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ABSTRACT

Project Redirection was designed to help pregnant and parenting adolescents progress toward eventual self-sufficiency by linking them with community agencies and volunteers at four geographically and ethnically diverse sites in the United States. Distinctive features of the program include: (1) a broad scope of services including employability training, parenting and educational counselling, and (2) the inclusion of paid women drawn from the local community to act as primary supports to the teens and help them achieve short-term goals. This report discusses impact findings 12 months after the teens first enrolled in Project Redirection. The hypothesis tested is that participants in the program will experience better education, employment, family planning, and health outcomes than a group of similar non-participants. Results show that the Project positively resulted in improvements in educational, employment and fertility areas. The program was deemed effective for teens in the major ethnic, age, and parity subgroups included in the sample. The most pronounced effects were found among teens who came to the program with the least favorable education and employment histories. (LHW)

MANPOWER DEMONSTRATION RESEARCH CORPORATION

SCHOOL, WORK AND FAMILY PLANNING

Interim Impacts in Project Redirection

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MDRC

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The Authors

PRESIDENT'S STATEMENT

Project Redirection, the program which these MDRC studies document, is one of the few efforts mounted today on a national scale to help teens who are either pregnant or young mothers achieve self-sufficiency. As a research program, it is unique in its scope of study to show which strategies work best, for the least cost, in promoting this long-term goal. Redirection's short-term results have been encouraging, and now a replication effort is underway to learn more about the program's feasibility.

Fortunately, the problem of teenage pregnancy is a more manageable one than some confronting this country. As a nation, we have not found the correct strategies to reshape the lives of all persons receiving public assistance. However, we can be of help to this smaller group -- to their own and to this society's advantage. If we can create with these young mothers the route to self-sufficiency -- both on a personal and economic level -- we are likely to forestall a pattern of welfare dependency that in the past has seemed inevitable for this group.

Because sound research takes time, we must at this point reserve judgment on the program's ultimate effectiveness. We can but anticipate the final report. Nevertheless, the first encouraging round of research reports has alerted us -- as well as public officials and program planners -- that in fact real progress has been made. Project Redirection is well worth watching.

Barbara B. Blum

PREFACE

Project Redirection was created out of concern for an issue that has assumed increasing importance on this nation's agenda: the high rate of teenage pregnancy, particularly among the disadvantaged. The costs are both human and societal; teen mothers are more likely than other adolescents to drop out of school, be unemployed, have more children at an early age, and become dependent on welfare. Project Redirection, begun in 1980 in four sites with support from the Ford Foundation and the U.S. Department of Labor, constitutes an attempt to learn how best to reduce these costs.

Project Redirection enrolls pregnant teenagers and teen mothers who are under 18 years of age, without high school diplomas, and for the most part, living in families receiving welfare. Although a number of programs already serve this needy group, Redirection takes several fresh approaches. In the belief that treating one problem alone in a population experiencing so many difficulties will result in, at best, only short-term gains, Redirection offers teens a comprehensive range of services, including educational, health,

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MDRC is publishing simultaneously School, Work and Family Planning, the interim impact report on Project Redirection, and Choices and Life Circumstances: An Ethnographic Study of Project Redirection Teens. This preface introduces both reports.

employability and family planning services. Together they are intended to help teens develop the personal and economic self-sufficiency that is so necessary in the long run. Reinforcing this goal for each teen is a community woman, an older role model who guides the teens through an individualized plan of activities.

As a national demonstration program, Redirection is unusual in its intent to test seriously whether this particular service program can make a difference. The Manpower Demonstration Research Corporation (MDRC) holds responsibility for managing the demonstration and carrying out a rigorous analysis of its implementation and effectiveness. An implementation study, with one report already issued and another scheduled for release later this year, examines the way in which the program is structured and managed, the feasibility of operating the model, and its costs.

The impact study, conducted by the American Institutes for Research in the Behavioral Sciences, assesses the program's effects on the teens' schooling and work experiences, attitudes towards family planning, and rates of subsequent pregnancy. To do this, a group of program participants was matched with an equal number of similar "comparison" teens -- young women who would have been eligible for the program but lived in communities in which it was unavailable. Care was taken to ensure that the comparison group communities and teens were well matched

with those in Project Redirection. Harlem, a program site, was paired with Bedford-Stuyvesant (also in New York City); Phoenix with San Antonio; Riverside, California with Fresno; and Boston with Hartford. Ultimately, the sample will include 900 teenagers who will be interviewed up to three times: at baseline (before program participation), and at 12 and 24 months later.

More or less concurrently with the baseline interviews, a group of three ethnographers in Riverside, Harlem and Phoenix began a nine-month observation of the lives of program participants, ultimately focusing on a group of 18 teens for whom they developed in-depth case studies. This ethnographic research was designed to amplify the results of the impact and implementation studies by shedding light on the particular life circumstances that teens face as they move through the program.

The two studies now being released by MDRC report on some of the results of these research efforts. Choices and Life Circumstances is the final report on the ethnographic study; School, Work and Family Planning is the second in a series of three impact studies. Following the baseline report, this impact report examines the experiences of 400 teens one year after program enrollment. The interim nature of the study should be emphasized: many of the teens were still in the program, the period of follow-up was short, and the sample was relatively small. Nevertheless, School, Work

and Family Planning, considered in conjunction with its companion volume, Choices and Life Circumstances, presents some early, important evidence on the program.

The story is a complex one. The ethnographic study portrays the hardships faced by adolescent low-income mothers and mothers-to-be. Many of the teens studied live in stressful family situations and have had negative school histories. Although most do not enter the program with clearly formulated aspirations, a feeling of uncommon strength among the group is their distrust of marriage, at least an early marriage to the fathers of the babies, as a solution to their dilemmas. These young women's attitudes illuminate some of the reasons underlying the alarming increase in the proportion of black families headed by single parents. Also, within these households characterized by welfare dependency and poverty, the study highlights the difficulties that confront these teens as they attempt to carve out better futures for themselves.

But while the findings of the ethnographic study point out that we should not expect to find easy solutions to the problems of teen mothers, the impact report leaves the reader with hope that, with a carefully structured intervention, progress is possible. The report indicates significant improvements in the educational and work behavior of program participants and a moderate, but significant, downward trend in the rate of subsequent pregnancy during this short

period. Although it is true that even the reduction found here would translate nationally into an avoidance of thousands of unintended repeat pregnancies, the findings of both studies indicate that a means must be found to motivate sexually active adolescents to contracept more effectively.

Encouraged by the initial promise of the program as reported in these volumes, the Ford Foundation has joined with a group of local community foundations to launch a number of new, smaller Redirection programs at sites around the country. With further information to emerge from this program replication and from the final reports on the original demonstration sites, policymakers and practitioners should be able to pinpoint far more accurately which strategies are most effective in helping teen mothers shape better futures for themselves.

Judith Gueron
Executive Vice-President
MDRC

EXECUTIVE SUMMARY

The Project Redirection Program

Project Redirection is a demonstration program of services for low-income teenage mothers and mothers-to-be. The demonstration, which began enrolling participants in several sites in mid-1980, offers, or brokers, comprehensive services designed to redirect the lives of young, disadvantaged women whose early parenthood is often a barrier to future economic self-sufficiency. Teens are eligible to participate in Project Redirection if they are 17 years or younger; pregnant or a mother; have not obtained a high school diploma or GED; and receive welfare or are living in a welfare-dependent family.

One distinctive aspect of Project Redirection is the broad scope of its services, including activities that focus on employability training and educational counseling. Another innovative feature is the inclusion of community women, paid volunteers who are drawn from the local community to act as primary supports to the teens and to help them achieve short-term goals. The community women, along with program staff and the teens, develop Individual Participant Plans (IPP). These plans specify the teens' scheduled schooling, child care, and other activities that may lead to better self-sufficiency.

The program was fully implemented as a demonstration in four sites: Boston, New York (Harlem), Phoenix and Riverside, California. Baseline interviews conducted with participants shortly after program enrollment revealed that they were predominantly young (mean age of about 16), unmarried, and composed of minority teens from disadvantaged backgrounds. At that point, nearly half the teens were not in school, and the majority were at least one year behind in grade level for their age. Although most participants had only one child or were about to deliver their first baby, some one-fourth of the teens had already had two or more pregnancies. Half of these teens said they had never practiced contraception, and among those who had, consistent use of birth control was the exception. The majority had grown up in households headed by mothers who themselves had been teenage mothers and had not completed high school.

While all of the teens had been receiving some social services, substantial percentages of them reported service needs that were not being met at the time of their entry into Project Redirection. In short, the Redirection participants at enrollment represented a clear target for social concern and intervention.

The Project Redirection Impact Analysis

This report discusses impact findings 12 months after the teens were first enrolled in Project Redirection; a

subsequent report will examine 24-month impacts. The general hypothesis being tested in the impact analysis is that participants in the program will experience better educational, employment, family planning and health outcomes than a group of similar nonparticipants.

The research design used for the impact analysis calls for the collection of both pre- and post-test data from a sample of program participants (the experimental group) and comparable nonparticipants (the comparison group). Twelve-month follow-up data were obtained from 400 teens who had been interviewed at baseline. Demographically, the experimental and comparison groups were well-matched at follow-up: on none of the major indices such as age, marital status, income, ethnicity, household structure, and number of children were the two groups significantly different from each other.

Table I presents selected program impacts from the 12-month follow-up interview data, in each case comparing the outcome for Project Redirection participants with that of the comparison group. These impacts are discussed in more detail below.

Program Participation

At the 12-month follow-up interview, 56.8 percent of the experimental teens were still participating in Project

TABLE I

SELECTED PROGRAM IMPACTS

Outcome Variables	Comparison Group	Project Redirection	Difference	Percent Increase/ Decrease
I. Service Receipt				
Number of Services	3.8	5.6	1.8***	+ 47
Number of Job-Readiness Activities	1.7	3.1	1.4***	+ 82
II. Education				
Percent enrolled in school or with a high school diploma, all teens	50	66	16***	+ 32
Percent enrolled in school or with a high school diploma, teens out of school at baseline	20	49	29***	+145
III. Employment				
Percent holding a post-baseline job, all teens	40	52	12***	+ 30
Percent holding a post-baseline job, teens with no prior work experience	18	44	26***	+144
IV. Fertility				
Birth control knowledge scores	9.5	10.6	1.1**	+ 11
Rate of subsequent pregnancies	22.4%	16.8%	-5.6*	- 25

Source: Tabulations are from AIR 12-month follow-up interviews with Project Redirection participants and comparison group members.

Notes: All means and percentages have been adjusted statistically for important background characteristics.

* Statistically significant at the .05 level, one-tailed test.
** Statistically significant at the .025 level, one-tailed test.
*** Statistically significant at the .01 level, one-tailed test.

Redirection. For the experimental group as a whole, the average length of participation was 10.5 months. Even among those who had terminated from the program, the average length of stay was 7.5 months. These figures compare favorably with participation data in other teen parent programs.

Early Impacts on Educational Outcomes

Generally, the teens in the impact analysis sample continued to experience educational deficits during the post-enrollment period. There were, however, important experimental improvements at follow-up.

When background and baseline characteristics were statistically controlled, a significantly higher percentage of experimental group teens (66 percent) than comparison group teens (50 percent) were either enrolled in, or had completed, an educational program at follow-up. Length of enrollment was even more powerfully related to a positive school status than experimental/comparison group membership. Net of other factors, each month of program enrollment was associated with a 1.8 percent increase in the percentage of teens who were enrolled in, or had graduated from, school.

Further analysis revealed that the program was especially effective in encouraging school dropouts to return to school. Among those teens who were not in school or a GED

program at program start-up, 49 percent of the experimental group, but 20 percent of the comparison group, had a positive school status one year later. When separate analyses were conducted for six subgroups -- blacks, Hispanics, older teens (16 or 17 at enrollment), younger teens (15 or younger), teens pregnant and not pregnant -- Project Redirection participation was again associated with the higher probability of each subgroup being in or having completed school or a GED program at follow-up.

The educational aspirations of these teens were also examined. The majority of teens in both groups wanted at least a high school diploma (97 percent); a full 40 percent of the sample said that they hoped for education beyond a diploma or GED. While the difference was not great, the experimental group teens were somewhat more likely than the comparison group teens to aspire to post-secondary education.

Early Impacts on Employment Variables

At the time of the follow-up interviews, program impacts on employment behavior were substantial, showing that the acquisition of post-enrollment work experience was increased significantly by participation in Project Redirection. Fifty-two percent of the experimental group, but 40 percent of the comparison group, had held a job at some point subsequent to program start-up. Employment in the

follow-up period was also significantly related to the teens having received training directly from Project Redirection rather than from another source.

Program participation was found to have an especially large effect on teens who had never worked prior to program start-up. Net of other factors, more than twice as many experimental as comparison teens (44 percent versus 18 percent) had gained their first employment experience during the 12-month follow-up period.

When experimental/comparison group differences were studied for the ethnic, age and parity subgroups, participation in Project Redirection was again associated with higher rates of employment. The experimental group advantage was significant for three of the six comparisons: Hispanics, older teens, and teens not pregnant at baseline.

Two measures of the teens' job readiness were also analyzed, using tests of career maturity and employability knowledge. In neither case did participation in Project Redirection result in significant improvement over baseline test scores. However, further analyses suggest that teens who received training directly from the Project Redirection program had significantly higher follow-up scores on the employability knowledge test than those who did not.

Early Impacts on Family Planning and Health Variables

A substantial number of teens in the total sample had gained some experience with contraception in the 12 months between program inception and follow-up; 82.8 percent reported having used contraception at least once at follow-up, compared to 47.3 percent one year earlier. Birth control pills were by far the most commonly tried method, having been used by 83.7 percent of the teens with any history of contraceptive use.

Despite the teens' widespread familiarity with at least one form of contraception, actual use tended to be inconsistent. Nearly one out of three sexually active teens at follow-up had not been protected against a repeat pregnancy at last intercourse. Others had relied on relatively ineffective methods; in fact, there were essentially no differences between the experimental and control group in the types of methods used. As a result, many teens in both the experimental and comparison groups had become pregnant again in the 12-month follow-up period.

However, even in this early report, several positive outcomes were found to result from Project Redirection participation. First, there were significant experimental/comparison group differences in scores on a 16-item test of birth control knowledge. Teens in the experimental group scored higher on this test at follow-up (mean score =

10.6) than teens in the comparison group (mean score = 9.5). Teens who received contraceptive counseling directly from Project Redirection scored, on average, over two points higher than teens who did not. Black teens were especially likely to improve their test scores after participation in the program.

Of greatest importance among the fertility-related outcomes was the reduced rate of subsequent pregnancies. Project Redirection participants experienced a significantly lower rate of post-baseline pregnancies (16.8 percent) than the comparison group teens (22.4 percent). This 5.6 percentage point difference represents a decline of 25 percent. While it is acknowledged that 12 months is too short a time in which to judge accurately this long-term phenomenon, the downward trend is promising. On a national basis, this would translate into an avoidance of thousands of unintended repeat pregnancies.

Health-related pregnancy outcomes for teens pregnant at baseline were also examined. At follow-up, nearly all of the teens in the sample (95.6 percent) reported having visited a doctor five or more times for prenatal care. There were no important experimental-comparison group differences on the measures studied, and in general, both experimental and control group teens appeared to be receiving adequate medical attention.

Receipt of Services

Retention rates and length of participation in Project Redirection were generally very good and resulted in substantial differences in most of the outcomes studied. The final important question is whether teens were, in fact, obtaining the services the program was designed to provide. If there were no evidence of that, rival explanations could account for the interpretation of program success reached in this report.

Both experimental and comparison group teens, when asked about services they had received in the previous 12 months, indicated that they were continuing to receive a range of formal services. However, group differences in service receipt during the follow-up period were significant. Overall, of eleven specific types of services, (e.g., birth control, education, educational counseling, parenting workshops), the experimental group received an average of 5.6 services; the comparison group, 3.8. This difference persisted even when enrollment characteristics (including the number of services used and needed at program start-up) were statistically controlled. In the experimental group, the difference between the average number of services used at follow-up (5.6) and at program start-up (3.0) is comparable to the number of services that these teens reported receiving directly from the Redirection program (2.2).

Group differences in receipt of job-readiness training were especially large. For the seven types of activities offered, Redirection participants, in almost every case, had taken part in twice as many classes or workshops as had the comparison group. Overall, experimental teens received 3.1 job-readiness services as opposed to 1.7 for the comparison group.

Conclusion

As seen in this 12-month follow-up report, Project Redirection positively affected service receipt and has resulted in improved educational, employment and fertility outcomes for its participants. Significant program impacts were detected in every major area examined. The program also proved effective for teens in the major ethnic, age and parity subgroups included in the sample. The most pronounced effects of Project Redirection were found among teens who came to the program with the least favorable educational and employment histories.

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SCHOOL, WORK AND FAMILY PLANNING

Chapter 1

Introduction

The United States has the highest rate of teenage childbirth of any industrialized country, despite the fact that contraceptives have been increasingly available to young people. Social concern about the number of teenage pregnancies began to grow in the 1970s as the adverse consequences of young parenthood became more evident.[1] Studies began to consistently show that early childbearing results in less education, higher rates of unemployment, lower wages, higher rates of welfare dependency, less successful childbirth experiences, and higher rates of divorce. Early repeat pregnancies are also not uncommon, resulting in larger than average families in the population of women giving birth in their teen years.

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[1] The literature on the consequences of early parenthood is extensive. Particularly good summaries and discussions are available in the following: Alan Guttmacher Institute, 1981; Haggstrom et al., 1981; Moore, Hofferth, Caldwell & Waite, 1979; and Phipps-Yonas, 1980.

Project Redirection is one of several interventions designed in recent years to address the needs of this group of teens and to attempt to help them overcome some of their early handicaps. This report describes the impacts of the program after a year and a half of operations.

A. The Project Redirection Demonstration

Project Redirection, a demonstration program of services for low-income teenage mothers and pregnant teenagers, began enrolling participants in several sites in mid-1980. The major purpose of the demonstration is to assess the feasibility and impacts of a comprehensive service program that attempts to "redirect" the lives of young women from low-income backgrounds, those most at risk of welfare dependency because of their early parenthood.

Project Redirection offers, or brokers, comprehensive services addressing a broad range of needs. Young mothers and pregnant teens are provided with services and activities in support of "continued schooling, the development of marketable skills, acceptance and use of needed health care and social services, and planning for

eventual employment and self-sufficiency."[2] This broad goal includes a number of specific objectives, the most critical of which are completion of a school or GED program, delay of subsequent pregnancy, attainment of job skills, and improved maternal and infant health.

Project Redirection began operations as a demonstration in five sites: Boston, New York (Harlem), Phoenix, Detroit, and Riverside, California. Because the Detroit program was subsequently discontinued due to management difficulties, this document reports on data gathered in the remaining four project sites.

Although some inter-site differences are allowed in program operations, there are core similarities in the functioning of local Project Redirection programs. First, all sites serve a specific clientele. Teens eligible to enroll in the program must be (1) under the age of 18; (2) pregnant or a mother; (3) without a high school diploma or GED; and (4) receiving welfare or living in a welfare-dependent family. A more detailed description of the Redirection participants is presented in a subsequent section of this chapter.

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[2]Planning document, Manpower Demonstration Research Corporation, 1980.

A second cross-site similarity is that all Project Redirection programs offer the same essential kinds of services, a comprehensive mix that distinguishes this program from many others for teen parents. The service components include:

- educational counseling and referral to a variety of continued schooling options;
- employability training and employment counseling;
- birth control and family planning counseling;
- referral to health care services;
- parenting education;
- personal counseling;
- life management education (e.g. nutrition education, budgeting of resources); and
- recreational activities

Inter-site variation, however, is allowed in the delivery of these services, and participants vary in their use of the different components.

Several other aspects of Project Redirection make it a distinctive program for teen parents. The first is the inclusion of "community women" on program staff. These women are not professional caseworkers, but rather adults drawn from the local community to act as primary supports

to the teens. Assigned to the participants upon enrollment, the community women meet with the teens weekly and work with them during the program to develop short-range goals that guide teens in the use of available services. They also often assist the teens in overcoming personal difficulties that may interfere with program activities.

A second distinctive component is the Individual Participation Plan (IPP), a working document drawn up by program staff, the community woman and the teen herself. This plan, which can change as the teen progresses in the program, specifies her goals for schooling, child care, and the use of other services designed to help her achieve self-sufficiency. It is a monitoring document, as well as a schedule of activities specifying which goals the teen should be striving toward.

Another unusual component of the Redirection program at its inception was its linkage to a local Work Incentive Program (WIN). While teens of this age would normally be exempt from WIN, national officials were interested in promoting the employability of these teens in the long run. In two sites (Harlem and Phoenix), a WIN social service worker was assigned to assist teens in gaining access to

the traditional WIN services of job placement, training, and child care.

B. The Implementation Experience

Project Redirection is supervised by the Manpower Demonstration Research Corporation (MDRC), the nonprofit organization with overall responsibility for the implementation of the demonstration and its extensive research component. One aspect of MDRC's coordinating function is the monitoring of local programs by operations staff to ensure that core services are being delivered to the teens. MDRC also maintains a management information system to collect data on participants and community women, and compiles detailed financial records.

At the end of the first year of operations, MDRC published a report on Project Redirection's implementation (Branch et al., 1981). This document noted that, while there were the inevitable start-up problems during the first few months of operation, the program model proved to be both operationally feasible and capable of providing the needed services to teenage mothers.

After the first few months, an effective referral network was in place, enabling sites to approach their targeted enrollment levels. The community woman component was functioning in all sites, and the host agencies had geared up quickly to offer those types of services with which they were most familiar, notably life management services such as parenting, nutrition education, and family planning. The most difficult services to provide or broker initially were employability training and educational services for teens under age 16, who are too young to enter many alternative educational programs. At the end of the first year, sites were being encouraged by MDRC to devote considerable energy to the provision of these two important, but unfamiliar, services.

C. The Clients of Project Redirection[3]

The teens at enrollment were predominantly young (mean age about 16), unmarried minority teens residing in urban areas. Nearly all of the teens came from disadvantaged backgrounds. The majority had grown up in households headed by mothers who themselves had been teenage mothers

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[3]The description below is based on data gathered in baseline interviews with program participants within one month of program enrollment.

and had not completed high school. Almost two-thirds lived in households where the estimated monthly income was under $600, for an average household size of 5.6 members. Most of the teens (72 percent) were living in households that included their own mothers.

Despite their youth, nearly half were not in school or in a GED program, and 40 percent of the dropouts had been out of school for over a year. A substantial majority were at least one year behind in grade level for their age. While pregnancy and child care were the most common reasons cited for leaving school, over one-fourth of the teens had left school one or more times prior to the pregnancy.

Although few of the teens were employed at the time of enrollment, more than half had held a paying job at some time. Most jobs had been low-paying part-time or summer jobs such as household work, cashiering, clerical, waitressing, or child care assistance. The majority of teens had a positive orientation to future work, but few said they were trained in specific skills.

Over 90 percent of the teens either had only one child or were about to deliver their first baby. However, about one-fourth of them had had two or more pregnancies. Virtually all of the teens acknowledged that birth control

was accessible to them, but approximately half said they had never practiced contraception. Among the sexually active teens using birth control at baseline, irregular or infrequent use was not uncommon.

Most of the teens appeared to be in good health and to be receiving medical attention. Compared to the medical experience of other teen parents as reported in the literature of this field, a high percentage (over 50 percent) of both the pregnant teens and mothers reported first-trimester medical care. Nevertheless, a relatively large number (15 percent) of the infants born to these young women were low birth-weight babies (under 5.5 pounds), and a substantial proportion of mothers (21 percent) left the hospital without their infants.

Families provided a particularly important form of assistance to these teens through child care. Maternal grandmothers were the most frequently cited child care providers. Formal, paid child care arrangements were seldom used. The teens had, however, used a broad range of other formal services, most frequently medical care, the WIC program (a food supplement program), and food stamps. Nevertheless, substantial percentages of teens reported service needs that were not being met at the time of their

Redirection enrollment. The services asked for most often were job training, job counseling, assistance in obtaining infant goods, tutoring, and educational counseling.

In summary, the Redirection participants at enrollment represented a clear target for social concern and intervention. These young women were poor, unmarried and already had substantial educational deficits for their age. Many were at-risk to repeat pregnancies during their teen years, and many reported needing services they did not receive from other sources.

D. Introduction to the Impact Analysis

The Project Redirection demonstration includes an extensive assessment of the feasibility, costs, and impacts of the program model. This report discusses the impacts 12 months after the teens were first enrolled; a later report will cover an additional year. The general hypothesis being tested is that participants in the program will experience better educational, employment-related, family-planning, and health outcomes than a group of similar non-participants.[4] Impacts associated with

[4] The research design and analytic strategy of the impact analysis are described in Chapter 2.

receipt of particular services, as well as length of participation in the program, are also examined.

In social programs whose aims are as broad as those of Project Redirection, it is often useful to have a conceptual framework to integrate in a meaningful way the diverse characteristics and behaviors under scrutiny. The framework here, also used in the baseline report (Polit et al., 1982), construes the goal of Project Redirection as encouraging young women to make an "investment" in their own future well-being. Investments can be made by obtaining educational credentials, developing employment skills, practicing effective contraception, and maintaining adequate health.

The investment concept is not dissimilar to the construct used in economic models of human capital accumulation, where investments may be defined as behaviors that constitute an expenditure of time, money, or energy for some expected future return. Virtually all of Project Redirection's objectives and all of the components of the program can be interpreted as promoting personal investments, providing the conditions that facilitate investments, or removing those that interfere with them.

This analysis of interim program impacts will examine the extent to which Project Redirection has been successful in working toward these objectives, based on data obtained from a partial sample. However, the impact analysis will be able to analyze investment behaviors more fully in the next report, which will be based on 24-month follow-up data with the complete research sample. This report is limited to consideration of the preliminary outcomes, which are nevertheless critical components in a hypothesized sequential chain leading from service receipt to self-sufficiency and personal satisfaction. The framework assumes that, to be a successful "investment" program, Project Redirection must set in motion a sequence such that the program would:

- provide the needed services to young teenage parents, and thereby
- affect the teens' knowledge, values, motivation, and willingness to make personal investments and, in so doing,
- produce investment behaviors in various areas, which
- lead to economic self-sufficiency and better life circumstances.

The last step, which is the payoff of investment behaviors, cannot be directly observed within the time frame of this demonstration. The impact analyses can,

however, collect data bearing on the question of whether the first three steps were set in motion.

One chapter of this report (Chapter 3) is devoted to the first step. If participation in Project Redirection is low, if service receipt is small, or if the teens drop out of the program before obtaining many needed services, then the expected impacts on motivations (Step 2) and on investments (Step 3) would be expected to be small. It is, therefore, important to look directly at program participation, and to compare service receipt of participants with that of a comparable group of non-participants. Subsequent chapters (Chapters 4 through 6) examine impacts relevant to Steps 2 and 3 in the educational, employment, pregnancy and health areas.

The next chapter of this report (Chapter 2) describes the research methods used in the impact assessment.

Chapter 2

The Impact Analysis Research Design

A. Overview of the Design

The impacts of Project Redirection are being measured using a quasi-experimental design wherein data are obtained from a sample of participants (the experimental group) and non-participants (the comparison group). The comparison group in this case consists of teens meeting program eligibility criteria in cities matched to the Redirection sites on sociodemographic characteristics, but not offering a Redirection program. The matched experimental/comparison sites are as follows: Boston--Hartford; Harlem--Bedford-Stuyvesant, New York; Phoenix--San Antonio; and Riverside--Fresno, California.

The research design calls for interviews with two samples of experimental and comparison group respondents. The Sample I experimental group consists of those teens who enrolled in Project Redirection from the time it began operations (August, 1980) to the spring of 1981. Sample I

experimental teens were administered a baseline interview within about one month of program enrollment. Comparison group teens in Sample I were also interviewed in late 1980 and early 1981. A total of about 450 Sample I respondents were included in the baseline sample. Follow-up data were obtained from the Sample I teens 12 months following the baseline interview.[1] The second wave of follow-up interviews, scheduled for 24 months after baseline, began in the fall of 1982. This volume reports on the preliminary results (based on 12-month follow-up interviews) with Sample I subjects only.

