
	Coh III	Coh II
Constant	-1.98 (0.74)	-0.61 (0.60)
Treatment	-0.21 (0.21)	0.33 (0.19)
Asian	-0.67 (0.57)	-1.16 (0.42)
Black	0.48 (0.37)	0.05 (0.28)
Hispanic	-0.23 (0.47)	-0.24 (0.36)
Age at Follow-up	0.14 (0.22)	-0.05 (0.19)
Family Size	0.09 (0.06)	0.02 (0.05)
Lives In a Female Headed Household	0.26 (0.27)	-0.62 (0.27)
Birth Order	-0.14 (0.07)	0.00 (0.06)
Has Problems with English	0.36 (0.39)	0.11 (0.34)
Was Absent 10 or More Days of School	0.53 (0.24)	0.12 (0.22)
Had A Sex Education Course Prior to STEP	0.05 (0.27)	-0.38 (0.22)
Did Not Respond to Sex Education Course Question at Baseline	0.03 (0.88)	-0.12 (0.55)

TABLE G.3 (continued)

ESTIMATED LOGIT COEFFICIENTS FOR
THE PROBABILITY OF HAVING A CHILD
FOR FEMALES

	Probability of Being a Mother	
	Coh III	Coh II
Ever Had Sex Prior to STEP	0.97 (0.24)	0.82 (0.22)
Did Not Respond to Sex Course Question at Baseline	-0.80 (0.77)	0.24 (0.36)
Baseline Measure of Contraceptive Knowledge (12-point scale)	0.05 (0.08)	0.12 (0.06)
Baseline Measure of Contraceptive Availability (8-point scale)	-0.07 (0.07)	0.00 (0.06)
Baseline Measure of the Consequences of Teenage Pregnancy (2-point scale)	-0.10 (0.17)	-0.07 (0.15)
Baseline Judgement About Joint Responsibility for Birth Control (1=yes, 0=if otherwise)	0.15 (0.28)	-0.40 (0.21)

TABLE G.3 (continued)

ESTIMATED LOGIT COEFFICIENTS FOR
THE PROBABILITY OF HAVING A CHILD
FOR FEMALES

	Probability of Being a Mother	
	Coh III	Coh II
Boston	-0.10 (0.35)	-0.10 (0.31)
Fresno	0.41 (0.36)	0.43 (0.31)
Portland	-0.59 (0.39)	0.01 (0.31)
San Diego	-0.06 (0.35)	-0.18 (0.32)
Frequency of Contraceptive Use	-0.05 (0.12)	-0.14 (0.13)
Sample Size	619	610
Fit Measure[3]	.104	.098

[3] This is the [-2 (log likelihood function with the intercept only) minus -2 (log likelihood function with the intercept and the covariates)] divided by -2 (log likelihood function with the intercept only).

TABLE G.4

ESTIMATED LOGIT COEFFICIENTS FOR
THE PROBABILITY OF HAVING A CHILD MODEL
FOR FEMALES WITH TREATMENT INTERACTIONS

	Probability of Being a Mother	
	Coh III	Coh II
Constant	-2.50 (0.84)	-0.49 (0.64)
Treatment*Asian	1.09 (0.82)	0.97 (0.56)
Treatment*Black	-0.58 (0.28)	0.20 (0.25)
Treatment*Hispanic	-0.22 (0.48)	0.64 (0.45)
Treatment*Other	0.82 (0.68)	-0.09 (0.49)
Asian	-1.00 (0.96)	-1.70 (0.59)
Black	1.23 (0.57)	-0.06 (0.38)
Hispanic	0.37 (0.67)	-0.63 (0.51)
Age at Follow-up	0.13 (0.22)	-0.04 (0.19)
Family Size	0.09 (0.06)	0.02 (0.05)
Lives In a Female Headed Household	0.23 (0.27)	-0.62 (0.27)
Birth Order	-0.13 (0.07)	0.01 (0.06)
Has Problems with English	0.39 (0.40)	0.10 (0.35)
Was Absent 10 or More Days of School	0.56 (0.24)	0.13 (0.22)

TABLE G.4 (continued)

ESTIMATED LOGIT COEFFICIENTS FOR
THE PROBABILITY OF HAVING A CHILD
FOR FEMALES WITH TREATMENT INTERACTIONS

	Probability of Being a Mother	
	Coh III	Coh II
Had A Sex Education Course Prior to STEP	-0.01 (0.28)	-0.38 (0.22)
Did Not Respond to Sex Education Course Question at Baseline	-0.07 (0.87)	-0.11 (0.55)
Ever Had Sex Prior to STEP	0.98 (0.24)	0.82 (0.23)
Did Not Respond to Sex Course Question at Baseline	-0.82 (0.77)	0.25 (0.36)
Baseline Measure of Contraceptive Knowledge (12-point scale)	0.06 (0.08)	0.13 (0.06)
Baseline Measure of Contraceptive Availability (8-point scale)	-0.06 (0.07)	0.00 (0.06)
Baseline Measure of the Consequences of Teenage Pregnancy (2-point scale)	-0.12 (0.18)	-0.08 (0.15)
Baseline Judgement About Joint Responsibility for Birth Control (1=yes, 0=if otherwise)	0.16 (0.28)	-0.39 (0.21)

TABLE G.4 (continued)

ESTIMATED LOGIT COEFFICIENTS FOR
THE PROBABILITY OF HAVING A CHILD
FOR FEMALES WITH TREATMENT INTERACTIONS

	Probability of Being a Mother	
	Coh III	Coh II
Boston	-0.16 (0.36)	-0.10 (0.31)
Fresno	0.33 (0.36)	0.45 (0.32)
Portland	-0.65 (0.39)	0.04 (0.32)
San Diego	-0.12 (0.36)	-0.16 (0.33)
Frequency of Contraceptive Use	-0.07 (0.12)	-0.14 (0.13)
Sample Size	619	610
Fit Measure[4]	.115	.102

[4] This is the [-2 (log likelihood function with the intercept only) minus -2 (log likelihood function with the intercept and the covariates)] divided by -2 (log likelihood function with the intercept only).

TABLE G.5

ESTIMATED LOGIT COEFFICIENTS FOR
THE AFDC RECEIPT MODEL
AT THE TIME OF THE INTERVIEW

	Probability of Receiving AFDC	
	Coh III	Coh II
Constant	-15.37 (7.30)	4.31 (6.92)
Treatment	0.28 (0.42)	0.39 (0.32)
Scaled Reading Score (in tens of points)	.038 (.050)	-.032 (.040)
Scaled Math Score (in tens of points)	.011 (.041)	-.020 (.033)
Reading Enjoyment Scale	-0.17 (0.20)	-0.07 (0.16)
Math Enjoyment Scale	0.00 (0.18)	0.03 (0.14)
Repeated a Grade	-0.39 (0.47)	0.43 (0.39)
Was Absent 10 or More Days of School	-0.34 (0.47)	0.10 (0.37)
Expects to Pursue a Post-High-School Education	-0.69 (0.50)	-0.58 (0.37)
Family Size	-0.07 (0.11)	0.03 (0.09)
Birth Order	-0.12 (0.14)	-0.07 (0.09)
Has Problems with English	0.04 (0.80)	-0.36 (0.61)
Lives In a Female Headed Household	0.21 (0.53)	0.32 (0.49)
Did Not Respond to Household Question	n.a.	0.25 (0.90)
Asian	0.14 (1.11)	-0.83 (0.79)

TABLE G.5 (continued)

ESTIMATED LOGIT COEFFICIENTS FOR
THE AFDC RECEIPT MODEL
AT THE TIME OF THE INTERVIEW

	Probability of Receiving AFDC	
	Coh III	Coh II
Black	0.32 (0.68)	0.29 (0.49)
Hispanic	0.65 (0.83)	-0.10 (0.60)
Age at Follow-up	0.73 (0.35)	0.01 (0.32)
Boston	-1.20 (0.66)	-0.26 (0.54)
Fresno	1.00 (0.63)	0.46 (0.55)
Portland	-0.39 (0.70)	-0.11 (0.52)
San Diego	1.27 (0.63)	0.02 (0.54)
Sample Size	154	218
Fit Measure[5]	.200	.066

[5] This is the [-2 (log likelihood function with the intercept only) minus -2 (log likelihood function with the intercept and the covariates)] divided by -2 (log likelihood function with the intercept only).

