

---

# Longer-term Impacts of Mentoring, Educational Services, and Incentives to Learn: Evidence from a Randomized Trial in the United States

Núria Rodríguez-Planas<sup>1</sup>

Universitat Autònoma de Barcelona,  
MOVE, IZA and FEDEA

First version: December 15<sup>th</sup>, 2010  
This version: October, 2010

## Abstract

This paper is the first to use a randomized trial in the United States to analyze the short- and long-term impacts on education, employment, wages and behavior of an after-school program that offered disadvantaged high-school youth: mentoring, educational services, and financial rewards (on both inputs and outputs), with the objective to improve high-school graduation and postsecondary schooling enrollment. The short-term hefty beneficial average impacts on high-school graduation and postsecondary education enrollment quickly faded away as some control group members eventually caught up to the QOP group. There are important gender differential effects of the program. QOP helped female students get them through high-school and post-secondary training (not necessarily college) quicker than their counterparts in the control group. Moreover, five years after the end of the program, female QOP enrollees had better employment outcomes than control group members. In contrast, QOP had some adverse short-term impacts on male students' academic performance and substance abuse, while leaving their rate of high-school graduation unaffected. Moreover, males showed worse adult employment outcomes and higher involvement on adult criminal activity than members of the control group. Finally, encouraging results for younger youth are also found.

**Key words:** short-, medium- and long-term effects, after-school programs, educational and employment outcomes, risky behaviors, cognitive and non-cognitive skills.

**JEL classification:** C93, I21, I22, I28, J24

---

<sup>1</sup> The Quantum Opportunity Program demonstration study was conducted under Contract No. K-5547-5-00-80-30 from the U.S. Department of Labor or the Ford Foundation. The views expressed herein do not necessarily reflect the policies or opinions of the U.S. Department of Labor. Allen Schirm, Myles Maxfield, Elizabeth Stuart, Allison Mckie, and Laura Castner from Mathematica Policy Research, as well as Vida Maralani from UCLA, and Mary Vencill from Berkeley Policy Associates, made important contributions to the study. I also welcome the useful comments and advice in the analysis and the exposition of the paper from Samuel Bentolila, Caterina Calsamiglia, David Dorn, Bernd Fitzenberger, Michael Lechner, Robert Moffit, Peter Mueser, Kenneth Troske, Sunde Uwe, Conny Wunsch, and participants at seminars at Saint Gallen, Switzerland; CEMFI, Spain; and ZEW Mannheim, Germany. Author's e-mail: [Nuria.Rodriguez@uab.es](mailto:Nuria.Rodriguez@uab.es)

---

## I. Introduction

In response to the observed increasing earnings differential between the most and the least educated workers since the late 1970s, policies aiming at improving high-school graduation and post-secondary education enrollment have recently received renewed attention from policy makers, practitioners and researchers. As a consequence, there has been a new wave of interventions whose main objective is to improve the school performance of disadvantaged youth.<sup>1</sup> Most of these interventions involve one or the combination of the following services: (i) a mentoring component; (ii) an educational component; and (iii) a financial incentive component; and they generally find promising results if not for all participants, for some particular subgroups.

While most of these studies analyze the effects of the interventions during or shortly after the students have been exposed to the program, the evidence on the medium- or longer-term impacts is very scarce. In addition, and probably because of the shorter-term focus, all of these studies look at educational outcomes as opposed to employment outcomes and wages.<sup>2</sup> However, knowing the long-term impacts of these interventions is key to disentangle the following questions: Do the short-term changes generated by the intervention persist or do they quickly fade away? Do they translate into longer-term payoffs as measured by post-secondary schooling, employment, earnings, and attitudes towards life (risky behaviors and welfare use)? These are the questions this paper aims to address for the first time in the United States.

Using a randomized experimental design, this study evaluates the short-, medium-, and long-term effects of a five-year after-school program, the Quantum Opportunity Program (QOP hereafter), on education, employment, wages and behavior in the United States. QOP involved the combination of the following three services: a mentoring, an educational, and a financial incentives component. The program's main objectives were to increase the likelihood that youth completed high-school and engaged in post-secondary education or training. In addition, QOP had a secondary

---

<sup>1</sup> For experimental or quasi-experimental designed evaluations targeting primary schools, see Jacob and Lefgren, 2004; Banerjee *et al.*, 2007; and Kremer *et al.*, 2008; James-Burdumy *et al.*, 2008; and Fryer, 2010. For studies focusing on high-school youth, see Machin *et al.*, 2004, and 2007; and Lavy and Schlosser, 2005; Angrist and Lavy, 2009; Holmlund and Silva, 2009; and Fryer, 2010. And for evaluations aiming at improving college students' performance, see Bettinger and Long, 2005; Leuven *et al.*, 2003; Angrist *et al.*, 2009, and Scrivener *et al.*, 2009. These studies complement the literature on employment and training programs for disadvantaged youth (Bloom *et al.*, 1996; Lalonde, 1995; Kemple and Willner, 2008; Schochet *et al.*, 2008).

<sup>2</sup> To the best of my knowledge, only three papers look at longer horizons as in the present paper: Leuven *et al.*, 2003, Bettinger and Long, 2005, and Angrist and Lavy, 2009, measure outcomes three, four and five years after the end of the intervention, respectively. However, the two first ones only look at academic achievement, while Angrist and Lavy analyse the effect on passing the Bagrut in Israel.

objective of reducing risky behaviors such as substance abuse, crime, and teenage childbearing. The outcome variables involved high-school completion and academic performance, post-secondary education or training enrollment and completion, employment and earnings, and risky behaviors measured at three points in time: (i) during the fifth year of the demonstration while the students were still in, or just completing, high school; (ii) three years later when most sample members were about 21 or 22 years old; and (iii) five years after the end of the demonstration when most sample members were about 24 or 25 years old. To examine the effectiveness of this program, the Department of Labor and the Ford Foundation funded this demonstration in eleven schools across seven sites across the United States between 1995 and 2001.

This paper presents impact findings from this evaluation.<sup>3</sup> The first finding is that QOP seemed to have helped youth take advantage of opportunities to get ahead in life, since the treated got their high-school diplomas and engaged in post-secondary education sooner than youths in the control group. Similar to recent studies, QOP appears to be quite effective in the short-run in terms of increasing the likelihood of high-school graduation and of attending college by 17.5% and 23%, respectively.<sup>4, 5</sup> Although QOP's positive effect on high-school completion is no longer statistically significant by the time youths are in their early twenties, QOP did raise the likelihood of ever attending college and post-secondary education by 23% and 17%, respectively. By the time youths are in their mid-twenties, QOP has no statistically significant educational or employment impacts on its enrollees. The reason is that some control group members eventually caught up to the QOP

---

<sup>3</sup> See Maxfield *et al.*, 2003 and 2003b; Schirm *et al.*, 2003; Schirm and Rodríguez-Planas, 2004; and Schirm *et al.*, 2007 for detail description of program design and implementation as well as thorough analysis of the demonstration's impacts.

<sup>4</sup> Rigorous studies finding *average* beneficial short-term impacts of interventions aiming at improving educational performance of youth include Hahn, 1994; Hahn *et al.*, 1994; Machin, *et al.*, 2004; Jacob and Lefgren, 2004; Lavy and Schlosser, 2005; Bloom and Sommo, 2005; Banerjee *et al.*, 2007; Scrivener, *et al.*, 2009; Kremer *et al.*, 2008; and Fryer, 2010. All these evaluations—except for Kremer *et al.*, 2008, which evaluated a merit awards program; and Fryer, 2010, which evaluate several programs offering financial incentives on students achievement—, studied the effects of a variety of educational services offered to youth. Other rigorous evaluations find positive impacts for certain subgroups *only*—see Bettinger and Long, 2005; Machin *et al.*, 2007, for evaluations of educational services; Leuven *et al.*, 2003; and Angrist and Lavy, 2009, for evaluations of financial rewards; and Angrist *et al.*, 2009, for a combination of services and financial rewards.

<sup>5</sup> Worth highlighting is the evaluation of the QOP pilot conducted between 1989 and 1993 in five sites with funding from the Ford Foundation (Hahn, 1994; Hahn *et al.*, 1994). While the results from the QOP pilot were slightly more promising than those from the large-scale evaluation demonstration presented in this paper, they were measured at most several months after participants should have graduated from high-school, and therefore it is unclear whether these findings would have persisted over time as youth grew older. Other important differences between the pilot and the demonstration included the sample size, which was smaller in the pilot, and the targeted population, which, in the pilot, were low-income students (as opposed to academically low-performing students as in the demonstration). Finally, results from the Philadelphia site in the pilot were exceptional and frequently this site was the only one to produce statistically significant results.

---

group in terms of high-school completion and post-secondary enrollment.

A second finding is that we find, similar to recent studies, a consistent pattern of stronger positive female response to services and financial incentives in education.<sup>6</sup> The sizeable impact on high-school graduation when youths were in their late-teens is driven entirely by a large and significant effect on women. In addition to helping female students by getting them through high school earlier, QOP also got them through post-secondary training (not necessarily college) quicker than their counterparts in the control group. Moreover, five years after the end of the program, female QOP enrollees had better employment outcomes than control group members, although the opposite was true for educational outcomes (measured as college attendance). In contrast, QOP had some adverse short-term impacts on males' academic performance. Although these adverse effects were short-lived and QOP enrollees ended up with higher GED receipt by the time they were in their mid-twenties, the longer-term employment impacts continued to be detrimental five years after the end of the program. In addition, males experienced higher rates of substance abuse in their late-teens—similar to the Moving to Opportunity experiment (Kling *et al.*, 2005)—, and a larger criminal activity in their mid-twenties relative to the control group.

Finally, we also find that QOP had long lasting beneficial educational outcomes for the younger enrollees, giving some hope for interventions in secondary school education. The promising findings for the younger youth contribute to the debate over whether investments in the later stages of a child's development have positive payoffs. The results suggest that the earlier the intervention the better—consistent with many academics and practitioners' beliefs that early childhood interventions are preferred (Currie, 2001; Currie and Thomas, 2001; Krueger and Whitmore, 2001; Garces *et al.*, 2002; and Carneiro and Heckman, 2003). In addition, they provide some hope for high-school interventions—if they are implemented on the entering class of freshmen students—, and in contrast with most findings from service-oriented dropout-prevention programs that present quite discouraging results (Dynarski and Gleason, 2002). Recently two rigorous studies have also found positive returns for interventions offering educational services targeted to youth at a relatively late stage of schooling (Machin, McNally and Meghir, 2004; and Lavy and Schlosser, 2005). In contrast with the present paper, both of these studies use a quasi-experimental design, present only short-term impacts of the services offered, and focus on high-school students in Israel and the

---

<sup>6</sup> Robert J. Lalonde, 1995; Joshua Angrist, *et al.*, 2002; Jeffrey R. Kling, Jeffrey B. Leibman, and Lawrence F. Katz, 2007; Dynarski, 2008; Garibaldi *et al.*, 2007; Anderson, 2008; Joshua Angrist, Daniel Lang, and Philip Oreopoulos, 2009; and Angrist and Lavy, 2010.

---

United Kingdom, respectively.

This paper is organized as follows. The next section describes the program implementation and the data. Section III provides a theoretical background motivating the intervention. Section IV explains the evaluation framework and analyzes the results. Section V concludes with suggestions on how to improve program design and evaluation.

## **II. The Quantum Opportunity Program Demonstration Project**

### ***A. Program Description***

QOP differed from other interventions aiming at improving disadvantaged youth educational outcomes in at least two important ways. First, it was an intensive, long-term after-school program aiming to overcome the many serious challenges facing disadvantaged youth. It lasted five years and was offered year-round to low-achieving students from low performing high-schools entering in 9<sup>th</sup> grade in 1995 in the United States.<sup>7</sup> It should therefore not come as a surprise that it was an expensive program. At almost \$25,000 per enrollee for the whole demonstration, QOP has been the most expensive Federal youth program offered. By comparison, the operating costs of the also-expensive Job Corps were approximately \$16,500 per participant in 1998 (Schochet *et al.*, 2008).

Second, QOP offered more comprehensive services than other programs. While most programs offer mentoring, educational services, or financial rewards, QOP offered all these services combined. As we shall discuss below, although the core of the QOP model was intensive case management and mentoring, its educational and development services, on the one hand, and the financial rewards, on the other, were similar in design and intensity as those implemented in other evaluations.

Case managers had small caseloads of only 15 to 25 youth and were to develop with each youth a highly personal, long-lasting connection that mirrored the relationship between a teenager and a nurturing, supportive older relative. As such, the case manager would make every effort to sustain a strong relationship with the youth regardless of behavior or status, including if the youth disengaged from the program, dropped out of school, became incarcerated, or moved out of the area. Case managers were also to manage the provision of supportive services for addressing all barriers to success that enrolled youth faced, whether related to school, family, or

---

<sup>7</sup> Enrollees who graduated on time received some mentoring and assistance in enrolling in postsecondary education or training between graduation and the end of the fifth year of the demonstration.

friends.<sup>8</sup>

In addition to case management, the program engaged youth in: (i) developmental activities that aimed to develop their social and employment-readiness skills; (ii) community service activities to develop a sense of community belonging, trustworthiness, and respect; and (iii) educational services to improve their academic performance. Examples of such types of activities are displayed in Table 1.

TABLE 1  
QOP's Developmental Activities, Community Services and Educational Services

| <i>Activity</i>      | <i>Examples of such types of activities</i>  |
|----------------------|--|
| Developmental        | Life skills activities/ discussion topics (such as, family planning, nutrition, personal hygiene, managing anger, avoiding drug behaviors, among others); pre-employment training; cultural activities; and recreational activities.   |
| Community services   | Visits to the residents of a local nursing home, or volunteering at a local food bank.   |
| Educational services | Academic assessment, development of individualized education plans, one-on-one tutoring, and computer-assisted instruction in specific coursework as well as basic reading and mathematics. Making the youth aware of, and helping them plan for, college and other postsecondary education or training. |

Finally, QOP also offered financial incentives to students. Such incentives had two components: an incentive on inputs and another one on outputs.<sup>9</sup> First, youth received a stipend of \$1.25 for every hour devoted explicitly to educational activities, developmental activities (excluding recreational activities), and community service. This component is equivalent to providing incentives on inputs rather than outputs, which is more common in the student

<sup>8</sup> These barriers could be addressed either directly by the case manager or by referral to a community resource, such as a substance abuse program or local agencies that provide housing, food, income support, or child care.

<sup>9</sup> As Fryer, 2010, explains, under certain assumptions, traditional price theory predicts that providing incentives based on output is socially optimal because each student decides which input from their production function to subsidize. Assuming that student's have superior knowledge about how they learn, it is socially optimal to allow them to allocate their time across inputs. However, if this assumption is violated, then it can be more effective to provide incentives for inputs. Fryer, 2010, finds that incentives for output did no increase achievement, while incentives for certain inputs did. He explains that the leading theory behind these findings is that students do not understand the educational production function and, thus, lack the know-how to translate their excitement about the incentive structure into measurable output.

---

incentives literature.<sup>10</sup> Second, a matching amount was promised to the youth when he or she earned a high school diploma or GED *and* enrolled in post-secondary education or training (including vocational training or military service).<sup>11</sup> This was clearly an incentive on output as the student received the economic incentive only if they graduated from high-school *and* enrolled in post-secondary education. As explained in the implementation sub-section below, by the end of the demonstration, this represented for most youths receiving between \$1,000 to \$3,000 after high-school graduation and enrollment in post-secondary education. Although some may question whether QOP's financial rewards were sufficiently large, numerous studies examining the impact of various types of tuition and financial aid policies on college-going show that students respond to changes in college cost (Leslie and Brinkman, 1988; Cornwell, Mustard, Cameron and Heckman, 1993; and Kane, 1998; Sridhar 2006; Dynarski, 2003; and Deming and Dynarski, 2009). A consensus estimate associates a \$1,000 change in college costs with an approximately 5 percentage point difference in college enrollment rates. Moreover, according to a recent study by Kane, 2007, there would be differential effects by race, being stronger for African American.

### ***B. Target Population and Sample Selection***

In the summer of 1995, QOP was implemented in eleven high schools across seven sites in the United States.<sup>12</sup> In each of these schools, entering 9<sup>th</sup> grade students—except those with a GPA from 8<sup>th</sup>-grade above the 66<sup>th</sup> percentile—, were randomly assigned to QOP or a control group. Youth assigned to the program group were enrolled in QOP. Youth assigned to the control group could not enroll in QOP, but could enroll in other youth programs offered in the community. Thus the counterfactual is *other* available programs that the study population would

---

<sup>10</sup> In Kremer *et al.*, 2008; and Angrist and Lavy, 2009; and most other studies on student incentives, the objective is to pass a test and students are paid if they complete the objective. Fryer, 2010, measures the effect of four different financial incentives on student achievement: two of them are “output” experiments (the ones in Chicago and New York city) and the other two (in Dallas and in Washington, DC) are “input” experiments.