The second sample of respondents was added to the impact analysis research design later. It consists of approximately 175 teens enrolled in Project Redirection from April 1981 to January 1982, and 175 matched comparison teens. This second sample was not administered a baseline interview, but will be given "12-month" and "24-month" interviews. Sample II 12-month interviews were completed in early 1983. The subsequent report on program impacts will include data from both Sample I and Sample II respondents. The aggregation of data from both samples will permit more refined analyses of site-specific impacts than is possible in the present report.

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[1]The actual mean elapsed time between the baseline and follow-up interviews was 12.4 months.

Prior to program implementation, the feasibility of a true experimental design, wherein teens would be randomly assigned to an experimental or control group, was assessed. Several constraints emerged, notably an insufficient pool of eligible teens in some sites and lack of agency cooperation in others. Because of the quasi-experimental nature of the design, the possibility of biased impact estimates exists due to the initial non-equivalence of the experimental and comparison groups. Selection bias may be defined as the difference in mean outcome scores between the two groups that would have been observed in the absence of the Redirection program. If the comparison group had more favorable outcomes at follow-up than the experimental group would have had without actual participation in the program, then this negative selection bias would make the program look less effective than it actually is. If, on the other hand, the experimental group would have done better than the comparison group even without the program, this positive selection bias would make the program appear to be more favorable than it actually is.

Given the design, there is no known methodological strategy to completely eliminate the risk of selection bias. There are, however, several ways to minimize its threat. Short of using a true experimental design, this

design and analysis plan has used virtually every known methodological strategy to reduce selection biases.

Several features of the design deserve special mention because they affect the selectivity problem and hence the interpretation of the results reported in subsequent chapters of this document:

- Eligibility. Only young women who met program eligibility criteria were included in the comparison sample. This means that the teens in both groups were young, poor, had not completed their basic schooling, and were either pregnant or a parent. The net result is a research sample that is homogeneous with respect to many variables known to be related to life outcomes.

- Site Selection. Comparison sites were matched to experimental sites in terms of general region of the country, teen pregnancy rates, ethnic distribution, income, density, AFDC rates, and service availability for teen parents. The use of communities other than the experimental sites themselves guaranteed a sample that would not be composed entirely or predominantly of refusers (i.e. those who self-selected themselves out of the program).

- Recruitment. Teens in the comparison sites were recruited into the sample in a manner analogous to the recruitment of experimental group teens into the Redirection program. The principal means of recruitment was through referral from

community agencies[2] (hospitals, schools, social service agencies) and word-of-mouth referral from teens already in the sample.

- Matching. Although pair-matching of individual teens was not considered a viable option because of constraints on the pool of eligible teens in each site, efforts were made to "balance" the two groups on four dimensions: age, ethnicity, baseline parity, and receipt of services from teen parent programs. In other words, similar proportions of teens in different age, ethnic, parity and service receipt groups were recruited for each pair of matched sites. While perfect matching was not possible because of a restricted pool of eligible teens, the balancing procedure prevented any extreme dissimilarities of the two groups on these four important dimensions.

- Data Collection. The baseline interview was a 60-90 minute interview that gathered pre-treatment data on the respondents' living situation, education, employment, pregnancy, contraception, aspirations and service utilization. Because these data were collected prior to any programmatic intervention, it is possible both to assess the initial equivalence of the two groups on a wide range of characteristics, and to statistically control for differences that could create selection biases (see Section B below).

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[2]The recruitment of comparison group teens from service providers rather than through random household procedures or use of birth records was considered legitimate beyond the parallelism of recruitment efforts. The teens in the Redirection program were ones who were already "connected" with the social service delivery system. The appropriate comparison group, therefore, is not a "no treatment" group, but rather a group receiving some services--but not Redirection services.

Despite the above efforts, analysis of the Sample I baseline data (see Polit et al., 1982) revealed that the experimental and comparison groups did differ initially on several observed characteristics. Table 2.1 summarizes the major experimental/comparison group variables at baseline for the subjets re-interviewed at follow-up. Comparison group teens, relative to experimental group teens, were on average more likely to be in school at baseline, were less likely to be planning a return if they had dropped out, had lower educational aspirations, had had fewer paid jobs and lower scores on a test of employability knowledge, had more pregnancies, and were more likely to have already participated in a teen parent program.

On the other hand, the two groups were comparable on a very large number of other characteristics. For example, the two groups were similar in age, marital status, ethnicity, highest grade completed, employment status at baseline, use of contraception, knowledge of contraception, number of services used and needed, household structure, and AFDC receipt. Taker in the aggregate, the baseline experimental/comparison group differences seem to be neither substantial in magnitude (with one or two exceptions), frequent in number (although higher than would be expected by chance), nor consistent in direction (some

TABLE 2.1

COMPARISON OF EXPERIMENTAL AND COMPARISON GROUP MEMBERS ON SELECTED VARIABLES AT BASELINE

Variable	Percentages or Means, By Group		
	Experimental	Comparison	Both Groups
• Demographic			
Mean age	15.9 (185)[a]	15.9 (215)	15.9 (400)
Percent Married	3.8 (7)	8.4 (18)	6.3 (25)
Percent Pregnant, Not a Parent	55.7 (103)	58.6 (126)	57.3 (229)
Percent Black	48.1 (89)	44.2 (95)	46.0 (184)
Percent Hispanic	37.8 (70)	44.7 (96)	41.5 (166)
Percent White	13.0 (24)	9.3 (20)	11.0 (44)
• Educational			
Percent in School at Baseline	52.1 (88)	70.1 (141)	69.9** (229)
Mean Highest Grade Completed	8.7 (178)	8.5 (213)	8.5 (391)
Percent of Dropouts Planning to Return	89.7 (78)	75.3 (55)	83.1* (133)
Percent Who Left School More Than 12 Months Before Baseline Interview	40.0 (34)	38.6 (27)	39.4 (61)
Percent Wanting More Than High School Diploma/GED	46.6 (82)	33.5 (67)	39.6* (149)
• Employment			
Percent Employed at Baseline	9.2 (17)	11.2 (24)	10.3 (41)
Percent Ever Worked	73.0 (135)	68.8 (148)	70.8 (283)
Mean Number of Jobs Held	1.3 (185)	1.1 (215)	1.2* (400)
Mean Score, Employability Knowledge Test	11.4 (185)	10.6 (214)	11.0* (399)
• Family Planning/Fertility			
Mean Number of Pregnancies	1.2 (185)	1.3 (214)	1.2* (399)
Percent Ever Used Birth Control	43.2 (80)	50.7 (109)	47.3 (189)
Mean Number of Birth Control Methods Used	0.6 (185)	0.7 (215)	0.6 (400)
Mean Score, Birth Control Knowledge Test	9.2 (184)	9.3 (215)	9.2 (399)
• Services/Supports			
Mean Number of Services Used	5.6 (185)	6.1 (215)	5.9 (400)
Mean Number of Services Needed	5.7 (185)	5.4 (215)	5.5 (400)
Percent Having Been in Teen Parent Program	23.0 (42)	44.4 (95)	34.5*** (137)
• Health-Related			
Mean Days in Hospital for Childbirth	3.4 (80)	3.9 (87)	3.6 (167)
Mean Weight of Infants in Ounces	106.8 (81)	108.8 (86)	107.8 (167)
Percent Visited Doctor First Trimester	52.4 (43)	58.0 (51)	55.3 (94)
• Psychological			
Mean Self-Esteem Score	18.8 (181)	18.8 (213)	18.8 (394)
Mean Locus-of-Control Score	14.4 (181)	13.8 (210)	14.0 (391)
• Home Environment			
Percent With Neither Parent Present at Baseline	31.4 (58)	28.8 (62)	30.0 (120)
Percent in AFDC Household	73.4 (135)	73.0 (157)	73.2 (292)

SOURCE: Tabulations from AIR baseline interviews with experimental and comparison group members in Sample I.

NOTES: [a]The numbers in parentheses represent frequencies on which the statistic is based. For means, the number indicates the total number of respondents to the calculation of the mean. For percentages, the number is the actual number giving the specific response.

*Two-tailed chi-square test (for percentage comparisons) or t-test (for mean comparisons) is statistically significant at the .05 level.

**Two-tailed chi-square test (for percentage comparisons) or t-test (for mean comparisons) is statistically significant at the .01 level.

***Two-tailed chi-square test (for percentage comparisons) or t-test (for mean comparisons) is statistically significant at the .001 level.

variables suggest positive selection bias while others suggest negative bias).

In summary, the design appears to have resulted in reasonably comparable groups. Nevertheless, it was clear that statistical controls would be needed to remove existing group differences, and to improve the precision (i.e., efficiency) of the estimates. Analytic strategies are described in the next section.

B. Basic Analytic Strategies

Given the quasi-experimental nature of the research design, the analyses of program impacts must deal with the problems of the initial non-equivalence of the comparison and experimental groups on some factors. In the present situation, selection biases were controlled using several multivariate statistical procedures, which are summarized briefly below and discussed in more detail in Appendix A.

The basic approach was to use analysis of covariance (ANCOVA), which is the most widely used analytic technique in quasi-experimental designs. ANCOVA is used to statistically adjust estimates of treatment effects for measured differences in the pre-treatment characteristics of the experimental and comparison groups. To the extent that selection bias is associated with the statistically

controlled characteristics (covariates), the selectivity problem will be reduced and possibly eliminated.

Several considerations, however, call for a departure from the basic ANCOVA approach in certain situations. First, there are outcome analyses for which a linear model is not the most appropriate estimation technique. When the assumptions of the ANCOVA model are violated, estimates of program and other effects may not make efficient use of the data and may also be biased. For example, when the outcome of interest is a binary (dichotomous) variable, use of the linear model may not be warranted. This is especially true for outcomes whose mean values deviate sharply from .5 (i.e., mean values less than .2 or greater than .8). Nonlinear maximum likelihood estimation techniques, such as logit or probit analysis, are often more appropriate for binary outcome variables, especially when mean values are in the extremes.

Generally, except in cases of an extreme distribution of outcome scores, the linear regression model was used in a preliminary analysis to develop the best possible specification for a particular binary outcome. Final results of coefficients and statistical significance were then obtained using logit analysis. For outcomes with

extreme mean values, the logit procedure was used in both the preliminary and final stages.

A second consideration led to the use of additional analyses to supplement both the ANCOVA and maximum likelihood procedures. One of the difficulties of these methods is that the covariates used may not control completely for selection bias. If important pre-treatment differences, such as motivation or ability, are unmeasured or not included as covariates, the regression procedure may only reduce, but not eliminate, the selection problem.

Because of this shortcoming, an additional technique was tested. This approach, recently developed by economists, is a sophisticated two-step statistical procedure that attempts first to model the selection process and then to use the results of the first step to correct for selection factors in the second step. In the present situation, it proved to be difficult to model the selection process with any accuracy. The resulting estimates of program effects were, therefore, considered not to be robust. A fuller description of the procedures is presented in Appendix A, and the analytic results are summarized in Appendix D.

In addition to using several alternative analytic techniques, treatment effects were also examined in several ways. The basic procedure was to create a dichotomous variable indicating membership in either the experimental or comparison group. A second approach was to examine whether amount of program exposure (i.e., time enrolled in the program) affected the outcomes of interest. Since the effects of program exposure may not be linear (e.g., a short length enrollment may be totally without impact), nonlinear effects were also tested by using a squared term for length of enrollment. Finally, in some analyses, receipt of specific Redirection services was substituted for program enrollment.

In summary, a variety of analyses were conducted to evaluate the effectiveness of participation in Project Redirection. The fundamental approach was to use a dummy treatment variable (experimental versus comparison group) in an ANCOVA model, using pre-treatment characteristics (including the baseline status on the outcome variable) as covariates. Additional analyses were performed to (1) deal with situations in which assumptions of the ANCOVA model were likely to be violated; (2) further explore handling of selection biases; and (3) refine the measurement of the treatment variable.

C. Description of the Follow-Up Sample

Twelve-month follow-up data were obtained from 400 teens who had been interviewed at baseline, out of 449 who could have been interviewed,[3] thus rendering the overall response rate of 89.1 percent. Table 2.2 shows the number of teens in the baseline and follow-up samples for each of the eight sites. Given the nature of the sample, which is highly mobile and not likely to have a telephone, the response rate is reasonably high. Differences between those who were and were not re-interviewed, based on an analysis of baseline characteristics, were generally small and uniformly non-significant (see Appendix B).

Demographically, experimental and comparison groups were fairly well matched at follow-up, as shown in Table 2.3. The teens were, on average, 17.4 years old when re-interviewed, and only a small percentage were married. Nearly nine out of 10 girls in the sample were in a racial/ethnic minority group. While some 72.0 percent of the teens at baseline reported living in a household that included their mothers, only 57.5 percent of the teens at follow-up did so. The percentage of teens with a father present did not change much in the one year interval.

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[3] Teens interviewed at baseline from Detroit (N=147) have been dropped from the Sample I research sample.

TABLE 2.2

SAMPLE SIZES AND RESPONSE RATES FOR THE IMPACT ANALYSIS AT BASELINE AND FOLLOW-UP, BY SITE

Site	Number in Baseline Sample	Number in Follow-Up Sample	Actual Response Rate[a]	Effective Response Rate[b]
Experimental Sites:				
Boston	36	27	75.0	81.8
Harlem	56	40	71.4	71.4
Phoenix	89	82	92.1	92.1
Riverside	42	36	85.7	85.7
Comparison Sites:				
Hartford	35	31	88.6	96.9
Bedford-Stuyvesant	62	57	91.9	91.9
San Antonio	89	88	98.9	98.9
Fresno	40	39	97.5	97.5
All Sites	449	400	89.1	90.2

SOURCE: Tabulations are from AIR baseline and follow-up interviews with experimental and comparison group members in Sample I.

NOTES: [a]The actual response rate is the number of respondents at follow-up, divided by the number of respondents at baseline (times 100).

[b]The effective response rate is the number of respondents at follow-up, divided by the number of respondents at baseline who could reasonably have been re-interviewed (times 100). This excludes from the denominator respondents who moved more than 100 miles from an interview site without being accessible for a phone interview.

TABLE 2.3

SUMMARY OF MAJOR DEMOGRAPHIC CHARACTERISTICS

OF THE RESEARCH SAMPLE AT FOLLOW-UP,

BY EXPERIMENTAL VERSUS COMPARISON GROUP

Demographic Characteristic[a]	Mean or Percentage, by Group		
	Experimental	Comparison	Both Groups
Mean Age	17.4	17.4	17.4
Percentage Married	10.3	12.1	11.3
Percentage Black	48.1	44.2	46.0
Percentage Hispanic	37.8	44.7	41.5
Percentage White	13.0	9.3	11.0
Mean Number of Children Living With Teen at Follow-Up	1.04	1.06	1.05
Percentage Whose Households Included Their Mothers	54.6	60.0	57.5
Percentage Whose Households Included Their Fathers	15.7	20.5	18.3
Mean Number of Household Members	5.3	5.5	5.4
Percentage of Households With AFDC Recipient	75.4	73.0	74.1
Percentage Whose Monthly Household Income was $500 or Less	45.2	50.0	47.9
Number of Respondents	185	215	400

SOURCE: Tabulations are from AIR 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: All of the variables are characteristics at the time of the follow-up interview.

None of the group differences is statistically significant at or beyond the .05 level.

These teens continued to live in conditions of poverty: nearly three out of four were in households that had at least one AFDC recipient, and nearly half lived in households where the monthly income was $500 or less, supporting an average of 5.4 household members. None of the experimental/comparison group differences at follow-up was statistically significant.

Chapter 3

Program Participation and Service Receipt

The extent to which enrollees participate in Project Redirection and make use of its various services is critical to the success of the program. If youths do not actively take part in the program, or if they drop out after only a few months, it would be unreasonable to expect Redirection to shape the investment behaviors of these young women. Looked at another way, if the analyses reveal substantial experimental-comparison group differences favoring Redirection participants--but there is no evidence that services were received by the participants--rival explanations (such as selection bias) will compete with the interpretation of program success.

Thus, before examining actual impacts, this report examines the extent of program participation and service utilization by Redirection participants. A later section of this chapter compares service receipt in the experimental versus the comparison groups.

A. Length of Enrollment in Project Redirection

At the time of the 12-month follow-up interview, more than half of the teens in the experimental group (56.8 percent) were still participating in Project Redirection.[1] For the experimental group as a whole, the average length of participation was 10.5 months. Among those teens no longer participating in Project Redirection, the average length of program enrollment had been 7.5 months.

These figures compare favorably with participation data for other teen parenting programs. Among the 28 project sites currently funded by the Office of Adolescent Parent Programs, teens who entered programs as mothers and subsequently left them tended to depart in the first six months (57 percent). Among teens who entered pregnant and delivered while still in the program, length of stay was longer: 35 percent terminated in the first six months, while an additional 28 percent left after 7 to 12 months.[2]

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[1] Among the 38 experimental group teens interviewed at baseline but not interviewed at follow-up, 89.8 percent were program terminees. The problem of attrition is discussed in Appendix B.

[2] Personal communication, M. Kimich, Urban Institute, February 1983.

When an analysis was performed to determine whether characteristics at baseline could be used to predict length of enrollment in Project Redirection,[3] the results indicated that pre-program characteristics were poor predictors of a teen's length of enrollment. Using such variables as age, ethnicity, household structure, marital status, school status, educational aspirations, AFDC receipt, service receipt, and parity at baseline as predictors, only 6 percent of the variance in number of enrolled months could be explained.[4]

Only one characteristic was important: being black significantly increased the teen's average length of stay. The mean adjusted length of participation for blacks in Project Redirection was 11.3 months, while for Hispanics and whites, it was 10.0 and 8.2 months, respectively. No other background variable was significantly related to length of enrollment at the .05 level. However, since ethnicity and program site are confounded in this analysis

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[3]A primary function of this ordinary least-squares (OLS) regression analysis was to determine whether there were any strong selection factors in program retention. If substantial systematic tendencies were evident, the analysis of program outcomes would have to address itself to the problem of program attrition bias.

[4]The actual regression tables for this and subsequent regression analyses are presented in Appendix C.

(black teens were primarily from Harlem), either of the two factors--site variation or ethnic differences--could account for program retention.

B. Project Redirection from the Teens' Perspective

Nearly half (44.1 percent) of the respondents said they were very satisfied with the program, and another 44.1 percent reported being fairly satisfied. Not surprisingly, satisfaction was related to length of stay in the program. Among those who expressed strong satisfaction, the mean number of months of enrollment was 11.4 months, compared with 8.3 months for those who were dissatisfied ($F=5.0$, $p<.01$). In their self-reports, teens characterized their program involvement as very active (33.3 percent) or fairly active (38.3 percent). Age, ethnicity, and baseline parity were unrelated to the degree of involvement or satisfaction with the program.

Only eight teens out of 185 claimed they had not been assigned a community woman. For the majority of participants, interaction with the community women tended to be frequent. More than half the teens said that they spoke to their community woman at least once a week on the telephone (62.1 percent) or saw her in person one or more times a week (67.8 percent).

Responses to other questions further suggest that the community woman component was a particularly successful part of the Redirection program. Nearly half of the teens (45.4 percent) rated the community woman as "very important" to them. For example, when teens were asked which component they enjoyed most and which helped them to the largest degree, teens ranked the community woman component a close second behind "parenting education." As Table 3.1 indicates, the employment training component--another innovative aspect of the program--was also rated highly by a sizeable percentage of teens.

C. Service Utilization

Thus far it has been shown that Redirection participants were enrolled in the program for a reasonable length of time and that the majority claimed to be actively involved in the program. A more important question is whether they were, in fact, obtaining the services the program was designed to provide.

Table 3.2 presents information on Redirection participants' use of, and need for, 11 selected services. Because of Redirection's special emphases on employment-related services, these services are reviewed

TABLE 3.1

PARTICIPANT PERCEPTIONS REGARDING COMPONENT OF REDIRECTION THAT WAS MOST ENJOYABLE AND MOST HELPFUL

Program Component	Percentage Distribution of Teens	
	Component Perceived as Most Enjoyable	Component Perceived as Most Helpful
Parenting Education	25.1	26.7
Community Woman	21.1	25.0
Employment Training	15.2	14.0
Personal Counseling	6.4	9.3
Family Planning	6.4	7.6
Education	2.9	7.0
Social Activities	14.6	3.5
Health Care	4.1	2.9
Nutrition Education	1.2	2.9
Child Care	1.8	1.2
Total	100.0	100.0
Total Number of Respondents	171	172

SOURCE: Tabulations are from AIR 12-month follow-up interviews with experimental group members in Sample I.

NOTES: The totals may not add to 100.0 percent due to rounding error.

TABLE 3.2

POST-ENROLLMENT UTILIZATION OF SELECTED SERVICES BY REDIRECTION PARTICIPANTS

Type of Service	Percentage of Participants Receiving Service in Past 12 Months	Percentage of All Participants Receiving Service from Redirection	Percentage of Participants Receiving Services from Redirection More Than Five Times	Percentage of Participants Needing Service Who Did Not Receive It[a]
Parenting Classes	66.8	47.7	33.2	3.3
Tutoring for School Work	10.8	5.4	3.8	25.9
Medical Care for Baby	98.4	0.0	0.0	1.6
Medical Care for Self	81.6	0.0	0.0	4.9
Recreational Program	41.3	35.3	19.6	17.4
Birth Control Counseling	75.5	20.1	5.4	4.9
Educational Counseling	36.6	30.6	12.0	18.0
Nutrition Education	56.2	28.6	13.0	6.5
Personal Counseling	41.1	28.1	14.1	10.2
Housing Assistance	16.8	9.7	2.7	25.4
Pregnancy Counseling	29.9	15.2	8.7	4.9

SOURCE: Tabulations are from AIR 12-month follow-up interviews with experimental group members in Sample I.

NOTES: The numbers do not add to 100.0 percent because respondents could cite multiple services used.

[a]All percentages are based on responses from 184 Project Redirection participants. The base for all calculations was this full sample. Thus, for parenting classes, 86 participants (46.7 percent) received the service directly from the program. Of the 123 teens who obtained any parenting education (66.8 percent), the majority (70 percent) got it from Project Redirection.

separately in the next section.[5]

The first column of Table 3.2 indicates the total percentage of teens who had received (from any source) the given service since the baseline interview. As indicated, the majority of teens had obtained medical services for themselves and their babies, birth control counseling, nutrition education, and parenting classes. More than a third of the teens had received personal and educational counseling, and had taken part in a recreation program.

The second column of Table 3.2 shows that Project Redirection was a major provider of most of these services, with the exception of health-related services. For example, 36.6 percent of the participants had received some educational counseling in the past year, and of these over 80 percent (30.6 percent of the entire sample) had obtained the counseling from Project Redirection. It is likely that many of those teens who obtained services elsewhere were referred to that service by program staff.

The third column provides information about the intensity of service receipt by all participants. The data

[5] It has recently been noted (Brown, 1982) that the most serious deficiency of teen parent programs is their lack of services that are directly related to the future economic well-being of teenage mothers.

indicate that, for the most part, service provision to these teens tended not to be a one-time affair; in fact, teens who obtained program services often received them on more than five occasions. For example, 46.7 percent of all participants received parenting education directly from the program, and of these over 70 percent (33.2 percent of all participants) said they had attended more than five classes.

The last column shows the total percentage of experimental group teens who felt they needed a given service but had not received it in the preceding 12 months. With two exceptions--tutoring for school work and housing assistance--the percentage needing and not receiving the service was substantially lower than the percentage of teens who presumably needed the service and obtained it. For example, about 80 percent of the sample felt they needed birth control counseling; however, 75.5 percent received it while 4.9 percent did not. The greatest unmet service need was tutoring for school work. Over one third of the sample felt tutoring was needed, but only one teen in ten obtained it.

The data reviewed in this section thus indicate that Redirection participants are obtaining a number of needed

services, and that the program itself is directly providing many of them. A key question is, however, whether participants are better served than they otherwise might have been in the absence of Project Redirection. This question is addressed by comparing service utilization in the experimental versus comparison groups and by examining baseline and follow-up utilization patterns.

Table 3.3 presents service use rates for the 11 services discussed above for both experimental and comparison group members. As seen here, rates were higher for the experimental group teens than the comparison teens for every service, and significantly higher for 8 of the 11. In many cases, the differences were substantial: twice as many participants had received personal counseling and tutoring for school work. Even for some services for which the comparison group rate was high, such as birth control counseling, the experimental group rate was significantly higher.

Table 3.3 suggests that Project Redirection is responsible for these differences. The first column figures in parentheses indicate the percentage of experimental teens who obtained the service from Project Redirection. For example, of the 10.8 percent of the teens

TABLE 3.3

PERCENTAGE OF EXPERIMENTAL AND COMPARISON GROUP MEMBERS RECEIVING SELECTED SERVICES SINCE BASELINE INTERVIEW

Type of Service Received	Percentage Utilizing Given Service, by Group			
	Experimental		Comparison	Difference
Parenting Classes	66.8	(71.1)[a]	35.8	31.0***
Tutoring for School Work	10.8	(52.6)	4.7	6.1*
Medical Care for Baby	98.4	(0.0)	94.3	4.1
Medical Care for Self	81.6	(0.0)	77.1	4.5
Recreational Program	41.3	(89.0)	7.9	33.4***
Birth Control Counseling	75.5	(26.8)	61.9	13.6**
Educational Counseling	36.6	(81.2)	20.9	15.7***
Nutrition Education	56.2	(52.5)	42.5	13.7**
Personal Counseling	41.1	(69.3)	17.2	23.9***
Housing Assistance	16.8	(56.3)	9.3	7.5*
Pregnancy Counseling	29.9	(52.8)	21.9	8.0

SOURCE: Tabulations are from AIR 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: The percentages do not add to 100.0 percent because respondents could cite multiple services used.

These data are based on responses from 184 Redirection participants and 215 comparison group members.

[a]The numbers in parentheses represent the percentage of experimental group teens receiving the specified service who received it from the Redirection program itself.

*The unadjusted group difference is statistically significant at the .05 level.

**The unadjusted group difference is statistically significant at the .01 level.

***The unadjusted group difference is statistically significant at the .001 level.

who obtained tutoring for school work in the past year, about half (52.6 percent) received it from Redirection. In the absence of the program, only some 5.1 percent might have obtained tutoring, a figure similar to the 4.7 percent rate in the comparison sites.

Table 3.4 presents some baseline and follow-up information on this same set of 11 services for both the experimental and comparison groups. Here, an index was created by counting the number of services used and needed out of the list of 11 possible services. The first row indicates that at baseline experimental group teens were receiving somewhat fewer services (0.5 on average) than the comparison groups teens. By follow-up, however, the experimental group was substantially better served than the comparison group, with Redirection teens obtaining 1.6 more services than other teens, on average. These same teens were receiving 2.5 more services than they had at baseline.

Again, the data suggest that the change is directly attributable to program participation. The difference between baseline and follow-up service receipt (2.5 services) is almost the same as the number of services the program provided directly (2.2 services). Furthermore, the experimental-comparison group difference continues even

TABLE 3.4

COMPARISON OF UNADJUSTED MEAN NUMBER OF SERVICES USED AND NEEDED AT BASELINE AND FOLLOW-UP, BY EXPERIMENTAL VERSUS COMPARISON GROUP

Services Used or Needed[a]	Mean Number of Services, by Group		
	Experimental	Comparison	Difference
Mean Number Used at Baseline	3.0	3.5	-0.5*
Mean Number Used at Follow-Up[b]	5.5	3.9	1.6***
Mean Number of Redirection Services Used	2.2	-	-
Mean Number Needed at Baseline	3.1	2.6	0.5*
Mean Number Needed at Follow-Up[b]	1.2	1.7	-0.5
Total Number of Respondents	185	215	--

SOURCE: The data are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: [a]Based on questions regarding the utilization of and need for 11 specific services.