TABLE G.6

ESTIMATED LOGIT COEFFICIENTS FOR
THE AFDC RECEIPT MODEL AT THE TIME OF THE INTERVIEW
WITH TREATMENT INTERACTIONS

	Probability of Receiving AFDC	
	Coh III	Coh II
Constant	-17.84	3.87
	(7.66)	(6.91)
Treatment*Asian	3.64	0.51
	(1.76)	(1.09)
Treatment*Black	0.25	0.13
	(0.51)	(0.42)
Treatment*Hispanic	0.69	0.54
	(1.11)	(0.76)
Treatment*Other	-1.83	1.31
	(1.22)	(0.95)
Scaled Reading Score (in tens of points)	.047	-.028
	(.051)	(.040)
Scaled Math Score (in tens of points)	-.002	-.022
	(.042)	(.033)
Reading Enjoyment Scale	-0.25	-0.07
	(0.21)	(0.16)
Math Enjoyment Scale	0.07	0.03
	(0.18)	(0.15)
Repeated a Grade	-0.54	0.43
	(0.48)	(0.40)
Was Absent 10 or More Days of School	-0.29	0.11
	(0.49)	(0.37)
Expects to Pursue a Post-High-School Education	-0.86	-0.58
	(0.52)	(0.37)
Family Size	-0.09	0.03
	(0.11)	(0.10)
Birth Order	-0.12	-0.07
	(0.14)	(0.10)

TABLE G.6 (continued)

ESTIMATED LOGIT COEFFICIENTS FOR
THE AFDC RECEIPT MODEL AT THE TIME OF THE INTERVIEW
WITH TREATMENT INTERACTIONS

	Probability of Receiving AFDC	
	Coh III	Coh II
Has Problems with English	-0.23	-0.34
	(0.84)	(0.64)
Lives In a Female Headed Household	0.33	0.39
	(0.54)	(0.50)
Did Not Respond to Household Question	n.a.	0.34
		(0.91)
Asian	-3.16	-0.63
	(1.76)	(1.12)
Black	-0.73	0.72
	(0.95)	(0.61)
Hispanic	-0.53	0.07
	(1.11)	(0.82)
Age at Follow-up	0.95	0.01
	(0.37)	(0.32)
Boston	-1.25	-0.24
	(0.69)	(0.54)
Fresno	0.93	0.49
	(0.64)	(0.55)
Portland	-0.08	-0.13
	(0.72)	(0.53)
San Diego	1.36	0.04
	(0.65)	(0.54)
Sample Size	154	218
Fit Measure[6]	.232	.071

[6] This is the [-2 (log likelihood function with the intercept only) minus -2 (log likelihood function with the intercept and the covariates)] divided by -2 (log likelihood function with the intercept only).

TABLE G.7

ESTIMATED LOGIT COEFFICIENTS FOR
THE PROBABILITY OF BEING EMPLOYED

	For In-School Youth During		For Out-of-School Youth During	
	Months 39-41	Months 49-51	Months 39-41	Months 49-51
Constant	-3.86 (0.83)	-2.50 (1.46)	-2.79 (1.57)	-2.66 (1.49)
Treatment	-0.01 (0.10)	0.11 (0.19)	0.33 (0.21)	-0.14 (0.20)
Age at Follow-up	-0.10 (0.11)	-0.18 (0.20)	-0.30 (0.22)	-0.16 (0.21)
Worked Before STEP	0.18 (0.12)	0.12 (0.23)	0.31 (0.24)	-0.07 (0.23)
Hours Worked While in High School Before The Relevant Quarter (in 00s)	0.26 (0.02)	0.12 (0.02)	0.17 (0.02)	0.07 (0.01)
Hours Worked After High School Before The Relevant Quarter (in 00s)	0.33 (0.05)	0.14 (0.03)	0.23 (0.03)	0.08 (0.01)
Cohort II Dummy	-0.98 (0.11)	n.a.	-0.60 (0.25)	n.a.
Scaled Reading Score (in tens of points)	.022 (.010)	.039 (.023)	-.027 (.024)	.045 (.022)
Scaled Math Score (in tens of points)	.026 (.090)	-.022 (.018)	.031 (.022)	-.008 (.019)
Reading Enjoyment Scale	-0.08 (0.05)	-0.10 (0.09)	0.25 (0.12)	0.00 (0.10)
Math Enjoyment Scale	0.02 (0.05)	0.09 (0.09)	-0.02 (0.11)	-0.00 (0.09)
Repeated a Grade	0.04 (0.12)	0.23 (0.24)	0.18 (0.25)	0.00 (0.22)
Was Absent 10 or More Days of School	-0.23 (0.14)	-0.00 (0.27)	0.47 (0.24)	-0.13 (0.23)

TABLE G.7 (continued)

ESTIMATED LOGIT COEFFICIENTS FOR
THE PROBABILITY OF BEING EMPLOYED

	For In-School Youth During		For Out-of-School Youth During	
	Months 39-41	Months 49-51	Months 39-41	Months 49-51
Expects to Pursue a Post-High-School Education	0.21 (0.14)	0.03 (0.24)	0.15 (0.25)	0.33 (0.23)
Family Size	-0.01 (0.03)	-0.02 (0.05)	0.04 (0.06)	0.01 (0.05)
Birth Order	-0.03 (0.03)	0.02 (0.06)	0.03 (0.07)	-0.07 (0.07)
Has Problems with English	-0.06 (0.17)	-0.00 (0.29)	0.44 (0.41)	-0.50 (0.36)
Lives In a Female Headed Household	0.21 (0.15)	-0.27 (0.28)	-0.23 (0.33)	-0.47 (0.29)
Did Not Respond to Household Question	-0.18 (0.34)	0.22 (0.39)	-0.75 (0.44)	-0.37 (0.37)
Male	0.23 (0.11)	0.29 (0.19)	0.35 (0.22)	0.67 (0.20)
Asian	0.43 (0.23)	0.67 (0.40)	-0.69 (0.53)	-0.42 (0.45)
Black	0.08 (0.17)	0.75 (0.33)	-0.43 (0.29)	-0.85 (0.27)
Hispanic	0.10 (0.21)	0.57 (0.42)	-0.83 (0.37)	0.12 (0.34)

TABLE G.7 (continued)

ESTIMATED LOGIT COEFFICIENTS FOR
THE PROBABILITY OF BEING EMPLOYED

	For In-School Youth During		For Out-of-School Youth During	
	Months 39-41	Months 49-51	Months 39-41	Months 49-51
Boston	-0.20	-0.72	0.62	-0.72
	(0.17)	(0.30)	(0.39)	(0.33)
Fresno	-0.02	-0.26	0.59	-0.40
	(0.19)	(0.33)	(0.38)	(0.34)
Portland	-0.10	-0.01	0.96	-0.15
	(0.18)	(0.31)	(0.34)	(0.32)
San Diego	0.26	0.04	1.06	-0.08
	(0.17)	(0.29)	(0.36)	(0.33)
Sample Size	2053	649	557	614
Fit Measure[7]	.210	.222	.278	.218

[7] This is the [-2 (log likelihood function with the intercept only) minus -2 (log likelihood function with the intercept and the covariates)] divided by -2 (log likelihood function with the intercept only).

