Conditional Cash Transfers: The Case of Progresa/Oportunidades

Susan W. Parker

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Conditional Cash Transfers: The Case of Progresa/Oportunidades†

Susan W. Parker and Petra E. Todd*

Conditional cash transfer (CCT) programs innovate by conditioning transfers to poor families on investments in the human capital of children and other family members. The Mexican CCT program Progresa/Oportunidades began in 1997 and has served as a model for many of the now over sixty countries with CCTs around the world, in large part due to its initial evaluation with an experimental design and numerous follow-up studies. This article reviews the literature on the development, evaluation, and findings of Progresa/Oportunidades, summarizing what is known about program effects, taking into account corrections for multiple-hypothesis testing. (JEL H23, I18, I28, I32, I38, O15)

1. Introduction

Conditional cash transfer (CCT) programs were first introduced in Brazil and Mexico more than a decade ago and have since spread around the world. These programs aim to alleviate current poverty and, in addition, reduce future poverty by augmenting human-capital levels of children and youth from poor families, thus increasing their lifetime earnings potential. The

Mexican Progresa/Oportunidades1 program began in 1997 and, in its first two years, was rigorously evaluated using a randomized design. The program, which is still ongoing, conditions transfers to poor families on children’s school attendance and family visits to health clinics.

In a time when experimental evaluations of social policy in developing countries were rare, a large-scale randomized evaluation was carried out that demonstrated substantial program effects on human-capital accumulation and poverty alleviation. The program’s novelty and the finding of positive initial impacts contributed to both a large scaling up within Mexico and an impressive

*Parker: University of Maryland, College Park and Centro de Investigación y Docencia Económicas (CIDE). Todd: University of Pennsylvania. We are grateful to Ana Cristina Perez-Gea for very helpful research assistance and to three anonymous referees for very helpful comments. We also thank James Heckman and seminar participants at the Center for Global Development and the University of Maryland for useful comments and suggestions.

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1The program began in 1997 as Progresa (Programa de Educación, Salud y Alimentación) and was renamed Oportunidades at the start of the Vincente Fox administration in 2001.
spreading of the program’s key features to new programs around the world. Many governments were persuaded by the idea of simultaneously reducing current poverty and inhibiting its intergenerational transmission. CCT programs have now been implemented in over sixty countries on five continents, ranging from among the poorest countries in the world, such as Malawi, to recent initiatives in developed countries including England and the United States.

Because of the program’s emphasis on human-capital accumulation and because the randomized-evaluation data were quickly made available to researchers, the program generated great interest among academic economists. Over one hundred articles in economics and health journals have been published using the Progresa/Oportunidades evaluation datasets, making it perhaps the most studied social program in a developing country. In this article, we review the literature on the development, evaluation, and findings of the Progresa/Oportunidades program. The literature shows that the program affected a diverse set of outcomes, including, among others, income, savings, poverty, health, obesity, children’s school enrollment and attendance, migration, women’s status, and voting.

This review demonstrates how a pioneering social program can impact well-being and provide a model for development policy initiatives, based on rigorous evaluation and scaling up. It also provides a broader vision of the potential scope of CCT program impacts and their costs.\(^2\) Conditional transfers relative to unconditional transfers entail additional costs related to compliance monitoring. Economists may view conditioning payments as paternalistic and potentially welfare reducing relative to unconditional transfers, a point considered later in this article.

It should be noted that Progresa/Oportunidades and CCT programs more generally are not the first to introduce some conditionality requirements for receiving benefits. A number of welfare programs in the United States and Canada, prior to the beginning of Progresa/Oportunidades, have work requirements as a condition for receiving or continuing to receive benefits (see Blundell and Hoynes 2004 and Card and Hyslop 2005). The main innovation in Progresa/Oportunidades lies not in the conditionality per se, but rather in tying benefits to human-capital investment.

We begin in section 2 with a description of the program, and in section 3 provide basic economic theory underlying CCT program designs and their expected impacts. We then turn in section 4 to the evaluation design and methodology. We review the design of the initial randomized evaluation, which most of the impact studies use to analyze short-run program impacts. We also discuss the diverse methodologies pursued to estimate the longer-term program impacts, given that the original experimental design lasted about eighteen months, at which point eligible households in the control group began receiving benefits.

In section 5, we discuss the studies of program impacts and results, divided into the following general areas: (a) education and time use; (b) health and nutrition; (c) household income, consumption, investment, and saving; (d) gender and demography; and (e) political effects. Given the large number of studies and hypotheses tested on the impacts of Progresa/Oportunidades, we present a correction for multiple-hypotheses testing. We

\(^2\)There are some previous reviews of CCT programs, one of the most comprehensive being Fiszbein and Schady (2009), a World Bank publication. Our review differs not only by providing a more up-to-date review and focusing exclusively on the pioneering Mexican program (there has been an explosion of articles since 2009 on many previously unstudied topics), but also in its approach, which is oriented to academics, both specialists and nonspecialists in the area.
then turn to a discussion of the broader conclusions of the studies, and, in particular, what the studies suggest about how the program might be improved. In section 6, we present the results from several structural-modeling studies, which consider the effectiveness of alternative program designs, and we present evidence on the overall program benefits and costs. Section 7 concludes by suggesting directions for future research and policy.

2. Progresa/Oportunidades Description

The Progresa/Oportunidades program began just subsequent to a major macroeconomic crisis in Mexico in 1995 in which real GDP fell by 6 percent, contributing to a context where government officials began to seek greater efficiency in antipoverty spending. Progresa/Oportunidades was financed from the elimination of general food subsidies (principally to tortillas and milk), as part of a move toward implementing more narrowly targeted antipoverty programs. The majority of the budget for food subsidies was generally directed toward urban areas, and so the elimination of food subsidies and the effective substitution of Progresa/Oportunidades represented a major shift in antipoverty spending in Mexico from primarily urban to primarily rural (Levy 2006). The targeting of the poor and the empirical determination of the program-eligible population were also integral parts of the development of Progresa/Oportunidades. The program began operating in small rural communities in 1997 and gradually expanded to urban areas in 2000 and 2001. It remains predominantly a rural program, with about 70 percent of all beneficiaries in 2013 from rural areas. As of 2013, the program covers almost six million households, about 20 percent of all households in Mexico.

As previously noted, the program’s principal innovation is to condition monetary transfers on human-capital investment. In particular, it provides cash payments to families that are tied to children regularly attending schools and family members visiting health clinics for checkups. Additionally, nutritional supplements are given to pregnant women and to children under the age of five. Program take-up was exceedingly high when the program first began in rural areas, with 97 percent of families who were offered the program electing to participate.

The program provides benefits in education, health, and nutrition. The program’s design aimed to exploit synergies in investments in these three areas. For instance, children who suffer from malnutrition might be more likely to drop out of school, implying that insuring that children go to school will be more effective if combined with health or nutrition interventions. Based on a review of the literature on the determinants of education, health, and nutrition investments from around the world, Behrman (2000) suggests that synergies in the Progresa/Oportunidades program might be substantial. Program documents also cite the conditioning of benefits as important for helping beneficiaries to develop a sense of “ownership” of the program and to avoid the transfers being perceived as a handout (Levy 2006).