[b]The follow-up means in the table are unadjusted. When statistical controls for background variables (including baseline service needs and utilization) are introduced, the adjusted number of follow-up services used is 5.6 in the experimental group and 3.8 in the comparison group; for services needed, the adjusted means remain 1.2 and 1.7, as shown. Levels of significance remain the same in both cases.

*The unadjusted group difference is statistically significant at the .05 level.

***The unadjusted group difference is statistically significant at the .001 level.

when both baseline characteristics (including services used and needed) and participation in a teen parent program other than Redirection are statistically controlled. Being a Redirection participant added about 1.8 services to the total number used at follow-up, net of other factors (see Table C.2 in Appendix C). With background factors controlled, participation in the program was also associated with a reduction of about .5 in the number of services needed at follow-up.

D. Employment-Related Services

The follow-up interview, as well as the baseline interview, asked teens to indicate whether they had received training in specific job-related areas: how to find a job, how to fill out a job application, how to act in a job interview, how to get along with people at work, how to act on the job, training for specific job skills (e.g. typing), and how to decide on a type of job. The percentage of respondents who reported training in these areas in the past 12 months is presented in Table 3.5. As shown in this table, the experimental group teens were significantly more likely than the comparison group members to have received training in all areas at follow-up.

TABLE 3.5

PERCENTAGE OF EXPERIMENTAL AND COMPARISON GROUP MEMBERS RECEIVING SELECTED EMPLOYMENT TRAINING EXPERIENCES SINCE BASELINE INTERVIEW

Type of Training Experience	Percentage Receiving Given Training, by Group		
	Experimental	Comparison	Difference
How to Find a Job	51.4 (62.4)[a]	19.7	31.7***
How to Fill Out a Job Application	55.4 (50.0)	33.2	22.2***
How to Act on a Job Interview	53.0 (50.5)	27.6	25.4***
How to Get Along With People at Work	42.6 (55.1)	25.5	17.1***
How to Act on the Job	45.7 (51.2)	25.2	20.5***
Training for Specific Job Skills	34.1 (21.3)	21.0	13.1**
How to Decide on the Kind of Job You Want	37.0 (48.6)	17.2	19.8***

SOURCE: Tabulations are from AIR 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: The percentages do not add to 100.0 percent because respondents could cite multiple forms of job training.

These data are based on responses from 184 Redirection participants and 214 comparison group members.

[a]The numbers in parenthesis represent the percentage of Redirection participants receiving the specified training who received it from the Redirection program itself.

**The unadjusted group difference is statistically significant at the .01 level.

***The unadjusted group difference is statistically significant at the .001 level.

Again, the pattern of service receipt suggests that Project Redirection was responsible for this result. The parenthetical numbers in the first column specify the percentage of experimental teens who had received the training from the Project Redirection program. For example, 53.0 percent of these teens had received training on job interview behavior, and of these about half (50.5 percent) received the training from Redirection. Only about 26 percent of the experimental group might have obtained this training without Project Redirection, a figure comparable to the comparison group rate (27.6 percent).

A composite index was formed by totaling the number of different types of training experiences an individual teen had received. Scores on this index ranged from 0 for no training to 7 for exposure to all seven areas. The mean number of training experiences for the experimental group teens--adjusted for age, ethnicity, baseline training experiences, and baseline scores on an employability knowledge test--was 3.1, nearly double that for the comparison group (1.7). This difference was significant at the .0001 level.

E. Child Care

One of the Redirection service components is the brokering of child care arrangements for those participants who need it. Potentially, child care could be a critical factor in the success of a teen parent program. If these young mothers could not participate in program activities, attend school, or take jobs because of their child care needs, then the program would not be expected to have much of an impact.

The baseline report indicated that the most prevalent source of child care assistance was the young mothers' own mothers. Table 3.6 indicates that, at follow-up, the [illegible]al grandmother continued to be an important provider of child care to both experimental and comparison group teens. Approximately 10 percent more of the comparison group members than experimental teens relied on their own mothers, but this difference was not statistically [illegible]gnificant. At baseline, about 15 percent of the teens used some form of formal child care (paid baby sitter, family day care, or day care center) while attending school. While this percentage was fairly stable from baseline to follow-up in the comparison group, the experimental group rate had increased to 25.4 percent. Again, however, the difference was not significant.

TABLE 3.6

RESPONSES ON SELECTED CHILD-CARE VARIABLES AT FOLLOW-UP, BY EXPERIMENTAL VERSUS COMPARISON GROUP

Selected Child Care Variable	Percentage with Given Response, by Group		
	Experimental	Comparison	Difference
Percent Using Own Mother for Child Care While in School or at Work	34.5	44.7	-10.2
Percent Using Paid Babysitter/Day Care	25.4	16.3	9.1
Percent Indicating Current Arrangements Meet Needs	90.7	90.6	0.1
Percent Indicating Desire to Change Current Arrangements	24.9	18.6	6.3
Percent Not Interested in Formal Child Care Because of Cost	15.5	23.7	- 8.2
Percent Not Interested in Formal Child Care Because of Distrust of Strangers	28.9	32.4	- 3.5
Percent Ever Missed School Because of Child Care	34.9	29.0	5.9

SOURCE: Tabulations are from AIR 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: The percentages do not add to 100.0 percent because respondents answered each question independently.

These data are based on responses from 174 Redirection participants and 204 comparison group members.

None of the group differences is statistically significant at or beyond the .05 level.

While the vast majority of teens (90.6 percent) said at follow-up that their current child care arrangements met their needs, about one in five of these young mothers expressed a desire for a change in their arrangements. When asked to explain what change they had in mind, the teens were most likely to say they wanted to find a day care center or a paid babysitter.

However, some of the teens expressed concerns about formal child care when asked specifically why they had not used it. The most frequent response was a lack of trust in unknown babysitters (30.8 percent). Formal child care cost was another prevalent reason, cited by one-fifth of the teens. The experimental and control group differences on these questions were not sizeable.

Finally, respondents were asked if they had ever missed school because of a child care problem. As shown in Table 3.6, about one-third of the teens in both groups stated they had. However, the majority of teens (76.2 percent) indicated that they had missed school on fewer than five occasions because of a child care problem.

Thus, the experimental group teens appear to have child care arrangements that are similar to those of comparison group mothers. The teens have most often used

relatives as babysitters, especially their own mothers. In neither group was the demand for formal child care great. As seen in Table 3.1, only a small minority of the experimental group teens rated the child care component of the program as most helpful, but a general conclusion cannot be reached; child care assistance was available only in the Harlem and Phoenix programs.

F. Conclusions

If a program such as Redirection is to have any impact on the lives of its participants, it clearly must provide them with the services that are designed to induce changed attitudes and behaviors. The data presented in this chapter suggest that Project Redirection is, in fact, delivering services to its participants.

Compared to the non-participating teens--even those who had taken part in another teen parent program--the experimental group teens had received more services and been exposed to more job-related training experiences in the previous 12 months. While the program did not play an important role in providing child care, this does not seem to have impeded program participation. More than half of the teens interviewed 12 months earlier were still enrolled

in the program at follow-up, and most reported a fairly high level of participation. The first "link" in the posited chain of events leading to program impact appears to have been realized.

Chapter 4

Early Impacts on Educational Outcomes

Educational attainment is a critical factor in determining future occupational status and economic self-sufficiency. Wage rates are lower, unemployment is higher, and the likelihood of welfare dependency is greater for individuals who do not finish high school or obtain a GED certificate. In recognition of the importance of continued education, Project Redirection places a heavy emphasis on this program component. Encouraging young mothers to invest in their future economic well-being by returning to and completing an educational program is a major objective of Project Redirection.

The need for programmatic emphasis on education is supported by many research studies. According to figures cited in a report by the Alan Guttmacher Institute (AGI), about 60 percent of 18-year-old mothers nationwide have not completed high school; 40 percent of 19-year-old mothers

have also failed to do so. Pregnancy is, in fact, the leading cause of high school dropout among teenaged girls in this country (AGI, 1981). The fact that lifelong educational attainment is curtailed for teenage mothers, even when other factors contributing to schooling are held constant, has been abundantly documented.[1]

At baseline, the educational deficits of this research sample were already striking. Despite the fact that these teens were 16 years old, on average, only slightly more than half were enrolled in a school or GED program at their first interview. It was estimated that about 80 percent were a year behind grade for their age, and 40 percent were two or more years behind. Most of the teens said they wanted to return to school and complete a diploma or GED, but, in fact, approximately 40 percent of the sample had been out of school for more than a year. The prognosis for school completion under these circumstances (with a long period of school absence and the responsibilities of parenthood), do not seem promising in the absence of some intervention.

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[1]For example, see Card & Wise, 1978; Moore & Waite, 1977; McCarthy et al, 1982.

In the follow-up interviews, additional information was collected on the effects of pregnancy and child care responsibilities on school attendance. Detailed educational histories of the sample teens were also collected. In the first section of this chapter, the overall schooling experiences of these teens are examined. In this first section descriptive information is provided, including experimental/comparison group and ethnic group trends, without adjusting for other background characteristics. Next, educational outcomes are analyzed to see if participation in Project Redirection affected the educational investment behaviors of these young mothers. A third section examines program impacts on educational aspirations. In the last two sections statistical adjustments are made for important baseline and background characteristics so that a more accurate assessment of program impacts can be made.

A. Overview of Teen Parents' Schooling at Follow-Up

During the follow-up interview, teens were asked to reconstruct their school histories from the time they entered first grade. Table 4.1 presents school enrollment data for the experimental and comparison groups from Fall, 1978 (when these girls were, on average, 13-14 years old)

TABLE 4.1

PERCENTAGE OF TEENS WHO WERE ENROLLED IN SCHOOL OR GED PROGRAM OR WHO HAD COMPLETED SCHOOL, BY SEMESTER (FALL, 1978 TO SPRING, 1982) AND BY EXPERIMENTAL VERSUS COMPARISON GROUP

Semester	Percentage with Positive School Status, by Group		
	Experimental	Comparison	Difference
Fall, 1978	83.6	89.3	- 5.5
Spring, 1979	78.4	86.5	- 8.1*
Fall, 1979	73.0	76.7	- 3.7
Spring, 1980	61.6	66.5	- 4.9
Fall, 1980[a]	57.3	61.9	- 4.6
Spring, 1981[a]	65.4	52.1	13.3**
Fall, 1981	65.9	52.1	13.8**
Spring, 1982[b]	65.9	55.5	10.4
Total Number of Respondents	185	215	--

SOURCE: Tabulations are from AIR 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: [a]The period of enrollment in Project Redirection for Sample I teens coincided with the Fall, 1980 and Spring, 1981 school semesters.

[b]Since many teens completed their follow-up interviews in the Fall of 1981, the tabulations for the Spring of 1982 are based on responses from only 88 experimental group and 146 comparison group teens.

*The unadjusted group difference is statistically significant at the .05 level.

**The unadjusted group difference is statistically significant at the .01 level.

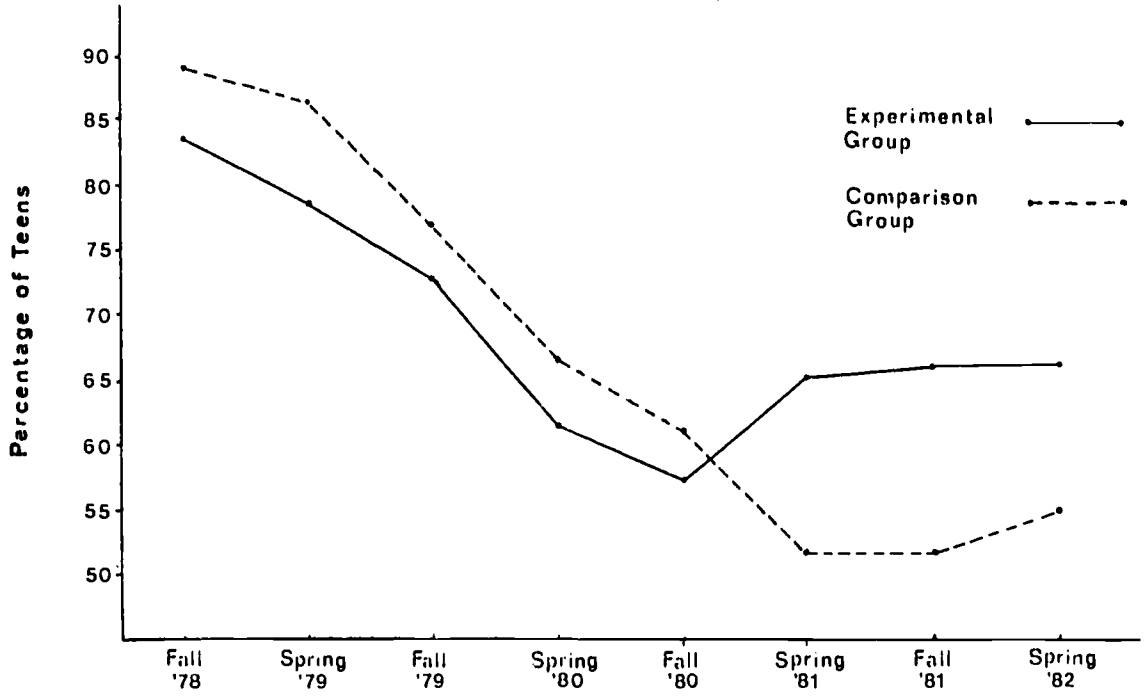
to Spring, 1982. Substantial percentages of teens were not enrolled in an educational program during the entire four-year period, but for both groups, school enrollment declined most sharply from 1978 to 1980. These two years correspond to the period during which most of these teens became pregnant.

These same results are displayed graphically in Figure 4.1. This figure shows more clearly that the two groups experienced almost parallel declines in school enrollment until the Fall of 1980. An abrupt change occurred between that point and Spring, 1981--the exact time period in which the majority of the experimental group teens enrolled in the Redirection program. The comparison group continued on the same downward path it had been taking for several years, while the experimental teens' path was diverted upwards.

At the time of the follow-up interview, 43.0 percent of the teens were not enrolled in an educational program, nor had they completed one. School enrollment levels, however, varied by ethnicity; enrollment was significantly lower among the Hispanic respondents than among the black or white teens. As Table 4.2 indicates, Hispanic teens were consistently and significantly less likely to be in

Figure 4.1

PERCENTAGE OF TEENS WITH POSITIVE SCHOOL OUTCOMES, 1978 TO 1982[a]



SOURCE: Tabulations are from AIR 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: [a]A positive school outcome was defined as either being in or having completed a school or GED program.

[b]The experimental group teens enrolled in Project Redirection between August, 1980 and July, 1981.

TABLE 4.2

PERCENTAGE OF TEENS WHO WERE ENROLLED IN SCHOOL OR GED PROGRAM OR WHO HAD COMPLETED SCHOOL, BY SEMESTER (FALL, 1978 TO SPRING, 1982) AND ETHNICITY

Semester	Percentage With Positive School Status, by Ethnicity			
	Hispanic	Black	White	All Teens
Fall, 1978	80.7	92.4	86.4	86.8
Spring, 1979	72.9	92.4	81.8	83.0
Fall, 1979	62.0	86.9	77.3	75.4
Spring, 1980	51.8	73.4	75.0	64.5
Fall, 1980	45.2	72.8	61.4	59.9
Spring, 1981	43.3	72.3	61.4	58.9
Fall, 1981	39.0	65.6	65.0	54.3
Spring, 1982[a]	33.3	63.4	71.4	52.6
Total Number of Respondents	166	184	44	394

SOURCE: Tabulations are from AIR 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: The ethnic group differences were statistically significant at or beyond the .05 level for all eight semesters.

[a]The tabulations for the Spring of 1982 are based on responses from 84 Hispanic, 90 black, and 35 white teens.

school from Fall, 1978 to Spring, 1982, and this gap widened over the four-year period. School status at follow-up was unrelated to the teens' age or pregnancy status at baseline.

A total of 31 teens (7.8 percent) had completed an educational program at follow-up. Of the 31 completers, half (48.4 percent) had obtained a GED certificate, while the other half (51.6 percent) had received a high school diploma. Older teens were more likely to have completed school than younger ones and white teens had a higher completion rate (15.9 percent) than Hispanic (9.0 percent) or black (4.3 percent) teens.

The majority (71.9 percent) of teens enrolled in an educational program at follow-up reported being in a regular school program rather than in a GED program. However, enrolled experimental group teens were more likely to be in a GED program (40.0 percent) than their counterparts (16.2 percent). The program type was unrelated to ethnicity or pregnancy status at baseline, but younger teens were more likely to be enrolled in a regular program (81.4 percent) than those 17 or older (66.7 percent).

A fairly high percentage of the sample had been enrolled in a teen parent or pregnant teen school program at one point or another (Table 4.3). Comparison group members were more likely than experimental group members to have spent one or more semesters in such a program prior to baseline. On the other hand, during the post-baseline period those in the experimental group were more likely to be enrolled in a special teen parent program (33.7 percent) than comparison group teens (22.3 percent).

As a whole, this sample of teens, despite their very young age, displayed a pattern of repeated dropout behavior. In fact, only 28.5 percent of the teens at follow-up had consistently stayed in school. On average, teens had dropped out of school 1.04 times. Some 20 percent had already left school twice, and an additional 6 percent had dropped out three or more times. Some ethnic group differences were also observed: Hispanic teens (85.5 percent) were more likely than black (59.2 percent) or white (68.2 percent) teens to have dropped out of school at least once.

Among the 282 teens in the sample who had at some time dropped out of school, 62.4 percent of them indicated that they had left school during a pregnancy; 86.8 percent said

TABLE 4.3

PERCENTAGE OF TEENS HAVING SPENT ONE OR MORE SEMESTERS IN A TEEN PARENT SCHOOL PROGRAM AT BASELINE AND FOLLOW-UP, BY EXPERIMENTAL VERSUS COMPARISON GROUP

	Percentage of Teens in Special Program, by Group		
Time of Enrollment	Experimental	Comparison	Difference
Ever Enrolled at Baseline	32.6	51.2	-18.7**
Enrolled from Baseline to Follow-Up	33.7	22.3	11.4**
Total Number of Respondents	184	215	--

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTE: **The unadjusted group difference is statistically significant at the .01 level.

the pregnancy was directly responsible for their decision to leave. When asked to describe more specifically why the pregnancy caused them to leave, the most prevalent response was physical illness (27.1 percent), followed by embarrassment to be seen pregnant in school (19.3 percent). Another 10.7 percent said they just did not want to attend any more. Pregnant teens were most likely to leave in their first (37.0 percent) or their second trimester (34.9 percent).

Although experimental and comparison groups recorded similar school dropout rates, return-to-school rates were higher for the experimental group teens. Among all teens in the sample who had dropped out of school during a pregnancy, 64.7 percent returned following the baby's birth: the return rate for the experimental group was 74.6 percent, compared with 57.8 percent among the comparison group. A similar trend was noted among the 77 teens who dropped out of school <u>after</u> childbirth. One quarter of these dropouts subsequently returned to an educational program, but 36.5 percent of the experimental group did so compared to 20.5 percent of the comparison group.

The majority of teens (59.0 percent) who were not in an educational program at follow-up planned to return to

school within the next six months, with the experimental and comparison groups having similar expectations. Hispanic teens (47.7 percent) were less likely to have definite plans for an early return to school than white (64.3 percent) or black (79.4 percent) teens not enrolled at follow-up.

As the data on school dropouts suggest, the teens in this sample continued to be substantially behind grade level for their age. Among those currently enrolled in a graded school program, the average educational deficit was 1.32 years. For those not in school, the gap was even greater.

B. Redirection Impacts on School Status

A question of major importance in this impact assessment is whether or not participation in Project Redirection results in improved educational outcomes. This section examines whether the experimental group teens were better off educationally after one year of program operations than they otherwise might have been in the absence of the program.

As shown in Figure 4.1, a higher percentage of teens in the experimental group than in the comparison group were enrolled in or had completed an educational program in the post-baseline period, despite the pre-baseline advantage of the comparison group. This longitudinal perspective suggests that Project Redirection was, in fact, exerting an influence on the school behavior of the experimental group.

A more sophisticated analysis was used to determine whether the experimental and comparison groups differed significantly on school enrollment rates after adjustments for any pre-baseline differences.[2] This analysis showed that school status at baseline, length of time out of school, and highest grade completed at baseline were significantly related to school status at follow-up, as were the teens' ethnicity and household composition (see Table C.3 in Appendix C).

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[2]Our strategy for analyzing program impacts was described in Chapter 2. In the case of school status, logit analysis was used in controlling for background variables that potentially affect one's schooling. Logit analysis was selected because the outcome variable was "positive school status," a binary variable defined as either being enrolled in or having completed school or a GED program or not enrolled in either. The results are essentially the same as those presented above when current status at follow-up (in school/not in school) is substituted for this "positive school status" variable. All logit and regression tables appear in Appendix C. See also Appendix D regarding supplementary analyses performed in connection with the selection bias issue.

Even more importantly, with these and other characteristics controlled, participation in Project Redirection was significantly related to school status at follow-up. Approximately 16 percent more of the experimental group (66 percent) than the comparison group (50 percent) were either enrolled in or had completed an educational program 12 months after the baseline interview, as shown in Table 4.4. Program participation was the most powerful predictor of follow-up school status.

The association between program participation and a possible educational outcome was yet stronger in an analysis examining the effect of the teens' length of enrollment in Project Redirection. Each month of program enrollment[3] was associated with a 1.8 percent increase, on average, in the percentage of teens with a positive school outcome. Thus, compared to the 50.0 percent of teens with a positive school status in the comparison group, approximately 72 percent of those in the experimental group with 12 months of program participation were in school or

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[3]In an analysis not shown, we tested for the possibility of a non-linear effect of length of participation by adding a term for the square of months enrolled. The hypothesis being tested was that the effect of additional months in the program peaks at some point and that additional months of participation beyond that point have no incremental effects. The analysis failed to support this hypothesis.

TABLE 4.4

ADJUSTED PERCENTAGES OF EXPERIMENTAL AND COMPARISON GROUP TEENS WITH A POSITIVE SCHOOL STATUS[a] AT FOLLOW-UP, FOR TEENS IN VARIOUS SUBGROUPS

Subgroup	Adjusted Percentages of Teens, by Group		
	Experimental	Comparison	Difference
Teens in School at Baseline (N=234)	77	67	10+
Teens Not in School at Baseline (N=157)	49	20	29**
Black Teens (N=181)	75	63	12*
Hispanic Teens (N=162)	51	34	17+
Teens Age 15 or Younger at Baseline (N=134)	58	50	8
Teens Age 16 or Older at Baseline (N=257)	68	50	18**
Teens Pregnant at Baseline (N=237)	66	51	15**
Teens Not Pregnant at Baseline (N=154)	55	46	20**
All Teens (N=391)	66	50	16***

SOURCE: Tabulations are from AIR 12-month follow-up interviews with experimental and comparison group members.

NOTES: The percentages shown have been adjusted for school status at baseline, ethnicity, highest grade completed at baseline, age of youngest child, and presence of the teens' mother, other women, or husband/boyfriend in the household at follow-up. The full logit analyses results are presented in Tables C.3 to C.7 in Appendix C.

[a]Positive school status was defined as either enrollment in a completion of a school or GED program at follow-up.

+Statistically significant at the .10 level for a two-tailed test.

*Statistically significant at the .05 level for a two-tailed test.

**Statistically significant at the .01 level for a two-tailed test.

***Statistically significant at the .001 level for a two-tailed test.

had completed a school program at follow-up.[4]

Program impacts on schooling were also examined for various subgroups. Of particular interest was the possible effect of program participation on in-school versus out-of-school teens at baseline. As Table 4.4 shows, participation in Project Redirection had a more powerful effect on the school status of teens out of school at baseline: 29 percent more of the experimentals (49 percent) than comparison (20 percent) respondents had returned to school 12 months after baseline. This difference was highly significant. Among those in school at baseline, the adjusted difference was only 10 percent, a difference significant at the .10 level (.05 level for a one-tailed test).

Separate logit analyses were also performed for ethnic, age, and parity subgroups. Table 4.4 summarizes experimental/comparison group differences for blacks,[5] Hispanics, teens 15 or younger at baseline, teens 16 or

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[4]A separate analysis was performed with the experimental group only, using length of enrollment to predict school outcomes. Number of months of enrollment continued to significantly and positively affect school status at follow-up, other things being equal.

[5]The subgroup of white teens was too small to perform a logit analysis.

older at baseline, and teens pregnant or not pregnant at baseline. This table shows that, for all six subgroups, participation in Project Redirection was associated with a higher probability of a positive school status at follow-up. This difference was not statistically significant for one subgroup only (younger teens), and this subgroup was small (126 teens). However, even within this small subgroup, length of participation in the program was significantly correlated with a positive school outcome.

C. Redirection Impacts on Educational Aspirations

Another area of investigation was whether participation influenced the teens' plans or hopes for further education. Table 4.5 presents the unadjusted responses to the follow-up question: "Right now, how far do you think you want to go in school?"

The overwhelming majority of teens in both groups wanted at least a high school diploma, but more of the teens in the experimental group (45.6 percent, than in the comparison group (33.2 percent) wanted education beyond a diploma or GED certificate. Experimental/comparison group differences were particularly marked for teens whose initial aspirations were relatively low. For example,

TABLE 4.5

EDUCATIONAL ASPIRATIONS OF TEENS AT FOLLOW-UP,

BY EXPERIMENTAL VERSUS COMPARISON GROUP

Educational Aspirations	Percentage Distribution of Teens, by Group		
	Experimental	Comparison	Difference
11th Grade or Less	2.7	4.4	- 1.7
Diploma/GED	48.4	61.0	-15.6
Trade/Technical School	14.7	7.1	7.6
Two-Year College	15.2	11.4	3.8
Four-Year College	11.4	11.4	0.0
Graduate Degree	4.3	3.4	0.9
Don't Know	3.3	1.4	1.9
Total	100.0	100.0	0.0
Total Number of Respondents	184	184	--

SOURCE: Tabulations are from AIR 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: The percentages may not add to 100.0 percent due to rounding error.

The unadjusted group differences for this distribution are statistically significant at the .05 level.

among the teens who at baseline aspired to only a high school diploma or GED, 31 percent of the experimental, but only 18 percent of the comparison group, reported wanting more education at follow-up.

However, these percentages and those shown in Table 4.5 are uncorrected for any background characteristics. When baseline and other relevant variables were controlled, the experimental/comparison group differences largely disappeared.[6] The results of one analysis are summarized in Table 4.6. Here follow-up aspirations are shown separately for those whose baseline aspirations were either high or low. At follow-up, the experimental group teens had higher aspirations than those in the comparison group, regardless of how much education they wanted at baseline. Nevertheless, these differences, after first adjusting for

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[6] Several OLS and logistic regression analyses were performed in which educational aspirations at follow-up were regressed on a range of baseline characteristics. In one analysis the dependent variable was the difference between aspirations at baseline and follow-up. In others, a dichotomous dependent variable was created in which teens whose aspirations had increased since follow-up were assigned a score of 1, while those whose aspirations either declined or remained the same were assigned a score of 0. Follow-up aspirations were also regressed on length of program participation. In none of these analyses was participation in Redirection significantly related to educational goals at follow-up, net of other factors. The full regression table for the results summarized in Table 4.6 is presented in Table C.8, Appendix C.

TABLE 4.6

ADJUSTED EDUCATIONAL ASPIRATIONS AT FOLLOW-UP FOR TEENS WANTING OR NOT WANTING MORE THAN DIPLOMA AT BASELINE, BY EXPERIMENTAL VERSUS COMPARISON GROUP

Educational Aspirations at Follow-UP	Wanting Diploma or Less at Baseline			Wanting More than Diploma at Baseline		
	Percentage Distribution of Teens, by Group			Percentage Distribution of Teens, by Group		
	Experimental	Comparison	Difference	Experimental	Comparison	Difference
High School Diploma/ GED or Less	68.0	77.0	-9.0	39.0	45.0	-6.0
More Than a High School Diploma/GED	32.0	23.0	9.0	61.0	55.0	6.0
Total	100.0	100.0	0.0	100.0	100.0	0.0
Total Number of Respondents	88	131	--	90	82	--

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: The percentages shown have been adjusted for school status at baseline, highest grade completed at baseline, age, ethnicity, presence of mother in household at follow-up, and number of school semesters repeated pre-baseline. The full logit analysis results are presented in Table C.8 of Appendix C.