<sup>11</sup> This is a similar design to the one currently applied in The Paper Project, which rewards high-school students for core class grades, in that half of the reward is given to the student immediately, the other half is distributed at graduation.

<sup>12</sup> DOL awarded demonstration grants to implement this QOP model in five sites: Cleveland, Ohio; Fort Worth, Texas; Houston, Texas; Memphis, Tennessee; and Washington, DC. The Ford Foundation funded two sites: Philadelphia, Pennsylvania and Yakima, Washington. Four of these seven sites operated in one school; two sites (Houston and Washington D.C.) operated in two schools each, and the Memphis site operated in three schools. Six of the seven demonstration sites operated QOP between 1995 and 2000; the Washington, DC, site began one year later and operated the program through summer 2001. A local community-based organization (CBO) implemented the QOP model in each site.

enroll in if QOP were not an option. Random assignment involved four steps as described in Table 2.

TABLE 2  
Random Assignment Protocol

|        |  |
|--------|--|
| Step 1 | Generate a list of all eligible 9 <sup>th</sup> graders at each participating school.  |
| Step 2 | Because the number of eligible students was larger than the target sample size in all but two schools, the second step was to randomly select students who would participate in the evaluation (in either the program or control group) from among all eligible students. This sampling was done independently for each school. This minimized the burden of the evaluation on students, parents, and schools; and it limited the number of disappointed students. For each school, we drew a sample of eligible students that was 10 percent larger than the target size for the evaluation sample. |
| Step 3 | Obtain consent for participation in the study from students' parents. We obtained denied consent from 2% of the study sample. Another 7% of the parents never responded.   |
| Step 4 | Randomly assign students within each school to either the QOP group or the control group.  |

Table 3 shows how the evaluation sample was developed for each school. The first row shows the number of slots allocated to each school. The second row in the table—headed “GPA Eligibles”—shows the number of students in each school who: (i) were attending the school, (ii) were entering ninth grade for the first time, (iii) were not so disabled that the school viewed participation in the program as inappropriate, and (iv) had a grade point average (GPA) from the eighth grade below the 67<sup>th</sup> percentile among the students in the school meeting the first three requirements. The number of eligible students ranged from 82 to 523 across the QOP schools. Using the procedures described in Table 2, we randomly selected from the list of GPA Eligibles an “Initial Sample” consisting of the number of students shown in the third row. This sampling was done independently for each school. Then, we instructed QOP staff to obtain consent for participation in the evaluation for all students in the initial sample. About 5% of the students in the initial sample—the students in the row headed “Ineligibles”—were determined to be ineligible for QOP based, in most instances, on evidence from school records indicating that a student had never attended the QOP school or had left the school early in the school year before QOP eligibility was determined. The parents/guardians of about another 7% of the students in the initial sample never responded to QOP staff’s attempts to obtain consent. There was strongly suggestive evidence from school staff or other sources—but not definitive evidence from school records—that many of these students were, in fact, ineligible. However, in some instances, the failure to respond probably was a passive denial of consent. Parents/guardians actively denied



**TABLE 3**  
**Development of the Evaluation Sample**

|                                  | Cleveland  |           | Washington, D.C. |       | Fort Worth | Houston |       |       | Memphis |          |           | Philadelphia | Yakima   | All Sites |       |
|----------------------------------|------------|-----------|------------------|-------|------------|---------|-------|-------|---------|----------|-----------|--------------|----------|-----------|-------|
|                                  | Collinwood | Anacostia | Easter           | Total | Paschal    | Austin  | Yates | Total | Carver  | Hamilton | Hillcrest | Total        | Franklin | Davis     | Total |
| QOP slots                        | 100        | 40        | 40               | 80    | 100        | 50      | 50    | 100   | 35      | 27       | 38        | 100          | 50       | 50        | 580   |
| GPA eligibles                    | 175        | 130       | 165              | 295   | 398        | 523     | 305   | 828   | 82      | 225      | 108       | 415          | 210      | 229       | 2,550 |
| Initial Sample                   | 175        | 88        | 88               | 176   | 220        | 110     | 110   | 220   | 82      | 58       | 88        | 228          | 110      | 110       | 1,239 |
| - Ineligibles                    | 9          | 11        | 4                | 15    | 18         | 5       | 7     | 12    | 0       | 0        | 1         | 1            | 9        | 0         | 64    |
| Net Eligible Sample              | 166        | 77        | 84               | 161   | 202        | 105     | 103   | 208   | 82      | 58       | 87        | 227          | 101      | 110       | 1,175 |
| Consenters                       | 158        | 72        | 82               | 154   | 177        | 92      | 94    | 186   | 70      | 54       | 75        | 199          | 95       | 100       | 1,069 |
| Denied Consenters                | 1          | 1         | 0                | 1     | 8          | 5       | 4     | 9     | 0       | 0        | 3         | 3            | 2        | 0         | 24    |
| Did Not Respond                  | 7          | 4         | 2                | 6     | 17         | 8       | 5     | 13    | 12      | 4        | 9         | 25           | 4        | 10        | 82    |
| Consent Probability <sup>a</sup> | 95         | 94        | 98               | 96    | 88         | 88      | 91    | 89    | 85      | 93       | 86        | 88           | 94       | 91        | 91    |
| QOP Enrollees                    | 100        | 40        | 40               | 80    | 100        | 50      | 50    | 100   | 35      | 27       | 38        | 100          | 50       | 50        | 580   |
| Controls                         | 58         | 32        | 42               | 74    | 77         | 42      | 44    | 86    | 35      | 27       | 37        | 99           | 45       | 50        | 489   |
| QOP Probability <sup>b</sup>     | 63         | 56        | 49               | 52    | 56         | 54      | 53    | 54    | 50      | 50       | 51        | 50           | 53       | 50        | 54    |

<sup>a</sup>100 × Consenters/Net Eligible Sample

<sup>b</sup>100 × QOP Enrollees/Consenters

consent for another 2% of the initial QOP sample. Before we would conduct random assignment for a school, QOP staff had to verify that they had made substantial efforts to contact and obtain consent from the nonrespondents.

The “Consenters” row in Table 3 gives the number of students who were eligible for random assignment and therefore constitute our evaluation sample. From among these students, we filled the available QOP slots independently for each school by simple random sampling without replacement. Students who were selected for QOP became QOP enrollees. Students who were not selected for QOP became the control group. The final sample for the QOP demonstration consists of 1,069 students, 580 in the QOP group and 489 in the control group.

As expected, random assignment produced treatment and control groups whose distributions of characteristics prior to random assignment were similar. The only statistically significant difference was the proportion of youth in the middle third of the distribution, which was a bit larger for the control group, as shown in Table 4 below.

TABLE 4

Group mean baseline characteristics by treatment group  
(Percentages)

|   | <i>QOP group</i><br>(1) | <i>Control group</i><br>(2) |
|---|-------------------------|-----------------------------|
| <i>Pre-program Characteristics</i>      |                         |                             |
| Male                                    | 52                      | 56                          |
| Age when entering 9 <sup>th</sup> grade |                         |                             |
| < 14                                    | 11                      | 11                          |
| 14                                      | 53                      | 57                          |
| > 14                                    | 36                      | 31                          |
| Hispanic                                | 26                      | 26                          |
| Black                                   | 68                      | 68                          |
| Rank based on 8 <sup>th</sup> grade GPA |                         |                             |
| Bottom third                            | 37                      | 34                          |
| Middle third                            | 31 <sup>†</sup>         | 36 <sup>†</sup>             |
| Top third                               | 32                      | 30                          |
| Sample size                             | 580                     | 489                         |

Note: † Significantly different from the mean for the other group at the 90% confidence level, two-tailed test

As discussed with more detail at the end of Section III.F., spill-over effects did not seem to be an issue in this intervention for the following two reasons. First, although QOP provided

tutoring and computer-assisted instruction to its enrollees, it was not designed to influence the structure, policies, or operation of the high schools with which local QOP programs were associated. Second, QOP did not operate within the school or within school hours. Instead it was an after-school program, and its activities were scheduled *outside* the high-schools from 3 to 6 pm during weekdays, and for one half day over the weekend. That said, QOP could still have stimulated control group members to work harder because they knew that some of their classmates were receiving additional help. In the results section, we discussed why we do not think this may have occurred.

### C. The database

One of the highlights of the QOP demonstration was its intense data collection. The evaluation was designed to estimate the short-, medium-, and long-term impacts of the program by collecting survey data on youth's outcomes at four different points in time, as shown in Table 5. Most of the analysis is based on data from a series of three telephone surveys, two of which were conducted two and five years *after* the end of the program.

TABLE 5

Timing of QOP implementation and survey data collection

| <i>QOP demonstration implementation</i> |   |                                     |                           | <i>Post-demonstration</i>                   |   |  |
|---|---|-------------------------------------|---------------------------|---|---|--|
| <i>Fall 1995</i>                        | <i>Spring 1999<br/>On time<br/>graduation</i>   | <i>November 1999-<br/>June 2000</i> | <i>September<br/>2000</i> | <i>December<br/>2000</i>                    | <i>September<br/>2002- April<br/>2003</i> | <i>January-<br/>September<br/>2005</i> |
| Youth entered<br>9 <sup>th</sup> grade  | Paper survey on<br>resiliency<br>factors and<br>Achievement<br>tests in math<br>and reading | 1 <sup>st</sup> telephone<br>survey | End of the<br>program     | High-<br>school<br>transcript<br>collection | 2 <sup>nd</sup> telephone<br>survey       | 3 <sup>rd</sup> telephone<br>survey    |

*Note:* All events occurred one year later for the Washington DC site with the exception of the two post-demonstration surveys, which were collected at the same time in the DC site than in the other sites.

In addition, by the usual standards for observational evaluation studies, this data set is exceptionally rich and informative, as it contains information on math and reading achievement tests, high-school completion status, engagement in post-secondary education and training, employment (including earnings and benefits), risky behaviors, welfare use, and resiliency factors. Finally, data on program implementation, participation, and costs, as well as (baseline)

---

information on the youth (including their 8<sup>th</sup>-grade GPA) and high-school transcripts from *all* the high schools a sample member attended were obtained.

There are three drawbacks with the data at hand. First, due to data collection costs, no baseline survey was collected. As a consequence, the pre-program information available is reduced to the characteristics displayed in Table 4. Albeit the limited baseline information available, it is important to highlight that 8<sup>th</sup>-grade GPA, which is a good proxy for youth's cognitive and non-cognitive skills, as well as unobserved ability, is available.<sup>13</sup> Second, as these are (mainly) survey data, differential non-response between treatment and control group members could potentially bias the results. Table 6 shows the survey effort is equiparable to that of other studies (Schochet *et al.*, 2008; Banerjee *et al.*, 2007; and Kremer *et al.*, 2008, among others). However, we did observe that attrition rates were higher across control group members than treatment group members. The response rate to the 1<sup>st</sup>, 2<sup>nd</sup> and 3<sup>rd</sup> telephone survey was 84%, 75%, and 76%, respectively—87% , 80%, and 77% for the QOP group and 80%, 70%, and 74% for the control group in the 1<sup>st</sup>, 2<sup>nd</sup>, and 3<sup>rd</sup> telephone surveys, respectively. To address potential non-response bias, all of the estimates in the paper adjust for survey non-response.<sup>14</sup> In addition, a thorough sensitivity analysis on whether (and if so how) non-response may be affecting the results is also provided at the end of the results section. Overall, we do not find evidence that differential non-response bias between treatment and control group is driving the results.

Third, data on participation were recorded for the purpose of computing periodic stipend payments and accrual contributions for each enrollee, making its research use difficult for the following two reasons. First, given that mentoring time did not count toward stipends or accrual account contributions, data on time spent being mentored were *not* recorded. Second, bonuses hours (50 extra hours, for example) were given when an enrollee achieved a significant milestone, such as earning a B average or higher on his or her report card. The extra hours

---

<sup>13</sup> The main pre-program variable that is missing (compared to similar evaluations) is parent's education level. Fortunately, this variable is likely to be correlated with pre-program GPA, which we do have.

<sup>14</sup> Non-response weights were estimated using response propensity scores for the treatment and the comparison group, separately. The predictors used in the response propensity scores included school dummies, baseline characteristics, interactions between the previous school and baseline characteristics and between any two baseline characteristics, and outcomes measured in any of the earlier surveys.

TABLE 6

Response Rates for Data Collection Activities by Site

|  | Fort Worth | Cleveland | Washington, D.C. | Houston | Memphis | Philadelphia | Yakima | All Sites |
|--|------------|-----------|------------------|---------|---------|--------------|--------|-----------|
| <i>Response rates for data collection activities (percentages)</i> |            |           |                  |         |         |              |        |           |
| <b>Achievement tests</b>   |            |           |                  |         |         |              |        |           |
| <b>Reading</b>   |            |           |                  |         |         |              |        |           |
| Overall  | 82         | 83        | 84               | 89      | 84      | 89           | 77     | 84        |
| QOP  | 87         | 85        | 90               | 93      | 86      | 92           | 82     | 88        |
| Control  | 77         | 79        | 77               | 85      | 83      | 87           | 71     | 80        |
| <b>Mathematics</b>   |            |           |                  |         |         |              |        |           |
| Overall  | 82         | 83        | 84               | 89      | 84      | 89           | 75     | 84        |
| QOP  | 86         | 85        | 90               | 93      | 86      | 92           | 80     | 88        |
| Control  | 77         | 79        | 77               | 85      | 83      | 87           | 69     | 80        |
| <b>First telephone survey</b>                                      |            |           |                  |         |         |              |        |           |
| Overall  | 84         | 87        | 79               | 89      | 83      | 82           | 85     | 84        |
| QOP  | 86         | 87        | 90               | 91      | 87      | 84           | 84     | 87        |
| Control  | 82         | 86        | 66               | 86      | 79      | 80           | 86     | 80        |
| <b>Transcripts</b>   |            |           |                  |         |         |              |        |           |
| Overall  | 87         | 70        | 89               | 89      | 77      | 79           | 79     | 82        |
| QOP  | 93         | 68        | 95               | 95      | 81      | 82           | 88     | 86        |
| Control  | 79         | 72        | 82               | 83      | 74      | 76           | 70     | 77        |
| <b>Second telephone survey</b>                                     |            |           |                  |         |         |              |        |           |
| Overall  | 80         | 75        | 76               | 71      | 73      | 63           | 83     | 75        |
| QOP  | 84         | 80        | 90               | 73      | 74      | 64           | 84     | 80        |
| Control  | 75         | 66        | 61               | 68      | 71      | 61           | 82     | 70        |
| <b>Third telephone survey</b>                                      |            |           |                  |         |         |              |        |           |
| Overall  | 82         | 72        | 73               | 73      | 82      | 67           | 71     | 76        |
| QOP  | 82         | 75        | 79               | 74      | 86      | 69           | 67     | 77        |
| Control  | 83         | 69        | 67               | 73      | 79      | 65           | 76     | 74        |

resulted in an increased stipend payment and accrual account contribution. Unfortunately, these bonus hours could not be distinguished from regular hours, and thus result in overestimates of the amount of time spent on program activities for some enrollees. While this prevents me from using participation hours to apply quasi-experimental methods to estimate the impacts by subgroups based on their predicted probability of participating, analysis of these data does provide some reliable information on how much these services were taken, and who was likely to be taking them, as discussed below.

**D. Implementation of QOP and Service Use**

In many respects, the sites were successful in implementing QOP. As designed, QOP served

---

youth who faced many barriers to academic success. In addition, the core component of QOP—case management and mentoring—was also well implemented across the sites. Case managers were hired for the whole length of the program and with prior expertise on social services.<sup>15</sup> Most of them reported developing close mentoring relationships with the majority of the youth assigned to them, and they all provided access to services regardless of an enrollee’s behavior or status (such as becoming incarcerated, moving to another community, or dropping out of high school) as originally planned by the program.

Although the educational, community services, and development activities component fell short of the targeted original design, the participation achieved was still a substantial investment of time—especially compared to other similar youth programs.<sup>16</sup> The initially planned target consisted of 750 hours of services annually (equally distributed among the three different activities), which (if achieved) would have represented about three-quarters of the hours required for in-school instruction per year.<sup>17</sup> As it was, the average amount of time (708 hours) enrollees spent on QOP activities during the first four years—including summers—corresponds to about 72% of an extra school year, still a substantial investment of time (in addition to the time spent with the mentor), as shown in Table 7. Finally, the fact that QOP did not achieve its extremely ambitious target is not a concern in terms of the external validity of this evaluation as if the program were to be implemented on a broader scale, it is likely that its implementation would not differ much from how it was implemented during the demonstration.