Table 1 provides a general overview of the evolution of the program over time with respect to growth of beneficiaries, the importance of the program in the federal budget, and average amounts of benefits. The program has grown substantially over time and by 2010 its budget represented about half of 1 percentage point of GDP. Average monetary benefits received by beneficiary households in the beginning years of the program were about US$300 per year and have increased substantially over time, largely reflecting that additional monetary benefits have been progressively added to the program (although the majority of the evaluations focus only on the early years). Interestingly, average
benefit receipt, which varies somewhat from year to year, is only at about half the level of potential benefit receipt. This reflects both the demographic structure of beneficiary households, as well as the less than 100 percent take up of education and health benefits, for instance by eligible children who do not enroll in school. We now turn to a more detailed description of program benefits.

2.1 Education

_Progresa/Oportunidades_ provides monthly educational grants and in-kind school supplies for children under twenty-two years of age who are enrolled in school between the third grade of primary and the third grade of secondary school (e.g., up until twelfth grade).\(^3\) Originally, the program provided grants only for children in the third through ninth grades, but in 2001, the grants were extended to grades ten through twelve. Table 2 shows the monthly grant levels available for children between the third grade and the twelfth grades in the first semester of 2003. Grants increase as children progress to higher grades and, beginning at the junior high level, are slightly higher (by 10 to 15 percent) for girls than for boys. Higher grants for girls were motivated by the observation that in rural areas, girls tended to have a higher drop-out rate than boys after finishing primary school.\(^4\)

\(^3\)The Mexican education system has grades one through six as primary school (primaria), seven through nine as junior high school (secundaria), and ten to twelve as senior high school (media superior).

\(^4\)However, Behrman, Sengupta, and Todd (2005) show that actual attainment of girls in terms of grades of completed schooling in rural areas preprogram averaged as high or higher than that of boys. That enrollment rates, nevertheless, were higher for boys reflects that boys
The specific grant amounts range in 2003 from US$9.50 (105 pesos) in the third grade of primary to about US$53 (580 pesos) for boys and US$60 (660 pesos) for girls in the third year of senior high school (grades ten–twelve). For comparison, the minimum wage in Mexico was 44 pesos per day in 2003 (with some minor variations by region), corresponding to about 966 pesos monthly for full-time work (22 days). By the senior year of high school, the grant amount represents about two-thirds of a minimum wage. To receive the grant, parents must enroll their children in school and ensure their regular attendance, defined as 85 percent of days. The grant is child-specific. Program rules allow students to fail each grade once, but students are not allowed to repeat a grade twice and continue to receive the grant.\(^5\) Enrollment and attendance are verified before grants are paid. All monetary grants are given to the mother of the family, with the exception of scholarships for upper-secondary school, which the youth can receive themselves subject to the mother’s authorization.

\(^5\)In Mexico, students usually take exams that they have to pass to advance to the next grade. In the evaluation sample, failing grades is fairly common.
2.2 Health and Nutrition

The health-care component provides basic health care for all members of the family, with some emphasis on preventive care (table 3). The services are provided by public-health institutions in Mexico. The nutritional component includes a fixed monthly monetary transfer equal to about US$14.00 (155 pesos) (specified to be for “improved food consumption,” although the expenditures of beneficiaries are not monitored), as well as nutritional supplements, which are principally targeted to children between the ages of four months and two years, and pregnant and lactating women. Nutritional supplements are also given to children aged two to four who show signs of malnutrition. To receive the health transfers, all members of beneficiary families must comply with a regular schedule of clinic visits (table 4). Beneficiaries (generally mothers) are also required to attend monthly health and nutrition talks at the clinic on topics such as nutrition, hygiene, and immunization. When education grants were extended to senior-high school in 2001 (grades ten–twelve), high-school students are also required to attend (separate) talks on adolescent themes.

2.3 Size of Monetary Transfers

There is a limit of monthly benefits for each family equivalent to about US$80 for families with children in primary school and junior-high school and US$146 for those with at least one child in high school (grades ten–twelve). Benefits are provided directly to female beneficiaries by wire transfer to their bank accounts or to offices physically located near the communities. The design feature that benefits are provided to women, generally mothers, was motivated by the early literature on intra-household allocations, e.g., Thomas (1990), that showed that income in the hands of women has greater effects on child well-being than income in the hands of men.6

6Beginning in 2006, a number of extensions to program benefits have been added including a fixed benefit for each elderly member in the household (in 2006), a fixed monetary benefit linked to energy consumption (2007),

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**TABLE 3**

INTERVENTIONS IN THE BASIC HEALTH-SERVICES PACKAGE: PROGRESA

- Basic hygiene
- Family planning
- Prenatal, childbirth, and postnatal care
- Supervision of nutrition and children's growth
- Vaccinations
- Prevention and treatment of outbreaks of diarrhea
- Antiparasite treatment
- Prevention and treatment of respiratory infections
- Prevention and control of tuberculosis
- Prevention and control of high blood pressure and diabetes mellitus
- Accident prevention and first aid for injuries
- Community training for health care self-help

*Source: Oportunidades, 2004 (Program Operating Rules) oportunidades.gob.mx.*
2.4 Targeting and Eligibility

The program is means tested with both geographic and household-level targeting. In both rural and urban areas, the first stage of targeting is geographic, using aggregate local indicators to select poor rural communities and urban census blocks. In the second stage of targeting in rural areas, Progresa/Oportunidades carries out a survey of socioeconomic conditions for all households in the selected communities (*Encuesta de Caracteristicas Socio-economicos de los Hogares* or ENCASEH survey). With these data, discriminant analysis is used to distinguish eligible from noneligible households using household characteristics that include dwelling conditions, dependency ratios, ownership of durable goods, animals and land, and the presence of disabled individuals to predict household income. A cutoff divides eligible from ineligible households and benefits are offered to all those above the cutoff. Eligible households are offered the program and, once they agree to participate, they receive registration forms for schools and the family clinic.\(^7\) Nearly all

<table>
<thead>
<tr>
<th>Age group</th>
<th>Frequency of check-ups</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Children</strong></td>
<td></td>
</tr>
<tr>
<td>Less than 4 months</td>
<td>3 check-ups: 7 and 28 days, and at 2 months</td>
</tr>
<tr>
<td>4 months to 24 months</td>
<td>8 check-ups: 4, 6, 9, 12, 15, 18, 21, and 24 months with 1 additional monthly weight and height check-up</td>
</tr>
<tr>
<td>2 to 4 years old</td>
<td>3 check-ups a year: 1 every 4 months</td>
</tr>
<tr>
<td>5 to 16 years old</td>
<td>2 check-ups a year: 1 every 6 months</td>
</tr>
<tr>
<td><strong>Women</strong></td>
<td></td>
</tr>
<tr>
<td>Pregnancy</td>
<td>5 check-ups: prenatal period</td>
</tr>
<tr>
<td>Postpregnancy</td>
<td>2 check-ups: 1 immediately following birth and 1 during lactation</td>
</tr>
<tr>
<td><strong>Adults and youths</strong></td>
<td></td>
</tr>
<tr>
<td>17 to 60 years old</td>
<td>One check-up per year</td>
</tr>
<tr>
<td>Over 60 years old</td>
<td>One check-up per year</td>
</tr>
</tbody>
</table>

Source: Oportunidades, 2004 (Program Operating Rules) oportunidades.gob.mx.
selected families enrolled in the program in rural areas, so that self-selection in program participation is not a significant evaluation concern for studies using the experimental data.\(^8\) Skoufias, Davis, and de la Vega (2001) analyze the rural targeting, comparing the actual targeting to potential targeting based on household consumption levels, as well as other alternatives including purely geographic targeting. They show the program is generally well-targeted to the poorest households. In fact, beneficiary households show larger reductions with the program in the severity of poverty (or average distance to the poverty line) than in the proportion of households in poverty. The simulations also show, however, that almost as large reductions in poverty would be achieved with a targeting mechanism based only on geographic or community-level targeting.\(^9\)

In urban areas, the process by which individuals enroll in the program differs from that in rural areas. Individuals apply for the program at modules set up throughout the country, which are typically only open for a few weeks. Individuals answer a basic socioeconomic survey (similar to the ENCASEH) and, on the basis of these self-reports, an initial eligibility classification is determined. For those declared initially eligible for Progresa/Oportunidades, there is a home visit to verify the socioeconomic information given on the application form and confirm eligibility. Self-selection in program participation is an issue in urban areas, as many eligible households do not apply, in part because they are unaware of the program or of their eligibility for the program. A smaller fraction of eligible households participate than in rural areas (Coady and Parker 2009). There are also strong incentives to underreport economic conditions at the time of application to become eligible for the program. Martinelli and Parker (2009) show that underreporting of durable goods is rampant, but that, curiously, there is some overreporting of basic dwelling conditions, likely reflecting embarrassment in reporting the lack of basic conditions such as running water.