The experimental-comparison group differences were not statistically significant for either of the two comparisons.

such background characteristics as baseline school status, age, and ethnicity, were not statistically significant.

D. Summary and Conclusions

This chapter revealed that the sample of teens in this study continues to be disadvantaged educationally. Only 57 percent were enrolled in or had completed an educational program at follow-up. Pregnancy and child care responsibilities were cited as the major deterrents to school enrollment.

The follow-up findings suggest that participation in Project Redirection had a substantial effect on these teens' school status at follow-up. The effect was particularly pronounced among teens who were dropouts at baseline. Program participation was associated with an estimated 29 percent increase in the likelihood of school enrollment or completion among dropouts, net of a wide range of background factors. The longer the period of participation in Redirection, the greater the effects.

However, the program appeared not to have a significant impact on educational aspirations. This finding does not necessarily imply that the program failed to motivate these teens. It could mean, for example, that the program

is doing a better job of persuading teens to take steps toward the actual attainment of a modest goal, rather than developing in them unrealistically high aspirations. This hypothesis can be better tested at the time of the 24-month follow-up.

Chapter 5

Early Impacts on Employment Variables

The employment-related outcomes of the sample teens are of particular interest for several reasons. First, given the fact that the ultimate long-term goal of Project Redirection is the teens' economic self-sufficiency, their current work behavior, employment knowledge, and attitudes toward work are important indications of their potential for future achievement. Early job experience and the acquisition of employability skills are viewed as critical investments in the future marketability of these young women. Research evidence supports the view that work experience during the teen years is associated with higher levels of adult employment (e.g., Adams & Mangum, 1978; Johnston & Bachman, 1973).

A second reason for focusing on short-term employment outcomes is the generally bleak occupational prognosis for teen mothers. Early childbearing has consistently been

found to be related to low occupational status and income, and high rates of unemployment, and welfare dependency, (e.g., Card & Wise, 1978; McLaughlin, 1977; Moore et al., 1979). McLaughlin, in his path analysis of National Longitudinal Survey data, found that early work experience had a particularly strong effect on the earning potential of women who became mothers before age 19.

This chapter on employment-related outcomes consists of four major sections. First is an overview of the employment situation of the sample at the time of the 12-month follow-up. This first section presents the data in raw form, unadjusted for any differences between the two groups. The second focuses on program impacts on work behavior. The next section examines program impacts on job related "enabling factors"--i.e., job readiness factors that are presumed to facilitate better work investments. These two latter sections present experimental/comparison group differences after first adjusting statistically for important baseline and background characteristics. The fourth section summarizes the major findings.

A. Overview of Teen Parents' Work-Related Outcomes at Follow-Up.

In examining the employment status of this sample at follow-up, one must first keep in mind that these teens were predominantly school-age and that the interviews asking about their employment status were completed primarily during the academic year. Second, school attendance for this group is a higher priority goal of Project Redirection than actual employment, and teens were encouraged by counselors to pay particular attention to the educational component. The work status of the teens at follow-up therefore needs to be considered concurrently with their school status.

Table 5.1 presents the employment and schooling status of teens at follow-up. As shown in this table--and as expected--only a very small percentage of teens had completed their schooling and were employed. However, of the 31 completers, only five teens were working at all. The rest of the completers were neither in school nor working at follow-up. The experimental group teens were most likely to be in school, but unemployed (47.8 percent), while comparison teens were most likely to be neither in school nor working (43.2 percent). Across the two groups,

TABLE 5.1

SCHOOL/WORK STATUS OF TEENS AT FOLLOW-UP, BY EXPERIMENTAL VERSUS COMPARISON GROUP

School/Work Status at Follow-Up	Percentage Distribution of Teens, by Group		
	Experimental	Comparison	Difference
Completed School/GED and Working	2.2	0.5	1.7
Enrolled in School/ GED and Working	6.5	12.1	- 5.6
Enrolled in School/ GED and Not Working	47.8	34.0	13.8
Not Enrolled or Completed School but Working	6.0	4.7	1.3
Not Enrolled in or Completed School/ GED and Not Working	29.9	43.2	-13.3
Completed School/GED and Not Working	7.6	5.6	2.0
Total	100.0	100.0	0.0
Total Number of Respondents	184	215	--

SOURCE: Tabulations are from AIR 12-month follow-up interviews with experimental and comparison group members.

NOTES: The percentages may not add to 100.0 percent due to rounding error.

The unadjusted group differences for this distribution are statistically significant at the .05 level.

Hispanic teens (59.6 percent) were more likely than black (32.2 percent) or white (34.1 percent) teens to be neither working nor in school when re-interviewed.

Another way to look at current working status is to define labor force participation as either employment or job-seeking. In the follow-up interview, unemployed respondents were asked whether they were looking for employment and, if so, whether they wanted full- or part-time work. The results are presented in Table 5.2 according to experimental/comparison group status. About two out of five teens in the sample said they were looking for a job, and about half of these wanted full-time employment. More of the comparison teens (46.0 percent) than experimental teens (38.5 percent) were out of the labor force altogether.

Jobs held at follow-up, like earlier jobs, tended to be almost exclusively non-skilled and low-paying. The most frequently cited types of employment were day-care assistant/babysitter, domestic cleaner, fast food clerk, general or file clerk, camp counselor, waitress, cashier, and receptionist. On average, those working at follow-up earned $3.50 per hour. Both type of employment and hourly wage were similar for the experimental and comparison groups.

TABLE 5.?

LABOR FORCE PARTICIPATION OF TEENS AT FOLLOW-UP,

BY EXPERIMENTAL VERSUS COMPARISON GROUP

Labor Force Status at Follow-Up	Percentage Distribution of Teens, by Group		
	Experimental	Comparison	Difference
Employed Full-Time[a]	7.8	3.8	4.0
Employed Part-Time[b]	7.3	13.6	-6.3
Seeking Full-Time Employment	15.6	15.5	0.1
Seeking Part-Time Employment	21.8	19.2	2.6
Seeking Full- or Part-Time Employment	8.9	1.9	7.0
Not Working and Not Seeking Employment	38.5	46.0	-7.5
Total	100.0	100.0	0.0
Total Number of Respondents	179	213	--

SOURCE: Tabulations are from AIR 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: The percentages may not add to 100.0 percent due to rounding error.

The unadjusted group differences for this distribution are statistically significant at the .01 level.

[a]Full-time employment was defined as more than 30 hours of work per week.

[b]Part-time employment was defined as 30 hours or less of work per week.

Since most of the follow-up interviews took place when many of the teens were in school, it is important to look at the teens' employment experience other than at follow-up. Table 5.3 presents descriptive information on the teens' employment histories before and after the baseline interview.

On average, the teens had held 1.6 different jobs by the time they completed the follow-up interviews. The young women in the experimental group had worked in a larger number of different jobs than those in the comparison group both before and after follow-up. Older girls (age 16+ at baseline) had worked in 1.85 jobs compared with 1.26 jobs among the younger girls. Ethnicity and parity at baseline were not related to the total number of jobs held.

Only a minority of teens (26.2 percent) had never held a paying job, as shown in Table 5.4. Over one-fifth of the sample had already had three or more jobs at follow-up. Nearly twice as many comparison group teens (33.8 percent) as experimental group teens (18.0 percent) had never been employed.

As noted earlier, almost half of the unemployed teens unemployed at follow-up reported that they were seeking employment. Not all the teens who said they were job

TABLE 5.3

MEAN NUMBER OF JOBS HELD BEFORE AND AFTER BASELINE,

BY EXPERIMENTAL VERSUS COMPARISON GROUP

Work History	Mean Number of Jobs Held, by Group		
	Experimental	Comparison	Difference
Mean Number of Jobs Ever Held	1.8	1.3	0.5***
Mean Number of Jobs Held Prior to Baseline	1.1	0.8	0.3**
Mean Number of Jobs Held Subsequent to Baseline	0.7	0.5	0.2***
Total Number of Respondents	178	213	--

SOURCE: Tabulations are from AIR 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: **The unadjusted group difference is statistically significant at the .01 level.

***The unadjusted group difference is statistically significant at the .001 level.

TABLE 5.4

NUMBER OF PAYING JOBS EVER HELD AT FOLLOW-UP,

BY EXPERIMENTAL VERSUS COMPARISON GROUP

Number of Jobs Ever Held	Percentage Distribution of Teens, by Group		
	Experimental	Comparison	Difference
None	18.0	33.8	-15.8
One	27.5	29.6	- 2.1
Two	27.0	21.1	5.9
Three	15.2	9.9	5.3
Four or More	12.4	5.7	6.7
Total	100.0	100.0	0.0
Total Number of Respondents	178	213	--

SOURCE: Tabulations are from AIR 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: The percentages may not add to 100.0 percent due to rounding error.

The unadjusted group differences for this distribution are statistically significant at the .01 level.

hunting, however, were active in their search. Of those who were trying to find a job, some 71.1 percent had actually applied for one in the preceding month. Of those who had applied, about half had actually been given a job interview, and 19.5 percent had received a job offer. For the most part, teens relied on friends and relatives, the want ads, or signs in windows in their search for employment (Table 5.5). Some of the experimental group teens had turned to the Redirection program itself (12.8 percent).

Thus, despite their young age and despite the fact that this group has several characteristics associated with employment difficulties (e.g., minority group status, urban residency, and parenthood), the teens in this sample appear to have a positive orientation to the world of work and to have accumulated a sizeable amount of work experience. The experimental group teens appeared to have somewhat more job experience than the comparison group teens, an issue explored in greater depth in the next section.

B. Redirection Impacts on Labor Force Experience

In this section, Project Redirection's impact on the post-baseline work experiences of the teens in the sample is examined. The primary question is whether teens

TABLE 5.5

PERCENTAGE OF UNEMPLOYED TEENS USING VARIOUS METHODS TO FIND WORK, BY EXPERIMENTAL VERSUS COMPARISON GROUP

Method of Finding Work	Percentage of Teens, by Group		
	Experimental	Comparison	Difference
Employment Agency	12.8	25.0	-12.2
Reading Want Ads	52.3	51.3	1.0
Talking to Friends/ Relatives	43.0	36.3	6.7
Looking for Window Signs	33.7	35.0	- 1.3
Employment Program	8.1	10.0	- 1.9
Through Redirection	12.8	0.0	12.8
Direct Application to Employers	20.2	32.1	-11.9
Help from School	3.6	3.8	- 0.2
Other Method	8.3	3.8	4.5
Total Number of Respondents	86	80	--

SOURCE: Tabulations are from AIR 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: The percentages do not add to 100.0 percent because teens could use more than one method of job search.

None of the group differences is statistically significant at or beyond the .05 level.

accumulate more job experience and are more likely to be seeking employment if they have participated in Project Redirection.

It should again be noted that, for the 12-month follow-up interview, current employment was not considered a very meaningful outcome variable. While employment experience for these youths is desirable, actual employment during the academic year could conflict with school participation. School attendance can be assumed to be a better long-term investment for this group than working.

A second problem with using current employment as an outcome variable is that, as for all youths, employment among this sample tends to be unstable. Few of these teens were employed (16 percent of the sample), and most never held jobs longer than three months. Thus, responses given in the interview might differ depending on the specific time period covered. For these reasons, two other employment outcomes have been examined: post-baseline work experience and labor force participation at follow-up (defined as either current employment or current search for work).

Post-baseline work experience was defined as any paid employment subsequent to the baseline interview. Among the best predictors of such employment were pre-baseline work experience,[1] current pregnancy and school status, highest grade completed, and age of the teen's youngest child.[2] When these and other variables were statistically controlled, however, experimental group status was one of the strongest predictors of post-baseline employment. Participation in Redirection improved the likelihood of post-baseline employment to the point that 52 percent of the experimental group, as opposed to 40 percent of the comparison group, reported holding a job subsequent to baseline.

Two other measures of program participation were also examined: number of months enrolled[3] and having received

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[1]Several alternative indicators of baseline work experience were tested: number of jobs held at baseline, ever/never worked at baseline, number of weeks worked at baseline, and employed versus not employed at baseline. All were significantly related to post-baseline employment, and in all cases the Redirection participation variable was also significant.

[2]The logit analysis results for the full sample are shown in Table C.9 of Appendix C.

[3]As in the case of school-related impacts, we tested for the possibility of a curvilinear relationship between months of participation and employment. Our test suggests that the effects were linear.

job training on how to find a job, either through Project Redirection or through some other source. Both indicators of program participation were significantly related to paid employment in the preceding 12 months. However, job search training from a source other than Project Redirection was not significantly related to post-baseline employment.

As in the case of school outcomes, program impacts on employment were examined for various subgroups.[4] The results are summarized in Table 5.6, which shows that program participation had a very powerful effect on post-baseline employment among those who had never worked at the baseline interview. More than twice as many experimental as comparison teens (44 versus 18 percent) had gained their first employment experience during the 12-month follow-up period. Teens with pre-baseline work histories also had higher rates of post-baseline employment if they were in the experimental group, but this difference was not statistically significant.

Table 5.6 also summarizes the analyses performed for ethnic, age, and parity subgroups. In each case, participation in Project Redirection is associated with higher

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[4]The logit analyses for eight subgroups are presented in Tables C.10 to C-13 of Appendix C.

TABLE 5.6

ADJUSTED PERCENTAGES OF EXPERIMENTAL AND COMPARISON GROUP TEENS WITH POST-BASELINE EMPLOYMENT EXPERIENCE,[a] FOR TEENS IN VARIOUS SUBGROUPS

Subgroup	Adjusted Percentages of Teens, by Group		
	Experimental	Comparison	Difference
Teens With No Job Experience at Baseline (N=114)	44	18	26**
Teens With Some Job Experience at Baseline (N=277)	57	48	9
Black Teens (N=181)	52	46	6
Hispanic Teens (N=162)	56	35	21*
Teens Age 15 or Younger at Baseline (N=134)	49	37	12
Teens Age 16 or Older at Baseline (N=257)	54	42	12*
Teens Pregnant at Baseline (N=237)	47	39	8
Teens Not Pregnant at Baseline (N=154)	58	42	16*
All Teens (N=391)	52	40	12**

SOURCE: Tabulations are from AIR 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: The percentages shown have been adjusted for pre-baseline work experience, ethnicity, highest grade completed at baseline, pregnancy status at follow-up, school status at follow-up, and age of youngest child. The full logit analyses are presented in Table C.10 to C.13 in Appendix C.

[a]Post-baseline employment experience was defined as any paid employment subsequent to the baseline interview.

*Statistically significant at the .05 level for a two-tailed test.

**Statistically significant at the .01 level for a two-tailed test.

rates of post-baseline employment, even when pre-baseline employment and other background characteristics are controlled. Despite the relatively small number of teens in each subgroup, the experimental group advantage was statistically significant for three of the six subgroups: Hispanics, older teens, and teens not pregnant at baseline.

Again, when program impacts on labor force participation at follow-up were examined, three different measures of program participation were used: experimental versus comparison group status, length of enrollment, and receipt of training from the Redirection program on finding a job.[5]

The analyses revealed that a pregnancy at follow-up had especially strong negative effects on the teens' labor force participation, while pre-baseline work experience and baseline school enrollment had positive effects. Nevertheless, participation in Project Redirection continued to exert a significant positive influence on labor force participation at follow-up. Sixty percent of the experimental teens, but 52 percent of the comparison teens, were in the labor force at follow-up. The effect of

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[5]The logistic regression results for these three analyses are presented in Table C.14 of Appendix C.

having received training on how to find a job (from the program or from some other source) was especially pronounced.

When subgroup analyses were performed,[6] the most powerful program effects, shown in Table 5.7, were found among teens who had never held paying jobs prior to baseline: 61 percent of the experimental group, compared with 35 percent of the comparison group, were in the labor force at follow-up (after other key background variables were controlled). By contrast, the program had little effect on the labor force participation of teens who had worked prior to baseline.

For the other six subgroups, the results are mixed. Program effects on the labor force participation of blacks, older teens, and teens not pregnant at baseline were either nonexistent or negligible. Hispanic teens, on the other hand, were substantially affected by program participation: nearly 50 percent more of the Hispanic teens in the experimental group (62 percent) than in the comparison group (42 percent) were in the labor force at follow-up. Group differences were also found among teens pregnant at

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[6]The logit analyses for eight subgroups are presented in Tables C.15 to C.18 of Appendix C.

TABLE 5.7

ADJUSTED PERCENTAGES OF EXPERIMENTAL AND COMPARISON GROUP TEENS IN THE LABOR FORCE AT FOLLOW-UP[a] FOR TEENS IN VARIOUS SUBGROUPS

Subgroup	Adjusted Percentages of Teens, by Group		
	Experimental	Comparison	Difference
Teens With No Job Experience at Baseline (N=117)	61	35	26**
Teens With Some Job Experience at Baseline (N=283)	61	59	2
Black Teens (N=184)	63	63	0
Hispanic Teens (N=166)	62	42	20*
Teens Age 15 or Younger at Baseline (N=135)	63	50	13
Teens Age 16 or Older at Baseline (N=265)	58	55	3
Teens Pregnant at Baseline (N=237)	65	54	11+
Teens Not Pregnant at Baseline (N=158)	55	53	2
All Teens (N=393)	60	52	8+

SOURCE: Tabulations are from AIR 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: The percentages shown have been adjusted for pre-baseline work experience, age, ethnicity, school status at baseline, participation in a teen parent program at baseline, pregnancy status at follow-up, and age of youngest child. The full logit analyses are presented in Tables C.15 to C.18 in Appendix C.

[a]Labor force participation at follow-up was defined as either having a job or seeking employment at the time of the follow-up interview.

+Statistically significant at the .10 level for a two-tailed test.

*Statistically significant at the .05 level for a two-tailed test.

**Statistically significant at the .01 level for a two-tailed test.

baseline and among younger teens, though in the latter case the subgroup size was too small for the effect to be statistically significant at conventional levels.

In summary, even when work experience prior to baseline (as well as other characteristics) was standardized statistically, participation in Project Redirection was associated with improved post-baseline work experience and labor force participation. Program effects were especially marked in the case of teens with no prior work experience. Whether this effect reflects gains in knowledge about the world of work, increased job motivation as a result of program encouragement, or increased contact with potential employers via program personnel is a topic for further study.[7]

C. Redirection Impacts on Job Readiness

While employment prospects for youth are enhanced by early work experience, they presumably are also affected by

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[7]The possibility of selectivity bias still exists, although the statistical controls used in the regression analyses presumably controlled for such biases. We performed with the employment outcomes the 2-step selectivity analysis described in Appendix D, and obtained essentially the same results. Because the analyses were so similar, the results of this analysis are not presented.

the youths' knowledge about the world of work and through their orientation to work and career. Without information on job search, employer expectations, and procedures for job application, teens can have difficulty finding and keeping jobs. Unlike most teen parent programs, a component of the Redirection program is specifically designed to provide employability training and workshops. This section examines the extent to which the Redirection program had an impact on certain aspects of job readiness.

Two job readiness measures were administered to respondents at baseline and at follow-up, both part of a battery of employment-related tests prepared by the Educational Testing Service (ETS) for teenage students. The first is a 30-item measure of career maturity that taps decisiveness and personal planning about career choice.[8] Thirty is the highest possible (most "mature") score, and zero the lowest. At baseline, the mean score of the Redirection sample was comparable to the scores observed for other samples of minority youth.

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[8] Two typical items from this inventory include the following: "You shouldn't worry about choosing a job since you don't have anything to say about it anyway" and "Entering one job is about the same as entering another." For both items, agreement is scored as less "career mature" than disagreement.

The second ETS measure is a 17-item test of employability knowledge, which measures comprehension in such areas as completing a job application and reading want ads. Correctly answered items add one point to the respondent's test score, with scores ranging from 0 for no correct answers to 17 for all correct answers. At baseline, the mean score of the sample was one point lower than the mean score of the standardization sample for this instrument, a group, however, which was almost two years older than the Redirection research sample at baseline.

When adjustments were made for baseline test scores and other background characteristics, the program effects on the two job readiness measures were small.[9] On the Career Maturity Test, follow-up scores were, as expected, strongly related to baseline scores; however, participation in Project Redirection--regardless of how participation was defined--did not have a significant impact on follow-up scores. After adjusting for background factors, the mean experimental group score was 20.33, only about a quarter of a point higher than the comparison group mean of 20.04.

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[9] The linear regression results for these two tests are presented in Tables C.19 and C.20 of Appendix C.

The results for the Employability Knowledge Test were similar. Baseline test scores on the Knowledge and Career Maturity scale were both powerful predictors of follow-up test scores. Experimental group status and length of program participation were unrelated to follow-up performance. The adjusted mean follow-up scores were 11.41 for the comparison group and 11.50 for the experimental group.

The analyses do suggest, however, that certain types of job training experiences from Project Redirection might improve test scores. In particular, training from Project Redirection on how to decide on a job was found to be significantly related to performance on the Employability Knowledge Test (after other variables were controlled), while training from other sources was not. However, other types of job training offered by the program did not significantly improve test scores.

D. Summary and Conclusions

The majority of teens in the research sample had had some work experience by the time of the follow-up interview, although fewer than one in five were actually employed when interviewed. However, current employment

will be a more important outcome at the 24-month follow-up: the teens and their infants will be older and, presumably, more teens will have completed their basic schooling.

In this report, several indicators of job experience were considered to be investments in future employability. Both prior to and after baseline, teens in the experimental group had higher rates of employment than teens in the comparison group. Even when prior work experience was controlled, post-baseline employment was significantly higher for Redirection program participants, particularly those who had never been employed prior to enrollment. Program participation was associated with an estimated 26 percent impact on post-baseline employment among this group, even when other factors were controlled. The longer the time spent in Redirection, the stronger the programmatic effects. The results for labor force participation were similar in nature.

In contrast, participation in the Redirection program per se did not affect the teens' readiness for employment, as measured by tests of employability knowledge and career maturity. Nevertheless, those who participated in one certain type of employment training offered by the program--job decision-making--did perform better than those

who did not, and better than those who obtained such training from another source. The impact was not strong, but it was nonetheless noteworthy.

Chapter 6

Early Impacts on Family Planning and Health Variables

Many young mothers are hampered in making investments in their future earning power because of the responsibilities of parenthood. When these responsibilities increase as a result of an early repeat pregnancy, the handicaps can prove to be overwhelming. There is strong evidence that the negative consequences of teenage pregnancy, such as those found in the areas of education, employment and health, are even more severe among teens giving birth to more than one child (for example, Jekel et al., 1975; Furstenberg, 1976; Menken, 1975).

Unfortunately, early repeat pregnancies are not uncommon for teenage mothers. In a 1976 survey of a national sample of teenage women, Kantner and Zelnik found that 15 percent of the teens with a premarital pregnancy conceived again within a year; 30 percent did so within two years. Among those teens whose first pregnancy resulted in a live birth, 17.5 percent were pregnant again within 12 months (Zelnik, 1980). Broman (1978) reported, based on

data from the Collaborative Perinatal Project, that for white teenage parents aged 16 and 17 with a repeat pregnancy, the mean interval between the termination of one pregnancy and the onset of another was only 6.8 months. For those 15 or younger, the interval was 3.5 months.

In the baseline report for Project Redirection, (Polit et al., 1982), it was reported that nearly one-fourth of the teens had already had multiple pregnancies, although only about 6 percent of the sample were multiparous. Another 4 percent were expecting to deliver a second child. Only about half of the sample had ever used (or been with partners who used) any form of contraception.

Even among those with some birth control experience, contraceptive use tended to be inconsistent. Concern about side effects was the most frequently cited reason for non-use. Overall, the teens' knowledge of various contraceptive alternatives was limited, although they generally acknowledged that contraceptives were readily available. The baseline report concluded that, in the absence of any change in their behavior, a substantial proportion of the sample was at risk to a repeat pregnancy.

This chapter reports the results of the analyses on fertility and contraceptive behavior based on the 12-month follow-up data. While the 24-month follow-up interviews

are expected to yield more complete information about the incidence of repeat pregnancy, the matter is too important to ignore now despite the limited time frame. This analysis will take into account the entire sample's varying at-risk periods, since Zelnik et al. (1981) have concluded that the length of this period is a powerful determinant of premarital conception. However, it should be noted that because many of the Redirection teens in this study (almost half) were pregnant at baseline, their period at-risk to a subsequent pregnancy could be as short as three months. Nevertheless, Broman's data, cited above, suggest that, even given a brief at-risk period, repeat pregnancies are not uncommon.

This chapter follows the organization of the previous two chapters, first presenting an overview of the fertility and contraceptive experiences of the sample at follow-up, without making any statistical adjustments. Program impact on the incidence of repeat pregnancy is analyzed next, followed by an assessment of program impacts on contraceptive use and knowledge. Here, statistical controls were introduced to obtain more refined estimates of impact. The third section describes some health-related outcomes among teens pregnant at baseline, and the chapter then concludes with a summary.

A. Overview of Fertility and Contraception at Follow-Up

At the time of the follow-up interview, 11.3 percent of the teens acknowledged that they were pregnant again (9.7 percent in the experimental group, 12.6 percent in the comparison group). An additional 2.3 percent thought they might be pregnant but were not sure.

The percentage reporting a repeat pregnancy since baseline was even higher: a full 19.7 percent of the sample had become pregnant after the baseline interview (19.5 percent of the experimental group teens, 20.0 percent of the comparison group teens). Table 6.1 shows that 1.0 percent of the sample (two teens in each group) had become pregnant twice in the 12 months since baseline.

Of teens who were not currently pregnant, about one-third of those with a repeat pregnancy had terminated these pregnancies through abortions. Another third had had a miscarriage or a stillbirth. Teens pregnant at baseline were less likely than the other sample teens to have had a post-baseline pregnancy. Age and ethnicity were unrelated to the incidence of a repeat pregnancy.

Overall, the sample teens recorded a total of 1.4 pregnancies in their lifetimes. One-third of the sample had been pregnant on two or more occasions; nearly 6 percent had conceived three or more times by the time of

TABLE 6.1

NUMBER OF POST-BASELINE PREGNANCIES,

BY EXPERIMENTAL VERSUS COMPARISON GROUP

Number of Post-Baseline Pregnancies	Percentage Distribution of Teens, by Group		
	Experimental	Comparison	Difference
None	80.5	80.0	0.5
One	18.4	19.1	-0.7
Two	1.1	0.9	0.2
Total	100.0	100.0	0.0
Total Number of Respondents	185	215	--

SOURCE: Tabulations are from AIR 12-month follow-up interviews with Project Redirection participants and comparison group members.

NOTES: A teen was counted as having a post-baseline pregnancy if the beginning date of any of her pregnancies was subsequent to the date of her baseline interview.

The unadjusted group differences for this distribution were not statistically significant at or beyond the .05 level.

the follow-up interview. Teens aged 16 or younger at follow-up (77.8 percent) were more likely than older teens (61.6 percent) to have had only one pregnancy. Additionally, white (73.8 percent) and Hispanic (71.7 percent) teens were more likely than black teens (61.4 percent) to have had no more than one pregnancy.

Miscarriages and abortions were fairly common in the sample: 10.5 percent had had at least one abortion and 7.3 percent had experienced a miscarriage or stillbirth. While a higher proportion of older teens and black teens than younger and white or Hispanic teens had had an abortion, white teens (11.4 percent) were more likely than blacks (7.1 percent) or Hispanics (5.4 percent) to have had a miscarriage or stillbirth.

The total mean number of children born to these sample teens was 1.09 babies, with 1.12 born to teens 17 or older, compared to 1.03 born to the younger teens. There were no ethnic differences in the number of live births. The total number of pregnancies, abortions, miscarriages, and live births did not vary by experimental and comparison groups.