As explained earlier there were two components of the financial incentives: the one that rewarded program participation, and the one that rewarded high-school completion (including obtaining a GED) *and* post-secondary enrollment. The enrollee stipends were well implemented and appeared to be an effective way to attract the enrollees to program activities in the first year or two of the demonstration. As enrollees aged and could earn much more per hour by working, case managers found that other incentives, such as recognition, attention, and prizes, could replace the stipends. As explained earlier, the financial incentive to outputs was quite successful

---

<sup>15</sup> Most case managers stayed with the program for several years, and many stayed for the entire five years of the demonstration. Unfortunately, no information on sex, race or ethnicity of mentors was collected.

<sup>16</sup> For instance, the average participation in QOP activities (excluding mentoring) was more than half of the average instruction time received by Job Corps participants, the (by far) most intense education and training program for disadvantaged youths in the United States (Schochet *et al.*, 2008).

<sup>17</sup> In 2000, the average number of instructional hours spent in public school by 15-year-old youth was 990 hours (U.S. Department of Education 2005; Table 26-2).

TABLE 7  
Participation in QOP Activities

|   | <i>Cumulative Years<br/>1 through 4</i> | <i>Year 1</i> | <i>Year 4</i> |
|---|---|---------------|---------------|
| Average Number of Hours                       | 708                                     | 247           | 103           |
| Average Hours on Educational Activities       | 305                                     | 110           | 40            |
| Average Hours on Developmental Activities     | 306                                     | 105           | 41            |
| Average Hours on Community Service Activities | 97                                      | 32            | 22            |
| No Hours of Participation (percent)           | 1                                       | 1             | 26            |
| More Than 100 Hours (percent)                 | 88                                      | 73            | 29            |
| More Than 375 Hours (percent)                 | 62                                      | 23            | 11            |
| More Than 750 Hours (percent)                 | 36                                      | 1             | 0             |
| More Than 1,500 Hours (percent)               | 13                                      | 0             | 0             |

*Source:* QOP Demonstration Management Information System.

*Note:* Because QOP services in Period 5 differed substantially from those of the first four periods, we report trends over the first four periods. In Period 5, QOP offered enrollees who had graduated from high school only mentoring services, and hours spent being mentored were not recorded.

in that, by the end of the demonstration, enrollee's accrual account balances ranged from a few hundred dollars to nearly \$10,000, with most being in the range of \$1,000 to \$3,000.<sup>18</sup> The size of this incentive is comparable to the ones currently being offered in ongoing evaluations, such as, Capital Gains, where the average student will earn \$750 per year; Spark, where 7<sup>th</sup> graders can earn up to \$500 per year; or The Paper Project, where the average student will earn \$800 per year (up to a maximum of \$2,000 per year).

Table 7 shows that enrollees spent an average of 76 hours per year on education, 77 hours on developmental activities, and 24 hours on community service (in addition to time they spent with their mentor).<sup>19</sup> Not surprisingly, the average time spent on QOP activities fell steadily from 247 hours in the first year of the demonstration to 103 hours in the fourth year. About 30% (20%) of those who had participated in QOP activities early during the demonstration and then stopped participating before the end of the fourth year reported to do so because they left high school (worked). Similarly, among the reasons given for reducing participation in QOP

<sup>18</sup> Final payments were made directly to the enrollee rather than to the postsecondary institution or to the enrollee's parents.

<sup>19</sup> In the case of community services, the lower intake was due to enrollees' lack of interest in this type of activities and case managers' belief that enrollees needed other QOP services more. Most sites decided to reallocate their resources away from community service to mentoring, case management, and educational activities.

---

activities over time were having a job (40%), family responsibilities—including own child—(20%), and other after-school activities—such as sports—(13%).<sup>20</sup> It is important to highlight, however, that almost the totality of QOP youth engaged in QOP activities, as all but 1% of enrollees spent some time on QOP activities in the first year. As youth grew up, those not engaging in QOP activities in the fourth year amounted to 26%. Among the reasons given for not participating more in QOP activities were the lack of interest (25%) and their time commitment to a job (15%).

Analysis of baseline characteristics of QOP enrollees with higher and lower levels of participation reveals that those who attended more QOP activities during the demonstration tended to have higher grades at baseline, and be age 14 or younger upon entering the 9<sup>th</sup> grade than those with lower participation.<sup>21</sup> In addition, males were more likely to be among the heavy users (as measured by participating more than 1,500 hours during the demonstration) and the light users (as measured by participating 100 or fewer hours).

### **III. Results**

#### ***A. Evaluation Framework***

The estimates reported below are intention-to-treat effects that make no adjustments for remaining involved or service participation in QOP. Two sets of estimates are presented: difference-of-means and regression adjusted estimates that control for baseline differences between treatment and control group members. Estimates use student weights to adjust for survey nonresponse. To account for correlations in the data within schools, all standard errors are clustered at the school level.

To compute difference-of-means estimates, we first estimated the impact for a school by subtracting the mean outcome among youths in the control group for that school from the mean outcome among QOP enrollees for that school (regardless of whether they remained involved in QOP and of how much they participated in QOP activities). We then estimated the impact for a site by taking a weighted average of the impacts for the schools at that site. We based the school-level weights (the  $W_{school}$ ) on the allocation of slots observed in the demonstration. In fact,  $W_{school}$  was the fraction of the site's QOP slots allocated to the particular school. Thus,

---

<sup>20</sup> This information was retrieved from the paper survey that was taken at the time youth were between 18 and 19 years old.

<sup>21</sup> Notice that caution is needed when trying to infer from these results as it is likely that bonus hours for good grades may well be concentrated among the more able youth, that is those with higher 8<sup>th</sup>-grade GPA.



---

$W_{school}$  was 1.00 for Collinwood (Cleveland), Paschal (Fort Worth), Franklin (Philadelphia), and Davis (Yakima); 0.50 for Anacostia (Washington, D.C.), Eastern (Washington, D.C.), Austin (Houston), and Yates (Houston); 0.35 for Carver (Memphis); 0.27 for Hamilton (Memphis); and 0.38 for Hillcrest (Memphis). This was our best estimate of how slots would have been allocated had the sites been part of an ongoing, national program. In such a program, as in the demonstration, CBOs in some sites would work with just one school, while CBOs in other sites would have the same number of slots, but work with two or three schools. In the latter case, the CBOs would likely allocate slots in the same way that the CBOs in the demonstration did. Finally, we estimated the impact for the whole demonstration by taking the simple average of the seven site impacts.

In a randomized experiment, the unbiased estimation of the intention-to-treat coefficients does not require the inclusion of covariates in the model. However, we include baseline covariates to gain additional precision and to control for any chance differences between the groups.

### ***B. Measurement of Performance Outcomes***

The analysis in this paper focuses in four types of outcomes: those that measure youths' high-school performance, those that measure youths' post-secondary education, those that measure youths' employment, and those that measure youths' risky behaviors.<sup>22</sup> The outcomes have been measured at three different points in time: (i) during the fifth year of the demonstration while the students were still in, or just completing, high school; (ii) over seven years after the start of the program—or over two years after the end of the program; and (iii) ten (five) years after the start (end) of the demonstration.

---

<sup>22</sup> Achievement test scores are expressed as percentiles in the distribution of scores for tenth graders in the United States. Post-secondary education includes two- and four-year college, vocational or technical school, and the armed forces. Earnings are coded as zero if the person is reported not working. This measure of earnings is one of *realized* earnings and is frequently used in the literature, despite being a crude measure of productivity—since earnings are only observed for employed individuals.

TABLE 8. Treatment Effect When Youths Were in their Late Teens

| <i>OUTCOMES</i>   | <i>Control Mean</i> | <i>School dummies</i> | <i>All controls</i>  |
|---|---------------------|-----------------------|----------------------|
| <b><i>High-School Completion and Performance</i></b>                                      |                     |                       |                      |
| <b>Earned high-school diploma</b>   | 0.3957              | 0.0669*<br>[0.0342]   | 0.0656*<br>[0.0327]  |
| <b>Earned high-school diploma or GED</b>  | 0.4900              | 0.0463<br>[0.0351]    | 0.0522<br>[0.0323]   |
| <b>Math test scores (percentile)</b>  | 40.4696             | 0.3861<br>[0.4881]    | 0.5156<br>[0.4596]   |
| <b>Reading test scores (percentile)</b>   | 42.7315             | 0.5120<br>[0.5281]    | 0.5744<br>[0.5141]   |
| <b>GPA</b>  | 2.1927              | -0.0602<br>[0.0454]   | -0.0444<br>[0.0429]  |
| <b><i>Post-Secondary Activities</i></b>   |                     |                       |                      |
| <b>Attending or accepted in college</b>   | 0.2547              | 0.0561*<br>[0.0316]   | 0.0569*<br>[0.0325]  |
| <b>Attending college</b>  | 0.1809              | 0.0335<br>[0.0278]    | 0.0317<br>[0.0300]   |
| <b>Attending post-secondary education</b>   | 0.2612              | 0.0573*<br>[0.0320]   | 0.0511*<br>[0.0329]  |
| <b>Attending post-secondary education, high-school, GED class, or employed in any job</b> | 0.8037              | 0.0346<br>[0.0283]    | 0.0349<br>[0.0284]   |
| <b><i>Risky Behaviors</i></b>   |                     |                       |                      |
| <b>Binge drinking in the past 30 days</b>   | 0.2026              | 0.0408<br>[0.0297]    | 0.0289<br>[0.0365]   |
| <b>Used any illegal drug in the past 30 days</b>  | 0.2756              | 0.0653**<br>[0.0328]  | 0.0724**<br>[0.0351] |
| <b>Committed a crime in past 12 months</b>  | 0.2842              | 0.0305<br>[0.0333]    | 0.0357<br>[0.0340]   |
| <b>Ever arrested or charged</b>   | 0.2933              | -0.0478<br>[0.0327]   | -0.0288<br>[0.0287]  |
| <b>Have first child before age 18</b>   | 0.2572              | -0.0322<br>[0.0307]   | -0.0376<br>[0.0276]  |
| <b>Sample size<sup>a</sup></b>  | 891                 | 891                   | 891                  |

*Notes:* The table reports estimates of treatment effects on the dependent variables indicated in row headings. Estimated standard errors, clustered by school, are reported in brackets. “All controls” includes an indicator for being male, an indicator for being over age 14 when entering ninth grade, an indicator for being in the middle third of the eighth-grade GPA distribution, and an indicator for being in the top third of the eight-grade GPA distribution. \*, \*\* Estimate significantly different from zero at the 90% or 95% confidence level, two-tailed test.

<sup>a</sup>Except for high school diploma or GED, in which data has been complemented with high-school transcript information.

### ***C. Overall Results***

Table 8 presents overall estimates of the effect of QOP measured when youths were in their late teens. Similar to recent studies, QOP appears to be quite effective in the short-run in terms of high-school completion. About 40 percent of the youths in the control group had graduated from high-school at the end of the demonstration. At that time, youths in the treatment group were 7 percentage points more likely to have graduated from high-school than youths in the control

---

group, implying an improvement of 17.5 percent. This effect is significantly different from zero. However, beyond the statistically significant impact on high-school graduation, QOP did not significantly improve grades, or achievement test scores. Although QOP might not have raised grades if QOP enrollees were taking more challenging courses than youths in the control group, it ought to have increased standardized test scores if it had an impact on improvement of youths' achievement. These results are not far from those found by Lavy and Schlosser, 2005, who study the effect of targeted remedial education on underperforming teenagers, and find that it increased the probability of earning a matriculation certificate by 12 percentage points (corresponding to a 22% improvement), but had no effects on achievement. Although Lavy and Schlosser, 2005, did find that program participants gained on average two additional credits without lowering their average score; we found no evidence that QOP significantly increased the number of credits taken, as the estimated treatment effect on credits taken is 0.4556 (s.e. = 0.6579), with the control group mean being 15.7686 credits.

An alternative measure of program success is enrollment in post-secondary activities. At the end of the demonstration, 18 percent of the youths in the control group were attending college, and 26 percent were attending post-secondary education. QOP was successful in engaging youths in post-secondary activities, as youths in the treatment group were 6 percentage points more likely to attend (or being accepted in) college, and about 6 percentage points more likely to attend post-secondary education than youths in the control group, implying an improvement of 17.5 percent and 23 percent, respectively. Both of these estimates are statistically significantly different from zero.

In terms of risky behaviors, QOP did not significantly reduce the likelihood of binge drinking, committing a crime, being arrested or charged with a crime, or having a child before the age of 18. A quite puzzling result is that QOP had an overall detrimental effect on substance abuse. This estimate is statistically significant. One possible explanation for this result is that QOP enrollees were more accurate in reporting risky behaviors than youths in the control group. An alternative explanation is that QOP indirectly subsidized the engagement in risky behaviors by offering financial rewards for participating in educational activities.

TABLE 9. Treatment Effect When Youths Were in their Early Twenties

| <i>OUTCOMES</i>   | <i>Control Mean</i> | <i>School dummies</i>  | <i>All controls</i>    |
|---|---------------------|------------------------|------------------------|
| <b><i>High-School Completion and Performance</i></b>                              |                     |                        |                        |
| <b>Earned high-school diploma</b>   | 0.6356              | -0.0309<br>[0.0345]    | -0.0068<br>[0.0337]    |
| <b>Earned high-school diploma or GED</b>  | 0.7629              | -0.0016<br>[0.0365]    | 0.0179<br>[0.0284]     |
| <b><i>Post-Secondary Activities</i></b>   |                     |                        |                        |
| <b>Ever in a 4-year college</b>   | 0.1228              | 0.0308<br>[0.0251]     | 0.0381<br>[0.0357]     |
| <b>Ever in 2- or 4-year college</b>   | 0.3026              | 0.0668*<br>[0.0356]    | 0.0792*<br>[0.0377]    |
| <b>Ever in post-secondary education</b>   | 0.5303              | 0.0855**<br>[0.0390]   | 0.0957**<br>[0.0362]   |
| <b>Attending 4-year college</b>   | 0.0723              | 0.0240<br>[0.0201]     | 0.0251<br>[0.0213]     |
| <b>Attending 2- or 4-year college</b>   | 0.1592              | 0.0092<br>[0.0286]     | 0.0204<br>[0.0311]     |
| <b>Attending post-secondary education</b>   | 0.2602              | 0.0388<br>[0.0349]     | 0.0471<br>[0.0363]     |
| <b>Has a job</b>  | 0.7201              | -0.0670*<br>[0.0375]   | -0.059*<br>[0.0390]    |
| <b>Has a full-time job</b>  | 0.5623              | -0.1118***<br>[0.0392] | -0.1062***<br>[0.0371] |
| <b>Has a full-time job with health insurance</b>                                  | 0.3836              | -0.0756**<br>[0.0368]  | -0.0717**<br>[0.0334]  |
| <b>Has a full-time job with health insurance that pays at least \$10 per hour</b> | 0.1483              | -0.0203<br>[0.0267]    | -0.0175<br>[0.0270]    |
| <b>Attending post-secondary education, or employed in any job</b>                 | 0.7847              | -0.0102<br>[0.0342]    | -0.0067<br>[0.0350]    |
| <b>Usual hours worked per week</b>  | 27.2815             | -3.1013*<br>[1.5866]   | -3.1013*<br>[1.5866]   |
| <b>Hourly earnings at main current job (dollars)</b>                              | 8.2013              | -1.9664<br>[1.2767]    | -1.9664<br>[1.2767]    |
| <b><i>Risky Behaviors</i></b>   |                     |                        |                        |
| <b>Binge drinking in the past 30 days</b>   | 0.3075              | -0.0577<br>[0.0355]    | -0.0522<br>[0.0310]    |
| <b>Used any illegal drug in the past 30 days</b>                                  | 0.1786              | -0.0610**<br>[0.0294]  | -0.0600**<br>[0.0261]  |
| <b>Committed a crime in past 3 months</b>   | 0.0936              | -0.0179<br>[0.0241]    | -0.0077<br>[0.0181]    |
| <b>Arrested or charged in past 3 months</b>                                       | 0.0520              | -0.0018<br>[0.0194]    | -0.0077<br>[0.0181]    |
| <b>Have first child before age 18</b>   | 0.1548              | 0.0343<br>[0.0302]     | 0.0269<br>[0.0333]     |
| <b>Currently receiving welfare or food stamps</b>                                 | 0.2013              | 0.0412<br>[0.0326]     | 0.0308<br>[0.0355]     |
| <b>Sample size<sup>a</sup></b>  | 787                 | 787                    | 787                    |

*Notes:* The table reports estimates of treatment effects on the dependent variables indicated in row headings. Estimated standard errors, clustered by school, are reported in brackets. “All controls” includes an indicator for being male, an indicator for being over age 14 when entering ninth grade, an indicator for being in the middle third of the eighth-grade GPA distribution, and an indicator for being in the top third of the eighth-grade GPA distribution. \*, \*\*, \*\*\* Estimate significantly different from zero at the 90%, 95% or 99% confidence level, two-tailed test.