Beneficiary families remain in the program for three years without further verification of their economic status. After three years, a reinterview takes place, at which point, either their beneficiary status is renewed or they are transitioned to a scheme of partial benefits (called Esquema Diferenciado de Apoyos or EDA), which includes only secondary and high school educational grants but excludes primary-school grants and cash transfers associated with the health/nutrition component.

3. **Theoretical Considerations**

We next develop a simple theoretical model to show how CCTs imply both a price and an income effect. The price effect occurs because the subsidy reduces the shadow wage (or relative value) of children’s time in activities other than school (Martinelli and Parker 2008; Todd and Wolpin 2006; 2008; Attanasio, Meghir, and Santiago 2012). There is also an income effect, because the subsidy increases a family’s total potential income. Although school subsidies reduce the marginal cost of schooling and might therefore be expected to increase school-going, they can have a priori ambiguous effects on work and leisure.

Consider a simple two-period model, where in the first period, individuals are children and can allocate their time among leisure, work, and school. In the second period, they are adults and can choose between leisure and work, with a wage that depends on the time spent in school in the first period.

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8 However, Alvarez, Devoto, and Winters (2008) analyze the dropout program in the first five years of Progresa and show that relatively poorer beneficiaries are less likely to dropout, which they attribute to program conditionality potentially acting as a screening device.

9 As the program has expanded into more economically diverse areas, however, it is likely the gains for second stage targeting become larger.
Let $C_1$ and $C_2$ denote consumption of the individual as a child and as an adult, $L_1$ and $L_2$ denote leisure, $S$ the amount of schooling, $W_1$ the child wage rate, $W_2(S)$ the adult wage rate, and $A$ transfers to the individual from parents and other family members. Also, let $t_1$ denote amount of time spent working in period 1 and $t_2$ the amount of time spent working in period 2. $\beta$ denotes the discount rate, $r$ is the market interest rate, and $p_s$ the subsidy paid for amount of schooling attended. We assume diminishing marginal utility of consumption and leisure in each period and diminishing marginal productivity of schooling on the second-period wage rate. We also assume that consumption and leisure are normal goods. For simplicity, there is no direct utility from schooling, which solely provides a technology for transferring resources from the first to the second period and increasing the second-period wage rate.

Individuals maximize the objective function

$$U_1(C_1, L_1) + \beta U_2(C_2, L_2)$$

subject to the constraints

$$S + t_1 + L_1 \leq T$$
$$t_2 + L_2 \leq T$$
$$C_1 + C_2/(1 + r) = A + p_s S$$
$$+ (T - L_1 - S)W_1$$
$$+ (T - L_2)W_2(S)/(1 + r).$$

An optimality condition that holds in any interior solution of the problem is

$$MU_{L1} - MU_{C1}p_s = \beta MU_{C2}t_2 W_2'(S)$$

or equivalently,

$$MU_{C1}(W_1 - p_s) = \beta MU_{C2}t_2 W_2'(S).$$

The left side of the equation is the marginal cost of time spent in school, in terms of monetary direct costs and foregone leisure (or equivalently, foregone earnings from work, since another optimality condition for interior solutions equates the marginal utility from leisure with the marginal utility in added consumption derived from work). The right side is the marginal benefit of spending additional time in school, i.e., higher earnings as an adult.

The optimality condition shows that the subsidy affects the marginal costs of schooling by reducing the shadow wage (or relative value) of children’s time in activities other than school. We also assume that consumption and leisure are normal goods. For simplicity, there is no direct utility from schooling, which solely provides a technology for transferring resources from the first to the second period and increasing the second-period wage rate.

The program may also affect the time use of adults in the household, and the effects of a conditional transfer are likely to differ compared with an unconditional cash transfer. An unconditional cash transfer represents a pure income effect, which is expected to increase consumption of leisure (assuming leisure is a normal good) and reduce the time dedicated to work activities. On the other hand, a conditional transfer that subsidizes the time allocated to children’s schooling

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10 Assuming the marginal opportunity cost of boys’ schooling is not higher than for girls’ schooling.

11 However, schooling levels for girls have been rising relative to that of boys in many Latin American countries (Grant and Behrman 2010).
may lead to a cross-substitution effect on the time allocation of adults, assuming that work activities of children and adults are substitutes. Theoretically, the total effect of the program on time dedicated to work for adults is ambiguous (Rubio-Codina 2010).

Conditioning transfers on particular behaviors might appear to most economists as paternalistic and not likely to be welfare improving, relative to providing unconditional transfers. There are, however, several potential rationales for conditional transfers. One is that the private returns to school may be lower than the social returns, which would justify school-attendance subsidies. A second rationale is that students may not be well informed about the returns to education. If there is no good way of informing them, then conditional transfers can bring about a closer to optimal rate of attendance. Attanasio and Kaufmann (2014) provide evidence on perceptions about returns to schooling for the *Progresa/Oportunidades* population, demonstrating that mothers have significantly different expectations about the returns to education than their children and these expectations affect actual enrollment decisions. Chiapa, Garrido, and Prina (2012) further demonstrate that participation in the *Progresa/Oportunidades* increases parents’ educational aspirations for their children. A third motivation for conditioning transfers is the role of conditionality in implementing outcomes that are favorable to the child in the context of intrahousehold bargaining. For example, unconditional transfers to the child could be undone by a reduction in the resources transferred by family members to the child, as shown by Martinelli and Parker (2003).

Finally, Gahvari and Mattos (2007) point out that CCT programs may be useful for implementing redistribution without distortionary losses; that is, a combination of cash and in-kind transfers can be used as a screening device to distinguish rich from poor individuals. They consider a situation in which an indivisible good such as formal education or health services can be provided in different qualities and people must decide which of the qualities to consume. By accepting (self-selecting) lower-quality education, individuals reveal their type, e.g., whether they are in need of poverty-alleviation programs or not.

4. Evaluation Design and Methodology

Begun in 1998, the evaluation of *Progresa/Oportunidades* was a pioneer in the introduction of randomized designs for evaluating development programs. The evaluation was a model for many subsequent pilot programs and randomized experimental designs (Duflo and Kremer 2005). The initial impact-evaluation results were used to modify the program design and to justify scaling up the program, noteworthy at the time given that few of the pilot intervention initiatives of the previous decade had been scaled up. The *Progresa/Oportunidades* program has also been influential in the development of similar programs around the world and their rigorous evaluation (Fiszbein and Schady 2009).

4.1 Initial Experimental Evaluation Design

For the experimental evaluation, 506 rural communities from 7 Mexican states were selected in 1997, with 320 randomly assigned to receive benefits immediately and the other 186 to receive benefits later. The randomization was carried out by *Progresa/Oportunidades*.