TABLE G.8

ESTIMATED LOGIT COEFFICIENTS FOR
THE PROBABILITY OF BEING EMPLOYED
WITH TREATMENT INTERACTIONS

	For In-School Youth During		For Out-of-School Youth During	
	Months 39-41	Months 49-51	Months 39-41	Months 49-51
Constant	-3.78	-1.92	-3.17	-3.06
	(0.84)	(1.50)	(1.61)	(1.50)
Treatment*Asian	-0.02	0.75	0.58	0.22
	(0.21)	(0.34)	(0.80)	(0.63)
Treatment*Black	0.33	-0.03	0.31	-0.36
	(0.15)	(0.28)	(0.32)	(0.28)
Treatment*Hispanic	-0.80	-0.05	-0.02	-0.41
	(0.26)	(0.55)	(0.42)	(0.20)
Treatment*Other	-0.24	-1.10	0.72	-0.58
	(0.30)	(0.59)	(0.46)	(0.46)
Age at Follow-up	-0.08	-0.23	-0.29	-0.16
	(0.11)	(0.21)	(0.23)	(0.21)
Worked Before STEP	0.18	0.13	0.31	-0.05
	(0.12)	(0.23)	(0.24)	(0.23)
Hours Worked While in High School Before The Relevant Quarter (in 00s)	0.26	0.13	0.17	0.07
	(0.02)	(0.02)	(0.02)	(0.01)
Hours Worked After High School Before The Relevant Quarter (in 00s)	0.33	0.15	0.23	0.09
	(0.05)	(0.03)	(0.03)	(0.01)
Cohort II Dummy	-0.98	n.a.	-0.59	n.a.
	(0.11)		(0.25)	
Scaled Reading Score (in tens of points)	.022	.037	-.026	.046
	(.010)	(.024)	(.024)	(.022)
Scaled Math Score (in tens of points)	.026	-.022	.030	-.001
	(.090)	(.018)	(.022)	(.019)
Reading Enjoyment Scale	-0.08	-0.11	0.26	0.00
	(0.05)	(0.10)	(0.12)	(0.11)

TABLE G.8 (continued)

ESTIMATED LOGIT COEFFICIENTS FOR
THE PROBABILITY OF BEING EMPLOYED
WITH TREATMENT INTERACTIONS

	For In-School Youth During		For Out-of-School Youth During	
	Months 39-41	Months 49-51	Months 39-41	Months 49-51
Math Enjoyment Scale	0.02	0.10	-0.01	0.00
	(0.05)	(0.09)	(0.11)	(0.09)
Repeated a Grade	0.05	-0.26	0.19	-0.03
	(0.12)	(0.24)	(0.25)	(0.22)
Was Absent 10 or More Days of School	-0.24	0.02	0.46	-0.14
	(0.14)	(0.28)	(0.24)	(0.24)
Expects to Pursue a Post-High-School Education	0.21	0.08	0.15	0.31
	(0.14)	(0.24)	(0.25)	(0.23)
Family Size	-0.01	-0.02	0.04	0.01
	(0.03)	(0.05)	(0.06)	(0.05)
Birth Order	-0.03	0.03	0.03	-0.07
	(0.03)	(0.06)	(0.07)	(0.07)
Has Problems with English	-0.04	0.01	0.48	-0.47
	(0.17)	(0.30)	(0.41)	(0.36)
Lives In a Female Headed Household	0.19	-0.26	-0.22	-0.45
	(0.15)	(0.28)	(0.33)	(0.29)
Did Not Respond to Household Question	-0.19	0.20	-0.72	-0.30
	(0.34)	(0.39)	(0.44)	(0.37)
Male	0.23	0.27	0.34	0.65
	(0.11)	(0.20)	(0.22)	(0.20)

TABLE G.8 (continued)

ESTIMATED LOGIT COEFFICIENTS FOR
THE PROBABILITY OF BEING EMPLOYED
WITH TREATMENT INTERACTIONS

	For In-School Youth During		For Out-of-School Youth During	
	Months 39-41	Months 49-51	Months 39-41	Months 49-51
Asian	0.31	-0.18	-0.60	-0.24
	(0.29)	(0.50)	(0.66)	(0.57)
Black	-0.20	0.28	-0.21	-0.38
	(0.24)	(0.44)	(0.42)	(0.38)
Hispanic	0.41	0.12	-0.46	0.60
	(0.29)	(0.58)	(0.48)	(0.46)
Boston	-0.20	-0.70	0.66	-0.67
	(0.17)	(0.30)	(0.40)	(0.33)
Fresno	-0.03	-0.24	0.62	-0.33
	(0.19)	(0.33)	(0.38)	(0.34)
Portland	-0.11	0.06	0.98	-0.10
	(0.18)	(0.31)	(0.36)	(0.32)
San Diego	0.27	0.03	1.09	-0.01
	(0.17)	(0.29)	(0.36)	(0.33)
Sample Size	2053	649	557	614
Fit Measure[8]	.215	.231	.280	.223

[8] This is the [-2 (log likelihood function with the intercept only) minus -2 (log likelihood function with the intercept and the covariates)] divided by -2 (log likelihood function with the intercept only).

Table G.9

NUMBER OF TREATMENT AND CONTROL YOUTH
FOR WHOM THERE ARE SCHOOL DATA BY TERM

Terms After STEP Enrollment	Sample Size	Percentage of Total Sample Inexplicably Missing
In-Program		
Term 1	2,232	0.2%
Term 2	2,720	0.7
Postprogram		
Term 3	2,386	1.3%
Term 4	2,969	2.5
Term 5	2,324	4.6
Term 6	2,919	4.9
Term 7	1,139	8.1
Term 8	1,422	8.9

Note: Data for terms 7 and 8 based on Cohort II youth only.

APPENDIX H

ADDITIONAL ANALYSES OF OUTCOMES DATA

In Chapter 4, we presented some interesting analyses, such as those concerning the dropout, graduation and fertility-related rates. In this appendix, we present supporting analyses and analyses of related outcomes--in particular, multivariate analyses of grade progression, test scores, contraceptive knowledge, fertility-related outcomes, employment, earnings and hours.

GRADE PROGRESSION

The dropout analysis found that STEP treatments and controls were equally likely to drop out. Here, we investigate whether the treatments were able to progress further in school or acquire more basic skills than controls. Two indicators of grade progression were examined, credit accumulation and highest grade completed. Both variables reflect advancement, but the measures have different degrees of precision.[1] Credit accumulation is the finer measure because differences within grade can be observed. However, as STEP youth (both treatments and controls) have been progressively more likely to move to alternative high schools where credits were not recorded, the measure becomes less useful as graduation grows closer.[2] The highest grade completed is also a measure of school progression, but it can be somewhat misleading because in some school districts promotion is critically linked with credit accumulation while in others promotion is automatic regardless of credit accumulation.

To conclude that STEP positively affects grade progression, the analysis must show positive impacts on both of these outcomes. If only sporadic impacts are observed, we conclude that these sporadically significant estimates were due to chance.