<sup>a</sup>Except for high school diploma or GED, in which data has been complemented with 1<sup>st</sup> telephone survey and high-school transcript information.

---

An important consideration in the evaluation of educational interventions is whether or not the changes generated by the interventions persist over time and last beyond the period in which the intervention is administered. To investigate this question, Table 9 reports estimates two years after the end of the program (and three after youths' scheduled high-school graduation time).

At the time of the 2<sup>nd</sup> telephone survey, the average positive effect of QOP on high-school completion has faded away. The results on high-school graduation are explained by a larger number of control group members earning a high-school diploma between the 1<sup>st</sup> and 2<sup>nd</sup> telephone survey. While the likelihood of earning such a degree for QOP enrollees has increased by 14 percentage points (to 60%) during this time, the control group's percentage increased by 24 percentage points (to 64%). The fact that the schooling gap closes does not need to be necessarily a discouraging result as it appears that QOP got youths in the treatment group to get their high-school degrees sooner than youths in the control group. Moreover, it is likely that many of the late control diplomas are really GEDs even if respondents did not identify them as such. Indeed, when 2<sup>nd</sup> survey responses were contrasted (and corrected) with transcript information (as opposed to taking the survey responses as accurate), QOP's effect on high-school completion is a positive (albeit non significant) 0.0334 (s.e. = 0.0329) with the control group mean dropping to 49.31 percent of high-school graduates.

Some advantage remains for QOP youths as they are 7 percentage points (9 percentage points) more likely than youths from the control group to have ever attended college (post-secondary education), implying a 23% (17%) improvement. Both of these estimates are significantly different from zero. At the same time, QOP has a negative effect on overall employment, as participants were 7 percentage points less likely to have a job than youths in the control group, representing a 10% reduction in the likelihood of working. In addition, youths in the treatment group are 11 percentage points less likely to have a full-time job and 8 percentage points less likely to have a full-time job with health insurance than youths in the control group. These significant and negative impacts on employment may be due to the usual locking-in effects of training programs.<sup>23</sup> Further analysis on longer-term impacts may help interpreting these results.

In terms of risky behaviors, QOP enrollees in their early twenties were significantly less

---

<sup>23</sup> For a discussion on locking-in effects in the training literature, see Kluve 2006.

---

likely to use illegal drugs than youths in the control group. However, results based on the same survey found that QOP did not significantly reduce the likelihood of binge drinking, committing a crime, being arrested or charged with a crime, or having a child before the age of 18. It is worth mentioning that, although the insignificant coefficient on binge drinking shows a sizeable beneficial effect of QOP, this estimate is mainly driven by a suspiciously low probability among QOP youths from the Philadelphia school. Excluding this school leads to no impact on binge drinking (the impact drops to -1 percentage points and remains statically insignificant).<sup>24</sup>

Table 10 reports the longer-term impacts of QOP measured 5 years after the end of the demonstration. Not surprisingly the results on high-school graduation are not any different from those measured during the 2<sup>nd</sup> survey, since high-school graduation usually takes place during the late-teens or (at most) early-twenties. Indeed, they show that while QOP had a large and significant effect on high-school graduation at the end of the demonstration, this beneficial effect faded away a couple of years later. That said, QOP seemed to have gotten enrolled youths out of high-school and with a degree sooner than youths in the control group. Moreover, as suggested earlier, it is likely that many of the late control diplomas are really GEDs even if respondents did not identify them as such. Indeed, when responses from the 3<sup>rd</sup> survey were contrasted (and corrected) with transcript information (as opposed to taking the survey responses as accurate), QOP's effect on high-school completion is a non-significant 0.0380 (s.e. = 0.0330) with the control group mean dropping to 47.81 percent of high-school graduates—these estimates are very similar to those obtained with the corrected 2<sup>nd</sup> survey responses.

By the time of the 3<sup>rd</sup> survey, only the regression-adjusted coefficient on ever attending post-secondary education attendance is statistically significant indicating that youths in the treatment group were 7 percentage points (or 12.5%) more likely to have ever attended post-secondary training than youths in the control group. The departure of the long-term findings from the medium-term ones seems to be primarily due to a larger number of control group members than QOP enrollees engaging in post-secondary education in the two years between the 2<sup>nd</sup> and 3<sup>rd</sup> telephone surveys. While the percentage of QOP enrollees who were ever engaged in any post-secondary education or training increased by only 1 percentage point (to 62 percent) during this time, the control group percentage increased by 3 percentage points (to 56 percent).

---

<sup>24</sup> Only 5% of QOP youth reported binge drinking in the Philadelphia compared to a 28% average in the other schools. Moreover, in the Philadelphia school the percentage of youth reporting binge drinking when they were in their late teens and in their mid-twenties was 19% and 23%, respectively).

TABLE 10. Treatment Effect When Youths Were in their Mid-Twenties

| <i>OUTCOMES</i>   | <i>Control Mean</i> | <i>School dummies</i> | <i>All controls</i> |
|---|---------------------|-----------------------|---------------------|
| <b><i>High-School Completion and Performance</i></b>                                      |                     |                       |                     |
| <b>Earned high-school diploma</b>   | 0.5956              | 0.0038<br>[0.0335]    | 0.0250<br>[0.0331]  |
| <b>Earned high-school diploma or GED</b>  | 0.7543              | 0.0226<br>[0.0301]    | 0.0413<br>[0.0278]  |
| <b><i>Post-Secondary Activities</i></b>   |                     |                       |                     |
| <b>Earned a bachelor's degree</b>   | 0.0198              | 0.0056<br>[0.0107]    | 0.0120<br>[0.0117]  |
| <b>Earned a bachelor's or associate degree</b>  | 0.0676              | -0.0075<br>[0.0179]   | -0.0044<br>[0.0179] |
| <b>Completed 2 years of college or military service, or completed vocational training</b> | 0.3029              | 0.0495<br>[0.0378]    | 0.0570<br>[0.0387]  |
| <b>Ever in a 4-year college</b>   | 0.1510              | 0.0112<br>[0.0265]    | 0.0200<br>[0.0336]  |
| <b>Ever in 2- or 4-year college</b>   | 0.3434              | 0.0378<br>[0.0363]    | 0.0472<br>[0.0342]  |
| <b>Ever in post-secondary education</b>   | 0.5559              | 0.0580<br>[0.0395]    | 0.0695*<br>[0.0369] |
| <b>Ever employed</b>  | 0.9540              | 0.0017<br>[0.0169]    | 0.0022<br>[0.0219]  |
| <b>Attending 4-year college</b>   | 0.0904              | -0.0179<br>[0.0201]   | 0.0147<br>[0.0169]  |
| <b>Attending 2- or 4-year college</b>   | 0.1670              | -0.0375<br>[0.0267]   | -0.0347<br>[0.0238] |
| <b>Post-secondary education</b>   | 0.2398              | -0.0096<br>[0.0321]   | -0.0049<br>[0.0324] |
| <b>Has a job</b>  | 0.6764              | -0.0066<br>[0.0382]   | -0.0049<br>[0.0376] |
| <b>Has a full-time job</b>  | 0.5318              | -0.0039<br>[0.0404]   | 0.0012<br>[0.0385]  |
| <b>Has a job with health insurance</b>  | 0.4723              | -0.0325<br>[0.0396]   | -0.0256<br>[0.0387] |
| <b>Has a job with paid time off</b>   | 0.4507              | -0.0223<br>[0.0391]   | -0.0256<br>[0.0387] |
| <b>Has a job with a pension and retirement benefits</b>                                   | 0.3774              | -0.0129<br>[0.0382]   | -0.0063<br>[0.0368] |
| <b>Total earnings in past 12 months (dollars)</b>   | 13,198              | -522<br>[1,222]       | -663<br>[1,122]     |
| <b>Hourly earnings at main current job (dollars)</b>                                      | 9.1352              | -1.2035<br>[1.1982]   | -1.2436<br>[1.2326] |
| <b><i>Risky Behaviors</i></b>   |                     |                       |                     |
| <b>Binge drinking in the past 30 days</b>   | 0.3100              | 0.0020<br>[0.0376]    | 0.009<br>[0.0342]   |
| <b>Used any illegal drug in the past 30 days</b>  | 0.1275              | -0.0042<br>[0.0275]   | -0.0121<br>[0.0254] |
| <b>Committed a crime in past 3 months</b>   | 0.0206              | 0.0273*<br>[0.0162]   | 0.0181<br>[0.0133]  |
| <b>Arrested or charged in past 2 years</b>  | 0.0514              | 0.0609**<br>[0.0237]  | 0.0583<br>[0.0362]  |
| <b>Have first child before age 18</b>   | 0.1588              | 0.0235<br>[0.0299]    | 0.0177<br>[0.0294]  |
| <b>Currently receiving welfare or food stamps</b>   | 0.2447              | 0.0263<br>[0.0350]    | 0.0322<br>[0.0334]  |

| Sample size <sup>a</sup> | 791 | 791 | 791 |
|--------------------------|-----|-----|-----|
|--------------------------|-----|-----|-----|

*Notes:* The table reports estimates of treatment effects on the dependent variables indicated in row headings. Estimated standard errors, clustered by school, are reported in brackets. “All controls” includes an indicator for being male, an indicator for being over age 14 when entering ninth grade, an indicator for being in the middle third of the eighth-grade GPA distribution, and an indicator for being in the top third of the eighth-grade GPA distribution.

\* \*\*, \*\*\* Estimate significantly different from zero at the 90% , 95% or 99% confidence level, two-tailed test.

<sup>a</sup>Except for high school diploma or GED, in which data has been complemented with 1<sup>st</sup> telephone survey and high-school transcript information.

Similar differences are seen for college enrollment, with the gap between the QOP enrollees and the control group narrowing by 3 percentage points between the two surveys. The changes seen over time are perhaps not surprising given that many students do not engage in post-secondary education directly after high school. For example, over 10% of 1999-2000 bachelor’s degree recipients enrolled in college more than 2 years after high school graduation (U.S. Department of Education 2003). Finally, it is important to notice that by the time of the 3<sup>rd</sup> survey, the size of the estimates on having a job or a full-time job is negligible (and the coefficients are not significant) suggesting that the detrimental effects of QOP on employment observed in the 2<sup>nd</sup> survey were likely to be locking-in effects.

In terms of risky behaviors, although estimates from Table 10 show that QOP significantly increased the likelihood of committing a crime or being arrested when enrollees were in their mid-twenties, these findings are not robust. For instance, the impact on committing a crime in the three months before the survey is no longer significant after we adjust for multiple comparisons, and neither this impact nor the impact on being arrested or charged with a crime in the two year prior to the survey is robust to regression adjustment for random baseline differences between the QOP and control groups.

#### ***D. Gender Differential Effects***

Tables 11 through 13 consider the effects of heterogeneity with respect to sex. As is frequently found in the literature, QOP had a significantly differential effect by sex. Such differential pattern was statistically significant in the short-, medium-, and longer-term. Table 11 shows that the overall impact on high-school graduation when youths were in their late teens is driven entirely by a large and significant effect on women. Women in the treatment group were 9 percentage points more likely to have graduated from high-school at the end of the demonstration, implying a 21 percent improvement. In contrast, the estimated effect on men is



TABLE 11. Treatment Effect When Youths Were in their Late Teens by Gender

| <i>OUTCOMES</i>   | <i>Men</i>          |                                  | <i>Women</i>        |                                   |
|---|---------------------|----------------------------------|---------------------|-----------------------------------|
|   | <i>Control Mean</i> | <i>Treatment Effect</i>          | <i>Control Mean</i> | <i>Treatment Effect</i>           |
| <i>High-School Completion and Performance</i>   |                     |                                  |                     |                                   |
| <b>Earned high-school diploma</b>   | 0.3721              | 0.0352<br>[0.0478]               | 0.4202              | 0.0903*<br>[0.0510]               |
| <b>Earned high-school diploma or GED</b>  | 0.4892              | 0.0048<br>[0.0495]               | 0.4962              | 0.0667<br>[0.0520]                |
| <b>Math test scores (percentile)</b>  | 40.8452             | 0.7190<br>[0.7244]               | 40.1520             | 0.0806<br>[0.6909]                |
| <b>Reading test scores (percentile)</b>   | 41.7990             | 1.0994<br>[0.7503]               | 43.7407             | 0.0618<br>[0.7712]                |
| <b>GPA</b>  | 2.1796              | -0.1276**<br>[0.0641]            | 2.2294              | -0.0221<br>[0.0645]               |
| <i>Post-Secondary Activities</i>  |                     |                                  |                     |                                   |
| <b>Attending or accepted in college</b>   | 0.2420              | 0.0361<br>[0.0316]               | 0.2808              | 0.0520<br>[0.0458]                |
| <b>Attending college</b>  | 0.1628              | 0.0322<br>[0.0390]               | 0.2132              | 0.0193<br>[0.0430]                |
| <b>Attending post-secondary education</b>   | 0.2379              | 0.0687<br>[0.0443]               | 0.2958              | 0.0352<br>[0.0492]                |
| <b>Attending post-secondary education, high-school, GED class, or employed in any job</b> | 0.8449              | -0.0166 <sup>†</sup><br>[0.0400] | 0.7667              | 0.0920** <sup>†</sup><br>[0.0407] |
| <i>Risky Behaviors</i>  |                     |                                  |                     |                                   |
| <b>Binge drinking in the past 30 days</b>   | 0.2296              | 0.0721*<br>[0.0434]              | 0.1586              | -0.0000<br>[0.0390]               |
| <b>Used any illegal drug in the past 30 days</b>  | 0.3260              | 0.0739<br>[0.0476]               | 0.1935              | 0.0725<br>[0.0448]                |
| <b>Committed a crime in past 12 months</b>  | 0.3793              | 0.0537<br>[0.0507]               | 0.1611              | 0.0190<br>[0.0398]                |
| <b>Ever arrested or charged</b>   | 0.4107              | -0.0790<br>[0.0506]              | 0.1574              | 0.0026<br>[0.0387]                |
| <b>Have first child before age 18</b>   | 0.1756              | -0.0278<br>[0.0394]              | 0.3662              | -0.0543<br>[0.0502]               |
| Sample size <sup>a</sup>  | 470                 | 470                              | 421                 | 421                               |

*Notes:* The table reports estimates of treatment effects on the dependent variables indicated in row headings using school dummies. Estimated standard errors, clustered by school, are reported in brackets.

\*, \*\* Estimate significantly different from zero at the 90% or 95% confidence level, two-tailed test.

<sup>a</sup>Except for high school diploma or GED, in which data has been complemented with high-school transcript information.

smaller and not significantly different from zero. However, beyond the statistically significant impact on high-school graduation, QOP did not significantly improve grades, or achievement test scores for women. Perhaps more concerning, we find one significant detrimental effect on males' high-school achievement: QOP decreased their GPA average by 0.13 (s.e. = 0.0641). In terms of post-secondary education when youths were in their late-teens, there is no evidence of

---

gender differential effects except for women in the treatment group being 9 percentage points more likely of not being idle (defined as being engaged in a post-secondary activity, enrolled in a GED class, or working) than women in the control group. Again the estimated effect on men is small (negative) and not significant.

Finally, we find a detrimental statistically significant effect of QOP on males' binge drinking: QOP increased males likelihood of binge drinking in the past month by 7 percentage points. No effect is found among women. While stipends were intended to induce enrolled youth to participate in QOP activities, they may well have had a troublesome effect on substance abuse by providing income that could have been used to purchase alcohol and drugs. In other words, QOP may have indirectly financed the engagement in risky behaviors. There is a growing body of evidence showing that youths are very responsive to economic incentives, such as prices, when deciding to undertake risky behaviors (Nisbet and Vakil, 1972; Cook, 1981; Cook and Tauchen, 1982; Coate and Arluck, 1987; Saffer and Grossman, 1987a, 1987b; Coate and Grossman, 1988; Kenkel, 1993; Cook and Moore, 1993, 2000; Grossman et al., 1987, 1994; Chaplounka and Wechler, 1996; Ruhm, 1996; Evans and Huang, 1998; Markowitz and Grossman, 1998; Gruber, 2001; Gruber and Zinman, 2001; Pacula *et al.*, 2001; Levine, 2001; Ruhm, 2005; Ruhm and Black, 2005; and Grossman 2005). Moreover, Gruber and Zinman, 2001, find that substance abuse is much more responsive to price for black youths and disadvantaged youths than for white teens and teens from higher socio-economic backgrounds. Their findings suggest a strong correlation between price sensitivity and socioeconomic status (and in particular lower income)—see Evans *et al.*, 1999, and Gruber and Zinman, 2001, for evidence corroborating this hypothesis—, and implies that perverse effects of economic incentives may be particularly concerning among QOP's targeted population, which is an extremely disadvantaged group.