Substantially more teens at follow-up (82.5 percent) than at baseline (47.3 percent) had used some form of contraception at least once. A somewhat higher percentage of experimental group teens (87.0 percent) than comparison

group teens (79.1 percent) had used birth control on at least one occasion at follow-up. Group differences were especially marked for teens not using contraception prior to baseline. Among experimental group teens, 80 percent of those with no pre-program contraceptive practices had used some form of birth control at least once since baseline, compared to 67.9 percent of the comparison group teens.

Sexual activity increased the likelihood that a teen would try a contraceptive device (Table 6.2). Among the 299 teens who had had intercourse in the three-month period preceding the follow-up interview, 91.3 percent reported some contraceptive experience. Among the 99 teens reporting no recent sexual activity, only 57.6 percent had used contraception at least once. Teens pregnant at baseline (79.8 percent) were less likely than the other teens (87.3 percent) to have ever practiced some form of contraception.

Birth control pills were by far the most prevalent method used by both experimental and comparison groups, with 83.7 percent of the teens with any history of contraceptive use (Table 6.3) having tried them. The experimental group was somewhat more likely to have used an IUD, while comparison teens were somewhat more likely to have tried a diaphragm, but overall the behavior of the two groups was remarkably similar. On average, teens in the

TABLE 6.2

PERCENTAGE OF TEENS HAVING EVER USED CONTRACEPTIVES, BY SEXUAL ACTIVITY AT FOLLOW-UP AND BY EXPERIMENTAL VERSUS COMPARISON GROUP

User Category at Follow-Up	Sexually Active Teens[a]			Sexually Inactive Teens[b]		
	Percentage Distribution of Teens, by Group			Percentage Distribution of Teens, by Group		
	Experimental	Comparison	Difference	Experimental	Comparison	Differe
Never Used Any Contraception	5.6	11.5	-5.9	37.5	45.8	-8.3
Used Contraception at Least Once	94.4	88.5	5.9	62.5	54.2	8.3
Total	100.0	100.0	0.0	100.0	100.0	0.0
Total Number of Respondents	143	156	--	40	59	--

SOURCE: Tabulations are from AIR 12-month follow-up interviews with Project Redirection participants and comparison group members.

NOTES: The unadjusted group differences for these two distributions are statistically significant at or beyond the .05 level.

[a]A sexually active teen was defined as one who reported having had sexual intercourse within three months of the follow-up interview.

[b]A sexually inactive teen was defined as one who reported that her most recent sexual activity was more than three months prior to the follow-up interview.

TABLE 6.3

PERCENTAGE OF TEENS EVER HAVING USED VARIOUS CONTRACEPTIVES,

BY EXPERIMENTAL VERSUS COMPARISON GROUP

Type of Contraceptive	Percentage Having Used Method, by Group		
	Experimental	Comparison	Difference
Oral Contraceptives	83.0	84.4	-1.4
Condoms	39.0	37.3	1.2
Contraceptive Foam	23.3	22.6	0.7
Withdrawal	17.2	20.2	-3.0
Douching	17.2	15.2	2.0
IUD	19.0	13.3	5.7
Diaphragm	9.5	13.5	-4.0
Calendar Rhythm	5.1	3.7	1.4

SOURCE: Tabulations are from AIR 12-month follow-up interviews with Project Redirection participants and comparison group members.

NOTES: Use of a method includes both respondent's own use (e.g. oral contraceptives) or use by a partner (e.g. condom).

These data were obtained from 159 participants and 167 non-participants who had used at least one method of contraception.

The percentages do not add to 100.0 percent because some respondents had used more than one method.

None of the unadjusted group differences is statistically significant at or beyond the .05 level.

experimental group had tried 1.84 methods, compared with 1.67 methods for the comparison group, with no major differences by ethnic, age, or parity subgroup. This sample's preference of contraceptive methods is also comparable to that reported by teens nationally (Zelnik & Kantner, 1980).

While the majority of sample teens were familiar with at least one type of contraceptive, actual use tended to be inconsistent. Among those teens who were sexually active at follow-up, nearly 30 percent said they had not used any birth control at last intercourse. Table 6.4 shows the method used, if any. Comparable percentages of experimental and comparison group teens had used no contraception at all. A somewhat higher percentage of the experimental group than the comparison group had used the pill and IUD, the two most effective forms of birth control, but the differences were small. White teens (87.5 percent) were more likely than Hispanic (72.4 percent) or black (64.0 percent) teens to have used some form of birth control at last intercourse.

The right-hand panel of Table 6.4 presents figures estimating how effective each method actually is, as derived from data in several large-scale studies of contraceptive use (Hatcher et al., 1980). The index predicts how many women, on average, would become pregnant

TABLE 6.4

PERCENTAGE OF SEXUALLY ACTIVE TEENS USING VARIOUS METHODS OF CONTRACEPTION AT LAST INTERCOURSE, BY GROUP, AND USER EFFECTIVENESS OF EACH METHOD

Type of Contraception	Percentage of Teens Using Method, Last Intercourse		User Effectiveness Rating[a]
	Experimental	Comparison	
Oral Contraceptive	44.6	41.5	4
No Method	28.8	27.7	90
IUD	10.1	6.2	5
Diaphragm	2.2	9.2	17
Condom	5.8	3.4	10
Withdrawal	3.6	4.6	22
Contraceptive Foam	1.4	0.8	22
Douching	0.0	0.8	40
Combination	3.6	3.8	--[b]
Total	100.0	100.0	

SOURCE: Tabulations are from AIR 12-month follow-up interviews with Project Redirection participants and comparison group members.

NOTES: These data were obtained from 139 participants and 130 non-participants who had had intercourse within three months prior to the follow-up interview.

The unadjusted group differences for this distribution are not statistically significant at or beyond the .05 level.

[a]The index corresponds with the number of pregnancies expected over one year for 100 sexually active women using the specified method. Thus, lower scores represent more effective methods. For example, the effectiveness rating of oral contraceptives is 4; for no method it is 90 (Hatcher et al., 1980).

[b]No effectiveness rating can be given because different combinations were specified.

in one year, out of 100 women using the specified method. These numbers do not designate theoretical effectiveness (pills are virtually 100 percent effective if used properly); rather, the rates are based on the actual experiences of women, some of whom fail to use the method properly. Thus, in one year only four pill-users in 100 would be expected to become pregnant, while 90 sexually active non-contraceptors woul conceive.

Based on these ratings, an effectiveness score was assigned to each sexually active, nonpregnant teen according to the method last used. Assuming that current contraceptive patterns remained relatively constant, the analysis suggests that approximately 23 percent of the nonpregnant sexually active teens in the sample might become pregnant in the next 12 months.[1] This estimate is essentially the same for both experimental and comparison groups. However, white sexually active teens obtained better average effectiveness scores (16.7) than black (37.0) or Hispanic (31.5) teens.

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[1]The use-effectiveness scores in Table 6.4 are average rates based on samples of adult women using a given method over a one-year period, Therefore, the 23 percent projection for this population should be considered tentative. It may be inaccurate for various reasons. On the one hand, the figure might well be an underestimate: teens are less likely to use a method consistently and properly and they are more fertile than older woman. On the other hand, it might be an overestimate since the teens are less likely to have frequent and regular sexual contact than older, married women.

In summary, considerable improvement was found between baseline and follow-up in the number of teens using some contraceptive methods. Their use of a more effective method, such as birth control pills, was higher than that reported in national samples of sexually active teens (Zelnik & Kantner, 1980). Nevertheless, many teens continued to be at risk to a repeat pregnancy, and, in fact, one-fifth of the sample reported a repeat pregnancy in the 12 months since the baseline interview.

B. Redirection Impacts on Repeat Pregnancy and Contraceptive Use

The primary question addressed in this section is: Does participation in Project Redirection reduce the probability that these teens will experience an early repeat pregnancy? This outcome is of critical importance to the future lives of these young women.

In an analysis of early repeat pregnancies, an important factor is the amount of time a teen is at-risk to another pregnancy. As noted above, about half the sample was pregnant at the time of the initial interview, and in some cases, only a three-to-four month interval existed between the delivery of a teen's baby and the follow-up interview. On average, however, the teens in the sample were at-risk to a post-baseline conception some 364 days,

with the at-risk period defined in this analysis as the number of days from the termination of the most recent pre-baseline pregnancy to the date of the follow-up interview.

The analysis also included other factors that could potentially affect the teens' tendency to become pregnant: basic demographic variables, the teens' familial assets, the situation vis-a-vis a male partner at baseline, motivations for or alternatives to parenthood, and factors describing the teens' prior contraceptive and fertility history.[2]

The results indicated that the most powerful predictor of a repeat post-baseline pregnancy was the number of pre-baseline pregnancies. Apparently, one pregnancy was not always an effective deterrent to a second. Two or more early pregnancies, however, sharply reduced the liklihood of another pregnancy. As expected, the teens with the longest at-risk period were most susceptible, but the effect was surprisingly modest.

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[2]Two logit analyses were performed (see Table C.21). In the first analysis, Redirection participation is represented by a dichotomous variable (0 = comparison group member, 1 = experimental group member). In the second analysis, two dummy variables were entered into the equation to indicate whether the teen had obtained any post-baseline counseling in birth control either from Project Redirection or some other source.

An important finding in this report is that teens in the experimental group were significantly less likely to experience a post-baseline pregnancy than comparison group teens. When adjustments were made for period at risk, number of baseline pregnancies, baseline presence of a male in the household, school status and ethnicity, the percentage of teens with a post-baseline pregnancy was 16.8 percent for the experimental group and 22.4 percent for the comparison group.[3]

A second analysis indicated that teens who obtained birth control counseling directly from Redirection were somewhat less likely than others to become pregnant again, but this effect was not significant at conventional levels. However, obtaining birth control counseling from other sources (typically from a hospital or medical personnel) significantly reduced the probability of a conception within the 12-month period. Many experimental group teens obtained birth control counseling from other sources, and may have done so on referral from the Redirection program. Only about 10 percent of the teens received direct program counseling related to family planning.

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[3] This difference is statistically significant at the .05 level for a one-tailed test.

Table 6.5 summarizes the results of several additional analyses that compared subsequent pregnancy rates for the experimental and comparison groups in six subgroups. In most cases, group differences favored the Redirection participants, but these differences were generally too small to be significant given the sample size. The one exception was the subgroup of older teens: 26 percent of the comparison but only 17 percent of the experimental teens who were 16 or 17 years old at baseline had a follow-up pregnancy.

Program effects on several measures of contraceptive use were also examined, but the results were not encouraging. For example, an index was created that indicated adoption of a highly effective birth control method (the pill or IUD) in the post-baseline period. Both group differences and the overall relationshp were nonsignificant.[4] The effectiveness rating (see Table 6.4) of the birth control method used at last intercourse was also analyzed, with similar results.

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[4]In an additional analysis, a significant overall relationship was obtained when the dichotomous experimental/comparison variable was replaced with two variables: receipt of birth control counseling from Redirection and receipt of birth control counseling elsewhere. Both variables were significantly and positively related to adoption of a good form of contraceptive in the follow-up period ($p<.01$).

TABLE 6.5

ADJUSTED PERCENTAGES OF EXPERIMENTAL AND COMPARISON GROUP TEENS WITH A POST-BASELINE PREGNANCY,[a] FOR TEENS IN VARIOUS SUBGROUPS

Subgroup	Adjusted Percentages of Teens, by Group		
	Experimental	Comparison	Difference
Black Teens (N=183)	17	25	-8
Hispanic Teens (=164)	16	21	-5
Teens Age 15 or Younger at Baseline (N=132)	18	14	4
Teens Age 16 or Older at Baseline (N=259)	17	26	-9+
Teens Pregnant at Baseline (N=238)	12	17	-5
Teens Not Pregnant at Baseline (N=153)	26	30	-4
All Teens (N=391)	17	22	-5+

SOURCE: Tabulations are from AIR 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: The percentages shown have been adjusted for length of time at-risk to a subsequent pregnancy, number of baseline pregnancies, presence of a male in the household at baseline, use of contraception at baseline, baseline school status, and ethnicity.

[a]A post-baseline pregnancy was defined as any pregnancy that commenced after the date of the baseline interview.

+Statistically significant at the .10 level (.05 level for a one-tailed test).

In summary, Project Redirection's impact thus far in the area of fertility and contraceptive behavior appears to be modest. Having received birth control counseling from Redirection (or any other source) improved the likelihood that teens would begin to use an effective form of birth control, but experimental group status per se did not have much effect on contraceptive usage.

There were, however, significantly fewer repeat pregnancies in the experimental group than in the comparison group during the follow-up period--a difference of about 5.6 percentage points. Much of this effect appeared to be concentrated in the subgroup of older teens. It should be noted that, because many of these teens were recently pregnant when re-interviewed, the current data do not address the question of differences in live births. This outcome is one that can be more clearly examined in the 24-month follow-up period.

C. Redirection Impacts on Birth Control Knowledge

Teenagers who are well-informed about the availability of birth control and about the types of birth control options available to them are presumably better able to make informed decisions on contraceptive use. Project Redirection's family planning component is designed in part to provide contraceptive knowledge and counseling so that

such decisions get made and implemented. In this section, contraceptive knowledge is examined as it relates to participation in Project Redirection.

At baseline and at follow-up, teens were administered a 16-item true-false test of contraceptive knowledge.[5] Each item correctly answered added one point to the respondents' total scores, which could range from 0 (no correct answers) to 16 (all correct answers). At baseline, the mean score for the sample was 9.2; 12 months later the average score was 10.0.

In the program impact on analysis, it was found that teens with higher follow-up scores tended to be high scorers at baseline, and to have fewer pregnancies prior to baseline. They were more likely to be white, not pregnant at follow-up, and enrolled in a teen parent program at baseline. With these factors controlled, participation in Project Redirection was associated with an increase of about 1.1 points in the respondents' follow-up test scores, an effect statistically significant at the .001 level. Each month of program participation further added about one-tenth of a point.

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[5]Two typical items on this test (which was based on a test administered nationally by Kantner & Zelnik), are as follows: "Condoms can be obtained only from a doctor" and "An IUD can be purchased from a drugstore." Disagreement with both items is scored as the correct response.

The most dramatic effect stemmed from the teens' receipt of contraceptive counseling from the Redirection program. Teens who had received such counseling scored, on average, over two points higher than teens who did not. Birth control counseling from other sources also improved the teens' performance on the Birth Control Knowledge Test.

Program impact on test scores was also seen in all six subgroups based on ethnicity, age, and baseline parity, as shown in Table 6.6. At follow-up, in all six subgroups, experimental group teens scored one point higher than comparison group teens, even when baseline scores, number of baseline pregnancies, and other background chracteristics were statistically controlled. Black teens were especially likely to have improved their test scores after participating in the program.

In summary, these analyses suggest that even when a substantial number of background factors are controlled, including factors relating to a teen's early sexual experiences and birth control knowledge, Project Redirection is associated with significantly greater gains in the teens' understanding of birth control. These gains were particularly strong for those who obtained birth control counseling from the program.

TABLE 6.6

ADJUSTED MEAN FOLLOW-UP SCORES ON THE BIRTH CONTROL KNOWLEDGE TEST, BY EXPERIMENTAL VERSUS COMPARISON GROUP, FOR VARIOUS SUBGROUPS

Subgroup	Adjusted Mean Scores, by Group		
	Experimental	Comparison	Difference
Black Teens (N=181)	11.04	9.65	1.39**
Hispanic Teens (N=158)	9.77	8.66	1.11*
Teens Age 15 or Younger at Baseline (N=130)	10.40	9.45	.95+
Teens Age 16 or Older at Baseline (N=254)	10.62	9.58	1.04**
Teens Pregnant at Baseline (N=230)	10.52	9.46	1.06**
Teens Not Pregnant at Baseline (N=154)	10.72	9.58	1.14**
All Teens (N=384)	10.59	9.54	1.05***

SOURCE: Tabulations are from AIR 12-month follow-up interviews with experimental and comparison group members.

NOTES: The percentages shown have been adjusted for baseline scores on the Birth Control Knowledge Test, ethnicity, number of baseline pregnancies, presence of father in the household at baseline, enrollment in a teen parent program at baseline, and follow-up pregnancy status. The full logit analyses results are presented in Tables C.22 to C.25 in Appendix C.

+Statistically significant at the .10 level for a two-tailed test.

*Statistically significant at the .05 level for a two-tailed test.

**Statistically significant at the .01 level for a two-tailed test.

***Statistically significant at the .001 level for a two-tailed test.

D. Health Outcomes

There is widely documented evidence that young parents and their offspring are at greater risk to a variety of health problems than older mothers and their infants. One of the most serious medical problems associated with teenage pregnancy is the increased risk of prematurity and low-birthweight babies (Broman, 1978; Graham, 1981; Menken, 1975; Stickle, 1981). Infants born to teen mothers are also more likely than other babies to have low Apgar scores (Broman, 1978; Jones & Placek, 1979; NCHS, 1981), and to experience a higher-than-average number of health problems that sometimes persist in later years (Hollingsworth & Kotcher, 1981; Klerman & Jekel, 1973).

Poor diets and late or inadequate prenatal care have been suggested as contributing causes to these health problems (Bonham & Placek, 1978; Forbes, 1981; Menken, 1975; Carruth, 1978). National data, in fact, have shown that young women receive less prenatal care than older pregnant women. For example, a report from the National Center for Health Statistics (1979) indicated that only 3 percent of teens age 15 or younger obtained prenatal care in the first trimester, compared with 74 percent of all women.

In the present sample, the health status of these teens at baseline appeared to be somewhat better than average. Nearly three out of four of the pregnant teens reported having received medical care during the first three months of their pregnancy. The majority of mothers (82 percent) reported post-partum care within ten weeks of delivery. Few teens reported major long-term problems for either themselves or their infants. On the other hand, 15.3 percent of the babies born to these young mothers weighed under 5 1/2 pounds at birth, which is the normal cut-off point for low birthweight infants. This high percentage of low birthweight babies is consistent with national figures: 15 percent of non-white teen mothers, compared with 12 percent of non-white mothers aged 20-24, give birth to babies under 5 1/2 pounds, according to the National Center for Health Statistics (1980).

In the follow-up interview, teens pregnant at baseline were asked about the outcomes of their pregnancies. Although the data are not extensive and were gathered by self-report, without verification by medical reports, they have interest as general indicators of the sample's health.

The overwhelming majority (95.6 percent) of the 222 pregnant teens at baseline reported at follow-up that they had visited a doctor five or more times for prenatal care. Nearly 80 percent reported eight or more prenatal visits.

The amount of prenatal care was virtually identical for both the experimental and comparison group teens. There were no age or ethnic group differences.

These teens had spent an average of 4.1 days in the hospital during and after delivery, which is comparable to the 3.9 days reported by teen mothers at baseline. White teens had a shorter mean length of stay (3.1) than other teens, but experimental-comparison, ethnic and age group differences were not statistically significant.

The babies born to this group of young women weighed, on average, 7.0 pounds at birth. Only 9.3 percent of the babies weighed under 5 1/2 pounds, which is a substantially lower percentage than that reported by the teen mothers at baseline, and lower also than national percentages. A somewhat lower percentage of comparison teens (8.2 percent) than experimental teens (10.6 percent) had low birthweight infants, but this difference was not significant.[6] There

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[6]To further test group differences in relation to birthweight, a regression analysis was performed in which infant's birthweight was regressed on various background factors and participation in the Redirection program. Included in the regression model were such variables as amount and timing of prenatal care, household income, age, ethnicity, marital status, household composition, and number of prior pregnancies. The overall relation of these background characteristics and the program participation variable with the infant's birthweight was negligible: the highest adjusted R2 obtained was .01, and the overall model was not statistically significant. Thus, we conclude that there was no experimental/comparison difference in birthweight after controlling key background variables.

were also some differences (again nonsignificant) among the ethnic groups: none of the white mothers, compared with 11.0 percent of the Hispanic and 9.7 percent of the black mothers had had infants weighing under 5 1/2 pounds at birth.

Thus, the follow-up data suggest that the research sample as a whole is receiving adequate medical care and that health outcomes are reasonably good.

E. Summary and Conclusions

The majority of teens in the research sample were familiar with at least one form of birth control by the time of the follow-up interview and had had experience using either the IUD or birth control pills, two highly effective forms of contraception. Nevertheless, a substantial percentage of teens continued to be at-risk to a repeat pregnancy, either because they used no contraception or used it inconsistently. Further, it seems likely that at least some teens exaggerated the extent to which they practiced effective birth control. Even without any misreporting, the analysis suggests that about a quarter of the sexually active teens will be pregnant again in the next 12 months, unless there are changes either sexual activity or contraceptive practices. And, in fact, a disappointingly large number of teens--about 20 percent

of the sample--had become pregnant during the previous 12-month period.

Participation in Project Redirection was associated with several positive outcomes in the fertility and contraceptive area, although the effects were modest compared to those found for education or employment. Of particular importance was the finding that, with other factors controlled, Redirection teens were less likely than the nonparticipants to have had a repeat post-baseline pregnancy. The difference was small, but statistically significant; adjusted repeat pregnancy rates differed by about 5.6 percentage points, and even five to six fewer births per 100 teens in one year could result in substantial societal savings. The 24-month results will be useful in indicating whether these early advantages can be sustained or improved.

Other fertility-related outcomes were studied, with similar results. Birth control counseling from the program (and from other sources) increased the likelihood of teens' adopting an effective means of birth control, especially for non-users at baseline. Counseling also significantly improved birth control knowledge scores in the post baseline period.

Chapter 7

Lessons of Project Redirection

This report has examined some early evidence regarding program impacts of Project Redirection. The main question was: Is the program "redirecting" the lives of these young women in such a way that self-sufficiency in the future can be attained? We have postulated that, in order for these teens to work toward economic self-sufficiency, they must begin now to make "investments" in themselves. That is, they must begin to engage in behaviors that will result in a higher accumulation of human capital. The principal aim of the Redirection program is to assist teen mothers and mothers-to-be in making such investments in themselves. The mechanism for this assistance is the provision of a comprehensive array of services, some of which are delivered through an older woman from the teen's own community, the community woman. The first set of analyses in this report examined the extent to which Redirection participants were, in fact, being served by the program.

- Provision of Needed Services - On average, participants in Project Redirection had been in the program ten and a half months when re-interviewed. Over half the teens (56.8%) were still enrolled at follow-up. Some 71% of the experimental group teens described their involvement in the program as very or fairly active. Two of the most innovative components of the program -- the community women and employment training services -- were rated among the top three in terms of services the teens found helpful and enjoyable.

  During their period of program participation, the Redirection teens received substantially more services than their counterparts from matched sites. Significantly more experimental than comparison teens received such services as parenting classes (67% versus 36%); birth control counseling (76% versus 62%); educational counseling (37% versus 21%); tutoring for school work (11% versus 5%); nutrition education (56% versus 43%); and personal counseling (41% versus 17%). The differences were especially pronounced with respect to

employment-related training. For example, 51% of the experimental teens but only 20% of the comparison teens said they had received training on how to find a job in the preceding 12 months; 53% of the experimental and 28% of the comparison teens received training on how to act on a job interview. The data suggested that, in the absence of services received directly from the program, the experimental group would have had similar rates of service receipt as the comparison group. Thus, of 11 specific types of services, the experimental group had received 5.5 since the baseline interview, 2.2 of which were provided directly by the program; comparison teens had received only 3.9 different services during the same period. This difference was maintained even when service receipt at baseline was controlled.

It may seem obvious that the ability of a program to have positive impacts upon the lives of its clientele will be in part a function of the extent to which it actually delivers the services it has designed. Yet, this "obvious" fact may be one key to the difference between the relative success of Project Redirection in comparison with other

programs for pregnant and parenting teens.[1] To have an effect, services must not only be offered, they must also be utilized. Many programs designed for this target population have had trouble in both attracting and maintaining their clientele. Thus, the relatively long mean length of stay in the program, and the number of service components actually utilized by participants are important factors to consider when analyzing Project Redirection's success. Several analyses revealed that length of program enrollment had a more powerful impact than program participation per se on important outcomes, so the program's ability to retain these young women over a fairly long period appears to have had important consequences.

An earlier study by researchers at American Institutes for Research (Cannon-Bonventre and Kahn, 1979) revealed an interesting discrepancy between the services valued and sought by teenage parents, and these deemed important by service providers. In general, the young parents expressed

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[1]Evaluations of teen parent programs, like those of other programs, have generally failed to provide much evidence of program effectiveness. This is at least partially attributable to the fact that such evaluations have relied on small samples, inadequate outcome measures, or faulty designs (cf. Klerman, 1979). It might also be, however, that few teen parent programs represent as strong or comprehensive a treatment as the Redirection program.

a desire for "hard" services (e.g., money, jobs, housing, etc.) while providers saw this population as being in need of many "soft" services (e.g., various forms of counseling). Project Redirection offers both of these to its participants. It might be that the relative success of Project Redirection's soft services such as educational counseling, employment counseling, and personal counseling rests in part on its ability to retain participants in the program by satisfying their immediate perceived needs for more concrete help.

It seems plausible that the community woman is still a vital figure in creating a participant's sense that the program is offering her real help with day-to-day problems, rather than just another opportunity to talk. Responses to an open-ended question about the community woman suggest that this is the case. These comments indicate that the community woman is available to the participants to assist in looking for an apartment, locating household goods or baby clothes, and other practical matters of daily life.

Another important aspect of the design of Project Redirection is the number of services actually offered on site. Prior research, and common sense, indicate that young mothers are less likely to use an array of services

if using them requires multiple appointments, multiple contact people, and complicated transportation and scheduling. The services that Redirection participants valued most, and those that were less likely to have been received by comparison respondents, were largely services that the Redirection program offered directly rather than through referral (i.e., parenting education, the community woman, and employment training).

The delivery of important services to this needy target group is one form of evidence that the program is "working." A more important question, however, is whether the program is resulting in actual benefits to the participating teens. In this assessment of the early (12 month) impacts of Project Redirection, three broad categories of "investment" outcomes were examined: educational, employment-related, and family planning/health-related.

- Program Impacts on Investments. The analyses indicate that Project Redirection has been particularly successful in the educational arena. Substantially more of the experimental group teens (66%) than comparison group teens (50%) were either in an educational program at

follow-up or had completed one. This impact was observed for smaller subgroups of teens such as blacks, Hispanics, older teens, younger teens, teens pregnant at baseline, and teens who were parents at baseline. The effect was especially powerful among those who at baseline had dropped out of school. Among the dropouts, 49% of the experimental group and 20% of the comparison group were either in or had completed an educational program at follow-up.

In the area of employment, experimental group teens were significantly more likely than comparison group teens to have worked on at least one job in the 12 months since the baseline interview (52% versus 40%). Teens who had never been employed at baseline were especially likely to be positively affected by program participation. Among those with no early work record, 44% of the experimental group and 18% of the comparison group gained their first work experience in the preceding 12 months. It was also learned that labor force participation at follow-up (defined as being employed or looking

for employment at the time of the interview) was positively related to program participation, net of other background factors. Some 60% of the experimental group compared with 52% of the comparison group were in the labor force at follow-up. With respect to employment-related "enabling factors" (i.e., characteristics believed to be antecedent to investment behaviors, such as motivation or skill acquisition), teens who received certain job-training experiences through Project Redirection gained about a point on a test of employability knowledge relative to those without the training.

In the area of family planning, there was a modest, significant tendency for repeat pregnancies to be higher in the comparison group. After controlling for background characteristics, 16.8% of the experimental teens and 22.4% of the comparison teens had a repeat pregnancy in the post-baseline period. The adoption of an effective form of birth control in the preceding 12 months was found to be related to receiving birth control counseling from the program (or

from another source). Furthermore, participation in the program was associated with gains in birth control knowledge. The mean adjusted score on a 16-item Birth Control Knowledge Test was 10.6 for the experimental group and 9.5 for the comparison group. There were no program effects on the teens' receipt of medical care or on the baby's birth weight: teens in both groups appeared to be receiving adequate medical care and to have low rates of low-birth-weight babies relative to national norms.

One of the most noteworthy aspects of this impact assessment is the fact that the vast majority of the analyses suggest that the program has beneficial effects. Unlike the evaluations of many social programs, the results reported here consistently indicated positive program impacts. We did not have to "dig" to discover a glimmer of evidence supporting the success of the program. Evidence of success was to be found almost everywhere, even when sophisticated analyses were performed to rule out the possibility of selection bias.