Table H.1 presents the longitudinal progression of STEP treatments through high school in terms of the highest grade enrolled and the percent of total graduation credits earned. Next to each of the treatment group means is the difference between the treatment group's level and the control group's level. The table indicates that the two groups progressed through school fairly

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[1] The problems associated with measures of these outcomes are discussed fully in Appendix F.

[2] During the year immediately after the youth's first summer of STEP, 94% of those enrolled in school had credits reported on their transcripts, compared with only 85% of those enrolled in school three years after enrollment and 83% of those enrolled in school four years after enrollment.

similarly until near the end of the follow-up period. Then treatment youth appear to have slowed down relative to control youth. Treatment youth earned fewer credits and remained in lower grade levels.

Table H.2 presents the estimated impact of STEP on graduation credits earned by the end of the third year post-enrollment (as reported on student transcripts) and the highest grade completed (as reported in the most recent follow-up interview). We found that the treatment group earned approximately the same percentage of graduation credits as controls by the end of Term 6. Credit accumulation did not differ by subgroup. "White and other" youth seemed to earn fewer credits but we cannot reject the hypothesis that STEP's impact on credit accumulation was the same for all racial/ethnic groups.

The analysis of the highest grade completed reveals no significant subgroup differentials. The 42-month postprogram impact was based on Cohort III and the 54-month postprogram impact was based on Cohort II.[3] Thus, we do not find evidence to support the hypothesis, which was posited based on the dropout analysis that STEP was more effective among ninth-grade enrollees.

TEST PERFORMANCE

Thus far, there is no evidence that STEP altered the treatment group's educational experience in the long term. Earlier reports of in-program impacts, however, did find that the treatment group's reading and math abilities increase relative to the control group's after the first summer of STEP. Perhaps a skill differential was maintained over the long run, even though similar numbers of treatments and controls ultimately dropped out.

In all sites except Portland, the school district administered standardized tests to ascertain the performance of their students relative to a national norm. The scaled test scores of treatments and controls were converted to normal curve equivalent scores (NCEs), which range from 1 to 99, so that data from the

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[3] One would expect that the average highest grade completed would be somewhat lower than the highest grade enrolled. However, this relationship does not hold between self-reported highest grade completed and school-reported last grade attended. The average grade enrolled for Cohort III treatments in Term 6 was 10.5. Cohort III's interview-based highest grade completed was 10.9. For Cohort II, the Term 8 school-based highest grade enrolled was 10.9 while the interview-based highest grade completed was 11.2. Although there appears to be "grade inflation," it is not differentially inflated by treatment status; thus, the estimated STEP impact should remain unbiased.

different tests used by the four sites could be pooled for the analysis.

The results of test scores comparisons must be interpreted with care. Table H.3 contrasts the math and reading abilities of STEP treatment and control youth who are still in school at the time of the test. We have no measure of the abilities of youth who have dropped out of school. Because youth who do worse in school are the most likely to drop out, treatment/control test differences of in-school youth may be misleading. If STEP encourages youth to stay in school, even at lower levels of achievement, in-school treatments may perform worse on tests than do in-school controls. However, we empirically tested whether treatment youth are more likely to stay in school with lower test scores, but found no evidence to support the hypothesis. Thus, while the average scores and impacts presented in Table H.3 represent only the abilities of in-school youth, there should be no bias in the impact estimate.

Table H.3 also shows the average standardized math and reading scores over time for the treatment and control youth who took the tests, with the difference between the two groups' mean levels. The table shows that STEP had no effect on in-school test performance. While the Term 8 impact on math scores is numerically large, we only have data on a very small portion of the sample. First, only Cohort II youth still in school could have taken the test at this time. Second, San Diego seniors were not tested. Finally, the transcripts were missing a fair amount of data. Thus, Term 8 results should be viewed as inconclusive.

Table H.4 presents the results of the subgroup analysis. It shows that STEP significantly increased test scores for none of the subgroups we considered.

NON-HIGH SCHOOL EDUCATION

In Chapter 5, we showed that treatments and controls were equally likely to receive a GED certificate. Table H.5 presents the results of multivariate analysis on this outcome for Cohort II dropouts. The table confirms that, for the sample as a whole, there were no impacts 54 months after enrollment. However, treatments who entered STEP after ninth grade but subsequently dropped out of high school appeared to be more likely to obtain a GED certificate than control youth. Among Cohort II dropouts, 19.2 percent of the control youth obtained GED certificates, while an estimated 30.7 percent (19.2 + 11.5) of treatment youth enrolling after ninth grade did so.

Table H.6 shows the analysis of STEP's impact on pursuing a college education for graduates. While there was no significant impact on obtaining a college education for the sample as a whole, Cohort III Hispanic graduates were much more likely to be

going to college 42 months post-enrollment than their control counterparts. However, Cohort II Hispanics were not more likely to be in college at 54 months. In addition, we cannot reject the hypothesis that STEP impacted all the racial/ethnic groups equally. Thus, there is no evidence strong enough to conclude that STEP led any subgroup to pursue a college education at higher rates than they would have in the absence of the program.

LIFE SKILL OUTCOMES

Knowledge of Contraception

The results presented in Table H.7 indicate that the in-program impact on knowledge of contraception has endured. As one would expect, the knowledge level among control youth increased as they matured; however, treatment youth still exhibited a higher average level of knowledge. Although the magnitude of the impact differs by gender, race/ethnicity and site, the impact is significant for nearly every subgroup at both 42 and 54 months after enrollment in the program.

Fertility Outcomes

Among female treatment group members, increased knowledge of contraception did not translate into fewer pregnancies or children. Multivariate analysis, shown in Table H.8, found no impact of STEP on the probability of having a pregnancy for the sample as a whole or for any subgroup. But analysis of having a child (shown in Table H.9) finds that Cohort II treatment youth pregnancies were more likely to result in a birth than among control youth.

Similarly, among male treatments, STEP had no impact on the probability of causing pregnancy (shown in Table H.10). Among boys who have been responsible for a pregnancy, participation in STEP had no impact on the likelihood that boys would have a child, as indicated in Table H.11.

WELFARE OUTCOMES

Next, we investigated whether treatment group mothers were more or less likely to go on AFDC after they became eligible. Table H.12 shows that no significant difference in welfare use exists between treatment- or control-group mothers. There were also no subgroup differentials.

The last welfare outcome considered is food stamp receipt. Table H.13 presents our food stamp receipt analysis. Overall, there was no difference in the probability that a treatment or control group youth was living in a household that received food stamps. We also cannot reject the hypothesis that all subgroup impacts were equal to the overall impact.

EMPLOYMENT

Chapter 5 discussed how the youth's employment patterns shifted over time. No difference was apparent between the treatment and the control groups.

To investigate subgroup impacts, employment, earnings, wages and hours were examined for two periods in time. The first period was the three-month quarter spanning months 37 to 39--the latest quarter for which employment information was collected for both Cohort II and III youth. The other period was the quarter spanning months 49 to 51, the latest quarter for which any employment data were available. Data for this quarter concern only Cohort II youth. Both of these three month periods were summers. Thus, employment activity can be observed among in-school youth as well as among out-of-school youth. However, since the employment behavior of in-school and out-of-school youth is likely to be quite different, we analyzed them separately.[4]

Employment Impacts on Youth Still In School, Months 37 to 39

Table H.14 presents the impact of STEP on the probability of being employed during months 37 to 39, on hours worked per week averaged over the three-month period for anyone who was employed, and highest weekly salary received during the period for youth who have neither dropped out nor graduated by month 39. The youth were 17 or 18 years old at this point, and approximately 80 percent were still in school.