An alternative possible explanation is that the developmental activities, which included many life skills training activities designed to reduce the youths' likelihood of engaging in risky behaviors, had a perverse effect on substance abuse. According to behavioral economists, raising perceptions of the severity of bad outcomes may increase the person's level of the risky activity if she reacts in a fatalistic way or if she engages in riskier substitute behaviors (O'Donoghue and Rabin, 2001). Indeed, preaching the dangers of marijuana use can cause more harm than good if: (*i*) it induces young people who use marijuana to exaggerate the degree to which their lives are ruined already and hence to increase the use of drugs if she uses them at all;

TABLE 12. Treatment Effect When Youths Were in their Early Twenties by Gender

| <i>OUTCOMES</i>   | <i>Men</i>          |                         | <i>Women</i>        |                         |
|---|---------------------|-------------------------|---------------------|-------------------------|
|   | <i>Control Mean</i> | <i>Treatment Effect</i> | <i>Control Mean</i> | <i>Treatment Effect</i> |
| <i>High-School Completion and Performance</i>                                     |                     |                         |                     |                         |
| <b>Earned high-school diploma</b>   | 0.5802              | -0.0109<br>[0.0505]     | 0.6889              | -0.0545<br>[0.0501]     |
| <b>Earned high-school diploma or GED</b>  | 0.6148              | -0.0127<br>[0.0628]     | 0.8144              | 0.0308<br>[0.0324]      |
| <i>Post-Secondary Activities</i>  |                     |                         |                     |                         |
| <b>Ever in a 4-year college</b>   | 0.1192              | 0.0334<br>[0.0383]      | 0.1267              | 0.0148<br>[0.0342]      |
| <b>Ever in 2- or 4-year college</b>   | 0.2474              | 0.0897*<br>[0.0495]     | 0.3710              | 0.0320<br>[0.0545]      |
| <b>Ever in post-secondary education</b>   | 0.5006              | 0.0590<br>[0.0562]      | 0.5699              | 0.0958*<br>[0.0530]     |
| <b>Attending 4-year college</b>   | 0.0502              | 0.0486*<br>[0.0281]     | 0.0938              | -0.0087<br>[0.0297]     |
| <b>Attending 2- or 4-year college</b>   | 0.1181              | 0.0548<br>[0.0374]      | 0.1963              | -0.0260<br>[0.0441]     |
| <b>Post-secondary education</b>   | 0.2492              | 0.0447<br>[0.0485]      | 0.2716              | 0.0331<br>[0.0513]      |
| <b>Has a job</b>  | 0.7825              | -0.1023*<br>[0.0531]    | 0.6551              | -0.0170<br>[0.0547]     |
| <b>Has a full-time job</b>  | 0.6225              | -0.0925<br>[0.0586]     | 0.4925              | -0.1243**<br>[0.0554]   |
| <b>Has a full-time job with health insurance</b>                                  | 0.4522              | -0.1015*<br>[0.0565]    | 0.3019              | -0.0380<br>[0.0490]     |
| <b>Has a full-time job with health insurance that pays at least \$10 per hour</b> | 0.1898              | -0.0258<br>[0.0445]     | 0.1017              | 0.0052<br>[0.0343]      |
| <b>Attending post-secondary education, or employed in any job</b>                 | 0.8294              | -0.0297<br>[0.0499]     | 0.7390              | 0.0176<br>[0.0496]      |
| <b>Usual hours worked per week</b>  | 30.0336             | -2,9544<br>[2,3707]     | 24.0723             | -2.7625<br>[2.1372]     |
| <b>Hourly earnings at main current job (dollars)</b>                              | 10.6069             | -4.1811**††<br>[2.1184] | 5.4438              | 0.8017†<br>[0.7046]     |
| <i>Risky Behaviors</i>  |                     |                         |                     |                         |
| <b>Binge drinking in the past 30 days</b>   | 0.4220              | -0.1204**<br>[0.0540]   | 0.1896              | -0.0243<br>[0.0437]     |
| <b>Used any illegal drug in the past 30 days</b>                                  | 0.2369              | -0.0822*<br>[0.0497]    | 0.1123              | -0.0320<br>[0.0325]     |
| <b>Committed a crime in past 3 months</b>   | 0.1493              | -0.0321<br>[0.0418]     | 0.0290              | -0.0100<br>[0.0172]     |
| <b>Arrested or charged in past 3 months</b>                                       | 0.0707              | 0.0051<br>[0.0354]      | 0.0375              | -0.0243<br>[0.0172]     |
| <b>Have first child before age 18</b>   | 0.0793              | 0.0518<br>[0.0410]      | 0.2558              | -0.0001<br>[0.0491]     |
| <b>Currently receiving welfare or food stamps</b>                                 | 0.1180              | 0.0332<br>[0.0452]      | 0.3135              | 0.0041<br>[0.0494]      |
| Sample size <sup>a</sup>  | 397                 | 397                     | 390                 | 390                     |

Notes: The table reports estimates of treatment effects on the dependent variables indicated in row headings. Estimated standard errors, clustered by school, are reported in brackets. "All controls" includes an indicator for being male, an indicator for being over age 14 when entering ninth grade, an indicator for being in the middle third of the eighth-grade GPA distribution, and an indicator for being in the top third of the eighth-grade GPA distribution.

\*, \*\*, \*\*\* Estimate significantly different from zero at the 90% , 95% or 99% confidence level, two-tailed test.

<sup>a</sup>Except for high school diploma or GED, in which data has been complemented with 1<sup>st</sup> telephone survey and high-school transcript information.

---

(ii) it leads them to underestimate the additional harm of cocaine or, worse yet, if (iii) it leads them to substitute cocaine for marijuana to begin with. According to O’Donoghue and Rabin, the fatalistic reaction identified above is likely to be important when a person suffers from an overly strong taste for immediate gratification or from projection bias—defined by Loewenstein *et al.*, 1999, as the capacity that people have to under appreciate the effects of changes in their states and hence falsely project their current consumption preferences onto their future preferences. Since males are known to have less self-discipline (Duckworth and Seligman, 2006), have a stronger taste for immediate gratification (Silverman, 2003), or have higher discount rates than young women (Warner and Pleeter, 2001), the perverse effects of raising perception of riskiness ought to be greater among them than females, as we find in our results.

When youths were in their mid-twenties, we continue to find a gender differential effect. In particular, we find that QOP’s positive overall effect on college enrollment is mainly driven by men, whereas QOP’s overall positive effect on post-secondary education enrollment is stronger for women, as shown in Table 12. Moreover, men in the treatment group are 5 percentage point more likely to attend a four-year college than those in the control group. This coefficient is statistically significant, whereas the estimated effect on women’s attendance to a four-year college is small (negative) and not significant. Again there is gender heterogeneity when looking at employment outcomes. The negative overall effects on employment and full-time employment in a job with health insurance is mainly driven by men. In each case, the estimated effect is a significant 10 percentage points for men, but a much smaller and not significant coefficient for women. In contrast, women in the treatment group are 12 percentage points less likely to be working full-time. We also find evidence that males in the treatment group were earning \$4 less per hour than males in the control group. For women, this coefficient is not significant (and positive). In terms of risky behavior, QOP statistically decreased the likelihood of binge drinking and using illegal drugs in the past month of males, indicating that any detrimental effect on substance abuse observed during the late-teens is short-lived and (even) reverse.

While the overall estimates do not show evidence of QOP increasing the likelihood of getting a GED degree by the time youths were in their mid-twenties, Table 13 shows a differential gender effect. QOP significantly increased the likelihood of men getting their GED

TABLE 13. Treatment Effect When Youths Were in their Mid-Twenties by Gender

| <i>OUTCOMES</i>  | <i>Men</i>          |                                       | <i>Women</i>        |                                      |
|--|---------------------|---------------------------------------|---------------------|--------------------------------------|
|  | <i>Control Mean</i> | <i>Treatment Effect</i>               | <i>Control Mean</i> | <i>Treatment Effect</i>              |
| <i>High-School Completion and Performance</i>                                      |                     |                                       |                     |                                      |
| Earned high-school diploma   | 0.54                | 0.01<br>[0.0333]                      | 0.66                | -0.01<br>[0.0333]                    |
| Earned high-school diploma or GED  | 0.71                | 0.07*<br>[0.0337]                     | 0.79                | -0.02<br>[0.0337]                    |
| <i>Post-Secondary Activities</i>   |                     |                                       |                     |                                      |
| Earned a bachelor's degree   | 0.0167              | -0.0055<br>[0.0118]                   | 0.0238              | 0.0202<br>[0.0201]                   |
| Earned a bachelor's or associate degree  | 0.0405              | 0.0015<br>[0.0192]                    | 0.1050              | -0.0220<br>[0.0339]                  |
| Completed 2 years of college or military service, or completed vocational training | 0.2833              | 0.0406<br>[0.0540]                    | 0.3379              | 0.0412<br>[0.0535]                   |
| Ever in a 4-year college   | 0.1335              | -0.0008<br>[0.0355]                   | 0.1709              | 0.0243<br>[0.0422]                   |
| Ever in 2- or 4-year college   | 0.2714              | 0.0648<br>[0.0495]                    | 0.4369              | -0.0135<br>[0.0363]                  |
| Ever in post-secondary education   | 0.5308              | 0.0359<br>[0.0574]                    | 0.6069              | 0.0564<br>[0.0545]                   |
| Ever employed  | 0.9425              | 0.0055<br>[0.0264]                    | 0.9635              | -0.0006<br>[0.0237]                  |
| Attending 4-year college   | 0.0595              | 0.0155 <sup>†</sup><br>[0.0248]       | 0.1220              | -0.0582* <sup>†</sup><br>[0.0322]    |
| Attending 2- or 4-year college   | 0.1109              | 0.0119 <sup>††</sup><br>[0.0333]      | 0.2332              | -0.1099*** <sup>††</sup><br>[0.0423] |
| Post-secondary education   | 0.1971              | 0.0428 <sup>†</sup><br>[0.0457]       | 0.2906              | -0.0788* <sup>†</sup><br>[0.0321]    |
| Has a job  | 0.7534              | -0.0660<br>[0.0541]                   | 0.5845              | 0.0611<br>[0.0564]                   |
| Has a full-time job  | 0.6373              | -0.0396<br>[0.0586]                   | 0.4067              | 0.0497<br>[0.0557]                   |
| Has a job with health insurance  | 0.5532              | -0.1460*** <sup>†††</sup><br>[0.0585] | 0.3838              | 0.1026* <sup>†††</sup><br>[0.0544]   |
| Has a job with paid time off   | 0.4898              | -0.1069* <sup>††</sup><br>[0.0576]    | 0.4035              | 0.0844 <sup>††</sup><br>[0.0544]     |
| Has a job with a pension and retirement benefits                                   | 0.4520              | -0.1077* <sup>††</sup><br>[0.0575]    | 0.2989              | 0.0906* <sup>††</sup><br>[0.0528]    |
| Total earnings in past 12 months (dollars)   | 16,012              | -1,479<br>[1,933]                     | 10,035              | 862<br>[1,582]                       |
| Hourly earnings at main current job (dollars)                                      | 10.1251             | -1.4146<br>[1.3759]                   | 7.5904              | -0.6756<br>[1.7465]                  |
| <i>Risky Behaviors</i>   |                     |                                       |                     |                                      |
| Binge drinking in the past 30 days   | 0.4552              | -0.0462<br>[0.0568]                   | 0.1497              | 0.0437<br>[0.0447]                   |
| Used any illegal drug in the past 30 days  | 0.2013              | -0.0364<br>[0.0466]                   | 0.0443              | 0.0260<br>[0.0244]                   |
| Committed a crime in past 3 months   | 0.0245              | 0.0461* <sup>†</sup><br>[0.0379]      | 0.0162              | -0.0059 <sup>†</sup><br>[0.0175]     |
| Arrested or charged in past 2 years  | 0.1635              | 0.1045*** <sup>†††</sup><br>[0.0464]  | 0.0571              | -0.0375* <sup>†††</sup><br>[0.0209]  |
| Have first child before age 18   | 0.0597              | 0.0334<br>[0.0293]                    | 0.2730              | -0.0014<br>[0.0510]                  |

|   |        |                    |        |                    |
|---|--------|--------------------|--------|--------------------|
| <b>Currently receiving welfare or food stamps</b> | 0.1286 | 0.0301<br>[0.0434] | 0.3943 | 0.0073<br>[0.0545] |
| <b>Sample size<sup>a</sup></b>                    | 406    | 406                | 385    | 385                |

*Notes:* The table reports estimates of treatment effects on the dependent variables indicated in row headings. Estimated standard errors, clustered by school, are reported in brackets. “All controls” includes an indicator for being male, an indicator for being over age 14 when entering ninth grade, an indicator for being in the middle third of the eighth-grade GPA distribution, and an indicator for being in the top third of the eighth-grade GPA distribution.

\*, \*\*, \*\*\* Estimate significantly different from zero at the 90% , 95% or 99% confidence level, two-tailed test.

<sup>a</sup>Except for high school diploma or GED, in which data has been complemented with 1<sup>st</sup> telephone survey and high-school transcript information.

diploma by 7 percentage points, but it had no statistically significant effect for women. Similarly, QOP also seemed to have affected post-secondary education and employment outcomes differently for its female than for its male enrollees. For women, the large, significant, and beneficial effect of QOP on their likelihood of ever attending post-secondary education observed at the time of the 2<sup>nd</sup> survey declines and is no longer statistically significant at the time of the 3<sup>rd</sup> survey when youths are in their mid-twenties. The departure of the longer-term findings from the medium-term ones seems to be primarily due to a larger number of control group members than QOP enrollees having engaged in post-secondary education in the two years between the 2<sup>nd</sup> and 3<sup>rd</sup> telephone surveys. While the percentage of QOP female enrollees who were ever engaged in any post-secondary education or training did not change during this time (staying at 67 percent), the control group percentage increased by 4 percentage points (from 57 to 61 percent). Moreover, estimates from Table 13 show that, at the time of the 3<sup>rd</sup> survey, women from the control group are significantly more likely to attend post-secondary education (by 8 percentage points), two- or four-year college (by 11 percentage points), or four-year college (by 6 percentage points) than women in the treatment group, confirming that the former are catching up to the latter in terms of post-secondary education enrollment. Indeed, the employment outcomes also suggest that QOP got its female enrollees to go through the system faster than those in the control group since, by the time of the 3<sup>rd</sup> survey, they were more likely to have a “good” job than women in the control group: QOP increased by a significant 10 and 9 percentage points the likelihood of having a job providing health insurance or retirement benefits, respectively. In contrast, for males, we continue to observe that those in the treatment were between 11 and 15 percentage points less likely to have a job with paid time off, a pension or health insurance at the time of the 3<sup>rd</sup> survey. All of these coefficients are statistically significant. This might be explained by the fact that, at that time, they were more likely (although not statistically significantly so) to attend post-secondary education (including college)

---

than males in the control group. However, because the percentage of males attending post-secondary education or college in their mid-twenties within this population is considerably low (and much more so than for women), we unfortunately lack precision in these male estimates. Finally, it is worth highlighting that we no longer find that QOP male enrollees earned significantly less than youth in the control group.

In terms of risky behaviors, we find that when youth were in their mid-twenties, QOP clearly had a significantly differential gender effect on criminal activity. As shown in Table 12, QOP significantly increased by 5 percentage points the likelihood of committing a crime in the past three months for males, while having no effect for females. Similarly, QOP significantly increased by 10 percentage points the likelihood of being arrested or charged in the past two years prior to answering the 3<sup>rd</sup> survey for males, while significantly decreasing the effect by 4 percentage points for females. It is worthwhile highlighting that when male enrollees were in their late teens, QOP reduced by a (not statistically significant) 8 percentage points the likelihood of being arrested or charged in the year prior to answering the survey. This finding seems to suggest that protecting difficult male youths in their late teens (in terms of helping them deal with the judicial system and preventing them from being arrested or charged) may backfire with longer-term detrimental consequences as youth age. Indeed we have qualitative evidence that mentors acted as advocates of QOP enrollees and negotiated on their behalf with the high school, the criminal justice and other public agencies when they got in trouble. It is likely that, by doing so, mentors ended up overprotecting youths in such a way that they mitigated the consequences of misbehaving and reduced youths' costs of engaging in risky behaviors (in particular, but not exclusively, criminal activity) leading to a perverse effect in that it prevented its male enrollees from internalizing the full costs of engaging in such types of risky behaviors, leading to higher involvement in such type of activities in the future. This view is consistent with Becker's 1968 economic model of crime in which crime can be deterred through punishment, and with studies that have found that youths are responsive to sanctions (see for instance, Pacula, 1998 a; Chaloupka *et al.*, 1999 a and 1999 b; Levitt, 1998; and Levitt and Lochner, 2001, among others).