Given that the long-term goal of Project Redirection is to help participants become self-sufficient adult women, the program's achievements in the areas of education and employment are most encouraging. These findings are important both for their face value, and for their broader implications. That is, in addition to the inherent value of attending school while one is still basically "school-aged," or being employed when jobs are difficult to find, these current behaviors potentially reflect the creation of habits and attitudes that will stand these women in good stead later in life. It appears that Project Redirection is helping its participants learn to manage the multiple roles of mother/student and/or mother/worker. Trends in employment and fertility indicate that whether married or single, these women can expect to be juggling such responsibilities for many years to come. The assumption of multiple rather than serial roles is essential to the goal of relatively early self-sufficiency.

As stated earlier, Project Redirection was only modestly successful in reducing the rate of rapid repeat pregnancies among participants. While a five percent reduction is somewhat disappointing, a few factors must be borne in mind when considering this finding. First, it is an improvement of about 25% over the non-program outcome.

On the national scale, this improvement would translate into an avoidance of thousands of unintended repeat pregnancies to teen mothers. Second, the matter of repeat pregnancies is of serious concern in large part because prior studies have indicated that a second birth to a teenage mother almost ensure a halt to her schooling and employment. Only the 24-month data will be able to tell us whether these adverse effects of early multiple births are mitigated by enrollment in Project Redirection.

Considering the relatively disappointing results in the area of repeat pregnancy and contraception, it might be advisable for program staff to devote more energy to this issue. However, it is unclear exactly what such a recommendation would imply. The literature to date, including these data, contradict the notion that birth control knowledge in and of itself leads to consistent, effective contraceptive use. Further, given the percentage of the sample who had used birth control at some point, it appears that access to contraceptives is also not a barrier. The issue appears to be finding a means to motivate sexually active adolescents to contracept effectively.

One might suggest that birth control counseling be undertaken as soon as possible after enrollment. Yet, such counseling probably needs to be more than information-sharing and encouragement. Given the data that correlate high educational and career aspirations with increased likelihood of using effective contraception, one strategy might be to focus on the teens' goals and the long-range payoffs of contraception. Further, it might be that in working with a population which by virtue of age, gender, and social class has experienced so little control over so many aspects of life, sensitive counselors may have to address the issue of control head-on. That is, the teens' failure to use effective contraception may be linked to a very central assumption that one cannot control one's life or avoid life's hazards. The community women may be able to play a vital role in reinforcing formal birth control counseling services by attacking this assumption.

In summary, based on the controls introduced in our research design and on the analytic strategies we have adopted, our conclusion is that Project Redirection positively affected service receipt, the development of motivations and knowledge in certain areas, and investment activities in the areas of schooling, employment, and subsequent pregnancy among its participants. In the next

report, which will analyze data collected 24 months after baseline, it will be possible to study whether these early impacts are sustained. And, because the sample will be nearly doubled, program impacts at the site level will also be studied.

APPENDIX A

Appendix A

Supplement to Chapter 2: Analytic Strategies

This appendix augments the discussion presented in Chapter 2 on the analytic methods used to assess the impacts of Project Redirection. Three analytic issues are discussed: the use of linear versus nonlinear models to estimate program impact; the use of alternative measures of program participation; and special procedures for handling selectivity bias.

A. Linear and Nonlinear Models

Multivariate statistical procedures are generally used to analyze data from quasi-experimental designs in which the initial non-equivalence of experimental and comparison groups is a major issue. Because the results produced by multivariate analysis may be sensitive to the particular statistical method employed, the proper determination of the most appropriate estimation technique for the purpose at hand is crucial to the validity of the results. The

statistical problem is to design an appropriate model that is capable of producing unbiased, efficient estimates of the effects of the program or of its various components.

The most widely used analytical technique in quasi experimental designs such as this one is the analysis of covariance (ANCOVA). ANCOVA is typically employed to adjust estimates of the treatment effect for known differences in the characteristics. The general ANCOVA regression model for the analysis can be stated:

$$(1) \quad Y_{ijt} = F\ (Z_m, X_{ni}, U_{ijt})$$

where

- Y = a vector of individual outcomes (i.e., return to school, scores on a Birth Control Knowledge Test, etc.)
- Z = a vector of dimensions or components of the program (e.g., the number of months spent in the program or the provision to the individual of career counseling services)
- X = a vector of personal characteristics (e.g., age, pregnancy history, school status at baseline, etc.)
- U = a vector corresponding to a stochastic disturbance or residual term

The subscripts denote that there are i individuals in the sample under consideration; j outcome measures in which we are interested; m dimensions of the program; n personal

characteristics; and t the time period (e.g., number of months) that has elapsed since the individual entered the program.

In this model, outcomes are posited to be a function of two major sets of variables: (1) predetermined factors or covariates; and (2) the effects of participating in the program. Covariates serve two important functions. First, they reduce error variance by attributing a portion of the variation in the dependent variable to exogenous factors. This decreases the standard error of the estimate, producing more efficient (or loosely speaking, precise) estimates of the treatment effects. Second, to the extent that selection differences are associated with specific exogenous variables, covariates will also reduce and possibly eliminate any selection bias present in the analysis.

A linear (ANCOVA) model is generally a useful first approximation at estimating program impacts. It is a relatively inexpensive procedure and permits experimentation in developing the best specification of a given relation. However, if the assumptions upon which the ANCOVA model is based are likely to be violated, the resulting estimates may be biased, inconsistent, and/or inefficient. In that case, either the necessary correc-

tions in the model must be made, or an alternative estimation technique to produce reliable estimates must be found and applied. In particular, maximum likelihood procedures are often substituted for ANCOVA to deal with problems of nonlinearity. Nonlinear maximum likelihood estimation techniques, such as logit or probit methods, are often appropriate, for example, when the measure of program outcome takes the form of a binary (dichotomous) variable.

The attraction of these nonlinear estimation techniques lies in the fact that they avoid two major statistical problems that arise in the use of the linear regression ANCOVA model in this situation. When the dependent variable is binary in nature, the error term will be heteroscedastic, resulting in unbiased but generally inefficient estimates of the parameters of the model, and in biased standard errors. An even more serious problem, however, is that the resulting coefficients may imply probability estimates for the dependent variable outside the zero to one range. Since probabilities are not defined outside that range, it is difficult to know how to interpret predictions that do not lie within its bounds. Trying to eliminate the problem by defining predictions of less than zero as equal to zero (or greater than one as equal to one) is not a uniformly acceptable strategy; in some

instances, it can produce an unreasonable clumping of predictions at either bound.

However, logit and probit methods are not without problems of their own. In addition to cost considerations, one rather unattractive feature has to do with the interpretation of the coefficients. Where coefficients obtained from the ANCOVA model have a simple interpretation (i.e., they indicate the effect on the dependent variable of a one-unit change in an independent variable holding other included variables constant), the interpretation of coefficients arising from the nonlinear forms are less straightforward. In particular, estimates of marginal probabilities obtained from such coefficients are dependent upon the mean values of all other covariates in the equation. While the linear regression coefficients provide an estimate of treatment effect that may apply to other samples, coefficients obtained from nonlinear estimation techniques cannot be directly interpreted in the same manner.

No estimation technique is, therefore, ideal. The proper choice of a method must take into account the specific objectives of a study and the constraints under which it operates. Information furnished in a recent paper

by Amemiya (1981) is quite useful in making a judgment in this matter. He demonstrates that, in most instances, logit and probit methods produce equivalent results. Pronounced differences between the methods appear only when the mean of the dependent variable lies near a boundary point (i.e., 0 or 1). More interestingly, he also shows that as long as the mean of the dependent variable lies within the 30 to 70 percent range, there is likely to be a clear and simple relationship between the coefficients (and hence resulting predictions) produced via the use of logit, probit, and the linear regression models. The conversion formula he presents indicates that the coefficient on a covariate produced by a linear regression is approximately equal to .4 and .25 times the corresponding coefficients produced via a probit and logit analysis, respectively.

Based upon all these considerations, we opted to pursue the following research strategy. When the dependent variable was binary and when its mean fell well within the 30 to 70 percent range, we devoted most of the available resources to applications of the linear regression model, in an attempt to pin down the best specification of a particular relationship. Nonlinear estimations were relied upon much more heavily in presenting the final regression estimates, but also used exclusively in the preliminary runs when the mean of the binary dependent variable lay

outside the 30 to 70 percent range.[1]

B. Measurement of Treatment Effects

The effectiveness of Project Redirection could be measured in various ways. The simplest method is to use a dummy variable to indicate whether an individual was a member of the experimental or comparison group. When coded in this manner, the resulting regression coefficient on this binary variable is an estimate of treatment effect; it represents the average difference[2] in the particular outcome measure for program participants relative to comparison group members after adjusting for individual differences.

While the simple dummy variable method offers a straightforward interpretation of the average program effect on an outcome measure, it does not take into account how much or which inputs of a program individual participants had been exposed to. It is plausible to expect individuals who were exposed only briefly to a program to benefit less than individuals who were exposed to that

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[1] Cost and other practical considerations led us to opt for the use of logit rather than probit analysis.

[2] In the case of logit analysis, it is a transformation of the average difference.

program longer. Therefore, the amount of time individuals spent in a program could be substituted instead of the simple dichotomous participation variable in the ANCOVA specification. The resulting coefficient on this time-in-program variable would then correspond to the change in outcome measure associated with one more unit of time of involvement.

There is, however, no strong a priori reason to impose the assumption that the effectiveness of a program is linear with respect to the amount of time spent in it. In particular, individuals who were enrolled for only a short period may have spent most of that time becoming oriented to the program. Later months might have been more productive in terms of impact on individual outcomes. But it might also be true that the marginal effectiveness of additional months spent in the program declined after some point--that is, after the participants were properly exposed to its most beneficial aspects. To test whether nonlinearities of this sort are present, both a linear (time enrolled) and second degree term (time enrolled squared) may be used on the right-hand side of the specification to measure program effect. Together, these terms are capable of approximating most kinds of nonlinearities that are likely to exist with respect to program effectiveness.

While the methods described above can provide an indication of program effectiveness, they cannot specify which features of a program work especially well and which do not. Certain features may be assessed, however, by employing additional dummy variables on the right-hand side of the specification to reflect whether subjects received particular services through the program or other agencies. For an examination of employment outcomes, for instance, one alternative is to add two additional dummy variables to the model-one to indicate first, whether a subject received employment training as part of the program and second, whether the subject reported receiving similar services from some other agency.

All three approaches were used to assess the early impacts of Project Redirection. Relevant statistical tables are presented in Appendix C. Tables for nonlinear effects of length of program enrollment are not included because in no case did the second degree term (time enrolled squared) prove to be statistically significant.

C. Selectivity Bias

Chapter 2 described several design strategies that were introduced to minimize the threat of selection bias.

Despite the research design, bas e differences were observed between the experimental and comparison groups, as shown in Table 2.1.

There are several possible approaches for dealing with this problem analytically. The most common is to use multivariate procedures such as ANCOVA to control baseline characteristics. A potential shortcoming of this approach, however, is that the available covariate information may not control for all relevant group differences contributing to (either program or self) selection bias. If certain relevant factors, such as entry-level aspirations, ability, or motivation, are not measured or included as covariates, the regression specification will result in only a partial adjustment for differences between groups. Remaining differences will be "forced" into the residual term, very likely violating the hypothesized characteristics of the distribution of that variable. Biased estimates will then be produced if the residual is correlated with program treatment, as would be the case if atypically motivated individuals were participating in the Redirection program.

There are several possible approaches for dealing with this problem. One is to develop and use some proxy to represent the aspiration, motivation, or capability

factors. A likely candidate is the baseline (pre-program) measure of the outcome under consideration. We would expect, for example, that girls employed at baseline would be more likely than other teens to be employed at follow-up, regardless of whether they were involved in Redirection or not; having a job at baseline is probably an indication of both the teen's employability and her motivation to seek out a job. This would likely be true even if typical job duration were short (i.e., even if the particular job at baseline did not last into the follow-up period). Standardizing for baseline employment represents an attempt to control for pre-program employability and motivation.

This is a relatively simple way of dealing with the selection problem, but it may not always represent the missing variables satisfactorily. Baseline measures may not conform purely to the "permanent" characteristic of interest, and may also be affected by "transitory" factors. For example, girls who were mothers at baseline might have dropped out of school to care for their young children. But among these young mothers, the desire or motivation to return to school at that point might have differed systematically, particularly between program participants and non-participants. If so, then controlling for school enrollment or absence at baseline would not necessarily standardize effectively for the school motivation factor.

A second, but not mutually exclusive, method of controlling for the motivational factor is also possible. Program participants and members of the control group were queri d at baseline about their educational apsirations, plan t› return to school, to seek employment, or become pregnant again. They were also presented with sets of questions on their knowledge of and use of birth control, career maturity, and employability knowledge. If these questions adequately captured remaining differences among individuals in motivation, attitudes and capabilities, then their inclusion on the right-hand side of the ANCOVA model would remove the complicating influence of important omitted factors.

The potential difficulty with this alternative lies in interpreting what additional information the responses to these questions actually convey. Attitudes and motivation are notoriously difficult to measure accurately. The questions may not be appropriately designed; they may be misinterpreted by the respondent; and--unlike more readily measurable characteristics such as schooling attainment--the responses are generally incapable of verification. In fact, it is possible that variables based upon these responses convey no more accurate information on attitudes or motivation than the more readily observable variables, and

may contain some misinformation as well (i.e., responses to questions might not correspond to the true state of the factor). This possibility does not imply that the approach should be abandoned. It does suggest, however, that statistical results from the use of these variables need to be interpreted with considerable care.

A third approach, again not mutually exclusive, attempts to capture motivational, attitudinal, or ability differences among individuals indirectly by moving beyond the single-equation regression specification. This alternative was recently developed by economists for investigating many aspects of individual behavior. It is a sophisticated statistical methodology involving an adjustment for selection bias by first modeling the selection process that segregates subjects into the treatment and comparison groups.

According to this approach, if unobserved variables, such as motivation or ability, affect both the outcomes of interest and the decision to participate in a program, then group status is potentially endogenous with behavioral outcomes. Since single-equation estimators will generally be biased and inconsistent in this case, a two-stage estimation procedure is necessary. First, the selection

process is modeled by performing a maximum likelihood logit or probit analysis of the relationship between the group status dummy variable and factors hypothesized to influence program participation. In the second stage, the first stage results are used to provide either an estimate of the probability that a subject will be in the treatment group (the Barnow et al. method), or a correction factor (the inverse of Mill's ratio) is constructed (the Heckman method). One of these is then inserted into the model's second equation,[3] thereby eliminating treatment effect bias attributable to the endogeneity of group status. Descriptions of this technique may be found in Heckman (1979) and Barnow, Cain and Goldberger (1980).

Estimates of treatment effect may then be derived by examining the resulting second stage coefficients. (Note that the second-stage regression is simply a modified version of the single equation model presented earlier.) The approach also allows for experimentation with the set of covariates incorporated into the model. To "identify" the model, we may divide the covariates into two separate (but probably nonexclusive) sets, using factors suspected

[3] In the Barnow et al. approach, the probability estimate replaces the dummy representing actual program enrollment; in the Heckman approach, the correction factor is included in addition to the dummy.

of influencing the decision to participate in the first set, and factors affecting outcomes in the second.

Although this procedure is attractive because it allows us to correct for the omission or improper measurement of certain variables that may be important in determining individual outcomes, it is not without its difficulties. For the procedure to be useful, it is necessary that the first-stage equation describe (i.e., predict) the selection process reasonably well. This is not always easy to achieve, regardless of how much experimentation takes place in preliminary analyses. If it cannot be achieved, resulting estimates of program effect that appear in the second stage are likely to be sensitive to the information incorporated from the first stage; i.e., the estimates are not "robust." If so, we can place little confidence in the second-stage estimates of program effect.

Because no single approach is without its limitations, we used all three methods in determining program effect. The purpose of this was not to provide a range of estimates, but rather to indicate which method (and which set of estimates) is best in this particular application. The results of the third technique are discussed in Appendix D.

Many of the analyses performed in connection with this report indicated strong program impacts. The impacts were especially likely to be observed in the case of behavioral measures such as school enrollment and work experience, and less likely to be observed on "softer" attitudinal or knowledge measures whose reliability is generally lower. The conclusion reached on the basis of the various analyses is that Project Redirection did, in fact, have impacts on several aspects of the teens' lives, and especially in the educational arena.

Despite the fact that these conclusions are not definitive, given the quasi-experimental nature of the design, the evidence in support of this position is, nevertheless, persuasive. The various approaches used to assess impact allow us to rule out or challenge competing explanations for the findings.

In a quasi-experimental design, the major threats to the internal validity of the study include selectivity, history, maturation, and mortality[4] (Campbell & Stanley, 1963). Maturation refers to changes over time resulting from developmental processes. Since the two groups were matched for both age and parity (and thus roughly for age

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[4]Mortality (attrition) is discussed in Appendix B.

of the children), differential maturation seems implausible.

The threat known as "history" refers to the occurrence of events external to and concurrent with the experimental treatment that can affect the outcome of interest. Examining Figure 4.1, for example, we must ask: is it plausible that something other than program enrollment occurred between Fall, 1980 and Spring, 1981 to bring about the observed shifts in school status? For example, school policies could change; other programs could be opened or shut down, and so forth. If the program had been implemented in one site only, with one matched comparison site, this possibility might bear close scrutiny. But the likelihood of such a history effect simultaneously occurring in four sites, at the same time as the implementation of Project Redirection, seems highly unlikely.

The remaining alternative explanation is that the groups were initially nonequivalent in potentially unknowable or unmeasurable ways, and that these pre-existing differences "caused" the differences at follow-up. In the regression analyses, we were able to control many of the known determinants of the outcome measures, and still program impacts were observed. The bulk of evidence ob-

tained from a supplementary analysis using a two-step procedure to correct for selectivity (Appendix D) suggested that whatever selectivity biases existed initially had been satisfactorily accounted for by the covariates used in the regression ar ,sis in which program participation was found to have a significant positive effect. Thus, we conclude that there were true program effects on educational, employment and subsequent pregnancy outcomes 12 months after enrollment.

APPENDIX B

Appendix B

Attrition in the 12-Month Follow-Up Sample

Overall, the response rate to the 12-month follow-up interview was reasonably high: 89.1 percent of the 449 teens in the baseline sample (excluding Detroit respondents) were re-interviewed 12 months after the initial interview. Given the characteristics of the sample (their youth, recent pregnancy, poverty, and their residence in primarily urban areas where a distrust of strangers is common), a 10 percent attrition rate can be considered low.

Nevertheless, a loss of 10 percent of the original sample could create a form of selection bias, that caused by differential (i.e., non-random) attrition. If teens who were highly disadvantaged were more likely to drop out of the experimental group than other teens, a positive selection bias could arise in the follow-up data if these were the teens "beyond the help" of program services. On the other hand, if the most disadvantaged teens were the

ones who would profit most from program participation, the same differential attrition could result in a negative selection bias. The converse argument could be made if attrition in the experimental group favored those who initially were least disadvantaged.

There were, in fact, sizeable group differences in the rates of attrition in the experimental and comparison groups. Seventeen percent of the participants, compared with 5 percent of the non-participants, were not re-interviewed, a difference significant at the .001 level. Nearly one-third of all non-interviews in the experimental group were refusals. On the other hand, in the comparison group refusals were rare. The primary reasons for non-interviews within that group were "unable to locate" (1.8 percent) and "moved out of the area" (1.8 percent).

This differential attrition rate could be explained in a number of ways. One possibility is that teens linked the interview effort directly to the Redirection program. That is, they may have viewed the AIR field team as being associated with Redirection staff. If this is true, they perhaps felt that they had made a break with the program (most non-interviewees were terminated) and no longer "owed" the program further cooperation. Or perhaps some

teens felt hostility toward the program if, for example, they were terminated against their wishes.

A second explanation is that comparison group teens, but not participants, were paid to complete interviews. Our initial rationale for paying non-participants was that we believed attrition would be higher in the comparison group, since non-participants would undoubtedly have little commitment to the research effort. The strategem may have worked in reverse.[1]

A third possibility is that different types of teens differentially self-selected themselves out of the experimental and comparison groups, thus creating the possibility of attrition bias mentioned above. We examined this possibility by comparing the follow-up and attrition samples on a range of important characteristics at baseline.

Comparisons were made both within the experimental and comparison groups and for the aggregated baseline sample. Table B.1 displays the results, broken down by the experimental and comparison group.

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[1]Field staff felt that the payment of a stipend to participants not enrolled in the program at follow-up would have greatly reduced the rate of refusals. If their impressions are valid, this second interpretation should be considered plausible.

TABLE B.1

Selected Baseline Characteristics of Teens Interviewed or Not Interviewed at Follow-Up, by Experimental Versus Comparison Group

Baseline Characteristic	Experimental Group		Comparison Group	
	Interviewed at Follow-Up	Not Interviewed at Follow-Up	Interviewed at Follow-Up	Not Interviewed at Follow-Up
Mean Age	15.9	15.8	15.9	15.9
Percent Never Married	95.1	89.5	87.9	100.0
Percent Hispanic	37.8	47.4	44.7	45.5
Percent Black	48.1	47.4	44.2	54.5
Percent Pregnant	58.4	65.8	52.3	71.8
Percent With Mothers Present	67.2	59.5	70	63.6
Percent With Both Parents Present	17.3	18.4	15.3	9.1
Percent on AFDC	73.4	84.2	73.0	90.0
Percent in School	51.4	50.0	59.3	54.5
Mean Highest Grade Completed	8.6	8.3	8.5	8.3
Percent Wanting More Than Diploma	46.6	44.1	33.5	40.0
Percent Employed	8.7	7.7	11.8	11.1
Mean Number of Jobs	1.3	1.2	1.1	1.0
Mean Number of Pregnancies	1.2	1.2	1.3	1.2
Percent Ever Used Birth Control	43.2	52.6	50.7	45.5
Mean Number of Services Used	5.6	6.2	6.1	6.9
Percent Having Been in Teen Parent Program	52.1	60.0	45.3	50.0
Total Number of Respondents	38	185	11	215

SOURCE: Tabulations are based on AIR baseline interviews with Project Redirection participants and comparison group members.

NOTES: For both the experimental and comparison groups separately and combined, none of the differences between the follow-up sample and the attrition sample is statistically significant at or beyond the .10 level.

Several observations about this table are in order. First, none of the differences between interviewees and non-interviewees even approaches statistical significance in either group, or for the two groups combined. The difference that comes closest is "highest grade completed" within the experimental group, which is associated with a significance level of .15. All other comparisons had probability levels higher than .20.

Nevertheless, the cell sizes, particularly for the comparison group, are generally so small that reaching statistical significance becomes difficult. If overall trends are observed, there does appear to be a tendency for the more disadvantaged teens not to participate in the follow-up interviews. The non-interviewees were somewhat more likely to have been on AFDC; were less likely to have lived in a household with the mother present; had attained a lower mean number of years of schooling; and were less likely to have been employed at baseline than the interviewees. On the other hand, there are some contrary tendencies. For example, re-interviewed teens were less likely to have been in a teen parent program, had received fewer services, and had had a higher mean number of pregnancies than those who were not re-interviewed. One further observation is especially important. With few

exceptions, the differences between interviewees and non-interviewees tended to run in the same direction for both the experimental and comparison groups. Thus, for all of the variables noted above indicating a greater degree of disadvantage among those not re-interviewed, a similar group difference was observed for both participants and non-participants. And, even when the two groups were combined, thereby increasing cell sizes, none of these differences were significant.[2] This suggests that, if there were attrition biases, they tended to be small and not to affect the experimental and comparison groups differently. We conclude, therefore, that estimates of program impact are unlikely to be affected by sample attrition.

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[2]Separate logistic regression analyses were performed for the comparison and experimental groups, using attrition status as the dependent variable and the variables in Table B.1 as the independent variables. In both analyses, none of the independent variables was statistically significant at the .10 level, and the overall equation also failed to reach statistical significance.

APPENDIX C

SUPPLEMENTARY TABLES

TABLE C.1

REGRESSION OF LENGTH OF PARTICIPATION IN PROJECT REDIRECTION AT FOLLOW-UP[a] ON BACKGROUND CHARACTERISTICS

Explanatory Variable[b,c]	Unstandardized Coefficient	Standard Error
Black	2.52***	.83
Hispanic	1.25	.84
Age	- .10	.26
Pregnant at Baseline	.74	.56
Number of Services Used at Baseline	.10	.08
Number of Services Needed at Baseline	- .10	.08
On AFDC at Baseline	.47	.60
Constant	10.46	
Adjusted R^2	.063	
Number of Respondents	180	

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental group members in Sample I.

NOTES: [a]Length of participation is measured in number of months.

[b]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[c]In preliminary analyses, the following additional explanatory variables were included in the model: number in household at baseline, presence of father or mother in household at baseline, number of siblings, number of baseline jobs, school status at baseline, educational aspirations at baseline, and number of children present at baseline. None of these variables was significant and their inclusion reduced the R^2 from .06 to .04.

***Two-tailed t-test is statistically significant at the .001 level.

TABLE C.2

REGRESSION OF NUMBER OF SERVICES USED AND NEEDED IN POST-BASELINE PERIOD[a] ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	Number of Services Used		Number of Services Needed	
	Unstandardized Coefficient	Standard Error	Unstandardized Coefficient	Standard Error
Age	- .27	.13	- .06	.10
Black	.21	.35	.25	.28
Hispanic	- .04	.34	.56*	.27
Mother Present in Household at Baseline	.11	.24	.01	.19
Both Parents Present in Household at Baseline	.36	.30	- .01	.24
Single	- .12	.40	- .62+	.32
Pregnant	- .11	.22	- .41*	.17
In School at Baseline	- .18	.24	- .34+	.19
Highest Grade Completed	.10	.11	- .16+	.09
Aspires to More Than Diploma/GED	- .18	.21	.09	.17
Family on Welfare/AFDC	.08	.24	- .17	.20
Household Income	.00	.00	- .00	.00
Number of Services Used at Baseline	.27***	.05	.04	.04
Number of Services Needed at Baseline	- .17	.05	.26***	.04
Ever Enrolled in a Teen Parent Program	- .24	.23	- .07	.18
Participated in Project Redirection	1.84***	.22	- .54**	.18
Constant	7.82		3.55	
Adjusted R^2	.22		.18	
Number of Respondents	378		378	

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: [a]Number of Services Used was the total of 11 specified services used by the teen in the preceding 12 months. Number of Services Needed was the total of the same 11 services not used but needed in the preceding 12 months.

[b]Participation in Project Redirection was coded 1 for the experimental group, 0 for the comparison group.

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

+Statistically significant at the .10 level.

*Statistically significant at the .05 level.

**Statistically significant at the .01 level.

***Statistically significant at the .001 level.

TABLE C.3

LOGISTIC REGRESSION OF SCHOOL STATUS AT FOLLOW-UP[a] ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	(1) Beta	(1) Standard Error	(2) Beta	(2) Standard Error
Age	- .22	.16	- .22*	.16
White	.72	.48	.87+	.48
Hispanic	- .51*	.29	- .39	.30
Age of Youngest Child	.00+	.00	.00+	.00
Mother Present in Household at Follow-Up	.68*	.31	.69*	.31
Other Females Present at Follow-Up	.56	.45	.62	.47
Husband/Boyfriend Present at Follow-Up	- .94*	.43	- .92*	.44
Mother's Education	- .22	.31	- .17	.31
Married at Follow-Up	.37	.54	.35	.54
Pregnant at Baseline	.07	.29	.13	.30
Pregnant Since Baseline	- .47	.39	- .37	.40
In School at Baseline	.96+	.59	.91	.60
Highest Grade Completed	.29+	.15	.30*	.15
Planning to Return to School, Dropouts	- .16	.52	- .24	.53
Absentee Rate from School, If in School at Baseline	- .05	.05	- .05	.06
Amount of Time Out of School, Dropouts	- .04*	.02	- .04*	.02
Ever Enrolled in Teen Parent Program	.22	.28	.19	.28
Participated in Project Redirection	1.00***	.29	--	--
Number of Months Participated in Project Redirection	--	--	.11***	.03
Constant	1.96		1.91	
D[e]	.238		.248	
Number of Respondents	362		362	

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: These analyses were based on data from those respondents who were not employed fulltime at follow-up.