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12 Time costs of complying with program requirements may fall disproportionately on women, who must attend a monthly health talk, as well as pick up transfers. Other time-compliance costs may be time spent taking children to school and required appointments at the health clinic.

13 Private returns to schooling may broadly include both standard monetary returns and nonpecuniary returns associated with greater schooling, e.g., greater happiness, better marriage decisions, and parenting etc. (Oreopoulos and Salvanes 2011).
Eligible households in the original treatment localities began receiving program benefits in the spring of 1998, whereas the eligible households in the control group began receiving benefits at the end of 1999, after which both groups continued to receive program benefits.

The International Food Policy Research Institute (IFPRI) was hired to coordinate the external evaluation between 1998 and 2000 and in turn hired several academic economists to carry out the initial principal studies, as well as assigning several IFPRI employees to be in-staff at Progresa/Oportunidades offices. The design of the questionnaires was a joint effort between IFPRI, Progresa/Oportunidades staff, and the external hired economists, and the survey work was hired by Progresa/Oportunidades with supervision by IFPRI staff. Evaluation questionnaires (called ENCELs) were applied every six months over the time period 1997 to 2000, with a final follow up in 2003. Table 5 presents a timeline of the experimental valuation. The majority of existing studies focus on the initial experimental period, when

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### TABLE 5

| Timeline for Progresa Rural Evaluation and Data Sources |
|-----------------|----------------|----------------|----------------|----------------|
|                | Fall, 97 | March, 98 | May, 98 | Nov, 98 | May, 99 |
| 1) ENCASEH survey to determine program eligibility | X | | | | |
| 2) Experimental design (randomization) 506 communities, 320 T1998, 186 T2000 | X | | | | |
| 4) Treatment (T1998) begins to receive benefits | | | X | | |
| 5) Follow-up ENCEL | | X | X | X |
| 5) Follow-up ENCEL | | | | | X |
| 6) Control group (T2000) begins to receive benefits | | | | X |
| 7) New comparison group added (C2003) to sample 152 new rural communities | | | | X |

**Definitions:**

- T1998 = original treatment communities under experimental design, began receiving benefits in May 1998.
- T2000 = original control communities under experimental design, began receiving benefits in January 2000.

**Source:** Oportunidades, 2004. *Nota metodológica de la muestra rural.*

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14 Petra Todd served as an outside observer at the time of the randomization of villages into the treatment and control groups in Mexico City. The detailed history of how the randomization and evaluation of the program came to be has been the subject of several papers and books (see Levy 2006, Behrman 2010, and Lustig 2014).
treatment group received benefits and the control group did not.\textsuperscript{15}

An important design feature of the rural evaluation is that while benefits were provided only to program-eligible households, the evaluation surveys interviewed all households in the villages, including non-eligible households. Some studies restrict impact analysis to only those eligible within the treatment and control communities (effectively a treatment on the treated estimator given very high take-up). A number of studies, however, take advantage of the data available on all households within the villages to separately analyze program impacts on eligibles from ineligibles, interpreting any impacts on the non-eligible population as program spillovers. In fact, Angelucci and De Giorgi (2009) argue that, because of spillovers, misleading impacts would have been obtained were randomization of treatment and control households to have occurred within the same villages.

Behrman and Todd (1999) evaluate the quality of the randomization by carrying out tests of the existence of significant differences in preprogram variables for a wide variety of indicators. In general, they conclude that at the community level, the level at which the randomization was done, treatment and control groups appear to be random. Nevertheless, at the individual level, where most of the analysis in the studies based on the experimental design were done, due to the larger sample size, they find some generally small but statistically significant differences in preprogram characteristics between the treatment and control group for a number of different characteristics.\textsuperscript{16}

4.2 Estimating Longer-Term Effects of the Program

Most of the Progresa/Oportunidades evaluation studies focus on the time period before the control group began to receive benefits, but long-term impacts of the program are of substantial interest, given its emphasis on intergenerational changes in poverty. Longer-term effects can, however, be measured directly only after a significant number of years of program operation. Short-term estimates based on one or two year’s exposure to a program may be used to extrapolate to long-run program impacts (for instance, Behrman, Sengupta, and Todd 2005; Schultz 2004; Todd and Wolpin 2006), but with a number of assumptions (King and Behrman 2009). The fact that the experimental control group began to receive benefits after eighteen months presents challenges for assessing long-term impacts directly, given most beneficiaries participate for a number of years. Several different strategies have, however, been pursued to estimate longer-term impacts, which we now briefly describe.

A first strategy are estimators based on follow-ups of the original treatment and control group, e.g., comparing a group offered $X$ years of benefits versus $X - 18$ months of benefits. These estimators have the advantage of preserving the randomized design and can show, for instance, whether initial impacts are maintained in the longer run or whether the control group “catches up”

\textsuperscript{15}There was also a later evaluation of Progresa/Oportunidades in urban areas, beginning with a baseline survey in 2002 and follow-up rounds in 2003 and 2004. The evaluation design included the selection of households in poor blocks and selection of comparison blocks using matching. There are only a few published studies using the urban data; we cite these in the review below.

\textsuperscript{16}At the individual level, a larger number of significant differences exist than would be expected by chance alone (32 percent of 187 characteristics studied). Behrman and Todd argue that this may in part reflect the large sample size at the individual level (e.g., 24,000 households and more than 100,000 individuals) and thus a tendency to reject the null hypothesis that differences are not significant at conventional significance levels.
(Behrman, Parker, and Todd 2009b, 2011; Biancha and Bobba 2013; Gertler, Martinez, and Rubio-Codina 2012). Relatedly, some evaluations focus on subpopulations that may have “passed the peak moment” for receiving benefits at the time they were offered benefits. An example is Fernald, Gertler, and Neufeld’s (2009) *Lancet* study of the ten-year impacts for children born into the program versus those who began to receive benefits at eighteen months or so of age and who would have missed receiving the early nutritional program components during the, as argued by the authors, critical zero-to-eighteen-month period. Note that these longer-term estimates of the original treatment and control groups include both the effect of being offered the program for an additional eighteen months and the effect of being offered benefits at an earlier time in the life cycle.

A second evaluation strategy for longer-term impacts of the program on the experimental treatment group is based on the addition of a new matched-comparison group of localities in which the program was not yet operating to the original experimental sample. In 2003, a group of nonparticipating communities was selected (through matching based on community-level socioeconomic characteristics from the 2000 Mexican Census) to be observably similar to the original treatment communities in an effort to allow estimation of receiving the program for a longer period of time against a benchmark of no treatment. All households in these 151 communities were interviewed in 2003, in addition to a follow-up interview of the original 506 communities. Retrospective information was also collected on the nonparticipating communities to determine their program eligibility status in 1997, when the program began in the treatment communities (table 5). This comparison group allows, in 2003, the estimation of impacts of about 5.5 years post-program (using the original treatment group) compared with the matched comparison group that never received the program (Behrman, Parker, and Todd 2011, Bianchi and Bobba 2013).17

A third approach used to evaluate longer-term impacts is to use nonexperimental administrative or household data, which cover a longer period of time and/or a more representative sample of households than did the experimental-evaluation sample that was drawn from seven states in Mexico. For instance, Barham (2011) and Barham and Rowberry (2013) use vital-statistics data and administrative information on program beneficiaries to construct a longitudinal municipal-level database on mortality. They use variation in the proportion of beneficiaries in a municipality across municipalities and over time to estimate program impacts.