### ***E. Impacts by Age when Entering 9<sup>th</sup> Grade***

Most of the youth in the QOP demonstration were 13 to 15 years old when the demonstration began. Youth aged 14 years old or younger (about two thirds of QOP enrollees) were those who

---

had begin 9<sup>th</sup> grade on time, whereas youth older than 14 years old had been most likely previously held back in school (although not in the 9<sup>th</sup> grade). Table 7 considers the effects of heterogeneity with respect to age by dividing youth into two groups, based on whether they were older than 14 years old when they entered 9<sup>th</sup> grade or not. The estimates show that QOP was effective for the younger enrollees, while no significant effects were found for the older ones. Although this differential result by age holds all along, the short-term impacts are not statistically significantly different from each other. In contrast, there is a statistically significant differential treatment effect by age in the medium- and long-run.

In the short-run, QOP increased by 12 percentage points the likelihood that younger enrollees graduated from high-school. Although this effect is slightly reduced in the longer-term, it remains sizeable, persistent, and statistically significant. Five years after the end of the program, younger QOP participants were 7 percentage points more likely to have a high-school degree and 6 percentage points more likely to have a high-school degree or a GED than younger members of the control group.<sup>25</sup> QOP also had a long-lived positive impact on post-secondary education attendance of younger enrollees as it increased their likelihood of attending college or post-secondary education by 7 percentage points in either case in the short-term, and it increased the likelihood of having ever attended college by 12 percentage points two years after the end of the program, and of having ever attended post-secondary education by 10 percentage points five years after the end of the program.

Although the medium-term employment impacts of QOP on younger enrollees are negative and statistically significant—QOP reduced the likelihood of being employed and of having a good job (defined as a job with health insurance) by 9 and 14 percentage points, respectively, the fact that they come hand in hand with positive and sizeable (non-significant) impacts of QOP on college attendance seems to suggest that the employment effects of QOP are due to locking-in effects of training (as opposed to detrimental effects of QOP that could have been driven by extrinsic incentives crowding out intrinsic motivation). This interpretation is confirmed by the longer-term impacts. Although none of the effects of QOP on employment for the younger enrollees are statistically significant, they are all positive reflecting a sharp shift from the effects observed three years earlier and opening the possibility that the returns from higher education

---

<sup>25</sup> However, no effects were found on short-term achievement tests, and GPA.



TABLE 7  
Short-, medium and long-term impacts of QOP, by age  
(Percentage points except where noted)

| <i><b>OUTCOMES</b></i>   | <i>Age entering 9<sup>th</sup> grade</i> |  |  |  |  |  |
|--|--|--|--|--|--|--|
|  | <i>Greater than 14 years old</i>         |  |  | <i>14 years old or less</i>            |  |  |
|  | <i>1<sup>st</sup> telephone survey</i>   | <i>2<sup>nd</sup> telephone survey</i> | <i>3<sup>rd</sup> telephone survey</i> | <i>1<sup>st</sup> telephone survey</i> | <i>2<sup>nd</sup> telephone survey</i> | <i>3<sup>rd</sup> telephone survey</i> |
| <b>High-school performance</b>   |  |  |  |  |  |  |
| Earned high-school diploma <sup>a</sup>                                      | 0  | -8 <sup>†</sup>                        | -8 <sup>†</sup>                        | 12 <sup>***</sup>                      | 7 <sup>*†</sup>                        | 7 <sup>*†</sup>                        |
| Earned high-school diploma or GED <sup>a</sup>                               | 6  | 0                                      | 0                                      | 7                                      | 6 <sup>*</sup>                         | 6 <sup>*</sup>                         |
| Math test scores (percentile)  | 0.55                                     | --                                     | --                                     | 0.72                                   | --                                     | --                                     |
| Reading test scores (percentile)   | 0.96                                     | --                                     | --                                     | 0.67                                   | --                                     | --                                     |
| GPA  | -0.06                                    | --                                     | --                                     | -0.02                                  | --                                     | --                                     |
| <b>Post-secondary training</b>   |  |  |  |  |  |  |
| Earned a bachelor's or associate's degree                                    | --                                       | --                                     | -2                                     | --                                     | --                                     | 0                                      |
| Attending college  | 3  | -7 <sup>††</sup>                       | -3                                     | 5                                      | 5 <sup>††</sup>                        | -4                                     |
| Attending or accepted in college   | 5  | --                                     | --                                     | 7 <sup>*</sup>                         | --                                     | --                                     |
| Attending post-secondary education   | 7  | -1                                     | 0                                      | 7 <sup>*</sup>                         | 6                                      | -1                                     |
| Attending post-secondary education or working                                | 10 <sup>*</sup>                          | 0                                      | -3                                     | 1                                      | -3                                     | 5                                      |
| Attending post-secondary education or working in a job with health insurance | 8  | 1                                      | --                                     | 6                                      | 2                                      | --                                     |
| Ever in college  | --                                       | -4 <sup>†</sup>                        | 0                                      | --                                     | 12 <sup>***†</sup>                     | 7                                      |
| Ever in post-secondary education   | --                                       | 4                                      | 2                                      | --                                     | 10 <sup>**</sup>                       | 10 <sup>**</sup>                       |
| <b>Employment</b>  |  |  |  |  |  |  |
| Has a job  | --                                       | -5                                     | -4                                     | --                                     | -9 <sup>**</sup>                       | 3                                      |
| Has a job with health insurance  | --                                       | -7                                     | -8                                     | --                                     | -14 <sup>*</sup>                       | 1                                      |
| Has a job that pays more than \$10 / hour                                    | --                                       | 0                                      | --                                     | --                                     | -2                                     | --                                     |
| Has a job with paid-off time   | --                                       | --                                     | -11 <sup>†</sup>                       | --                                     | --                                     | 4 <sup>†</sup>                         |
| Has a job with pension or retirement benefits                                | --                                       | --                                     | -7                                     | --                                     | --                                     | 2                                      |
| Ever employed  | --                                       | --                                     | -1                                     | --                                     | --                                     | 2                                      |
| Total earnings in the past 12 months (dollars)                               | --                                       | --                                     | -1524                                  | --                                     | --                                     | 465                                    |
| Hourly earnings (dollars)  | --                                       | --                                     | -4.24 <sup>†</sup>                     | --                                     | --                                     | 0.99 <sup>†</sup>                      |

*Notes:* All figures were calculated using weights to adjust for the sample and survey designs. Achievement test scores are expressed as percentiles in the distribution of scores for tenth graders in the United States. \*, \*\*, \*\*\* Estimate significantly different from zero at the 90%, 95%, or 99% confidence level, two-tailed test. † Significantly different from the impact for the other subgroups at the 90% confidence level, two-tailed test. <sup>a</sup>High school diploma or GED outcomes have been complemented with high-school transcript information.

achievements are beginning to feed in the employment and earnings outcomes.<sup>26</sup>

For older enrollees, QOP did not significantly impact high-school completion or post-secondary education or training. While QOP increased the short-term likelihood of attending post-secondary education or working by 10 percentage points for older enrollees, such effect fades away soon thereafter. Finally, although the longer-term effects of QOP on high-school completion and employment are negative, they are not statistically significant, implying no detrimental effects of QOP among this group.

#### ***F. Non-Response Bias***

Although the survey effort is equiparable to that of other studies (Schochet *et al.*, 2008; Banerjee *et al.*, 2007; and Kremer *et al.*, 2008; among others), a concern with the current paper would be that the observed differences between the shorter- and the longer-term outcomes were due to differential non-response bias between treatment and control group members that would vary across the surveys. More specifically, if non-respondents are more likely to be individuals with more difficult lives (and consequently worse outcomes), given that response rates are higher for the QOP members than for members of the control group, a concern is that the lack of positive results in the medium- and long-term outcomes is due to having a higher proportion of “difficult” youth responding in the QOP group relative to the control group.<sup>27</sup> To explore the internal validity of the results presented in the text we have done the following two robustness checks. First, columns 2 and 4 of Table A.1. present unweighted estimates that were derived by making the response rate for the QOP group equal to the response rate for the control group within each of the 11 QOP schools. That is, if the QOP group had a higher response rate, we treated enough QOP group respondents as nonrespondents to lower the implied response rate to the level of the control group. The QOP group respondents that were treated as nonrespondents were the last ones to respond to the survey—as these were those most difficult to find and who had eventually responded because of our intense survey effort. Columns 1 and 3 present the preferred estimates displayed in the paper for comparison purposes. Overall the estimates in columns 2 and 4

---

<sup>26</sup> This occurs because individuals who delay employment to obtain further education or training may initially have lower earnings upon entering the labor force than similarly aged but less educated individuals who have accrued more work experience (Mincer, 1974).

<sup>27</sup> Notice however that the differential response rate between the treatment and control groups does not always increase across surveys (it is 7 percentage points in the 1<sup>st</sup> telephone survey, 10 percentage points in the 2<sup>nd</sup> telephone survey, and 3 percentage points in the 3<sup>rd</sup> telephone survey.)

---

deliver practically the same results discussed earlier in the main text, suggesting that it is not the higher response rate among the treatment group youth that is driving the results.

A second robustness check is to compare the estimate of an outcome that does not change over time across different populations of respondents across surveys. The issue here is to identify an outcome that remains constant across surveys. Fortunately, we can identify such an outcome for the 2<sup>nd</sup> and 3<sup>rd</sup> telephone survey that is “high-school graduation”. Since by the time of the 2<sup>nd</sup> telephone survey, more than three years have elapsed between on-time high-school graduation and the survey date, it is very unlikely that individuals who had not yet earned a high-school diploma (and who are about 21 years old) are going to go back and receive a high school diploma. The first row of Table A.2. displays estimates of high-school graduation estimated using *only* the sample members who responded to the 3<sup>rd</sup> telephone survey, the same sample on which *all* other long-term outcomes reported in the main text are based. The second row of Table A.2 presents estimates of high-school graduation using information on the 3<sup>rd</sup> telephone survey respondents as well as non-respondents who had reported earning a high-school diploma in a previous survey. In both rows, all sample members’ responses to the 3<sup>rd</sup> telephone survey regarding their high-school completion status are assumed to be accurate. Therefore the only difference between the estimates in rows 1 and 2 is the people who responded, implying that any differences that may appear across the two rows will be due to differential non-response bias between the treatment and the control group changing between the 3<sup>rd</sup> and the other two surveys. As the differences are minimal, comparing the estimates from rows 1 and 2 suggests that non-response bias did not vary much between the last survey and the two previous ones. The third row of Table A.2 estimates high-school graduation using only information on respondents of the 2<sup>nd</sup> telephone survey (complemented with 2<sup>nd</sup> telephone survey non-respondents who had reported earning a high-school diploma in the 1<sup>st</sup> survey). In this case, comparing the estimates from rows 3 and 2 is equivalent to comparing estimates when only respondents from the first two surveys are observed (row 3) versus when respondents from *all* surveys are observed (row 2). Again the differences are minimal and if anything they suggest that, had the response rate been the same across surveys, the medium- and longer-term estimates would have been even more detrimental.<sup>28</sup>

---

<sup>28</sup> Notice that the response rate is lower for the comparison group members in the 2<sup>nd</sup> survey than in the 3<sup>rd</sup> survey, while the opposite is for the treatment group members.

---

Finally, Table A.3 presents the unweighted estimates. Overall, the results are quite similar affecting a little the magnitudes but not the significance of the estimates.

#### **IV. Conclusion**

Recently, a new wave of interventions whose main objective is to improve the educational outcomes of disadvantaged youth has found promising results if not for all participants, for some particular subgroups. While most of these studies analyze the effects of the interventions during or shortly after the students have been exposed to the program, this paper investigates not only the short-term, but also the medium- and longer-term impacts of a five-year intensive after-school program on at-risk youth in the United States using a randomized experimental design. The paper focuses on both educational and employment outcomes: (i) during the last year of the demonstration, while the students were still in, or just completing, high school; (ii) three years later when most sample members were about 21 or 22 years old; and (iii) five years after the end of the demonstration.

When estimating the average effects of the program, the study finds that the beneficial shorter-term effects of QOP on high-school completion and post-secondary education enrollment quickly fade away. Although the short-lived program benefits may seem disappointing given the program's cost of \$25,000 per enrollee for the full five years of the demonstration, QOP did manage to get its enrollees to graduate from high-school and enroll in post-secondary education sooner than youths in the control group. These results are consistent with short-term evidence on resiliency factors, which show that youths in QOP reported to be 7 percentage points (19%) more likely (and significantly so) to have an influential adult helping them to "take advantage of opportunities to get ahead" in life.

In addition, we find that the program fares significantly better in multiple dimensions for its female than its male enrollees. The sizeable overall impact on high-school graduation when youths were in their late-teens is driven entirely by a large and significant effect on women. In addition to helping female students by getting them through high school earlier, QOP also got them through post-secondary training (not necessarily college) quicker than their counterparts in the control group. Moreover, five years after the end of the program, female QOP enrollees had better employment outcomes than control group members, although the opposite was true for educational outcomes (measured as college attendance). In contrast, QOP had some adverse short-term impacts

---

on males' academic performance, while leaving high-school graduation unaffected. Although these adverse effects were short-lived and QOP enrollees ended up with higher GED receipt by the time they were in their mid-twenties, the longer-term employment impacts continued to be detrimental five years after the end of the program. In addition, males experienced higher rates of substance abuse in their late-teens—similar to the Moving to Opportunity experiment (Kling *et al.*, 2005)—, and a larger criminal activity in their mid-twenties relative to the control group.

This raises the important question of why women responded so differently to QOP than males. We have no simple explanation for this difference, and this field experiment was not designed to distinguish between the different mechanisms. The evidence seems to be consistent with models of youths' behavioral response to economic incentives for males, while it appears that females responded more to a mentoring model, in which having an adult helping the youth seems to help develop their non-cognitive skills. However, other explanations are also possible. For instance, it may well be that by offering a mentor and having youth participate in many after school activities, QOP may have weakened the ties between male enrollees and their parents, breaking important social bonds, and thus leading to the observed perverse effects among male enrollees. An alternative and related explanation is that because enrollees' parents trusted that another adult (the mentor) was also watching over their male children, they ended up investing less time with their male children and paying less attention to possible warning signs than parents of youths in the control group. In essence, QOP may have led to a substitution effect away from parents' attention, which could have explained these detrimental findings for males. Yet another hypothesis for these results is that QOP may have led male enrollees to be more aware of their relative disadvantaged situation in life, which may have brought upon them further disappointment, leading them to engage in diverse types of risky behaviors.

It is important to highlight that similar gender differences in response to services and incentives have been observed elsewhere.<sup>29</sup> Indeed, Anderson, 2008, shows that three well-known early childhood interventions (Abecedarian, Perry, and the Early Training Project) had substantial short- and long-term effects on girls but no effect on boys. Similarly, a number of public-sector training programs generated larger effects on women than men (Robert J. Lalonde, 1995). The Moving to Opportunity randomized evaluation of housing vouchers likewise generated clear benefits for girls,

---

<sup>29</sup> To our knowledge, the only exception is Fryer, 2010, who has recently found that in an intervention involving financial incentives on input (in this case reading books), boys seem to gain more from the experiment than girls.

---

with little or even adverse effects on boys (Jeffrey R. Kling, Jeffrey B. Leibman, and Lawrence F. Katz, 2007). Within the literature on financial incentives, Dynarski, 2008, estimates larger effects of tuition aid on college completion for women in the United States, while Garibaldi *et al.*, 2007, find that tuition affects the completion rates of women more than men in Italy. In a study of the effects of merit awards on Israeli high school students, Angrist and Lavy, 2010, find effects on girls only. Similarly, a recent randomized trial looking at cash payments for academic achievement among college freshman in Canada finds clear effects for females but no effect on males (Joshua Angrist, Daniel Lang, and Philip Oreopoulos, Forthcoming). A more modest but still marked gender differential crops up in the response to randomly assigned vouchers for private secondary schools in Colombia (Joshua Angrist, *et al.*, 2002). These vouchers incorporated an incentive component because voucher retention was conditional on academic performance. All of this research raises the need for further studies using experimental designs to answer whether and how services and cash incentives work differently across genders.

#### **REFERENCES (NEED TO REVISE)**

- Angrist, Joshua, Eric Bettinger, Erik Bloom, Beth King, and Michael Kremer. 2002. "Vouchers for Private Schooling in Colombia: Evidence from a Randomized Natural Experiment." *American Economic Review*, 92(5): 1535-1558.
- Angrist, Joshua, Eric Bettinger, and Michael Kremer. 2006. "Long-Term Educational Consequences of Secondary School Vouchers: Evidence from Administrative Records in Colombia." *American Economic Review* 96(3): 847-862.
- Angrist, Joshua, Daniel Lang, and Philip Oreopoulos. Forthcoming. "Incentives and Services for College Achievement: Evidence from a Randomized Trial." *American Economic Journal: Applied Economics*.
- Angrist, Joshua D., and Victor Lavy. Forthcoming. "The Effect of High School Matriculation Awards: Evidence from Randomized Trials." *American Economic Review*.
- Ash, Katie. 2008. "Promises of Money meant to heighten Student Motivation." *Education Week*, February 13.
- Ashworth, K., J. Hardman, et al. 2001. "Education Maintenance Allowance: The First Year, A Qualitative Evaluation". Research Report RR257, Department for Education and Employment.
- Attanasio, Orazio, Emla Fitzsimons, Ana Gomez, Diana Lopez, Costas Meghir and Alice Mesnard. 2006. "Child education and work choices in the presence of a conditional cash transfer programme in rural Colombia." Working Paper W06/13. London: Institute for Fiscal Studies.