Table H.14 shows that for the in-school sample as a whole, STEP participation did not change a youth's likelihood of being employed four summers after enrollment. However, the impact did differ among race/ethnicity groups. Black treatment youth were 8.3 percentage points more likely to be employed, while Hispanic youth were substantially--20.0 percentage points--less likely to be employed during that summer. Among those who were employed, there were no treatment/control differences in the number of hours worked or the wages earned for either the in-school sample as a whole or for any subgroup.

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[4] The multivariate analyses conducted controlled for differences in the youth at baseline. The standard set of factors, such as reading and math scores, self-reported attendance, number of siblings, living in a female-headed home, and post-high school expectations, are described in the methodological appendix. However, in addition, the economic analyses control for the amount of work experience a youth has had prior to the period under consideration.

Table H.15 summarizes STEP's impact on the earnings of in-school youth four summers after enrolling in STEP--two summers post-program. Again for the in-school sample as a whole there was no impact. However, the differential employment impacts translated into a positive $105 earnings impact on black youth and a negative $225 earnings impact on Hispanic youth.

Table H.15 also indicates that Cohort II treatments did not fare as well as Cohort III treatments, relative to their controls. Cohort II treatment youth earned $105 less than Cohort II controls, whereas Cohort III treatments earned $82 more than Cohort III controls. The differential impact may be related to the difference in labor market conditions faced by the two cohorts. Program months 37 to 39 spanned the summer of 1989 for Cohort II, while they spanned the summer of 1990 for Cohort III. Labor market conditions facing Cohort II were much poorer than those facing Cohort III. Only 48 percent of the Cohort II controls were employed during program months 37 to 39, while 56 percent of Cohort III controls were employed during their program months 37 to 39.[5] Given the poor economic conditions facing Cohort II youth in general, any advantage STEP may have provided the treatments would have had difficulty translating into greater employment opportunities.

Employment Impacts on Youth Out of School Months 37 to 39

The employment behavior of youth who had left school by month 39 is examined in Table H.16. For the out-of-school sample as a whole, treatments were no more likely to be employed than controls. However, when the impacts were calculated for Cohorts II and III, we found no impact on Cohort II youth but a large positive increase in employment for Cohort III treatments. Again, the difficult labor market conditions facing Cohort II youth may account for the lack of impact. Among the employed, there was no difference in either wages or hours between treatment and control youth.

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[5] The employment opportunities of the control group represent the best gauge of the labor market facing the treatment group because the controls share the same regional, age, racial/ethnic, and ability distributions as the treatments. However, to give additional context to the conditions facing the STEP youth, we find that the employment rate for all U.S. 16- to 19-year-olds was 47.7% in June 1989 and grew to 48.1% by August 1989. In June 1990, it was 45.4% and fell to 42.8% by August 1990. Thus, we see that while the labor market for STEP youth was about the same as the national labor market for youth in 1989, STEP youth (both treatments and controls) fared much better than the average youth in the summer of 1990.

Table H.17 presents STEP's aggregate impact on earnings for all out-of-school youth. Even though Cohort III treatments were more likely to be employed than their control counterparts, they earned no more relative to their control group than did Cohort II treatments relative to Cohort II controls.

Employment Impacts on Youth Still In School Months 49 to 51

Next, we examined economic activities during months 49 to 51, five summers after STEP enrollment. This follow-up period can be observed only for Cohort II, for whom these months spanned the summer of 1990, a summer with better employment possibilities than the previous one. The youth were 18 or 19 years old, and approximately half of them (51.4 percent) stated that they had not left school by month 51.

Table H.18 presents the impacts of STEP on the in-school sample of youth with respect to employment, hours and wages. STEP participation did not change an average Cohort II youth's likelihood of being employed five summers after enrollment. However, the impact does differ among race/ethnicity groups--as it did for the summer before. The groups affected were not the same as the previous summer, when black treatment youth were more likely to be employed and Hispanic youth were less likely to be employed. During the fifth post-enrollment summer, Asian in-school treatments were much more likely to be employed than their control counterparts, while white and "other" treatments were substantially less likely to be employed. STEP increased the employment rate among Asian treatments 18.4 percentage points over that of Asian controls. White and other treatments, on the other hand, had an employment rate 27.1 percentage points lower than their controls. Among those who were employed, there were no aggregate treatment/control differences in the number of hours worked or the wages earned. However, white and other treatment youth who were employed appeared to have worked more hours and earned higher weekly wages.

Table H.19 shows the overall impact of STEP on earnings for the in-school sample during months 49 to 51. On average, in-school control group youth earned $923 over the summer. The earnings of treatments was $82 higher, but this difference is not large enough to be certain that it was due to STEP rather than chance. The impact variation with respect to race/ethnicity did not carry through to earnings, primarily because white treatments who were employed earned enough to compensate for the lower employment rate among white treatments versus white controls.

Employment Impacts on Youth Out of School Months 49 to 51

The employment behavior of youth who had left school by month 51 is examined in Table H.20. As a whole and for all subgroups examined, out-of-school treatments were no more likely to be

employed than controls. Similarly, they worked the same number of hours over the summer and were paid approximately the same as controls. The only differential impact appears to be STEP's impact on hours worked by site. Boston and San Diego treatment youth worked more hours, and Fresno and Seattle treatments worked fewer hours than their control counterparts. This site differential is suspect, since it was not apparent among the in-school youth. The site differential is also not evident in STEP's aggregate impact on earnings during months 49 to 51, as seen in Table H.21. Fresno treatments appear to have earned less than Fresno controls but we cannot reject the hypothesis that all the site impacts were the same.

Summary of Employment Impacts

STEP did not affect wages or hours for any group. When it did affect a particular group, it did so by increasing the probability of employment for members of that group. The data collected so far indicate that STEP may have a differential impact on employment rates of various racial/ethnic groups. During the fourth summer after STEP enrollment, black youth were more likely to be employed than their control counterparts, while Hispanic treatments were less likely to be employed. During the fifth summer after STEP enrollment, Asian treatments were more likely to be employed than controls, while white and other treatments were less likely to be employed. Unfortunately, the inconsistency of these patterns over time makes it difficult to understand STEP's true impact.

Table H.1

HIGHEST GRADE ENROLLED AND PERCENTAGE
OF THE GRADUATION CREDITS EARNED FOR COHORTS II AND III
WITH TREATMENT IMPACT OVER TIME

Terms After STEP Enrollment	Highest Grade Enrolled		Percentage of Total Graduation Credits Earned	
	Treatment Mean	Impact	Treatment Mean	Impact
In-Program				
Term 1	9.49	0.00	25.4%	-0.1
Term 2	9.43	0.00	33.6	-0.3
Postprogram				
Term 3	10.15	-0.01	43.0	-0.3
Term 4	10.05	-0.04	49.1	-0.9
Term 5	10.75	-0.02	58.5	-0.7
Term 6	10.65	-0.04	63.0	-1.5
Term 7	10.97	-0.13*	64.5	-4.6**
Term 8	10.94	-0.07	67.4	-3.4*

Note: The sample size used in each term is shown in Table B.1 Data for terms 7 and 8 based on Cohort II youth only.

** Indicates that the impact is statistically different from zero at a 0.05 level of significance.

* Indicates that the impact is statistically different from zero at a 0.10 level of significance.