### 4.3 Estimating Effects of Alternative Program Designs

Because of the number of different program components, one challenge common to all of the evaluation studies is how to identify the source of program impacts, i.e., are positive effects on education due to the conditionality of the education grants, the increased household income, or the effect of mothers being the transfer recipients? Because the evaluation design in *Progresa/Oportunidades* compares the entire package of benefits versus the alternative of no program, isolating the impacts of specific program features is difficult. Some studies do, however, try to isolate the effect of conditionality versus income in health and education by using variance in transfer receipt or other characteristics affecting the degree of participation. Several estimation studies develop structural behavioral models that can be used to separate the effect of conditionality from the income

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17To distinguish in the discussion of results, the original treatment group we term T1998, the original control group T2000, and the new comparison group C2003.
effect and analyze the effects of cash transfer designs that differ from the one that was implemented, e.g., Todd and Wolpin (2006, 2008) and Attanasio, Meghir, and Santiago (2012). The behavioral models estimated in these papers provide ways of extrapolating from the observed data to alternative transfer amounts. We turn next to a discussion of the methodology of these studies.

There are two main approaches pursued in the literature: a nonparametric structural approach and a dynamic, fully parametric structural approach. Todd and Wolpin (2008) use nonparametric matching estimators combined with a structural model to perform an ex ante evaluation using the rural data from the initial Progresa experiment. They develop a simple optimizing model of whether to send a child to school or to work at a wage $w$, and they show that the effect of a school subsidy $\tau$ is analogous to simultaneously increasing the household’s income from $y$ to $y + \tau$ and decreasing the child’s wage offer to $w - \tau$. The matching estimator simulates the effect of introducing the program by matching program-eligible households with income $y$ and child-wage offer $w$ to other households with income $y + \tau$ and child-wage offers $w - \tau$ (using child-wage variation observed across villages). They find that program-impact estimates on schooling obtained using this nonparametric matching approach applied to control group data come close to the experimental estimates. Azevedo, Bouillon, and Yanez-Pagans (2009) use the same method to evaluate the urban Oportunidades program.

A fully nonparametric approach is not possible for more complex behavioral models. For example, as discussed in Todd and Wolpin (2009) and Wolpin (2013), the nonparametric approach breaks down when there are three mutually exclusive choices for children’s time: school attendance, leisure, and work. An alternative is to adopt a more conventional fully specified parametric model. Todd and Wolpin (2006) and Attanasio, Meghir, and Santiago (2012) develop dynamic discrete choice dynamic programming (DCDP) models that they estimate using the rural Progresa data. Wolpin (2013) provides a critical review of these two papers, comparing the modeling assumptions and results in detail.18

5. Studies of Progresa/Oportunidades’s Effects

We now turn to the evidence on Progresa/Oportunidades impacts. The initial evaluations focused primarily on short-run schooling, health, and expenditure effects, but over time the literature has expanded to include longer-term analysis of these variables and a variety of other indicators. We organize impact studies into the following categories: (a) education and time use; (b) health and

18 In Todd and Wolpin (2006), in each year, a married couple decides on whether each of their boys and girls between the ages of six and fifteen will attend school, remain at home, or, for those ages twelve to fifteen, work in the labor market (mutually exclusive options). The couple also decides on pregnancy. The couple receives flow utility in each period from consumption, the current stock of children, the children’s current schooling attainment, the children staying home, and the wife’s pregnancy status. Household income includes parental income and children’s wage income. The observed outcomes are (1) the decision made by the couple about whether to have a pregnancy, which children to send to school, which to work in the market, and which to remain home; (2) the wages received by children who work; (3) the success or failure of each child who attends school to complete the grade level; and (4) parental income. Model parameters are estimated using data on the randomized-out control group, incorporating these outcomes.

The Attanasio, Meghir, and Santiago (2012) model is also a dynamic discrete-choice model, but it differs significantly from the Todd and Wolpin (2006) model. Attanasio, Meghir, and Santiago (2012) consider the binary decision of whether the child goes to school or works, excluding the at-home option. They model the decision about each child independently of other children and do not model fertility. The flow utility depends on whether the child is attending school, the current school attainment, factors that affect the cost of attending primary school, factors that affect the cost of attending secondary school, and unobservable child- and time-varying factors.
nutrition; (c) household income, consumption, investment, savings; (d) gender and demography; and (e) political and environmental effects. The large number of papers altogether test a large number of hypotheses. For this reason, at the end of this section, we perform several multiple-hypothesis corrections of the principal impact studies.

5.1 Education and Time Use: Education

With respect to the direct objective of improving the level of schooling, the studies of Progresa/Oportunidades have been positive and little debate exists over both the sign and the magnitude of the results. Early evaluation studies using the experimental design and data demonstrated positive effects on improving school enrollment, reducing grade repetition, and increasing completed grades of schooling. Schultz (2004) studied program impacts on school enrollment in the first eighteen months of the program and demonstrated that the largest impacts occurred at the transition between primary and secondary school (sixth to seventh grade) with increases on the order of 4 to 5 percentage points for boys and 8 to 10 percentage points for girls. Behrman, Sengupta, and Todd (2005) use the same data to estimate a Markov schooling-transition model that compares transition matrices between the treatment and control groups, analyzing for each age program impacts on enrollment, repetition, dropout, and school reentry. There are few impacts on enrollment for younger children (ages six to ten) for whom enrollment rates are very high even in the absence of the program. However, younger children experience large reductions in grade repetition and better grade progression. At the junior-high-school level (grades seven–nine), the program reduces the dropout rate and also encourages reentry among those who have dropped out. Both Schultz (2004) and Behrman, Sengupta, and Todd (2005) use their short-term estimates to predict the effects on overall schooling levels. In spite of different methodologies, they both find an overall increase of 0.6 grades for a child receiving grants from primary school through junior high school (grades three–nine).

Younger children have also benefitted from the program. Both Behrman, Parker, and Todd (2009b) and Todd and Winters (2011) show that the program reduced the age of entry to primary school. This impact may reflect, at least partially, the impacts of the early nutrition interventions as well as possible income effects and the incentives that the program gives to enroll in child in school earlier to receive the education grant starting at grade three.

Dubois, de Janvry, and Sadoulet (2012) use the experimental data to estimate a dynamic model of schooling decisions along with program impacts. They demonstrate positive impacts on enrollment at the primary and secondary levels. They argue that, with respect to performance as measured by grade progression, the program increases performance at the primary level but reduces it at the secondary level. They offer a possible explanation in that education grants under the initial program (until 2001) were provided only up until ninth grade. Program rules allowed failure of one grade, providing a potential incentive to repeat secondary school to continue receiving program benefits.