- Banerjee, Abhijit, Rukmini Banerji, Esther Duflo, and Stuti Khemani. Forthcoming. "Pitfalls of Participatory Programs: Evidence from a randomized evaluation in education in India." *American Economic Journal: Economic Policy*.
- Banerjee, A., S. Cole, E. Duflo and L. Linden. 2007. "Remedying Education: Evidence from Two Randomized Experiments in India," *Quarterly Journal of Economics*, Vol. 122 (3), pp.1235-64.
- Becker, Gary. 1964. "Human capital; a theoretical and empirical analysis, with special reference to education." New York: Columbia University Press.
- Bénabou Roland, and Jean Tirole. 2003. "Intrinsic and Extrinsic Motivation." *Review of Economic Studies* 70, 489–520.
- Bettinger, Eric P., and Bridget Terry Long. 2005. "Addressing the Needs of Under-Prepared Students in Higher Education: Does College Remediation Work?" National Bureau of Economic Research Working Paper 11325.
- Behrman, Jere R., P. Sengupta, and P. Todd. 2000. "Final Report: The Impact of PROGRESA on Achievement Test Scores in the First year." Washington, DC: International Food Policy Research Institute, Food Consumption and Nutrition Division.
- Bettinger, Eric & Slonim, Robert, 2007. "Patience among children," *Journal of Public Economics*, Elsevier, vol. 91(1-2), pages 343-363, February.
- Binder, M., P. T. Ganderton, et al. 2002. "Incentive Effects of New Mexico's Merit-Based State Scholarship Program: Who Responds and How?," unpublished manuscript.
- Bloom, H.S., Bell, S.H., Doolittle, F., Lin, W., Cave, G., and Orr, L.L. 1996. "Does training for the disadvantaged work? Evidence from the national JTPA study." Washington, DC: The Urban Institute Press.
- Bloom, Dan and Colleen Sommo. 2005. "Building Learning Communities: Early Results from the Opening Doors Demonstration at Kingsborough Community College," New York: MDRC.
- Bonner, Sarah E. S., Mark Young, and Reid Hastie. 1996. "Financial Incentives and Performance in Laboratory Tasks: The Effects of Task Type and Incentive Scheme Type," Unpublished manuscript, University of Southern California Department of Accounting.
- Camerer, Colin, and Robin Hogarth. 1999. "The Effects of Financial Incentives in Experiments: A Review and Capital-Labor-Production Framework." *Journal of Risk and Uncertainty*, 19:1-3; 7-42.
- Cameron, J., K. M. Banko, et al. 2001. "Pervasive Negative Effects of Rewards on Intrinsic Motivation: The Myth Continues." *The Behavior Analyst* 24: 1-44.
- Cameron, S., & Heckman, J. 1993. The nonequivalence of high school equivalents. *Journal of Labor Economics*, 11(1), 1–47.
- Cascio, Elizabeth U., and Ethan G. Lewis. 2006. "Schooling and the Armed Forces Qualifying Test: Evidence from School-Entry Laws." *Journal of Human Resources* 41(2): 294–318.
- Cornwell, Christopher, Kyung Hee Lee, and David B. Mustard. 2005. "The effects of merit-based financial aid on course enrollment, withdrawal, and completion in college." *Journal*

- of Human Resources* 40, no. 4:895–917.
- Cornwell, Christopher, David B. Mustard, and Deepa J. Sridhar. 2006. “The Enrollment Effects of Merit-Based Financial Aid: Evidence from Georgia’s HOPE Program.” *Journal of Labor Economics*, 24(4): 761–86.
- Currie, Janet. 2001. “Early childhood education programs.” *Journal of Economic Perspectives* 15, no. 2:213–38.
- Deci, E. 1975. *Intrinsic Motivation* (New York: Plenum Press).
- Deci, E., Koestner, R. and Ryan, R. 1999. “A Meta-Analytic Review of Experiments Examining the Effects of Extrinsic Rewards on Intrinsic Motivation”, *Psychological Bulletin*, 125 (6), 627–668.
- Dearden, L., Emmerson, C., Frayne, C., and Meghir, C. 2005. “Education Subsidies and School Drop-Out Rates.” Institute for Fiscal Studies Working Paper 05/11.
- Deming, D. and S. Dynarski. 2009. “Into college, out of poverty? Policies to increase the postsecondary attainment of the poor”. NBER Working Paper 15387.
- DuBois, D.L., Holloway, B.E., Valentine, J.C. and Cooper, H. 2002. “Effectiveness of mentoring programs for youth: A meta-analytical review”, *American Journal of Community Psychology*, 30 (2), 157–197.
- Duckworth, Angela Lee, and Martin P. Seligman. 2006. “Self-Discipline Gives Girls the Edge: Gender in Self-Discipline, Grades, and Achievement Test Scores.” *Journal of Educational Psychology* 98(1): 198-208.
- Duflo, Esther, Pascaline Dupas and Michael Kremer, 2009. “Additional Resources versus Organizational Changes in Education: Experimental Evidence from Kenya.” Mimeo, MIT, Department of Economics.
- Duflo, Esther, Michael Kremer, and Jonathan Robinson, 2009. “Nudging Farmers to Utilize Fertilizer: Theory and Experimental Evidence from Kenya.” Mimeo, MIT, Department of Economics.
- Dynarski, Susan. 2003. “Does Aid Matter? Measuring the Effect of Student Aid on College Attendance and Completion.” *American Economic Review* 93(1): 279–88.
- Dynarski, Susan. 2008. “Building the Stock of College-Educated Labor.” *Journal of Human Resources*, 43(3): 576-610.
- Dynarski, Mark, and Gleason, Philip. 2002. “How Can We Help? What We Have Learned From Recent Federal Dropout Prevention Evaluations.” *Journal of Education for Students Placed at Risk*, 7 (1): 43-69.
- El País, 2009. “Chequera Contra el Fracaso Escolar.” Reportaje de J.A. Aunión, 14 October, 2009.
- Eckstein, Zvi and K.I. Wolpin. 1999. “Why Youths Drop Out of High School: The Impact of Preferences, Opportunities, and Abilities.” *Econometrica*, 67(6): 1295-1340.
- Finn, J. (1989). Withdrawing From School. *Review of educational research*, 59, 117–142.
- Frangakis, Constantine E., and Donald B. Rubin. 2002. “Principal stratification in causal inference.” *Biometrics*, 58: 21-29.



- Frey, B. S. 1994. "How Intrinsic Motivation Is Crowded Out and In." *Rationality & Society*, 6(3): 334-352.
- Frey, B. S. and Oberholzer-Gee, F. 1997. "The costs of price incentives: An empirical analysis of motivation crowding-out." *American Economic Review*, 87(4):746–755.
- Fryer, Roland. 2010. "Financial Incentives and Student Achievement: Evidence from Randomized Trials." Department of Economics, Harvard University Working Paper.
- Garibaldi, Pietro, Francesco Giavazzi, Andrea Ichino, and Enrico Rettore. 2007. "College Cost and Time to Obtain a Degree: Evidence from Tuition Discontinuities." NBER Working Paper 12863.
- Gerfin M., and M. Lechner, and H. Steiger. 2005. "Does Subsidized Temporary Employment Get the Unemployed Back to Work? An Econometric Analysis of Two Different Schemes." *Labour Economics*, Vol. 12, pp. 807-835.
- Gneezy, U. and Rustichini, A. 2000. "Pay Enough or Don't Pay at All", *Quarterly Journal of Economics*, 115 (3), 791–810.
- Gibbons, R. 1997. "Incentives and Careers in Organizations", in D. Kreps and K.Wallis (eds.) *Advances in Economic Theory and Econometrics*, Vol. II (Cambridge, U.K.: Cambridge University Press).
- Granger, Robert, and Rachel Cytron. 1998. "Teenage Parent Programs." MDRC report.
- Green, L., Fry, A. F., and Myerson, J. 1994. Discounting of delayed rewards: A life span comparison. *Psychological Science*, 5, 33-36.
- Greene, A. 1986. "Future Time Perspective in Adolescence: The present of things future revisited", *Journal of Youth and Adolescence*, 15: 99-113.
- Grossman, J. B., and J. P. Tierney. 1998. "Does Mentoring Work? An Impact Study of the Big Brothers Big Sisters program," *Evaluation Review*, 22(3), pp. 402–425.
- Gruber, Jonathan. 2001. *Risky Behavior Among Youth: An Economic Analysis*. Chicago: University of Chicago Press.
- Hall, J. 2003. "Mentoring and Young People: A literature review." *SCRE Research Report 114*. University of Glasgow.
- Hahn, A. 1999. "Extending the Time of Learning," in Douglas J. Besharov, ed., *America's Disconnected Youth*, Washington, D.C.: Child Welfare League of America, Inc.
- Hahn, A., T. Leavitt and P. Aaron. 1999. *Evaluation of the Quantum Opportunities Program*, Heller Graduate School, Center for Human Resources, Brandeis University, Waltham, Massachusetts.
- Heckman, James J., 2000. "Policies to foster human capital," *Research in Economics*, Elsevier, vol. 54(1), pages 3-56, March.
- Heckman, James J & Lochner, Lance & Taber, Christopher, 1998. "Tax Policy and Human-Capital Formation," *American Economic Review*, vol. 88(2), pages 293-97, May.
- Heckman, James J., and Yona Rubinstein. 2001. "The Importance of Non-Cognitive Skills: Lessons from the GED testing program. *American Economic Review* 91, no. 2:145–49.

- Hertwig, Ralph and Andreas Ortmann. 1998. "Experimental Practices in Economics: A Methodological Challenge for Psychologists," *Behavioral and Brain Sciences*.
- Holmlund Helena, and Olmo Silva. 2009. "Targeting Non-Cognitive Skills to Improve Cognitive Outcomes: Evidence from a Remedial Education Intervention." IZA DP No. 4476.
- James-Burdumy Susanne, Mark Dynarski and John Deke. 2008. "After-School Program Effects on Behavior: Results From the 21<sup>st</sup> Century Community Learning Centers Program National Evaluation." *Economic Inquiry*, Vol. 46, No. 1, January 2008, 13–18
- Jacob, B. and Lefgren L. 2004. "Remedial Education and Student Achievement: A regression Discontinuity Analysis", *Review of Economics and Statistics*, vol. 86, pp. 226-44.
- Jackson, C. K. 2007. "A Little Now for a Lot Later: A Look at a Texas Advanced Placement Incentive Program." [http://works.bepress.com/c\\_kirabo\\_jackson/1](http://works.bepress.com/c_kirabo_jackson/1).
- Jekielek, S.M., Moore, K.A., Hair, E.C. and Scarupa, H.J. 2002. *Mentoring: A promising strategy for youth development*. (Child Trends Research Brief.) Washington, DC: Child Trends.
- Jenkins, G. Douglas, Jr., Atul Mitra, Nina Gupta, and Jason D. Shaw. 1998. "Are Financial Incentives Related to Performance? A Meta-Analytic Review of Empirical Research," *Journal of Applied Psychology* 83, 777-787.
- Kane, Thomas J. 1998. "Savings Incentives for Higher Education." *National Tax Journal*, Vol. 51, No. 3, pp. 609-620.
- Kane, Thomas J. 2007. "Evaluating the Impact of the DC Tuition Assistance Grant Program." *Journal of Human Resources*.
- Kemple, James J., and Cynthia J. Willner. 2008. "Career Academies. Long-Term Impacts on Labor Market Outcomes, Educational Attainment, and Transitions to Adulthood" MDRC Report.
- Kirby, K. N. 1997. "Bidding on the future: Evidence against normative discounting of delayed rewards." *Journal of Experimental Psychology: General* 126, 54-
- Kling, Jeffrey R., Jeffrey B. Liebman, and Lawrence F. Katz. 2005. "Experimental Analysis of Neighborhood Effects." NBER Working Paper 11577, August.
- Kling, Jeffrey R., Jens Ludwig, and Lawrence F. Katz. 2005. "Neighborhood Effects on Crime for Female and Male Youth: Evidence from a Randomized Housing Voucher Experiment." *Quarterly Journal of Economics* 120(1):87–130.
- Kohn, A. 1993. *Punished by Rewards* (New York: Plenum Press).
- Kohn, Alfie. 1999. *Punished by Rewards the Trouble with Gold Stars, Incentive Plans, A's, Praise, and Other Bribes*. Bridgewater, NJ: Replica Books.
- Kremer, Michael, Edward Miguel, and Rebecca Thornton. Forthcoming. "Incentives to Learn." *The Review of Economics and Statistics*.
- Kruglanski, A., I. Friedman, and Zeevi. 1971. "The Effect of Extrinsic Incentives on Some Qualitative Aspects of Task Performance." *Journal of Personality and Social Psychology* 39: 608-617.
- Kluge, Jochen. 2006. "The Effectiveness of European Active Labor Market Policy." IZA Discussion Paper 2018.

- Laibson, David, 1997. "Golden Eggs and Hyperbolic Discounting," *The Quarterly Journal of Economics*, vol. 112(2), pages 443-77.
- LaLonde, Robert J. 1995. "The Promise of U.S. Employment and Training Programs." *Journal of Economic Perspectives*, 9(2): 149-68.
- Lazear, E. (2000), "Performance, Pay and Productivity", *American Economic Review*, 90 (5), 1346-1361.
- Lavy, Victor, and Analia Schlosser. 2005. "Targeted Remedial Education for Underperforming Teenagers: Costs and Benefits." *Journal of Labor Economics*, 23(4): 839-74.
- Lepper, M., D. Greene, et al. 1973. "Undermining Children's Interest with Extrinsic Rewards: A Test of the 'Overidentification Hypothesis.'" *Journal of Personality and Social Psychology* 28: 129-137.
- Lepper, M., Greene, D. and Nisbett, R. 1973. "Undermining Children's Interest with Extrinsic Rewards: A Test of the 'Overjustification Hypothesis'", *Journal of Personality and Social Psychology*, 28, 129-137.
- Leslie, L. L., and Brinkman, P. T. 1988. *The Economic Value of Higher Education*. San Francisco: Jossey-Bass.
- Leuven, E., H. Oosterbeek, and B. van der Klaauw. 2003. "The Effect of Financial Rewards on Students' Achievement: Evidence from a Randomized Experiment." CEPR Discussion Paper 3921.
- Machin, Steven, Sandra McNally, and Costas Meghir, 2004. "Improving Pupil Performance in English Secondary Schools: Excellence in Cities," *Journal of the European Economic Association*, MIT Press, vol. 2(2-3), pages 396-405, 04/05.
- Machin, Steven, Sandra McNally, and Costas Meghir, 2007. "Resource and Standards in Urban Schools", IZA DP 2653.
- Maxfield, Myles, Laura Castner, Vida Maralani, and Mary Vencill. 2003a. "The Quantum Opportunity Program Demonstration: Implementation Findings." Washington, DC: Mathematica Policy Research, Inc.
- Maxfield, Myles, Allen Schirm, and Nuria Rodriguez-Planas. 2003b. "The Quantum Opportunities Program Demonstration: Implementation and Short-Term Impacts." Mathematica Policy Research Report 8279-093. Washington, DC: Mathematica Policy Research, Inc.
- Mincer, Jacob. 1974. *Schooling, Experience, and Earnings*. New York: Columbia University Press for the National Bureau of Economic Research.
- Nurmi, J. 1991. "How Do Adolescents See Their Future? A review of the development of future orientation and planning", *Developmental Review*, 11:1-59.
- Rhodes, Jean. 1994. "Older and Wiser: Mentoring Relationships in Childhood and Adolescence," *The Journal of primary prevention*, v 14 n 3, Spring.
- Rhodes, Jean E., Jean B. Grossman, and Nancy L. Resch. 2000. "Agents of Change: Pathways through Which Mentoring Relationships Influence Adolescents' Academic Adjustment." *Child Development*, November/December, Volume 71, Number 6, Pages 1662-1671.