[a]School status at follow-up was defined as either in school/completed school or GED (code 1) or not in school or completed (coded 0).

[b]Participation was examined in two ways. In analysis (1) participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members).

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

[e]The D statistic is a goodness-of-fit measure that is asymtotically equivalent to a standard R^2.

+Statistically significant at the .10 level.

*Statistically significant at the .05 level.

**Statistically significant at the .01 level.

***Statistically significant at the .001 level.

TABLE C.4

LOGISTIC REGRESSION OF SCHOOL STATUS AT FOLLOW-UP[a] ON TEENS IN AND OUT OF SCHOOL AT BASELINE ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	Teens In School at Baseline				Teens Not In School at Baseline			
	(1)		(2)		(1)		(2)	
	Beta	Standard Error	Beta	Standard Error	Beta	Standard Error	Beta	Standard Error
Age	- .23	.22	- .22	.22	- .09	.27	- .12	.28
White	1.16+	.69	1.24+	.69	.44	.80	.63	.81
Hispanic	- .85*	.40	- .80*	.40	- .07	.49	.14	.50
Age of Youngest Child	.00	.00	.00	.00	.00+	.00	.00	.00
Mother Present in Household at Follow-Up	1.15**	.42	1.16**	.42	.19	.51	.14	.51
Other Females Present at Follow-Up	1.12+	.68	1.12+	.69	.06	.72	.20	.72
Husband/Boyfriend Present at Follow-Up	- .83	.61	- .84	.62	-1.07+	.65	-1.06	.62
Mother's Education	- .37	.41	- .35	.41	- .16	.56	- .03	.56
Married at Follow-Up	1.07	.85	1.20	.86	.01	.77	- .12	.77
Pregnant at Baseline	- .22	.43	- .18	.43	.23	.48	.31	.48
Pregnant Since Baseline	- .26	.56	- .29	.56	- .88	.65	- .60	.67
Highest Grade Completed	.15	.21	.15	.21	.49*	.24	.46*	.24
Planning to Return to School, Dropouts	--	--	--	--	.09	.59	.08	.24
Absentee Rate from School, If in School at Baseline	- .05	.06	- .05	.06	--	--	--	--
Amount of Time Out of School, Dropouts	--	--	--	--	- .04+	.02	- .04+	.02
Ever Enrolled in Teen Parent Program	- .02	.35	- .01	.35	.48	.55	.37	.58
Participated in Project Redirection	.66+	.38	--	--	1.44**	.46	--	--
Number of Months Participated in Project Redirection	--	--	.08*	.03	--	--	.13***	.04
Constant	-3.60		3.29		-1.10		- .61	
D[e]	.153		.165		.236		.240	
Number of Respondents	219		219		143		143	

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: These analyses were based on data from those respondents who were not employed fulltime at follow-up.

[a]School status at follow-up was defined as either in school/completed school or GED (code 1) or not in school or completed (coded 0).

[b]Participation was examined in two ways. In analysis (1) participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members).

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

[e]The D statistic is a goodness-of-fit measure that is asymtotically equivalent to a standard R^2.

+Statistically significant at the .10 level.

*Statistically significant at the .05 level.

**Statistically significant at the .01 level.

***Statistically significant at the .001 level.

TABLE C.5

LOGISTIC REGRESSION OF SCHOOL STATUS AT FOLLOW-UP[a] FOR BLACK AND HISPANIC TEENS ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	Hispanic Teens				Black Teens			
	(1)		(2)		(1)		(2)	
	Beta	Standard Error	Beta	Standard Error	Beta	Standard Error	Beta	Standard Error
Age	- .09	.26	- .08	.26	- .43+	.27	- .47+	.27
Age of Youngest Child	.00	.00	.00	.00	.00+	.00	.00+	.00
Mother Present in Household at Follow-Up	.82+	.48	.81+	.48	1.07*	.54	1.07*	.53
Other Females Present at Follow-Up	-1.20	.95	-1.10	1.02	1.01	.69	1.00	.68
Husband/Boyfriend Present at Follow-Up	- .60	.62	- .59	.63	-1.89*	.89	-1.94*	.91
Mother's Education	.69	.77	.83	.75	- .32	.44	- .34	.45
Married at Follow-Up	.12	.80	.24	.80	.57	1.06	.55	1.07
Pregnant at Baseline	- .21	.55	- .19	.55	.52	.43	.60	.43
Pregnant Since Baseline	- .35	.58	- .33	.64	-1.07+	.62	- .99	.63
In School at Baseline	1.48+	.86	1.29	.87	-1.39	1.42	-1.34	1.43
Highest Grade Completed	.64**	.26	.64**	.26	- .02	.24	- .02	.24
Planning to Return to School, Dropouts	.37	.74	.26	.75	-2.37+	1.39	-2.50+	1.40
Absentee Rate from School, If in School at Baseline	- .15	.09	- .13	.09	.03	.09	.02	.09
Amount of Time Out of School, Dropouts	- .05+	.03	- .06	.03	- .04	.04	- .04	.04
Ever Enrolled in Teen Parent Program	.14	.50	.07	.50	- .41	.43	- .43	.43
Participated in Project Redirection	.90+	.51	--	--	.95*	.46	--	--
Number of Months Participated in Project Redirection	--	--	.10**	.04	--	--	.11**	.04
Constant	-1.57		-1.64		7.94		8.69	
D[e]	.285		.292		.248		.262	
Number of Respondents	147		147		175		175	

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: These analyses were based on data from those respondents who were not employed fulltime at follow-up.

[a]School status at follow-up was defined as either in school/completed school or GED (code 1) or not in school or completed (coded 0).

[b]Participation was examined in two ways. In analysis (1) participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members).

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

[e]The D statistic is a goodness-of-fit measure that is asymtotically equivalent to a standard R^2.

+Statistically significant at the .10 level.

*Statistically significant at the .05 level.

**Statistically significant at the .01 level.

***Statistically significant at the .001 level.

TABLE C.6

LOGISTIC REGRESSION OF SCHOOL STATUS AT FOLLOW-UP[a] FOR YOUNGER AND OLDER TEENS ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

	Teens Age 15 or Younger at Baseline				Teens Age 16 or Older at Baseline			
	(1)		(2)		(1)		(2)	
Explanatory Variable[c,d]	Beta	Standard Error	Beta	Standard Error	Beta	Standard Error	Beta	Standard Error
Age	- .24	.46	- .24	.47	- .69*	.34	- .69*	.34
White	- .89	1.08	- .70	1.11	1.21*	.60	1.37*	.61
Hispanic	1.46*	.65	-1.43*	.67	- .24	.36	- .08	.36
Age of Youngest Child	.01**	.00	.01**	.00	.00	.00	.00	.00
Mother Present in Household at Follow-Up	1.92**	.70	2.02**	.72	.52	.37	.47	.37
Other Females Present at Follow-Up	1.03	1.18	.95	1.23	.26	.53	.35	.53
Husband/Boyfriend Present at Follow-Up	-1.84*	.97	-1.55	.97	- .84	.53	- .92+	.54
Mother's Education	.41	.74	.44	.78	- .58	.38	- .51	.38
Married at Follow-Up	2.01	1.28	2.02	1.26	.07	.65	.09	.66
Pregnant at Baseline	.78	.70	.75	.71	.11	.35	.16	.35
Pregnant Since Baseline	- .24	.88	- .39	.93	- .47	.46	- .30	.47
In School at Baseline	.33	1.37	.29	1.40	1.06	.73	1.06	.74
Highest Grade Completed	.10	.34	.08	.36	.33+	.17	.36*	.18
Planning to Return to School, Dropouts	- .33	1.31	- .14	1.38	- .17	.61	- .20	.63
Absentee Rate from School, If in School at Baseline	- .04	.10	- .01	.10	- .07	.08	- .06	.08
Amounts of Time Out of School, Dropouts	- .09	.07	- .10	.07	- .04	.03	- .04	.03
Ever Enrolled in Teen Parent Program	1.16*	.68	1.30+	.70	.15	.34	.07	.34
Participated in Project Redirection	.94	.61	--	--	1.12**	.35	--	--
Number of Months Participated in Project Redirection	--	--	.14*	.06	--	--	.10***	.03
Constant	.16		- .14		9.84		9.88	
D[e]	.398		.410		.232		.240	
Number of Respondents	126		126		236		236	

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: These analyses were based on data from those respondents who were not employed fulltime at follow-up.

[a]School status at follow-up was defined as either in school/completed school or GED (code 1) or not in school or completed (coded 0).

[b]Participation was examined in two ways. In analysis (1) participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members).

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

[e]The D statistic is a goodness-of-fit measure that is asymtotically equivalent to a standard R^2.

+Statistically significant at the .10 level.

*Statistically significant at the .05 level.

**Statistically significant at the .01 level.

***Statistically significant at the .001 level.

TABLE C.7

LOGISTIC REGRESSION OF SCHOOL STATUS AT FOLLOW-UP[a] FOR TEENS PREGNANT OR NOT PREGNANT AT BASELINE ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	Teens Pregnant at Baseline				Teens Not Pregnant at Baseline			
	(1)		(2)		(1)		(2)	
	Beta	Standard Error	Beta	Standard Error	Beta	Standard Error	Beta	Standard Error
Age	- .20	.21	- .19	.22	- .49	.30	- .50	.30
White	.14	.66	.25	.66	1.52*	.79	1.69	.82
Hispanic	- .91*	.39	- .78*	.40	.17	.49	.23	.50
Age of Youngest Child	.00	.00	.00	.00	.00	.00	.00	.00
Mother Present in Household at Follow-Up	.64	.42	.70	.43	.91+	.48	.87+	.48
Other Females Present at Follow-Up	.50	.64	.70	.69	.45	.72	.38	.71
Husband/Boyfriend Present at Follow-Up	- .85	.57	- .82	.57	-1.59*	.76	-1.48*	.76
Mother's Education	- .25	.41	- .23	.41	- .50	.54	- .39	.54
Married at Follow-Up	.09	.68	.13	.70	1.34	.97	1.16	.98
Pregnant Since Baseline	- .20	.55	.04	.57	- .91	.63	- .83	.63
In School at Baseline	.67	.77	.68	.79	1.63	1.13	1.50	1.15
Highest Grade Completed	.31	.21	.33	.22	.35	.25	.35	.25
Planning to Return to School, Dropouts	- .02	.69	- .02	.71	- .34	.90	- .48	.93
Absentee Rate from School, If in School at Baseline	.09	.07	- .10	.08	- .05	.10	- .05	.10
Amount of Time Out of School, Dropouts	- .07*	.03	- .08*	.03	- .02	.03	- .02	.03
Ever Enrolled in Teen Parent Program	.79+	.40	.84*	.40	- .57	.48	- .63	.48
Participated in Project Redirection	1.17**	.40	--	--	1.38**	.47	--	--
Number of Months Participated in Project Redirection	--	--	.14***	.04	--	--	.12**	.04
Constant	1.81		1.58		5.92		6.12	
D[e]	.262		.279		.290		.291	
Number of Respondents	220		220		142		142	

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: These analyses were based on data from those respondents who were not employed fulltime at follow-up.

[a]School status at follow-up was defined as either in school/completed school or GED (code 1) or not in school or completed (coded 0).

[b]Participation was examined in two ways. In analysis (1) participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members).

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

[e]The D statistic is a goodness-of-fit measure that is asymtotically equivalent to a standard R^2.

+Statistically significant at the .10 level.

*Statistically significant at the .05 level.

**Statistically significant at the .01 level.

***Statistically significant at the .001 level.

TABLE C.8

LOGISTIC REGRESSION OF EDUCATIONAL ASPIRATIONS AT FOLLOW-UP[a] FOR TEENS WITH LOW AND HIGH ASPIRATIONS AT BASELINE ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	Teens Wanting a Diploma or Less at Baseline		Teens Wanting More Than a Diploma at Baseline	
	Beta	Standard Error	Beta	Standard Error
Age	- .20	.23	- .38	.34
White	- .24	.57	.91	.80
Hispanic	- .88*	.44	- .25	.53
Mother Present in Household at Follow-Up	.14	.43	-1.14*	.59
Other Females Present at Follow-Up	- .30	.87	- .55	.65
Husband/Boyfriend Present at Follow-Up	.03	.60	- .58	.86
Mother's Education	.65	.47	.20	.47
Married at Follow-Up	.01	.70	- .57	1.01
Pregnant at Baseline	- .41	.37	.10	.46
Pregnant at Follow-Up	.07	.59	- .67	.59
Number of Children at Follow-Up	- .47	.52	- .59	.62
In School at Baseline	- .42	.65	- .18	.78
Highest Grade Completed	.30*	.14	.49*	.25
Number of Semesters Repeated	- .33+	.18	.25	.22
Number of Semesters Skipped	- .29	.23	- .42	.30
Absentee Rate From School If in School	- .00	.07	- .12	.09
Amount of Time Out of School, Dropouts	.00	.03	- .03	.04
Ever Enrolled in Teen Parent Program	.04	.42	- .16	.48
Participated in Project Redirection[b]	.20	.38	.03	.46
Constant	-3.47		7.67	
D[e]	.157		.172	
Number of Respondents	216		134	

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: [a]Educational aspirations at Follow-Up were coded 1 if the teen wanted more than a high school diploma/GED, and 0 if she wanted less.

[b]Participation was coded 1 for experimental group teens, 0 for comparison group teens. In additional analyses not shown, the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members). The results were essentially the same as those presented above; participation did not approach statistical significance when other factors were controlled.

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

[e]The D statistic is a goodness-of-fit measure that is asymtotically equivalent to a standard R^2.

+Statistically significant at the .10 level.

*Statistically significant at the .05 level.

TABLE C.9

LOGISTIC REGRESSION OF POST-BASELINE EMPLOYMENT EXPERIENCE[a] ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	(1) Beta	(1) Standard Error	(2) Beta	(2) Standard Error	(3) Beta	(3) Standard Error
Age	- .11	.14	- .12	.14	- .13	.14
White	- .32	.38	- .26	.38	- .28	.39
Hispanic	.23	.27	.26	.27	.28	.28
Age of Youngest Child	.00*	.00	.00+	.00	.00*	.00
Mother Present in Household at Follow-Up	- .13	.26	- .14	.26	- .14	.26
Other Females Present at Follow-Up	- .30	.40	- .28	.40	- .23	.40
Mother's Education	.23	.27	.25	.27	.25	.27
Pregnant at Follow-Up	- .99**	.39	- .95*	.39	- .99**	.39
In School at Baseline	.52	.13	.50	.34	.52	.34
In School at Follow-Up	.76**	.25	.72**	.26	.77**	.25
Highest Grade Completed	.26*	.12	.28*	.12	.28*	.12
Amount of Time Out of School, Dropouts	- .01	.02	- .01	.02	- .00	.02
Employed Pre-Baseline	.83**	.27	.84**	.27	.82**	.27
Other Person in Household Working at Follow-Up	- .14	.23	- .16	.23	- .15	.24
Enrolled in Teen Parent Program at Baseline	- .49+	.29	- .51+	.28	- .48+	.28
Participated in Project Redirection	.58**	.23	--	--	--	--
Number of Months Participated in Project Redirection	--	--	.04*	.02	--	--
Received Training on How to Find a Job--Project Redirection	--	-	--	--	.87*	.42
Received Training on How to Find a Job--Elsewhere	--	--	--	--	.46	.30
Constant	- .78		- .46		- .40	
D[e]	.145		.140		.145	
Number of Respondents	387		385		387	

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: [a]Post-baseline employment experience was coded 1 if the teen had had any paid employment subsequent to the baseline interview, 0 if she did not.

[b]Participation was examined in three ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection, coded 0 for comparison group members. In analysis (3), a variable was included for receipt of training on how to find a job, coded 1 if received from Project Redirection, 0 otherwise. In the third analysis, another variable was added for receipt of such training elsewhere.

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

[e]The D statistic is a goodness-of-fit measure that is asymtotically equivalent to a standard R^2.

+Statistically significant at the .10 level.

*Statistically significant at the .05 level.

**Statistically significant at the .01 level.

***Statistically significant at the .001 level.

TABLE C.10

LOGISTIC REGRESSION OF POST-BASELINE EMPLOYMENT EXPERIENCE[a] FOR TEENS WITH AND WITHOUT PRE-BASELINE EMPLOYMENT EXPERIENCE ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	Teens With No Job Experience at Baseline		Teens With Some Job Experience at Baseline	
	Beta	Standard Error	Beta	Standard Error
Age	.02	.37	- .12	.16
White	.22	.82	- .65	.46
Hispanic	1.66*	.72	- .09	.31
Age of Youngest Child	.00	.00	.00	.00
Mother Present in Household at Follow-Up	.47	.56	- .34	.30
Other Females Present at Follow-Up	-1.07+	.97	- .16	.46
Mother's Education	1.28+	.71	- .02	.31
Pregnant at Follow-Up	-1.14	.96	-1.00*	.45
In School at Baseline	.97	.80	.55	.41
In School at Follow-Up	.37	.61	.96***	.29
Highest Grade Completed	.64+	.35	.20	.14
Amount of Time Out of School, Dropouts	.03	.04	- .02	.02
Other Person in Household Working at Follow-Up	.11	.58	.01	.27
Enrolled in Teen Parent Program at Baseline	-1.05*	.76	- .35	.33
Participated in Project Redirection	1.79**	.57	.43	.28
Constant	-5.82		.76	
D[e]	.245		.133	
Number of Respondents	110		277	

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: [a]Post-baseline employment experience was coded 1 if the teen had had any paid employment subsequent to the baseline interview, 0 if she did not.

[b]Participation in Project Redirection was coded 1 for the experimental group, 0 for the comparison group.

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

[e]The D statistic is a goodness-of-fit measure that is asymtotically equivalent to a standard R^2.

+Statistically significant at the .10 level.

*Statistically significant at the .05 level.

**Statistically significant at the .01 level.

***Statistically significant at the .001 level.

TABLE C.11

LOGISTIC REGRESSION OF POST-BASELINE EMPLOYMENT EXPERIENCE[a] FOR BLACK AND HISPANIC TEENS ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	Black Teens		Hispanic Teens	
	Beta	Standard Error	Beta	Standard Error
Age	.08	.24	- .32	.21
Age of Youngest Child	.00*	.00	.00*	.00
Mother Present in Household at Follow-Up	- .37	.44	- .59	.40
Other Females Present at Follow-Up	- .44	.56	-1.08	.82
Mother's Education	- .15	.38	.96	.66
Pregnant at Follow-Up	-1.71**	.62	- .89	.69
In School at Baseline	- .38	.62	.99+	.55
In School at Follow-Up	1.23**	.41	.59	.42
Highest Grade Completed	.27	.21	.32+	.18
Amount of Time Out of School, Dropouts	- .07	.04	- .01	.02
Employed Pre-Baseline	1.67***	.46	.29	.41
Other Person in Household Working at Follow-Up	- .19	.36	- .15	.39
Enrolled in Teen Parent Program at Baseline	- .55	.40	- .76	.55
Participated in Project Redirection	.33	.35	.96*	.41
Constant	-4.08		2.79	
D[e]	.223		.199	
Number of Respondents	184		161	

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: [a]Post-baseline employment experience was coded 1 if the teen had had any paid employment subsequent to the baseline interview, 0 if she did not.

[b]Participation in Project Redirection was coded 1 for the experimental group, 0 for the comparison group.

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

[e]The D statistic is a goodness-of-fit measure that is asymtotically equivalent to a standard R^2.

+Statistically significant at the .10 level.

*Statistically significant at the .05 level.

**Statistically significant at the .01 level.

***Statistically significant at the .001 level.

TABLE C.12

LOGISTIC REGRESSION OF POST-BASELINE EMPLOYMENT EXPERIENCE[a] FOR YOUNGER AND OLDER TEENS ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	Teens Age 15 or Younger at Baseline		Teens Age 16 or Older at Baseline	
	Beta	Standard Error	Beta	Standard Error
Age	.02	.36	- .18	.31
White	- .25	.87	- .45	.45
Hispanic	.62	.53	.02	.34
Age of Youngest Child	.00	.00	.00*	.00
Mother Present in Household at Follow-Up	.59	.53	- .27	.32
Other Females Present at Follow-Up	- .71	.86	.03	.49
Mother's Education	.93+	.56	.08	.34
Pregnant at Follow-Up	.08	.72	-1.45**	.52
In School at Baseline	- .02	.63	.61	.44
In School at Follow-Up	.65	.54	.86**	.32
Highest Grade Completed	.15	.21	.31*	.16
Amount of Time Out of School, Dropouts	.04	.03	- .03	.02
Employed Pre-Baseline	1.30**	.46	.70*	.36
Other Person in Household Working at Follow-Up	.36	.41	- .02	.30
Enrolled in Teen Parent Program at Baseline	.15	.53	- .74*	.36
Participated in Project Redirection	.47	.42	.62*	.30
Constant	-3.38		.49	
D[e]	.182		.188	
Number of Respondents	130		257	

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: [a]Post-baseline employment experience was coded 1 if the teen had had any paid employment subsequent to the baseline interview, 0 if she did not.

[b]Participation in Project Redirection was coded 1 for the experimental group, 0 for the comparison group.

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

[e]The D statistic is a goodness-of-fit measure that is asymtotically equivalent to a standard R^2.

+Statistically significant at the .10 level.

*Statistically significant at the .05 level.

**Statistically significant at the .01 level.

***Statistically significant at the .001 level.

TABLE C.13

LOGISTIC REGRESSION OF POST-BASELINE EMPLOYMENT EXPERIENCE[a] FOR TEENS PREGNANT OR NOT PREGNANT AT BASELINE ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	Teens Pregnant at Baseline		Teens Not Pregnant at Baseline	
	Beta	Standard Error	Beta	Standard Error
Age	- .21	.18	- .07	.27
White	- .17	.51	- .75	.63
Hispanic	.17	.37	.57+	.47
Age of Youngest Child	- .00	.00	.00+	.00
Mother Present in Household at Follow-Up	.17	.35	- .39	.44
Other Females Present at Follow-Up	.26	.53	-1.17+	.67
Mother's Education	.15	.34	.73	.51
Pregnant at Follow-Up	- .89	.56	-1.23*	.64
In School at Baseline	.53	.45	.20	.59
In School at Follow-Up	1.01**	.34	.54	.43
Highest Grade Completed	.51**	.17	- .04	.21
Amount of Time Out of School, Dropouts	.01	.02	- .05+	.03
Employed Pre-Baseline	.35	.35	1.66***	.46
Other Person in Household Working at Follow-Up	- .17	.30	- .11	.41
Enrolled in Teen Parent Program at Baseline	- .45	.35	- .38	.55
Participated in Project Redirection	.35	.30	.84*	.40
Constant	.57		- .60	
D[e]	.138		.248	
Number of Respondents	233		154	

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: [a]Post-baseline employment experience was coded 1 if the teen had had any paid employment subsequent to the baseline interview, 0 if she did not.

[b]Participation in Project Redirection was coded 1 for the experimental group, 0 for the comparison group.

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

[e]The D statistic is a goodness-of-fit measure that is asymtotically equivalent to a standard R^2.

+Statistically significant at the .10 level.

*Statistically significant at the .05 level.

**Statistically significant at the .01 level.

***Statistically significant at the .001 level.

TABLE C.14

LOGISTIC REGRESSION OF FOLLOW-UP LABOR-FORCE PARTICIPATION[a] ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	(1) Beta	(1) Standard Error	(2) Beta	(2) Standard Error	(3) Beta	(3) Standard Error
Age	.13+	.14	.12	.14	.08	.14
White	-.61+	.38	-.56	.38	-.53	.39
Hispanic	-.28+	.27	-.24	.27	-.30	.27
Age of Youngest Child	-.00+	.00	-.00*	.00	-.00+	.00
Mother Present in Household at Follow-Up	.41	.25	.40	.25	.42+	.26
Other Females Present at Follow-Up	.59	.42	.73+	.43	.87*	.43
Mother's Education	.08	.28	.09	.28	.10	.28
Pregnant at Follow-Up	-1.58***	.39	-1.54***	.39	-1.74***	.40
In School at Baseline	.78*	.34	.79*	.34	.82*	.35
In School at Follow-Up	.05	.26	.00	.26	-.10	.26
Highest Grade Completed	-.14	.12	-.14	.12	-.15	.12
Amount of Time Out of School, Dropouts	.01+	.02	.01+	.02	.01	.02
Employed Pre-Baseline	.46+	.25	.45+	.25	.45+	.25
Other Person in Household Working at Follow-Up	-.05	.23	-.07	.23	-.03	.24
Enrolled in Teen Parent Program at Baseline	-.50+	.29	-.53	.29	-.52	.29
Participated in Project Redirection	.38+	.23	--	--	--	--
Number of Months Participated in Project Redirection	--	--	.04*	.02	--	--
Received Training on How to Find a Job--Project Redirection	--	--	--	--	.99**	.35
Received Training on How to Find a Job--Elsewhere	--	--	--	--	.79**	.31
Constant	-1.95		-1.73		-1.21	
D[e]	.128		.126		.148	
Number of Respondents	387		385		387	

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: [a]Labor-force participation at follow-up was a dichotomous variable, coded 1 if the respondent was either working or if she reported looking for work, and 0 if otherwise.

[b]Participation was examined in three ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection, coded 0 for comparison group members. In analysis (3), a variable was included for receipt of training on how to find a job, coded 1 if received from Project Redirection, 0 otherwise. In the third analysis, another variable was added for receipt of such training elsewhere.

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

[e]The D statistic is a goodness-of-fit measure that is asymtotically equivalent to a standard R^2.

+Statistically significant at the .10 level.

*Statistically significant at the .05 level.

**Statistically significant at the .01 level.

***Statistically significant at the .001 level.

TABLE C.15

LOGISTIC REGRESSION OF FOLLOW-UP LABOR FORCE PARTICIPATION[a] FOR TEENS WITH AND WITHOUT PRE-BASELINE EMPLOYMENT EXPERIENCE ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	Teens With No Job Experience at Baseline		Teens With Some Job Experience at Baseline	
	Beta	Standard Error	Beta	Standard Error
Age	.55+	.33	.07	.16
White	- .25	.74	- .79+	.47
Hispanic	.33	.58	- .54+	.32
Age of Youngest Child	- .00*	.00	- .00	.00
Mother Present in Household at Follow-Up	- .02	.49	.57+	.31
Other Females Present at Follow-Up	.86	.82	.67	.52
Mother's Education	.39	.58	- .03	.33
Pregnant at Follow-Up	- .65+	.73	-1.92***	.48
In School at Baseline	1.23+	.71	.69+	.41
In School at Follow-Up	- .28	.55	.13	.30
Highest Grade Completed	- .35	.28	- .07	.14
Amount of Time Out of School, Dropouts	- .01	.03	.02	.02
Other Person in Household Working at Follow-Up	- .09	.50	.17	.28
Enrolled in Teen Parent Program at Baseline	- .52	.60	- .48	.35
Participated in Project Redirection	1.46**	.49	- .02	.28
Constant	-8.13		- .71	
D[e]	.203		.136	
Number of Respondents	110		277	

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: [a]Labor-force participation at follow-up was a dichotomous variable, coded 1 if the respondent was either working or if she reported looking for work, and 0 if otherwise.

[b]Participation in Project Redirection was coded 1 for the experimental group, 0 for the comparison group.

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

[e]The D statistic is a goodness-of-fit measure that is asymtotically equivalent to a standard R^2.

+Statistically significant at the .10 level.

*Statistically significant at the .05 level.