Later studies based on actual (rather than simulated) impacts in the medium term (Behrman, Parker, and Todd 2009b, 2011) find that extended time participating in the program leads to significant improvements in grades completed, about one full grade for children who participate in the program for six years beginning at ages nine to twelve, compared to nonparticipating children. Studies based on structural estimation also find program effects of a similar magnitude (Todd and Wolpin 2006, Attanasio, Meghir, and Santiago 2012).
<table>
<thead>
<tr>
<th>Study authors, year</th>
<th>Method: Data and estimation techniques</th>
<th>Impacts</th>
</tr>
</thead>
<tbody>
<tr>
<td>Schultz (2000)</td>
<td>Difference-in-difference regression estimates of the impacts for the first 18 months of the program using randomized experiment.</td>
<td>1. Enrollment/attendance: No significant effects on days enrolled in school at the primary or secondary level.</td>
</tr>
<tr>
<td>Behrman, Sengupta, and Todd (2000)</td>
<td>After program difference estimators of impacts using experimental design 18 months after program for a subsample who took the tests, applied in schools.</td>
<td>2. Performance in school; grades of schooling/test scores: No significant impact on achievement tests. Impacts likely to be underestimated due to their application in schools.</td>
</tr>
<tr>
<td>Skoufias and Parker (2001)</td>
<td>Difference in difference before and after for first 18 months of program using experimental design.</td>
<td>3. Labor-force participation and time in work: Boys 8–11 enrollment impacts of 1.3–1.8 percentage points, girls 8–11 no significant effects. Boys 12–17 3.2–5.8 percentage points, girls 12–17 7.5–9.5 percentage points. Reductions in the probability of working of up to 6 percentage points for boys and 3 percentage points for girls for those aged 12–17 preprogram. Reductions of about 1 percentage point in participation in work for boys 8–11 preprogram, no impact on girls age 8–11 preprogram. Reductions in participation in domestic work of girls aged 12–17 pre-program in 4 percentage points.</td>
</tr>
<tr>
<td>Schultz (2004)</td>
<td>Difference in differences before and after estimates for first 18 months of program using experimental design.</td>
<td>Increase in primary school enrollment of 0.8 percentage points for boys and 0.9 percentage points for girls. Increase in secondary school enrollment of 6 percentage points for boys and 9 percentage points for girls. Simulation of long run impact of program: increase schooling by 0.6 years.</td>
</tr>
<tr>
<td>Coady and Parker (2004)</td>
<td>Difference in differences before and after estimates for first 18 months of program using experimental design.</td>
<td>Increase in average enrollment of 5–8 percentage points for boys and 11–12 percentage points for girls in secondary level.</td>
</tr>
<tr>
<td>Behrman, Sengupta, and Todd (2005)</td>
<td>Markov model, estimate impacts on transition matrices that estimate probability of transition between possible educational states including enrollment, dropout, failure, and repetition between two years. Use of random experiment design.</td>
<td>Significant impact on reducing repetition, dropout, and increasing progression in primary and secondary. Simulation of short-run impacts implies increase of 0.7 years of schooling in long run.</td>
</tr>
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</table>

(Continued)
### TABLE 6
**Education Impacts (Continued)**

<table>
<thead>
<tr>
<th>Study authors, year</th>
<th>Method: Data and estimation techniques</th>
<th>1. Enrollment/attendance</th>
<th>2. Performance in school; grades of schooling/test scores</th>
<th>3. Labor-force participation and time in work</th>
</tr>
</thead>
<tbody>
<tr>
<td>Behrman, Parker, and Todd (2009a)</td>
<td>Follow up after six years. Difference-in-differences estimates based on original experimental design and difference-in-differences matching using non-experimental comparisons based on groups with different program exposure. 5.5 years, 4 years and never receiving program. Children 0–8 prepilot.</td>
<td>Reduction in the age at entering school in those aged 1–2 years in (preprogram) 1997.</td>
<td>Children aged 12–14 in 2003 accumulate about 0.5 grades after 5.5 years of program benefits: increase of 8–9 percent of grades of current completed schooling.</td>
<td></td>
</tr>
<tr>
<td>Lalive and Cattaneo (2009)</td>
<td>With experimental design during first 18 months, uses IV regressions to estimate peer effects, the instrument is eligible children in the same school grade in the community.</td>
<td>10 percentage point increase in peer group school attendance leads to a 5 percentage point increase in individual school attendance.</td>
<td></td>
<td></td>
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<tr>
<td>Angelucci et al. (2010)</td>
<td>Difference-in-differences estimates from experimental design first 18 months. Papers compares impacts for those with other family members residing in the villages (connected) to those with no other family members in the village (isolated).</td>
<td>Increase of 8.3 percentage points on enrollment in secondary school for youth in connected households, insignificant results for isolated households.</td>
<td></td>
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</tbody>
</table>
### TABLE 6
**Education Impacts (Continued)**

<table>
<thead>
<tr>
<th>Study authors, year</th>
<th>Method: Data and estimation techniques</th>
<th>Impacts</th>
</tr>
</thead>
<tbody>
<tr>
<td>de Brauw and Hoddinott (2011)</td>
<td>Nearest neighbor matching comparing beneficiaries with completed grades 3–8 who receive enrollment forms with those who did not to measure conditionalities’ effect.</td>
<td>Those not receiving enrollment form show impacts lower by 4.5 percentage points than for the group who receives the form.</td>
</tr>
<tr>
<td>Behrman, et al. (2012)</td>
<td>Before and after program difference-in-differences matching estimator for years 2002–2004, urban sample.</td>
<td>Impacts on girls 6–20 of 2.5 percentage points after 2 years of benefits. Significant impacts on boys aged 6–20 of 0.1 additional grades, 0.12 additional grades for girls.</td>
</tr>
<tr>
<td>Behrman, Parker, and Todd (2011)</td>
<td>Follow up after six years. Difference-in-differences estimates based on original experimental design and difference in difference matching using nonexperimental comparisons based on groups with different program exposure: 5.5 years, 4 years, and never receiving program.</td>
<td>Early health intervention increases the probability of enrolling in primary-school at age 6 in 3 to 5 percentage points. Early health and nutrition intervention decreases missed days of school per month in 0.12.</td>
</tr>
<tr>
<td>Todd and Winters (2011)</td>
<td>Double-difference estimator on observations from two age cohorts of beneficiary children, one exposed to program at birth, the other at age 3.</td>
<td>Program increases grade progression at the primary level and reduces grade progression at the secondary level.</td>
</tr>
<tr>
<td>Rubio-Codina (2010)</td>
<td>Cross-sectional comparisons treatment and control after 12 months of program using experimental design.</td>
<td>Significant reductions in work hours of boys and girls aged 12 to 17 of 0.15 hours. Reduction in 0.15 hours of hours in domestic work for girls.</td>
</tr>
</tbody>
</table>
An important question is to what extent similar schooling impacts could have been achieved with unconditional payments. There are a few experimental studies for other countries (See Baird, McIntosh, and Ozler 2011 for Malawi; Benhassine et al. 2013 for Morocco; and Akresh, de Walque, and Kazianga 2013 for Burkina Faso) that compare the effects of conditional and unconditional transfers. The results from Malawi and Burkina Faso suggest higher impacts on schooling outcomes with conditional payments, relative to unconditional payments. For instance, in Malawi impacts on enrollment in the group with unconditional payments were 43 percent the size of impacts on enrollment for the conditional group. Impact results from Morocco, however, were similar for conditional transfers and a treatment arm where payments were loosely tied to education but attendance was not monitored.

In the Progresa/Oportunidades context, several studies offer nonexperimental evidence on the potential effects of conditionality. De Brauw and Hoddinott (2011) take advantage of the fact that there are a number of beneficiary households who report not receiving enrollment forms at the beginning of the program, arguing that for these households benefits are effectively unconditional, as the program cannot monitor students for whom they have no information on whether or not they are enrolled. The authors report impacts of the program are between 5 and 10 percentage points smaller than for those groups receiving the forms, implying that conditionality accounts for a large fraction of total impacts on education. Todd and Wolpin (2006) use a structural model to compare impacts from a conditional and an unconditional cash-transfer program design and find the conditionality explains most of the observed increases in schooling; unconditional transfers of the same size would lead to schooling impacts less than a quarter of the size of those from the actual program.