- 
- Schirm, Allen, Nuria Rodriguez-Planas, Myles Maxfield, and Christina Tuttle. 2003. "The Quantum Opportunity Program Demonstration: Short-Term Impacts." Washington, DC: Mathematica Policy Research, Inc.
- Schirm, Allen, and Nuria Rodriguez-Planas. 2004. "The Quantum Opportunity Program Demonstration: Short-Term Impacts." Washington, DC: Mathematica Policy Research, Inc.
- Schirm, Allen, Elizabeth Stuart, and Allison McKie. 2007. "The Quantum Opportunity Program Demonstration. Final Impacts." Washington, DC: Mathematica Policy Research, Inc.
- Schochet, Peter, John Burghardt, and Sheena McConnell. 2008. "Does Job Corps Work? Impact Findings from the National Job Corps Study." *American Economic Review*, 98:5, 1864-1886.
- Schultz, T. Paul. 2004. "School Subsidies for the Poor: Evaluating the Mexican Progress Poverty Program." *Journal of Development Economics*, 74(2): 199-250.
- Scrivener, Susan, Michael J. Weiss, and Jedediah J. Teres. 2009. "More Guidance, Better Results? Three-Year Effects of an Enhanced Student Services Program at Two Community Colleges." MDRC report.
- Silverman, Irwin W. 2003. "Gender Differences in the Delay of Gratification: A Meta-Analysis." *Sex Roles*, 49(9-10): 451-463.
- Titmuss, Richard M. 1970. *The Gift Relationship*. London: Allen and Unwin.
- Titmuss, Richard M. 1971. *The Gift Relationship: From Human Blood to Social Policy*. New York: Pantheon.
- U.S. Department of Education, National Center for Education Statistics. 2002. "A Descriptive Summary of 1999-2000 Bachelor's Degree Recipients 1 Year Later, With an Analysis of Time to Degree." NCES 2003-165, by Ellen M. Bradburn, Rachael Berger, Xiaojie Li, Katharin Peter, and Kathryn Rooney. Project Officer: James Griffith.
- U.S. Department of Education, National Center for Education Statistics. *The Condition of Education 2005*. NCES 2005-094. Washington, DC: U.S. Department of Education.
- Warner, John T., and Saul Pleeter. 2001. "The Personal Discount Rate: Evidence from Military Downsizing Programs." *The American Economic Review*, 91(1): 33-53.
- Wilson, T., Hull, J. and Johnson, J. 1981. "Awareness and Self-Perception: Verbal Reports on Internal States", *Journal of Personality and Social Psychology*, 40, 53-71.

---

## APPENDIX

*(Not for Publication)*

### A.I. Weights

Overall impacts of QOP were estimated using student-, school-, and site-specific weights.<sup>30</sup> Weights for each student were used to adjust for survey nonresponse and ensure that the sample of respondents reflects the experiences of all sample members.<sup>31</sup> The impacts for each school were calculated as the weighted difference in the outcomes of members of the QOP and control groups. The impacts for each site were calculated as a weighted average across schools using weights that reflected the proportion of QOP slots in each school. This weighting was selected because we believe that each program would have allocated slots across schools in the same way they did in the demonstration if they had been part of an ongoing, national program.<sup>32</sup> Finally, to obtain the overall demonstration impacts, the site-specific effects were averaged, with each site weighted equally. The equal weighting of sites was based on our best guess that if QOP were implemented as an ongoing, national program, each site would have roughly equal numbers of QOP slots.

---

<sup>30</sup> For thorough description on how the weighted averages were calculated see Maxfield *et al.*, 2003 a; Schirm *et al.*, 2004; and Schirm *et al.*, 2006.

<sup>31</sup> Non-response weights were estimated using response propensity scores for the treatment and the comparison group, separately. The predictors used in the response propensity scores included school dummies, baseline characteristics, interactions between the previous school and baseline characteristics and between any two baseline characteristics, and outcomes measured in any of the earlier surveys.

<sup>32</sup> Weighting each site in proportion to the number of students in the study did not lead to different study conclusions (Schirm *et al.* 2006).

TABLE A.1

Impacts Using Alternative Approaches to Adjusting for Non-Response  
(Percentage points)

| <i>OUTCOMES</i>  | <i>2<sup>nd</sup> survey</i> |                                      | <i>3<sup>rd</sup> survey</i> |                                      |
|--|------------------------------|--------------------------------------|------------------------------|--------------------------------------|
|  | <i>Unweighted</i><br>(1)     | <i>Same non-response rate</i><br>(2) | <i>Unweighted</i><br>(3)     | <i>Same non-response rate</i><br>(4) |
| <b>High-School Performance</b>   |                              |                                      |                              |                                      |
| Earned high-school diploma <sup>a</sup>                                      |                              |                                      |                              |                                      |
| <i>Control mean</i>  | 0.6267                       | 0.6281                               | 0.5981                       | 0.5963                               |
| <i>Impact</i>  | 0.0067<br>[0.0333]           | 0.0063<br>[0.0339]                   | 0.0241<br>[0.0327]           | 0.0233<br>[0.334]                    |
| Earned high-school diploma or GED <sup>a</sup>                               |                              |                                      |                              |                                      |
| <i>Control mean</i>  | 0.7502                       | 0.7501                               | 0.7601                       | 0.7581                               |
| <i>Impact</i>  | 0.0166<br>[0.0298]           | 0.0164<br>[0.0302]                   | 0.0301<br>[0.0286]           | 0.0248<br>[0.0294]                   |
| <b>Post-secondary Training</b>   |                              |                                      |                              |                                      |
| Earned a bachelor's or associate's degree                                    | --                           | --                                   |                              |                                      |
| <i>Control mean</i>  |                              |                                      | 0.0706                       | 0.0645                               |
| <i>Impact</i>  |                              |                                      | -0.001<br>[0.0192]           | 0.0064<br>[0.0191]                   |
| Attending college  |                              |                                      |                              |                                      |
| <i>Control mean</i>  | 0.1662                       | 0.1668                               | 0.1764                       | 0.1755                               |
| <i>Impact</i>  | 0.0090<br>[0.0282]           | 0.0163<br>[0.0292]                   | -0.0344<br>[0.0278]          | -0.0312<br>[0.0284]                  |
| Attending post-secondary education   |                              |                                      |                              |                                      |
| <i>Control mean</i>  | 0.2569                       | 0.2577                               | 0.2431                       | 0.2438                               |
| <i>Impact</i>  | 0.0507<br>[0.0336]           | 0.0458<br>[0.0346]                   | -0.0031<br>[0.0321]          | 0.0045<br>[0.0330]                   |
| Attending post-secondary education or working                                |                              |                                      |                              |                                      |
| <i>Control mean</i>  |                              | 0.7933                               | 0.7572                       | 0.7603                               |
| <i>Impact</i>  |                              | -0.0025<br>[0.0322]                  | 0.0285<br>[0.0327]           | 0.0236<br>[0.0332]                   |
| Attending post-secondary education or working in a job with health insurance |                              |                                      |                              |                                      |
| <i>Control mean</i>  |                              | 0.4442                               | --                           | --                                   |
| <i>Impact</i>  |                              | 0.0300<br>[0.0389]                   |                              |                                      |
| Sample size  | 670                          | 670                                  | 710                          | 710                                  |

TABLE A.1 (Continue)  
Impacts Using Alternative Approaches to Adjusting for Non-Response  
(Percentage points)

|  | <i>2<sup>nd</sup> survey</i> |                               | <i>3<sup>rd</sup> survey</i> |                               |
|--|------------------------------|-------------------------------|------------------------------|-------------------------------|
|  | <i>Unweighted</i>            | <i>Same non-response rate</i> | <i>Unweighted</i>            | <i>Same non-response rate</i> |
| <b>OUTCOMES</b>                                | (1)                          | (2)                           | (3)                          | (4)                           |
| Has a job                                      |                              |                               |                              |                               |
| <i>Control mean</i>                            | 0.7252                       | 0.7237                        | 0.7572                       | 0.6859                        |
| <i>Impact</i>                                  | -0.0608*                     | 0.0504                        | 0.0285                       | -0.0018                       |
|  | [0.0352]                     | [0.0362]                      | [0.0327]                     | [0.0363]                      |
| Has a full-time job                            |                              |                               |                              |                               |
| <i>Control mean</i>                            | 0.5686                       | 0.5682                        | 0.5339                       | 0.5401                        |
| <i>Impact</i>                                  | -0.1153***                   | -0.1023***                    | -0.0002                      | -0.0112                       |
|  | [0.0376]                     | [0.0386]                      | [0.0380]                     | [0.0386]                      |
| Has a job with health insurance                |                              |                               |                              |                               |
| <i>Control mean</i>                            | 0.4593                       | 0.4574                        | 0.4707                       | 0.4743                        |
| <i>Impact</i>                                  | -0.0302                      | -0.0169                       | -0.0023                      | -0.0063                       |
|  | [0.0381]                     | [0.0392]                      | [0.0382]                     | [0.0390]                      |
| Has a full-time job with health insurance      |                              |                               |                              |                               |
| <i>Control mean</i>                            | 0.3950                       | 0.3944                        |                              |                               |
| <i>Impact</i>                                  | -0.0848**                    | 0.0775**                      |                              |                               |
|  | [0.0361]                     | [0.0371]                      |                              |                               |
| Has a job with paid-off time                   |                              |                               |                              |                               |
| <i>Control mean</i>                            | --                           | --                            | 0.4435                       | 0.4472                        |
| <i>Impact</i>                                  |                              |                               | 0.0150                       | 0.0075                        |
|  |                              |                               | [0.0374]                     | [0.0382]                      |
| Has a job with pension or retirement benefits  |                              |                               |                              |                               |
| <i>Control mean</i>                            |                              |                               | 0.3766                       | 0.3778                        |
| <i>Impact</i>                                  |                              |                               | 0.0129                       | 0.0118                        |
|  |                              |                               | [0.0373]                     | [0.0381]                      |
| Total earnings in the past 12 months (dollars) |                              |                               |                              |                               |
| <i>Control mean</i>                            | --                           | --                            | 13,198                       | 13,326                        |
| <i>Impact</i>                                  |                              |                               | -66.2677                     | -349.48                       |
|  |                              |                               | [1,109]                      | [1,168]                       |
| Hourly earnings (dollars)                      |                              |                               |                              |                               |
| <i>Control mean</i>                            |                              |                               | 9.3466                       | 9.0378                        |
| <i>Impact</i>                                  |                              |                               | -1.2436                      | -0.9475                       |
|  |                              |                               | [1.2326]                     | [1.1671]                      |
| Ever in college                                |                              |                               |                              |                               |
| <i>Control mean</i>                            | 0.3234                       | 0.3245                        | 0.3688                       | 0.3691                        |
| <i>Impact</i>                                  | 0.0633*                      | 0.0786**                      | 0.0412                       | 0.0426                        |
|  | [0.0354]                     | [0.0366]                      | [0.0365]                     | [0.0373]                      |
| Ever in post-secondary education               |                              |                               |                              |                               |
| <i>Control mean</i>                            | 0.5428                       | 0.5452                        | 0.5637                       | 0.5684                        |
| <i>Impact</i>                                  | 0.0904**                     | 0.0399**                      | 0.07130*                     | 0.0661                        |
|  | [0.0373]                     | [0.0382]                      | [0.0370]                     | [0.0377]                      |
| Ever employed                                  |                              |                               |                              |                               |
| <i>Control mean</i>                            | --                           | --                            | 0.9577                       | 0.9611                        |
| <i>Impact</i>                                  |                              |                               | -0.0033                      | 0.0023                        |
|  |                              |                               | [0.0160]                     | [0.0152]                      |
| Sample size                                    | 670                          | 670                           | 710                          | 710                           |

TABLE A.1 (Continue)  
Impacts Using Alternative Approaches to Adjusting for Non-Response  
(Percentage points)

| <i><b>OUTCOMES</b></i>                         | <i>2<sup>nd</sup> survey</i> |                               | <i>3<sup>rd</sup> survey</i> |                               |
|--|------------------------------|-------------------------------|------------------------------|-------------------------------|
|  | <i>Unweighted</i>            | <i>Same non-response rate</i> | <i>Unweighted</i>            | <i>Same non-response rate</i> |
|  | <i>(1)</i>                   | <i>(2)</i>                    | <i>(3)</i>                   | <i>(4)</i>                    |
| <b>Substance Abuse</b>                         |                              |                               |                              |                               |
| Binge drinking in the past 30 days             |                              |                               |                              |                               |
| <i>Control mean</i>                            | 0.3035                       | 0.3040                        | 0.2956                       | 0.2968                        |
| <i>Impact</i>                                  | -0.684**<br>[0.0330]         | -0.0644*<br>[0.0339]          | 0.0009<br>[0.0342]           | .0067<br>[0.0351]             |
| Binge drinking on 8 or more days in past month |                              |                               |                              |                               |
| <i>Control mean</i>                            | 0.0545                       | 0.0545                        | 0.0530                       | 0.0535                        |
| <i>Impact</i>                                  | 0.0083<br>[0.0173]           | 0.0063<br>[0.0176]            | 0.0293<br>[0.019]            | 0.0275<br>[0.0193]            |
| Used any illegal drug in the past 30 days      |                              |                               |                              |                               |
| <i>Control mean</i>                            | 0.1700                       | 0.1706                        | 0.1283                       | 0.1264                        |
| <i>Impact</i>                                  | -0.0598**<br>[0.0262]        | -0.0538**<br>[0.0270]         | -0.0121<br>[0.0254]          | -0.0117<br>[0.0258]           |
| <b>Criminal Activity</b>                       |                              |                               |                              |                               |
| Committed a crime in past 3 months             |                              |                               |                              |                               |
| <i>Control mean</i>                            | 0.0844                       | 0.0846                        | 0.0203                       | 0.1171                        |
| <i>Impact</i>                                  | -0.0202<br>[0.0203]          | -0.0183<br>[0.0208]           | 0.0181<br>[0.0133]           | 0.0137<br>[0.0252]            |
| Arrested or charged (in last 3 months)         |                              |                               |                              |                               |
| <i>Control mean</i>                            | 0.0488                       | 0.0488                        | 0.0507                       | 0.0207                        |
| <i>Impact</i>                                  | -0.0029<br>[0.0169]          | -0.0034<br>[0.0173]           | 0.0467**<br>[0.0201]         | 0.0130<br>[0.0128]            |
| <b>Family Life and Welfare Use</b>             |                              |                               |                              |                               |
| Have first child before age 18                 |                              |                               |                              |                               |
| <i>Control mean</i>                            | 0.1538                       | 0.1547                        | 0.1676                       | 0.1702                        |
| <i>Impact</i>                                  | 0.0365<br>[0.0289]           | 0.0414<br>[0.0299]            | 0.0177<br>[0.0294]           | 0.0157<br>[0.0157]            |
| Currently receiving welfare or food-stamps     |                              |                               |                              |                               |
| <i>Control mean</i>                            | 0.2059                       | 0.2068                        | 0.2414                       | 0.2458                        |
| <i>Impact</i>                                  | 0.0451<br>[0.0320]           | 0.0491<br>[0.0330]            | 0.0322<br>[0.0334]           | 0.0375<br>[0.0343]            |
| Sample size                                    | 670                          | 670                           | 710                          | 710                           |

*Note:* Estimates were obtained using weights to adjust for differences between respondents and nonrespondents. Estimates in columns (2) and (4) were derived by making the response rate for the QOP group equal to the response rate for the control group for each of the 11 schools. That is, if the QOP group had a higher response rate, we treated enough QOP group respondents as nonrespondents to lower the implied response rate to the level of the control group. The QOP group respondents that were treated as nonrespondents were the last ones to respond to the survey. : (1) In the last 12 months if short-term impacts; and in the past 2 years if long-term impacts. (2) Ever arrested or charged if short-term impacts; in the past 3 months if medium-term impacts; and in the last 2 years if long-term impacts.

\*, \*\* Estimate significantly different from zero at the 90% or 95% confidence level, two-tailed test.

<sup>a</sup> High school diploma or GED outcomes have been complemented with high-school transcript information.



TABLE A.2

Means and Impacts on High-School Completion  
Using Alternative Samples of Respondents

| <i><b>OUTCOMES</b></i>  | <i>WEIGHTED MEANS</i>   |                             |                                      |
|---|-------------------------|-----------------------------|--------------------------------------|
|   | <i>QOP mean<br/>(1)</i> | <i>Control mean<br/>(2)</i> | <i>Percentage<br/>points<br/>(3)</i> |
| 3 <sup>rd</sup> telephone survey respondents only   | 58                      | 57                          | 1                                    |
| 3 <sup>rd</sup> telephone survey respondents plus 3 <sup>rd</sup> telephone survey non-respondents for whom we have information from earlier surveys or high-school transcripts | 60                      | 60                          | 0                                    |
| 2 <sup>nd</sup> telephone survey respondents plus 2 <sup>nd</sup> telephone survey non-respondents for whom we have information from earlier survey or high-school transcripts  | 58                      | 56                          | 2                                    |

*Note:* Each impact was derived by subtracting the control group mean from the QOP group mean prior to rounding those means; thus, an impact might not equal the difference between the rounded means that are displayed. Estimates were obtained using weights to adjust for differences between respondents and nonrespondents.