Table H.2

ESTIMATED IMPACT OF STEP ON CUMULATIVE GRADUATION CREDITS AND THE HIGHEST GRADE COMPLETED FOR COHORTS II AND III

	Treatment Impact, Percent of Graduation Credits Earned Six Terms After Enrollment	Treatment Impact, Highest Grade Completed by Month 42 (Cohort III)	Treatment Impact, Highest Grade Completed by Month 54 (Cohort II)
Overall	-0.6	-0.03	-0.01
Cohort II	-0.5	n.a.	n.a.
Cohort III	-0.8	n.a.	n.a.
Gender			
Male	-0.7	-0.03	0.01
Female	-0.5	-0.02	-0.02
Race/Ethnicity			
Asian	-0.5	0.10	-0.04
Black	-0.6	-0.06	0.09
Hispanic	-0.3	-0.10	-0.07
White and Other	-1.2*	0.04	-0.15
Site			
Boston	-0.5	-0.04	0.18
Fresno	-0.6	-0.02	0.04
Portland	-0.9	0.04	-0.12
San Diego	-0.4	-0.05	0.00
Seattle	-0.9	-0.08	-0.14
Grade Completed at Enrollment			
8th	-0.5	-0.03	0.01
9th	-0.8	-0.01	-0.03
Sample Size	14602	1327	1231

Note: On average, controls had completed 64.5% of the credits needed to graduate six terms after their STEP enrollment. At the time of the Wave III interview, Cohort III controls had completed an average of 10.87 years of school, while Cohort II controls had completed an average of 11.25 years of school.

Information for all terms and all individuals was used to develop these impacts. The estimates presented in the table are fixed-effect estimates adjusting for the non-independence of the observations. The actual number of transcripts available in Term Six was 2,810.

* Indicates that the impact is statistically different from zero at a 0.10 level of significance.

Table H.3

READING AND MATH SCORES WITH TREATMENT IMPACTS OVER TIME

Terms After STEP Enrollment	Reading Score		Math Score	
	Treatment Mean (in NCEs)	Impact (in NCEs)	Treatment Mean (in NCEs)	Impact (in NCEs)
In-Program				
Term 2	36.8	-0.5	48.3	-2.0
Postprogram				
Term 4	48.8	1.8	61.7	0.4
Term 6	52.9	-0.1	66.4	-0.7
Term 8	54.1	1.1	76.1	6.1

Note: The sample sizes that the treatment/control differences were based on are 966 for Term 2, 693 for Term 4, 416 for Term 6, and 65 for Term 8. The sample sizes are lower than for other analyses because only in-school youth are tested. Portland youth were excluded because they are not tested in a comparable manner and San Diego youth were only tested in 9th and 11th grades.

Statistically, all the impacts were equal to zero at standard levels of significance.

Table H.4

ESTIMATED IMPACT OF STEP ON READING AND MATH SCORES BY SUBGROUPS SIX TERMS AFTER ENROLLMENT

Group	Treatment Impact, Reading Scores (in NCEs)	Treatment Impact, Math Scores (in NCEs)
Overall	0.43	1.18
Cohort II	-0.12	1.58
Cohort III	1.20	0.64
Gender		
Male	0.32	2.00
Female	0.52	0.51
Race/Ethnicity		
Asian	1.52	1.13
Black	0.28	2.15
Hispanic	-0.67	-1.29
White and Other	-0.40	0.80
Site		
Boston	0.21	1.77
Fresno	-0.35	-0.76
San Diego	1.20	1.50
Seattle	-0.88	-0.54
Grade Completed At Enrollment		
8th	0.57	1.21
9th	-0.01	1.07
Sample Size	2,836	2,827

Note: Six terms after STEP enrollment, the average standardized test score was 48.2 NCEs in reading and 59.4 NCEs in math. The impacts are based on regressions including all test scores for all sample members who took tests.

Portland does not administer a nationally-normed test.

Table H.5

ESTIMATED IMPACT OF STEP ON THE PROBABILITY OF EARNING A GED
(54 MONTHS POST-ENROLLMENT)

Group	Treatment Impact, Cohort II 54 Months Post-Enrollment (in percentage points)
Overall	-0.2
Gender	
Male	-0.1
Female	-3.4
Race/Ethnicity	
Black	-1.0
Hispanic	-1.8
White and Other	1.4
Site	
Boston	9.1
Fresno	-17.3
Portland	6.6
San Diego	-1.7
Seattle	-7.2
Grade Completed at Enrollment	#
8th	-11.0
9th	11.5
Sample Size	299

Note: On average, 19.2% of the Cohort II control dropouts had a GED certificate by the time of the Wave III interview.

Asians have been grouped in the White and Other category due to the small size of the Asian sample.

# Indicates that the impact differs with respect to this characteristic at a 0.10 level of significance.

Table H.6

ESTIMATED IMPACT OF STEP ON RATES OF COLLEGE ATTENDANCE
(42 AND 54 MONTHS AFTER ENROLLMENT)

Group	Treatment Impact, Cohort III 42 Months Post-Enrollment (in percentage points)	Treatment Impact, Cohort II 54 Months Post-Enrollment (in percentage points)
Overall	4.4	-1.6
Gender		
Male	12.3	-0.9
Female	-0.5	-2.1
Race/Ethnicity		
Asian	-18.6	5.1
Black	3.5	-5.1
Hispanic	28.8**	-1.4
White and Other	11.6	-0.4
Site		
Boston	13.4	5.2
Fresno	6.2	-4.2
Portland	16.5	2.1
San Diego	-8.7	-3.8
Seattle	-2.5	-6.5
Grade Completed at Enrollment		
8th	-7.7	-5.0
9th	5.0	1.7
Sample Size	357	695

Note: Among high school graduates, 49.2% of Cohort III controls attended college, while 49.7% of Cohort II controls did so.

** Indicates that the impact is statistically different from zero at a 0.05 level of significance.

Table H.7

ESTIMATED IMPACT OF STEP ON KNOWLEDGE OF CONTRACEPTION
(42 AND 54 MONTHS AFTER ENROLLMENT)

Group	Treatment Impact, Cohort III 42 months post-enrollment	Treatment Impact, Cohort II 54 months post-enrollment
Overall	0.88***	0.66***
Gender	##	
Male	1.07***	0.60***
Female	0.71***	0.71***
Race/Ethnicity	###	###
Asian	1.54***	1.25***
Black	0.86***	0.56***
Hispanic	0.48***	0.44**
White and Other	0.63***	0.49**
Site	###	###
Boston	0.46**	0.14
Fresno	0.72***	0.07
Portland	0.71***	0.73***
San Diego	1.43***	1.62***
Seattle	1.05***	0.72***
Sample Size	1176	1107

Note: The mean score on the nine-item scale used to assess knowledge of contraception was 5.32 for the Cohort III control group and 5.40 for the Cohort II control group.

*** Indicates that the impact is statistically different from zero at a 0.01 level of significance.
** Indicates that the impact is statistically different from zero at a 0.05 level of significance.

Indicates that the impact differs with respect to this characteristic at a 0.01 level of significance.
Indicates that the impact differs with respect to this characteristic at a 0.05 level of significance.

Table H.8

ESTIMATED IMPACT OF STEP ON RATE OF FEMALES HAVING BEEN PREGNANT
(42 AND 54 MONTHS AFTER ENROLLMENT)

Group	Treatment Impact, Cohort III 42 Months Post-Enrollment (in percentage points)	Treatment Impact, Cohort II 54 Months Post-Enrollment (in percentage points)
Overall	-2.4	4.1
Race/Ethnicity		
Asian	14.8	17.6
Black	-6.4	3.4
Hispanic	-9.9	0.5
White and Other	18.5	-0.3
Site		
Boston	-0.2	-1.0
Fresno	7.4	8.9
Portland	0.1	1.0
San Diego	-10.9	4.4
Seattle	-10.4	7.7
Sample Size	594	598

Note: Among female STEP participants at the time of the Wave III follow-up interview, 34.7% of the Cohort III control group had been pregnant, while 43.4% of the Cohort II control group had been pregnant. None of the impacts were statistically significant at the .10 level.