The data gathered for the initial rural impact evaluation included both program eligible and ineligible households in both treatment and control communities, and several studies investigate whether there are spillover effects of the program on ineligible households. Spillover effects could be negative if, for example, higher school enrollment of program-eligible children leads to lower school quality for ineligible children. However, both Lalíve and Cattaneo (2009) and Bobonis and Finan (2009) report that the program increased significantly the school enrollment and progression of ineligible children and youth in the treatment communities. Relatedly, Angelucci et al. (2010) uses the Progresa/Oportunidades evaluation sample to test whether program impacts on secondary-school enrollment differ and finds effects are concentrated on those with extended family present in the village.

Overall, these diverse studies provide consistent evidence that the program increases schooling. The evidence from the medium-term evaluations suggests program impacts of up to an additional grade of schooling, an overall increase of 15 percent from levels in the absence of the program. A caveat, however, is that little is known about the impacts of the program on learning and achievement, e.g., if school quality is low, the returns to education on this additional schooling may

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19 However, unconditional transfers led to significant reductions in early marriage and teenage child bearing, whereas the conditional branch had no effect on these variables, which Baird et al. argue reflects the higher income received by dropouts in the unconditional branch versus the conditional branch.

20 It is unclear the extent to which these households did receive grants even though enrollment and attendance was not being verified and whether/when the program realized that the requirements to pay the grant were not being met.
be low and not lead to increases in lifetime income levels for beneficiaries when entering the labor market.

Two studies examine the impact of an increase in education by youth beneficiaries on their future labor-market outcomes. Behrman, Parker, and Todd (2011) estimate impacts of the program on labor-market indicators for young adults exposed to the program for about six years beginning in adolescence. Their main results show an increase in the probability of working for young women, who traditionally have very low labor-force participation rates in the experimental communities, and a shift away from participation in agricultural employment towards nonagricultural employment, as might be consistent with greater opportunities provided by higher education. McKee and Todd (2011) consider the question of how the *Progresa/Oportunidades* program will affect the earnings distribution once the children in the program become adults. They use a micro-simulation approach that adapts the nonparametric decomposition method originally developed in DiNardo, Fortin, and Lemieux (1996) for program evaluation purposes. They find that the additional human-capital investment under *Progresa/Oportunidades* will increase future mean earnings levels but will have only a modest effect on earnings inequality, because of the difficulty in predicting which children will become low-earning adults and because of nonlinearity in how health and education are priced in the labor market. Childhood poverty is a strong predictor of future low earnings, but there is also substantial intergenerational mobility that makes it difficult to target adult low earners on the basis of childhood characteristics. They also find that an additional year of secondary schooling has a higher monetary return than an additional year of primary school. Because of this nonlinearity, people at the upper deciles of the targeted population tend to benefit more from the program intervention.

5.1.1 *Labor-Force Participation and Time Use*

Several studies analyze the effects of the program on labor-force participation and, more generally, on time-use patterns. As discussed in section 3, through subsidizing schooling, the program likely increases the time of children spent in school and reduces time spent in work activities, which are substitutes, with an ambiguous effect on overall leisure time. For adults, the program has ambiguous effects on leisure and time spent in work activities because the income effect and the cross-substitution effect of school subsidies work in different directions. Unlike programs where amounts of transfers are effectively taxed by work, *Progresa/Oportunidades* does not continuously monitor beneficiary family economic conditions. Rather, every three years a follow-up is done to evaluate whether a household continues to be eligible. For this reason, the program does not create strong disincentives for working, at least in the short run.

Rates of children working in Mexico are relatively high in the *Progresa/Oportunidades* evaluation communities with more than 50 percent of twelve year olds on average reporting participation in work at baseline (including paid, as well as domestic and household agricultural work). Parker and Skoufias (2000) analyze program impacts in the first years of the program on labor-market participation and more detailed measures of time use. Estimates based on double-difference estimates before and after the program show significant reductions in children’s labor-force participation for both boys and girls, in both salaried and nonsalaried activities. Labor-force participation for boys shows reductions as large as 15 to 25 percent. Evidence from a cross-section time allocation survey applied
to the experimental sample allows a further disaggregation of impacts on work activities and shows that secondary-school-age boys reduce participation in both market and domestic work. Girls, on the other hand, show larger reductions in domestic work, in accordance with their much higher participation in domestic activities prior to the program. There are no apparent effects on reducing time of children dedicated to agricultural activities, such as caring for family animals. With respect to leisure, the increases in school enrollment and reductions in the work of boys are approximately equivalent, implying substitution between these activities and also implying no overall impact on boys’ leisure time. For girls, however, the reductions in work are less than the increases in school enrollment, implying that the leisure time of girls is slightly reduced under Progresa/Oportunidades.

With respect to adults, Parker and Skoufias (2000) find no reduction in labor-market participation rates. Although the time demands on women associated with satisfying program obligations are significant, the leisure time of both male and female adults in beneficiary households is not significantly affected by the program.

Rubio-Codina (2010) also studies program impacts on time use allocation of children and adults and carries out a similar analysis as Parker and Skoufias (2000), finding analogous effects on reducing child work in both market and nonmarket activities. She also further emphasizes the intra-household substitution of home production activities by decomposing the effects of the program on time in school and work activities of all household members (children and adults) into the own substitution effect, the cross-substitution effect (with other household members), and the income effect. Although the overall program effect on women’s leisure time is insignificant, she finds that the income effect increases overall leisure time, whereas the cross-substitution effect of the program reduces leisure, suggesting that women are more likely to substitute for children’s time within the household.

Relatedly, Dubois, and Rubio-Codina (2012) study effects of the program on the amount of time dedicated to child care in the household. Focusing on the subsample of households with teenagers twelve to seventeen and with children less than three years of age, they show that teenagers, particularly first teenage daughters, reduce significantly the time spent in child care, whereas women significantly increase time dedicated to child care. Overall, total household hours that adult women devote to child care increase for these types of households, although not for the overall sample of households. Women’s overall leisure time is unaffected by the program.

Overall, the program leads to significant changes in the time use of children by increasing time in school and also importantly reducing the time that children dedicate to work, both paid and domestic work. At least in the early program years where evidence exists and for the overall experimental sample, there are few effects on adult work and leisure time. Adult beneficiaries do not appear to use the benefits to work less and increase their leisure.

5.2 Health and Nutrition

In the area of health, early evaluations using the rural experimental data focused on program impacts on health care utilization, self-reported health conditions, and child height.