**Statistically significant at the .01 level.

***Statistically significant at the .001 level.

TABLE C.16

LOGISTIC REGRESSION OF FOLLOW-UP LABOR FORCE PARTICIPATION[a] FOR BLACK AND HISPANIC TEENS ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	Black Teens		Hispanic Teens	
	Beta	Standard Error	Beta	Standard Error
Age	- .04	.25	.23	.19
Age of Youngeat Child	- .00	.00	- .00	.00
Mother Present in Household at Follow-Up	.88*	.44	.33	.36
Other Females Present at Follow-Up	.72	.57	.51	.75
Mother's Education	- .06	.37	- .14	.62
Pregnant at Follow-Up	-2.11***	.56	-1.13*	.59
In School at Baseline	.83	.57	.91+	.52
In School at Follow-Up	- .02	.41	- .15	.40
Highest Grade Completed	.00	.22	- .18	.17
Amount of Time Out of School, Dropouts	.00	.04	.00	.02
Employed Pre-Baseline	.81*	.41	.20	.38
Other Person in Household Working at Follow-Up	- .50	.36	.24	.37
Enrolled in Teen Parent Program at Baseline	- .57	.41	- .66	.52
Participated in Project Redirection	- .04	.35	1.00**	.39
Constant	.19		-3.76	
D[e]	.161		.113	
Number of Respondents	184		161	

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: [a]Labor-force participation at follow-up was a dichotomous variable, coded 1 if the respondent was either working or if she reported looking for work, and 0 if otherwise.

[b]Participation in Project Redirection was coded 1 for the experimental group, 0 for the comparison group.

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

[e]The D statistic is a goodness-of-fit measure that is asymtotically equivalent to a standard R^2.

+Statistically significant at the .10 level.

*Statistically significant at the .05 level.

**Statistically significant at the .01 level.

***Statistically significant at the .001 level.

TABLE C.17

LOGISTIC REGRESSION OF FOLLOW-UP LABOR FORCE PARTICIPATION[a] FOR YOUNGER AND OLDER TEENS ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	Teens Age 15 or Younger at Baseline		Teens Age 16 or Older at Baseline	
	Beta	Standard Error	Beta	Standard Error
Age	- .24	.34	.43	.30
White	- .17	.87	- .64	.44
Hispanic	- .24	.52	- .20	.33
Age of Youngest Child	- .00+	.00	- .00	.00
Mother Present in Household at Follow-Up	.60	.50	.42	.31
Other Females Present at Follow-Up	1.40	.94	.56	.50
Mother's Education	.59	.56	- .15	.34
Pregnant at Follow-Up	-1.10	.75	-1.82***	.48
In School at Baseline	.74	.61	.75+	.43
In School at Follow-Up	.19	.53	.05	.32
Highest Grade Completed	.01	.21	- .24	.15
Amount of Time Out of School, Dropouts	.05	.03	- .01	.02
Employed Pre-Baseline	.68	.44	.45	.34
Other Person in Household Working at Follow-Up	- .19	.41	- .05	.30
Enrolled in Teen Parent Program at Baseline	- .67	.53	- .35	.36
Participated in Project Redirection	.60	.42	.24	.29
Constant	3.00		-6.36	
D[e]	.163		.153	
Number of Respondents	130		257	

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: [a]Labor-force participation at follow-up was a dichotomous variable, coded 1 if the respondent was either working or if she reported looking for work, and 0 if otherwise.

[b]Participation in Project Redirection was coded 1 for the experimental group, 0 for the comparison group.

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

[e]The D statistic is a goodness-of-fit measure that is asymtotically equivalent to a standard R^2.

+Statistically significant at the .10 level.

*Statistically significant at the .05 level.

**Statistically significant at the .01 level.

***Statistically significant at the .001 level.

TABLE C.18

LOGISTIC REGRESSION OF FOLLOW-UP LABOR FORCE PARTICIPATION[a] FOR TEENS PREGNANT OR NOT PREGNANT ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	Teens Pregnant at Baseline		Teens Not Pregnant at Baseline	
	Beta	Standard Error	Beta	Standard Error
Age	- .02	.17	.44+	.27
White	- .22	.53	-1.34*	.63
Hispanic	- .39	.37	- .16	.44
Age of Youngest Child	- .00	.00	- .00	.00
Mother Present in Household at Follow-Up	.49	.34	.54	.43
Other Females Present at Follow-Up	.89	.58	.77	.69
Mother's Education	.26	.36	.06	.48
Pregnant at Follow-Up	-1.73**	.55	-1.88**	.61
In School at Baseline	.65	.46	.86	.56
In School at Follow-Up	.14	.34	- .16	.43
Highest Grade Completed	- .01	.16	- .36+	.22
Amount of Time Out of School, Dropouts	- .02	.02	.03	.03
Employed Pre-Baseline	.21	.34	.92*	.41
Other Person in Household Working at Follow-Up	- .16	.31	.10	.39
Enrolled in Teen Parent Program at Follow-Up	- .76*	.38	.00	.54
Participated in Project Redirection	.57+	.31	.01	.39
Constant	.48		-6.64	
D[e]	.136		.197	
Number of Respondents	233		154	

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: [a]Labor-force participation at follow-up was a dichotomous variable, coded 1 if the respondent was either working or if she reported looking for work, and 0 if otherwise.

[b]Participation in Project Redirection was coded 1 for the experimental group, 0 for the comparison group.

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

[e]The D statistic is a goodness-of-fit measure that is asymtotically equivalent to a standard R^2.

+Statistically significant at the .10 level.

*Statistically significant at the .05 level.

**Statistically significant at the .01 level.

***Statistically significant at the .001 level.

TABLE C.19

REGRESSION OF FOLLOW-UP CAREER MATURITY SCORES[a] ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	(1) Unstandardized Coefficient	(1) Standard Error	(2) Unstandardized Coefficient	(2) Standard Error	(3) Unstandardized Coefficient	(3) Standard Error
Age	- .15	.24	- .16	.24	- .17	.24
White	1.71*	.68	1.72*	.68	1.71*	.68
Hispanic	.48	.42	.47	.43	.48	.43
Mother Present in Household at Baseline	.38	.43	.37	.43	.36	.44
Father Present in Household at Baseline	.64	.51	.67	.51	.67	.51
Single	-1.17	.71	-1.12	.71	-1.10	.70
Pregnant	.16	.39	.16	.39	.15	.39
In School at Baseline	.32	.43	.30	.43	.29	.43
Highest Grade Completed	.54**	.21	.55**	.21	.55**	.21
Number of Jobs at Baseline	- .01	.18	.02	.18	.02	.18
Employed Since Baseline	.31	.42	.33	.42	.34	.42
Household Income	- .00	.00	- .00	.00	- .00	.00
Career Maturity Scores at Baseline	.51***	.05	.51***	.05	.51***	.05
Employability Knowledge Scores at Baseline	.27***	.07	.27***	.07	.27***	.07
Ever Enrolled in a Teen Parent Program at Baseline	- .02	.42	- .05	.41	- .06	.41
Participated in Project Redirection	.28	.41	--	--	--	--
Number of Months Participated in Project Redirection	--	--	.01	.03	--	--
Received Training on How to Decide on a Job from Project Redirection	--	--	--	--	.19	.72
Received Training on How to Decide on a Job--Elsewhere	--	--	--	--	.14	.50
Constant	8.30		8.55		8.66	
R^2	.50		.50		.49	
Number of Respondents	355		355		355	

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: [a]The Career Maturity Inventory consists of 30 items each of which is scored as 1 point if it is answered correctly. Higher scores reflect greater career maturity.

[b]Participation was examined in three ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members). In analysis (3), a variable was included for receipt of training on how to decide on a job, coded 1 if received from Project Redirection, 0 otherwise. In the third analysis, another variable was added for receipt of such training elsewhere.

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

+Statistically significant at the .10 level.

*Statistically significant at the .05 level.

**Statistically significant at the .01 level.

***Statistically significant at the .001 level.

TABLE C.20

REGRESSION OF FOLLOW-UP EMPLOYABILITY KNOWLEDGE TEST SCORES[a] ON BASELINE CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	(1) Unstandardized Coefficient	(1) Standard Error	(2) Unstandardized Coefficient	(2) Standard Error	(3) Unstandardized Coefficient	(3) Standard Error	(4) Unstandardized Coefficient	(4) Standard Error
Age	- .45**	.17	- .46**	.17	- .45**	.17	- .43*	.17
White	- .35	.48	- .35	.49	- .33	.48	- .34	.48
Hispanic	- .15	.30	- .16	.30	- .14	.31	- .15	.30
Mother Present in Household at Baseline	.42	.31	.42	.31	.50	.31	.46	.31
Father Present in Household at Baseline	.43	.36	.44	.36	.38	.36	.40	.36
Single	.10	.51	.13	.51	.08	.50	.12	.50
Pregnant	- .15	.28	- .16	.28	- .16	.28	- .19	.28
In School at Baseline	- .05	.31	- .06	.31	- .05	.30	.02	.31
Highest Grade Completed	.38**	.15	.39**	.14	.37*	.15	.36	.15
Number of Jobs at Baseline	.05	.13	.06	.13	.03	.13	.06	.13
Employed Since Baseline	.28	.30	.30	.30	.27	.30	.28	.30
Household Income	- .00	.00	- .00	.00	- .00	.00	- .00	.00
Career Maturity Scores at Baseline	.20***	.03	.20***	.03	.20***	.03	.20***	.03
Employability Knowledge Scores at Baseline	.44***	.05	.44***	.05	.44***	.05	.44***	.05
Ever Enrolled in a Teen Parent Program at Baseline	.67*	.30	.65*	.30	.69*	.29	.71*	.30
Participated in Project Redirection	.09	.29	--	--	--	--	--	--
Number of Months Participated in Project Redirection	--	--	.00	.02	--	--	--	--
Received Training on How to Decide on a Job--Project Redirection	--	--	--	--	.84+	.51	--	--
Received Training on How to Decide on a Job--Elsewhere	--	--	--	--	- .15	.36	--	--
Received Training on How to Apply for Job--Project Redirection	--	--	--	--	--	--	- .53	.43
Received Training on How to Apply for Job--Elsewhere	--	--	--	--	--	--	- .22	.31
Constant	9.03		9.18		8.92		8.71	
R^2	.46		.46		.46		.46	
Number of Respondents	355		355		355		355	

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: [a]The Employability Knowledge Test consists of 17 items, each of which is scored as 1 point if it is answered correctly. Higher scores reflect greater knowledge.

[b]Participation was examined in four ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members). In the last two analyses, two types of post-baseline training experience were examined: training on how to decide on a job (column 3) and training on how to apply for a job. In these two analyses, two dummy variables were created indicating receipt of training from Project Redirection (coded 1 or 0) or from some other source (coded 1 or 0).

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

+Statistically significant at the .10 level.

*Statistically significant at the .05 level.

**Statistically significant at the .01 level.

***Statistically significant at the .001 level.

TABLE C.21

LOGISTIC REGRESSION OF A POST-BASELINE REPEAT PREGNANCY[a] ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable	(1) Beta	(1) Standard Error	(2) Beta	(2) Standard Error
Married at Baseline[c]	- .53	.78	- .11	.78
Age at Baseline	.28	.27	.28	.27
Mother Present in Household at Baseline	.53	.55	.58	.54
Husband/Boyfriend Present in Household at Baseline	1.02	.71	.93	.69
White	.41	.70	.30	.73
Hispanic	- .44	.54	- .53	.54
In School at Baseline	- .62	.53	- .37	.52
Highest Grade Completed	.04	.15	.05	.15
Employed at Baseline	- .86	1.14	- .55	1.14
Number of Pregnancies at Baseline	-5.92***	.75	-6.12***	.78
Ever Used Contraception at Baseline	- .55	.51	- .36	.53
Days at Risk to a Post-Baseline Pregnancy	.00*	.00	.00*	.00
Sexually Active at Baseline	.94+	.54	1.04*	.54
Ever Enrolled in Teen Parent Program at Baseline	- .83	.66	- .90	.66
Participated in Project Redirection	- .81+	.49	--	--
Received Birth Control Counseling from Project Redirection	--	--	- .75	.99
Received Birth Control Counseling Elsewhere Since Baseline	--	--	-1.26*	.51
Constant	-1.82		-1.45	
D[d]	.383		.387	
Number of Respondents	389		389	

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with Project Redirection participants and comparison group members.

NOTES: [a]The outcome variable was coded 1 if the teen <u>became</u> pregnant at any time after her baseline interview, 0 otherwise.

[b]Participation was examined two ways. In analysis (1), participation was coded 1 for all participants, 0 for nonparticipants. In analysis (2), we included a variable for receipt of counseling on birth control from the program, coded 1 if received from Project Redirection, 0 otherwise. In the second analysis, another variable was added for receipt of such counseling elsewhere during the follow-up period.

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]The D statistic is a goodness-of-fit measure that is asymtotically equivalent to a standard R^2.

+A two-tailed chi-square is statistically significant at the .10 level.

*A two-tailed chi-square is statistically significant at the .05 level.

**A two-tailed chi-square is statistically significant at the .01 level.

***A two-tailed chi-square is statistically significant at the .001 level.

TABLE C.22

REGRESSION OF FOLLOW-UP BIRTH CONTROL KNOWLEDGE TEST SCORES[a] ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	(1) Unstandardized coefficient	(1) Standard Error	(2) Unstandardized coefficient	(2) Standard Error	(3) Unstandardized coefficient	(3) Standard Error
Married	.19	.49	.25	.49	.26	.49
Age	-.04	.17	-.07	.17	-.00	.17
White	1.06*	.46	1.15*	.46	1.02*	.45
Hispanic	.03	.33	.06	.33	.10	.33
Mother Present in Household at Baseline	.39+	.30	.41	.30	.35	.29
Father Present in Household at Baseline	.60+	.35	.63+	.35	.65*	.34
Mother's Education	.07	.08	.08	.08	.09	.08
In School at Baseline	.17	.29	.10	.29	.10	.29
Highest Grade Completed	.13	.14	-.15	.14	.11	.14
Pregnant at Baseline	.13	.30	.17	.30	-.05	.29
Pregnant at Follow-Up	-.60*	.31	-.60*	.31	-.48	.31
Number of Pregnancies	-.52+	.31	-.50	.31	-.63*	.31
Used Contraception at Baseline	.23	.30	.23	.30	.13	.29
Frequency of Sexual Intercourse at Follow-Up	.01	.08	.02	.08	-.03	.08
Baseline Score on Birth Control Knowledge Test	.52***	.05	.52***	.05	.05***	.05
Ever Enrolled in Teen Parent Program at Baseline	.75**	.30	.71*	.30	.75*	.30
Participated in Project Redirection	1.05***	.27	--	--	--	--
Number of Months Participated in Project Redirection	--	--	.09***	.02	--	--
Received Birth Control Counseling--Project Redirection	--	--	--	--	2.11***	.48
Received Birth Control Counseling Elsewhsre Since Baseline	--	--	--	--	1.39***	.25
Constant	4.04		4.31		3.04	
R^2	.39		.38		.41	
Number of Respondents	376		376		376	

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: [a]The Birth Control Knowledge Test is a 16-item test designed to measure knowledge about various contraceptive methods and risk of pregnancy. Scores could range from 0 (no correct answers) to 16 (all correct answers).

[b]Participation was examined in three ways. In analysis (1), participation was coded 1 for experimental group teens, 0 for comparison group teens. In analysis (2), the participation variable was number of months enrolled in Project Redirection (coded 0 for comparison group members). In analysis (3), a variable for receipt of birth control counseling from the program was included, coded 1 if received from Project Redirection, 0 otherwise. In the third analysis, another variable was added for receipt of such counseling elsewhere during the follow-up period.

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

+Statistically significant at the .10 level.

*Statistically significant at the .05 level.

**Statistically significant at the .01 level.

***Statistically significant at the .001 level.

TABLE C.23

REGRESSION OF FOLLOW-UP BIRTH CONTROL KNOWLEDGE TEST SCORES[a] FOR BLACK AND HISPANIC TEENS ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory variable[c,d]	Black Teens		Hispanic Teens	
	Unstandardized Coefficient	Standard Error	Unstandardized Coefficient	Standard Error
Married	1.03	1.24	- .34	.64
Age	.14	.28	.01	.27
White	--	--	--	--
Hispanic	--	--	--	--
Mother Present in Household at Baseline	.34	.50	.45	.48
Father Preeent in Household at Baseline	.43	.56	.74	.63
Mother's Education	.15	.12	- .03	.14
In School at Baseline	.21	.47	.28	.49
Highest Grade Completed	- .04	.25	.06	.22
Pregnant at Baseline	.16	.46	.23	.57
Pregnant at Follow-Up	- .47	.48	- .52	.55
Number of Pregnancies	- .52	.49	- .98+	.53
Used Contraception at Baseline	.09	.44	.49	.56
Frequency of Sexual Intercourse at Follow-Up	.07	.14	.09	.13
Baseline Score on Birth Control Knowledge Test	.50***	.07	.55***	.08
Ever Enrolled in Teen Parent Program at Baseline	.59	.45	.98+	.57
Participated in Project Redirection	1.40**	.43	1.11*	.46
Constant	- .41		4.40	
R^2	.25		.33	
Number of Respondents	178		154	

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: [a]The Birth Control Knowledge Test is a 16-item test designed to measure knowledge about various contraceptive methods and risk of pregnancy. Scores could range from 0 (no correct answers) to 16 (all correct answers).

[b]Participation in Project Redirection was coded 1 for the experimental group, 0 for the comparison group.

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

+Statistically significant at the .10 level.

*Statistically significant at the .05 level.

**Statistically significant at the .01 level.

***Statistically significant at the .001 level.

TABLE C.24

REGRESSION OF FOLLOW-UP BIRTH CONTROL KNOWLEDGE TEST SCORES[a] FOR YOUNGER AND OLDER TEENS ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	Teens Age 15 or Younger at Baseline		Teens Age 16 or Older at Baseline	
	Unstandardized Coefficient	Standard Error	Unstandardized Coefficient	Standard Error
Married	1.26	.99	- .10	.57
Age	.36	.39	.15	.29
White	1.02	1.09	1.21*	.53
Hispanic	- .42	.70	- .03	.40
Mother Present in Household at Baseline	- .08	.58	.55	.36
Father Present in Household at Baseline	.67	.67	.51	.43
Mother's Education	- .03	.17	.10	.09
In School at Baseline	- .08	.56	.38	.36
Highest Grade Completed	.43	.28	.15	.17
Pregnant at Baseline	- .07	.59	.15+	.36
Pregnant at Follow-Up	- .38	.67	- .62+	.36
Number of Pregnancies	.16	.72	- .55	.35
Used Contraception at Baseline	.03	.58	.19	.36
Frequency of Sexual Intercourse at Follow-Up	- .21	.15	.10	.10
Baseline Score on Birth Control Knowledge Test	.55***	.08	.53***	.06
Ever Enrolled in Teen Parent Program at Baseline	.63	.56	.65+	.38
Participated in Project Redirection	.95+	.49	1.04**	.33
Constant	-1.29		- .17	
R^2	.33		.39	
Number of Respondents	129		247	

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: [a]The Birth Control Knowledge Test is a 16-item test designed to measure knowledge about various contraceptive methods and risk of pregnancy. Scores could range from 0 (no correct answers) to 16 (all correct answers).

[b]Participation in Project Redirection was coded 1 for the experimental group, 0 for the comparison group.

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

+Statistically significant at the .10 level.

*Statistically significant at the .05 level.

**Statistically significant at the .01 level.

***Statistically significant at the .001 level.

TABLE C.25

REGRESSION OF FOLLOW-UP BIRTH CONTROL KNOWLEDGE TEST SCORES[a] FOR TEENS PREGNANT OR NOT PREGNANT AT BASELINE ON BACKGROUND CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION[b]

Explanatory Variable[c,d]	Teens Pregnant at Baseline		Teens Not Pregnant at Baseline	
	Unstandard'zed Coefficient	Standard Error	Unstandardized Coefficient	Standard Error
Married	- .80	.66	1.98**	.74
Age	.03	.23	- .24	.27
White	1.37*	.65	1.09	.67
Hispanic	- .09	.47	.37	.49
Mother Present in Household at Baseline	.27	.40	.72	.45
Father Present in Household at Baseline	.59	.48	.40	.53
Mother's Education	.03	.10	.09	.12
In School at Baseline	.20	.40	- .09	.42
Highest Grade Completed	.12	.20	.06	.20
Pregnant at Baseline	--	--	--	--
Pregnant at Follow-Up	- .47	.50	.49	.39
Number of Pregnancies	-1.05*	.46	.26	.[illegible]
Used Contraception at Baseline	.58	.42	- .29	.44
Frequency of Sexual Intercourse at Follow-Up	- .00	.11	.05	.12
Baseline Score on Birth Control Knowledge Test	.48***	.06	.58***	.07
Ever Enrolled in Teen Parent Program at Baseline	1.01*	.40	.49	.52
Participated in Project Redirection	1.06**	.36	1.14**	.41
Constant	5.14		4.39	
R^2	.37		.43	
Number of Respondents	226		150	

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: [a]The Birth Control Knowledge Test is a 16-item test designed to measure knowledge about various contraceptive methods and risk of pregnancy. Scores could range from 0 (no correct answers) to 16 (all correct answers).

[b]Participation in Project Redirection was coded 1 for the experimental group, 0 for the comparison group.

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

+Statistically significant at the .10 level.

*Statistically significant at the .05 level.

**Statistically significant at the .01 level.

***Statistically significant at the .001 level.

APPENDIX D

Appendix D

Application of the Selection Modeling Techniques to Deal with Selectivity Biases

In the main body of this report, the analyses using either logistic or least-squares regression analyses to control for pre-existing experimental/comparison group differences were reported. This appendix discusses supplementary, exploratory analyses performed to further examine the selection bias issue.

In Chapter 4, the analyses revealed a program impact on school enrollment at the follow-up interview for the experimental group teens. A program effect was demonstrated despite elaborate controls for prior group differences. Nevertheless, there remains the possibility that unmeasured selection biases distinguished the experimental and comparison groups and that the unmeasured differences, rather than program participation per se, caused the observed effects. To explore this possibility, we performed some additional analyses based on concepts developed by Barnow, Cain, and Goldberger (1980) and

Heckman (1979).[1] Essentially, these analyses involved the development of a model to predict program participation. Then, in a second step, either the predicted value of being in Redirection (rather than actual participation) was used to predict school status at follow-up (the Barnow, Cain, & Goldberger approach); or a correction factor was added to the right-hand side of the equation, which already included a dichotomous experimental/comparison variable (the Heckman approach).

Table D.1 shows the results of these two approaches. For both methods, the results are shown for the analysis in which the first step of this approach (i.e., modeling program participation) used straightforward linear predictors: marital status, ethnicity, school status at baseline, number of times pregnant, enrollment in another teen parent program, attitudes toward work versus welfare, presence of mother in household, employment status, mother's education and household income (all measured at baseline).[2] Additional analyses (not shown in the table)

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[1]See Appendix A for a more complete discussion of our correction for selectivity.

[2]Other combinations of variables were tested in attempting to model experimental versus comparison group status. However, this specification resulted in the highest value of the D statistic (a goodness-of-fit statistic asymtotically equivalent to R^2).

TABLE D.1

LOGISTIC REGRESSION OF SCHOOL STATUS AT FOLLOW-UP[a] ON BASELINE CHARACTERISTICS AND PARTICIPATION IN PROJECT REDIRECTION, WITH SELECTIVITY CORRECTIONS[b]

Explanatory Variable[c,d]	(1) Beta	(1) Standard Error	(2) Beta	(2) Standard Error
Age	- .28+	.16	- .27	.17
White	.72	.47	.87+	.49
Hispanic	- .45	.37	- .35	.30
Age of Youngest Child	.00+	.00	.00	.00
Mother Present in Household at Follow-Up	.62*	.30	.69*	.31
Other Females Present at Follow-Up	.53	.44	.61	.47
Husband/Boyfriend Present at Follow-Up	- .69+	.42	- .96*	.44
Mother's Education	- .14	.31	- .20	.31
Married at Follow-Up	.34	.67	.45	.46
Pregnant at Baseline	.09	.29	.11	.30
Pregnant Since Baseline	- .58	.38	- .33	.40
In School at Baseline	1.00+	.58	.97+	.60
Highest Grade Completed	.33*	.15	.30*	.15
Planning to Return to School, Dropouts	- .21	.64	- .27	.53
Absentee Rate from School, If in School at Baseline	- .05	.05	- .63	.06
Amount of Time Out of School, Dropouts	- .03	.02	- .04	.02
Ever Enrolled in Teen Parent Program at Baseline	.33	.53	.21	.29
Predicted Value of Participating in Project Redirection	1.50	2.20	--	--
Participated in Project Redirection	--	--	1.00***	.28
Lambda-Correction for Selectivity via Heckman Approach	--	--	- .00	.00
Constant	2.43		2.65	
D[e]	.218		.250	
Number of Respondents	361		361	

SOURCE: Tabulations are from AIR baseline and 12-month follow-up interviews with experimental and comparison group members in Sample I.

NOTES: These analyses were based on data from those respondents who were not employed full-time at follow-up.

[a]School status at follow-up was defined as either in school/completed school or GED (code 1) or not in school or completed (coded 0).

[b]The corrections in column (1) follow the method described by Barnow et al. (1980), in which the predicted value of experimental group status is substituted for actual group status. The corrections in column (2) follow the Heckman (1979) approach in which a correction factor is added to the actual group status variable.

[c]All dummy variables are coded 1 for the variable as specified, 0 for the contrast.

[d]Unless otherwise specified, all explanatory variables are baseline characteristics.

[e]The D statistic is a goodness-of-fit measure that is asymtotically equivalent to a standard R^2.

+Statistically significant at the .10 level.

*Statistically significant at the .05 level.

***Statistically significant at the .001 level.

were also performed in which nonlinear interaction terms rather than just linear terms were used to model Redirection participation (e.g., school status x mother present; number of pregnancies x household income). The nonlinear terms yielded similar, but less satisfactory, results in the first step.

As shown in the first column of Table D.1 (corresponding to the Barnow et al. technique), the predicted value of being in Project Redirection was not a significant predictor of school enrollment/completion at the time of follow-up. In the second column, the Redirection participation variable is highly significant, while the correction factor (lambda) is nonsignificant. This second analysis suggests that there are true program effects, and that self-selection factors have been corrected in the other covariates.

The results of the first analysis could mean one of two things: (1) there are important selectivity biases that, when adjusted, eliminate the program participation effect on school status at follow-up; or (2) there is no selectivity effect, and the analysis is flawed either because of inadequate methods or a problematic specification. There is some rather strong evidence

suggesting that the second interpretation is the more plausible of the two. First and foremost, the analysis using the Heckman approach suggests no significant selection biases. Second, it would be surprising if it did, since the models tested in the first stage were not very successful in predicting program participation. Either because of the homogeneity of the population, the design used to match groups, or the failure to measure key variables, it proved to be very difficult to develop a prediction equation for experimental versus comparison group status. In fact, the highest amount of explained variance was .13, corresponding to a relatively poor fit and substantial errors of estimate.

A third problematic feature of the participation estimates is that they were primarily dependent on variables that were also being used in the model to predict school status. That is, the best predictors of participation (the first step) were also covariates in the second step of the analysis, causing a problem of redundancy that could mask true participation effects. In fact, when different variables were used in the first step (variables which, however, do not do as good a job at predicting experimental group status), the predicted value of Redirection participation did become a significant

variable in predicting school status in the second step of the analysis. Since there is little a priori reason to systematically exclude many variables used in the first stage from the second stage, however, these estimates are not very defensible.

Finally, the data showing teens' school enrollment over time (Figure 4.1) are not consistent with an interpretation that nonmeasured selectivity biases (such as higher motivation or better school attitudes) favored the Redirection teens. The comparison group teens were consistently more likely to be in school in the two and one-half year period prior to baseline than the program participants. If anything, one would have anticipated selectivity biases favoring the comparison group teens.

In summary, these supplementary analyses are not sufficiently persuasive to change our conclusion that participation in Project Redirection resulted in favorable school outcomes during the first 12 months of program participation. Furthermore, similar results were obtained in analyses in which employment and repeat pregnancy were the outcome variables, suggesting that the program impacts cut across a broad range of outcomes and did not spuriously reflect selection biases.

APPENDIX E

Appendix E

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