Table H.9

ESTIMATED IMPACT OF STEP ON RATE OF FEMALES EVER HAVING A CHILD
(42 AND 54 MONTHS AFTER ENROLLMENT)

Group	Treatment Impact, Cohort III 42 Months Post-Enrollment (in percentage points)	Treatment Impact, Cohort II 54 Months Post-Enrollment (in percentage points)
Overall	-3.9	6.8*
Race/Ethnicity	#	
Asian	20.1	19.6*
Black	-10.7**	4.1
Hispanic	-4.1	13.0
White and Other	15.1	-1.8
Site		
Boston	-10.7	4.6
Fresno	6.3	12.6
Portland	6.7	2.5
San Diego	-2.6	12.2
Seattle	-22.0**	0.7
Sample Size	619	610

Note: Among female participants in STEP at the time of the Wave III follow-up interview, 24.5% of the Cohort III control group had had a child and 28.3% of the Cohort II control group had had a child.

** Indicates that the impact is statistically different from zero at the 0.05 level.
* Indicates that the impact is statistically different from zero at the 0.10 level.

# Indicates that the impact differs with respect to this characteristic at a 0.10 level of significance.

Table H.10

ESTIMATED IMPACT OF STEP ON RATES OF MALES HAVING CAUSED A PREGNANCY
(42 AND 54 MONTHS AFTER ENROLLMENT)

Group	Treatment Impacts, Cohort III 42 Months Post-Enrollment (in percentage points)	Treatment Impacts, Cohort II 54 Months Post-Enrollment (in percentage points)
Overall	4.0	-3.9
Race/Ethnicity		
Asian	-	-11.2
Black	5.1	-8.1
Hispanic	-1.2	9.9
White and Other	2.1	-8.1
Site		
Boston	15.0*	4.4
Fresno	-0.9	-6.3
Portland	3.0	-23.2**
San Diego	4.0	-4.2
Seattle	1.7	9.5
Sample Size	533	480

Note: Among male STEP participants, 17.5% of the Cohort III control group had fathered a child or been responsible for a pregnancy by the time of the Wave III interview while 27.1% of the Cohort II control group had fathered a child or been reponsible for a pregnancy by the time of the Wave III interview.

** Indicates that the impact is statistically different from zero at a 0.05 level of significance.
* Indicates that the impact is statistically different from zero at a 0.10 level of significance.

Table H.11

ESTIMATED IMPACT OF STEP ON MALES EVER HAVING A CHILD
(42 AND 54 MONTHS AFTER ENROLLMENT)

Group	Treatment Impact, Cohort III 42 Months Post-Enrollment (in percentage points)	Treatment Impact, Cohort II 54 Months Post-Enrollment (in percentage points
Overall	-2.0	-3.8
Race/Ethnicity		
Black	-0.1	-0.2
Hispanic	-4.0	0.9
White and Other	-6.7	-16.6
Site		
Boston	-1.2	7.8
Fresno	-0.8	-11.0
Portland	-3.6	-10.6
San Diego	2.6	-4.9
Seattle	-5.8	3.5
Sample Size	551	493

Note: Among male participants in STEP at the time of the Wave III follow-up interview, 10.9% of the Cohort III control group had had a child and 14.3% of the Cohort II control group had had a child. None of these impacts was statistically significant at the .10 level.

Table H.12

ESTIMATED IMPACT OF STEP ON RATES OF WOMEN WITH CHILDREN EVER HAVING RECEIVED AFDC (42 AND 54 MONTHS AFTER ENROLLMENT)

Group	Treatment Impact, Cohort III 42 Months Post-Enrollment (in percentage points)	Treatment Impact, Cohort II 54 Months Post-Enrollment (in percentage points)
Overall	13.4	5.6
Race/Ethnicity		
Asian	71.2*	7.3
Black	8.7	-0.8
Hispanic	29.3	21.2
White and Other	-4.0	6.6
Site		
Boston	12.5	11.3
Fresno	5.0	8.2
Portland	-8.5	-2.2
San Diego	0.5	9.5
Seattle	55.2**	3.4
Grade Completed at Enrollment		
8th	15.3	6.5
9th	13.0	5.4
Sample Size	154	217

Note: At the time of the Wave III interview, 61.3% of female Cohort III controls with children had received AFDC and 72.4% of female Cohort II controls had received AFDC.

Table H.13

ESTIMATED IMPACT OF STEP ON FOOD STAMP RECEIPT
(42 AND 54 MONTHS AFTER ENROLLMENT)

Group	Treatment Impact, Cohort III 42 Months Post-Enrollment (in percentage points)	Treatment Impact, Cohort II 54 Months Post-Enrollment (in percentage points)
Overall	-2.0	1.5
Gender		
Male	-3.3	-1.2
Female	-0.9	3.5
Race/Ethnicity		
Asian	-4.5	4.6
Black	-6.2	-0.7
Hispanic	7.5	-1.3
White and Other	1.3	5.8
Site		
Boston	-1.7	-2.8
Fresno	9.5	-1.8
Portland	0.1	-4.2
San Diego	-12.2**	13.6**
Seattle	-6.8	1.6
Grade Completed At Enrollment		
8th	-2.2	-1.3
9th	-1.7	5.7
Sample Size	1324	1245

Note: Among Cohort III, 37.9% of control group youth were receiving food stamps 42 months after enrollment in STEP. Among Cohort II, 30.3% of control group youth were receiving food stamps 54 months after enrollment in STEP.

** Indicates that the impact is statistically different from zero at the 0.05 level.

Table H.14

ESTIMATED IMPACT OF STEP ON EMPLOYMENT, HOURS, AND PAY DURING MONTHS 37 TO 39 FOR COHORT II AND III HIGH-SCHOOL YOUTH

Group	Treatment Impact, Probability of Employment (in percentage points)	Treatment Impact Among Employed Youth	
		Average Weekly Hours	Average Weekly Pay
Overall	-0.1	-0.2	$3
Cohort II	-4.3	-1.6	-3
Cohort III	2.5	0.6	7
Gender			
Male	0.7	0.5	9
Female	-0.8	-1.0	-3
Race/Ethnicity	###		
Asian	-0.4	0.3	-2
Black	8.3**	-0.1	11
Hispanic	-20.0***	-2.4	-8
White and Other	-5.9	1.5	3
Site			
Boston	1.7	0.6	2
Fresno	-12.1**	-0.8	3
Portland	1.7	1.0	7
San Diego	3.1	-1.1	0
Seattle	4.5	-1.3	3
Grade Completed at Enrollment			
8th	-2.0	-0.8	-3
9th	3.3	0.6	12
Sample Size	2,053	1,061	1,061

Note: Among employed high-school youth in Cohorts II and III, control group youth worked an average of 22 hours a week during months 37 to 39. During this period, employed high-school youth in the control group from the two cohorts earned an average of $145 a week.

*** Indicates that the impact is statistically different from zero at a 0.01 level of significance.
** Indicates that the impact is statistically different from zero at a 0.05 level of significance.

Indicates that the impact differs with respect to this characteristic at a 0.01 level of significance.

Table H.15

ESTIMATED IMPACT OF STEP ON
AVERAGE QUARTERLY EARNINGS DURING MONTHS 37 TO 39
AMONG COHORT II AND III HIGH SCHOOL YOUTH

Group	Treatment Impact, Average Quarterly Earnings
Overall	$3
	##
Cohort II	-105*
Cohort III	82
Gender	
Male	56
Female	-44
Race/Ethnicity	##
Asian	-17
Black	105*
Hispanic	-225***
White & Other	-20
Site	
Boston	63
Fresno	-106
Portland	42
San Diego	-9
Seattle	23
Grade Completed at Enrollment	
8th	-38
9th	76
Sample Size	2,053

Note: Among in-school youth, controls earned, on average, $757 between months 37 and 39.

** Indicates that the impact is statistically different from zero at a 0.05 level of significance.
* Indicates that the impact is statistically different from zero at a 0.10 level of significance.

Indicates that the impact differs with respect to this characteristic at a 0.05 level of significance

Table H.16

ESTIMATED IMPACT OF STEP ON EMPLOYMENT, HOURS, AND PAY
DURING MONTHS 37 TO 39 AMONG COHORT II AND III OUT-OF-HIGH SCHOOL YOUTH

Group	Treatment Impact, Probability of Employment (in percentage points)	Treatment Impact Among Employed Youth	
		Average Weekly Hours	Average Weekly Pay
Overall	8.3 ##	1.3	7
Cohort II	0.1	1.9	18
Cohort III	26.3***	-0.4	-22
Gender			
Male	14.1*	-0.2	2
Female	3.1	2.8	12
Race/Ethnicity			
Asian	14.4	1.2	19
Black	7.8	2.7	-2
Hispanic	-0.4	2.1	-6
White and Other	17.8	-1.5	29
Site			
Boston	10.2	7.2	-15
Fresno	5.1	2.6	29
Portland	24.1**	-0.9	12
San Diego	-9.1	1.0	-27
Seattle	10.4	-1.6	30
Grade Completed at Enrollment			
8th	14.0*	2.3	-10
9th	4.2	1.0	19
Sample Size	557	257	257

Note: Among employed out-of-high school youth in Cohorts II and III, control group youth earned an average of $194 a week during months 37 to 39. During this period, employed out-of-high school control group youth in two cohorts worked an average 28.3 hours a week.

Indicates that the impact differs with respect to this characteristic at a 0.05 level of significance.

** Indicates that the impact is statistically different from zero at a 0.05 level of significance.

* Indicates that the impact is statistically different from zero at a 0.10 level of significance.

Table H.17

THE ESTIMATED IMPACT OF STEP ON AVERAGE QUARTERLY EARNINGS DURING MONTHS 37 TO 39 AMONG ALL COHORT II AND III OUT-OF HIGH SCHOOL YOUTH

Group	Treatment Impact, Average Quarterly Earnings
Overall	$143
Cohort II	173
Cohort III	68
Gender	
Male	235
Female	63
Race/Ethnicity	
Asian	21
Black	126
Hispanic	63
White and Other	311
Site	
Boston	364
Fresno	215
Portland	143
San Diego	-110
Seattle	144
Grade Completed at Enrollment	
8th	216
9th	101
Sample Size	557

Note: Among out-of-school youth, controls earned, on average, $956 between months 37 and 39. None of the impacts is statistically significant at a 0.10 level of significance.

Table H.18

THE ESTIMATED IMPACT OF STEP ON EMPLOYMENT, HOURS, AND PAY DURING MONTHS 49 TO 51 AMONG COHORT II HIGH SCHOOL YOUTH

Group	Treatment Impact, Probability of Employment (in percentage points)	Treatment Impact, Among Employed Youth	
		Average Weekly Hours	Average Weekly Pay
Overall	2.6	0.61	4
Gender			
Male	0.7	0.25	10
Female	4.0	0.98	-1
Race/Ethnicity	##		
Asian	18.4**	1.89	-1
Black	-0.9	-1.98	0
Hispanic	-1.3	-0.12	4
White and Other	-27.1*	8.24**	33
Site			
Boston	-0.7	3.84	-18
Fresno	1.6	4.14	-3
Portland	8.8	-0.79	14
San Diego	1.6	0.68	10
Seattle	3.3	-3.73	13
Grade Completed at Enrollment			
8th	7.1	0.82	9
9th	-5.1	0.28	-4
Sample Size	649	373	373

Note: Among employed high school youth in Cohort II, control group youth earned an average of $158 a week during months 49 to 51. During this period, they worked an average of 22.4 hours a week.

** Indicates that the impact is statistically different from zero at the 0.05 level.

* Indicates that the impact is statistically different from zero at the 0.10 level.

Indicates that the impact differs with respect to this characteristic at a 0.05 level of significance.

Table H.19

THE ESTIMATED IMPACT OF STEP ON AVERAGE QUARTERLY EARNINGS DURING MONTHS 49 TO 51 AMONG COHORT II HIGH SCHOOL YOUTH

Group	Treatment Impact, Average Quarterly Earnings
Overall	$82
Gender	
Male	89
Female	76
Race/Ethnicity	
Asian	188
Black	7
Hispanic	114
White and Other	36
Site	
Boston	116
Fresno	38
Portland	180
San Diego	72
Seattle	10
Grade Completed at Enrollment	
8th	147
9th	-32
Sample Size	649

Note: Among in-school Cohort II youth, controls earned, on average, $923 between months 49 and 51. None of the impacts was statistically different from zero at a 0.10 level of confidence.

Table H.20

THE ESTIMATED IMPACT OF STEP ON EMPLOYMENT, HOURS, AND PAY DURING MONTHS 49 TO 51 AMONG COHORT II OUT-OF-HIGH SCHOOL YOUTH

Group	Treatment Impact Probability of Employment (in percentage points)	Treatment Impact, Among Emp mployed Youth	
		Average Weekly Hours	Average Weekly Pay
Overall	-3.4	-0.02	-12
Gender			
Male	-7.0	-2.04	-27
Female	-0.3	2.55	7
Race/Ethnicity			
Asian	5.3	0.36	-1
Black	-8.5	0.72	8
Hispanic	-9.7	-1.68	-28
White and Other	13.7	0.65	-24
Site		##	
Boston	-17.0	5.56	20
Fresno	-6.4	-5.74**	-32
Portland	3.4	0.32	-19
San Diego	-3.0	5.86*	7
Seattle	6.0	-2.86	-18
Grade Completed at Enrollment			
8th	-6.7	-0.50	-4
9th	1.5	0.44	-20
Sample Size	614	357	357

Note: Among out-of-school youth in Cohort II, control group youth earned an average of $224 a week from month 49 to 51. During this period, youth worked an average of 29.2 hours a week.

** Indicates that the impact is statistically different from zero at a 0.05 level of significance.

* Indicates that the impact is statistically different from zero at a 0.10 level of significance.

Indicates that the impact differs with respect to this characteristic at a 0.05 level of significance.

Table H.21

ESTIMATED IMPACT OF STEP ON AVERAGE QUARTERLY EARNINGS
DURING MONTHS 49 TO 51 AMONG COHORT II OUT-OF-HIGH SCHOOL YOUTH

Group	Treatment Impact, Average Quarterly Earnings
Overall	-$84
Gender	#
Male	-287*
Female	108
Race/Ethnicity	
Asian	13
Black	-126
Hispanic	-276
White and Other	188
Site	
Boston	87
Fresno	-405*
Portland	8
San Diego	58
Seattle	-67
Grade Completed at Enrollment	
8th	88
9th	-77
Sample Size	614

Note: Among Cohort II out-of-high school youth, controls earned an average of $1,392 between months 49 and 51.

* Indicates that the impact is statistically different from zero at a 0.10 level of significance.

# Indicates that the impact differs with respect to this characteristic at a 0.10 level of significance