Since these early evaluations, the literature on estimating the health impacts of Progresa/Oportunidades has greatly expanded to include outcome measures ranging from child mortality, cognitive development, and behavioral problems to adult obesity, hypertension, depression, and use of
<table>
<thead>
<tr>
<th>Study authors, year</th>
<th>Method: Data and estimation</th>
<th>Nutrition</th>
<th>Health</th>
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</thead>
<tbody>
<tr>
<td><strong>Studies of children</strong></td>
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<tr>
<td>Rivera et al. (2004)</td>
<td>Subsample of communities from randomized treatment and control groups, random sample of infants selected and applied biomarkers. Treatments receive 2 years of benefits, controls receive 1 year.</td>
<td>Impact of 1 cm on height of infants younger than 6 months at baseline and who lived in the poorest households after 2 years of program implementation.</td>
<td>Anemia lower after 1 year of program 44 versus 55 percent in treatment/control group. No significant differences after 2 years of the program.</td>
</tr>
<tr>
<td>Gertler (2004)</td>
<td>After program difference regression estimates of the impacts randomized experiment for self-reported health and subsample of experiment for impacts on measured health.</td>
<td>Impact of 1 cm on height for children aged 1 to 3. Treatment children are 25.5 percent less likely to be anemic.</td>
<td>Treatment newborns were 25.3 percent less likely to be reported as being ill in the previous month. Treatment 0–3-year-olds were 22.3 percent less likely to be ill.</td>
</tr>
<tr>
<td>Behrman and Hoddinott (2005)</td>
<td>Subsample of children in randomized experiment data, child fixed effect estimates comparing children taking nutritional supplements with those who do not.</td>
<td>Impact of taking the supplement after 1 year = increase of 1 cm in child height for children aged 12 to 36 months.</td>
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<tr>
<td>Barber and Gertler (2008)</td>
<td>Comparison of original treatment and control of women experiencing birth between 1997 and 2003. Compares women with birth before receiving transfers to women with births after receiving transfers.</td>
<td>4.6 percentage point reduction in low birthweight, average increase in 127 grams in birthweight.</td>
<td></td>
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<tr>
<td>Fernald, Gertler, and Neufeld (2008)</td>
<td>Beneficiaries in original treatment and control communities, estimates impact of difference in total amounts of program transfers received by the family. Variation in transfers derived by differential time in receipt 1.5 years and differences in number and grades of children attending school.</td>
<td>Doubling of cash transfers associated with higher height-for-age Z score, lower prevalence of stunting, lower BMI, and lower prevalence of being overweight.</td>
<td></td>
</tr>
<tr>
<td>Fernald and Gunnar (2009)</td>
<td>After program difference none experimental estimators using original treatment and matched communities.</td>
<td>Children who had been in the Oportunidades program had lower salivary cortisol levels than comparison group, effects concentrated on mothers with depressive symptoms.</td>
<td></td>
</tr>
<tr>
<td>Study authors, year</td>
<td>Method: Data and estimation</td>
<td>Nutrition</td>
<td>Health</td>
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<tr>
<td>Fernald, Gertler, and Neufeld (2009)</td>
<td>Infants in original treatment and control communities interviewed 10 years later (differential exposure of 18 months to the program).</td>
<td>No significant effects on height or weight or cognitive development of all children. Impact of 1.5 cm in height for children of mothers without formal schooling.</td>
<td>Significant reduction in behavioral problems of children.</td>
</tr>
<tr>
<td>Andalon (2011)</td>
<td>Regression discontinuity using post-program data only on original experimental treatment communities. Adolescents aged 15–21.</td>
<td>LATE estimate, significant impacts on reducing obesity for adolescent girls only (32 percent), no effect on proportion overweight.</td>
<td></td>
</tr>
<tr>
<td>Studies of adults</td>
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<td></td>
<td></td>
</tr>
<tr>
<td>Fernald, Gertler, and Hou (2008)</td>
<td>Adult beneficiaries in original treatment and control communities, estimates impact of difference in total amounts of program transfers received by the family. Variation in transfers derived by differential time in receipt 1.5 years and differences in number and grades of children attending school</td>
<td>Doubling of cash transfers associated with higher body-mass index (BMI), higher blood pressure, and higher probability of being overweight.</td>
<td></td>
</tr>
<tr>
<td>Urequieta et al. (2009)</td>
<td>Difference-in-difference models using experimental data comparing treatment women experiencing a birth to control women experiencing a birth.</td>
<td></td>
<td>Treatment women having a birth are 11.4 percent-ages points more likely to have a doctor or nurse attending the birth.</td>
</tr>
</tbody>
</table>

(Continued)
### TABLE 7
**Health and Nutrition Effects of Progresa/Oportunidades (Continued)**

<table>
<thead>
<tr>
<th>Study authors, year</th>
<th>Method: Data and estimation</th>
<th>Nutrition</th>
<th>Health</th>
</tr>
</thead>
<tbody>
<tr>
<td>Lamadrid et al. (2010)</td>
<td>Difference-in-difference models comparing women from experimental treatment and control communities in 2000. Also use regression discontinuity to compare eligible with non eligible close to the cutoff.</td>
<td>Overall impact to increase probability of using contraceptives in 5 percentage points. Estimates based on RD show reduction in use of contraceptives for less-poor households.</td>
<td></td>
</tr>
<tr>
<td>Barber (2010)</td>
<td>Subset of communities from original evaluation communities selected, household/women randomly selected women interviewed women from original evaluation communities interviewed in 2003. Not clear whether randomization was preserved.</td>
<td>5.1 percentage point increase in probability of cesarean for those women having births after program began (between 1998–2003).</td>
<td></td>
</tr>
<tr>
<td>Ozer et al. (2011)</td>
<td>After program difference nonexperimental estimators using original treatment and matched communities.</td>
<td>Program participation associated with lower incidence of depression among women, 10 percent in depressive symptoms.</td>
<td></td>
</tr>
<tr>
<td>Behrman and Parker (2013)</td>
<td>After program difference experimental and nonexperimental estimators using eligible elderly population from original treatment, original control, and matched communities. Matching estimators.</td>
<td>Significant reduction for women 50+ in sick days, proportion with high blood pressure and increase in participation in vigorous activity. No health effects on men 50+.</td>
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</tbody>
</table>
contraceptives. Significant effects appear not only among children to whom the program is heavily oriented, but also among adolescents and adults.

Most studies of the health impacts are published in public-health journals and are based on data gathered using supplemental health modules applied to subsamples of children/adults in the original communities in the experimental evaluation. Compared with the literature on education impacts, the health literature tends to rely on smaller samples. Additionally, because the experimental evaluation collected limited baseline information on health outcomes, many studies of health indicators rely on post-program treatment control comparisons without information on, or the ability to adjust for, preprogram differences. Loss of the sample to followup is also often an important issue.

Studies using a different approach include Barham (2011) and Barham and Rowberry (2013), which use municipality-level data to estimate program impacts on child mortality and mortality for those aged sixty-five and over. They do so by constructing municipal rates of mortality from vital statistics over the period and municipal measures of program coverage. Program impacts are identified from variation in the proportion of beneficiaries across time and municipalities, deriving from differential phase-in of the program across geographical areas. One advantage of such an analysis is that it provides more nationally representative estimates of program effects than does the rural experimental evaluation, which was based on data gathered from seven states.

5.2.1 Impacts on Child Health

Early health studies compare the original treatment outcomes with the original control outcomes during the initial eighteen months of the evaluation, e.g., before the control group also began to receive benefits. These evaluations showed that, in addition to the expected positive impact on clinic visits, there were also reductions in self-reported illness and small but positive effects on child height on the order of 1 cm over the eighteen-month initial treatment period (Gertler 2004; Behrman and Hoddinott 2005; Rivera et al. 2004). Gertler (2004) reports that treatment newborns and children zero to three were 25.3 percent and 22.3 percent less likely than the controls to be reported as being ill in the previous month during the first year of the program. Rivera et al. also show effects during the first year of the program on reducing significantly the proportion of young children with anemia, on the order of 10 percentage points. Barber and Gertler (2008) find that infants born to beneficiaries have significantly higher birthweight, 125 grams more on average than non-beneficiaries.

Turning to medium-term impacts, Fernald, Gertler, and Neufeld (2009) use data following up the experimental communities to compare T1998 treatment infants versus T2000 control infants in 2007 after a decade of program participation on a number of cognitive and growth variables, when children were eight to ten years old. Treatment infants began to receive benefits at the zero-to-eighteen-month period, whereas control infants began to receive benefits eighteen months later. Although they find significant reductions in the incidence of behavioral problems among children, they do not find significant impacts on mean height-for-age Z scores, BMI-for-age Z scores, assessment scores for language, or other measures of cognition. Given the earlier presented initial evidence of significant impacts observed on height, these findings suggest, at least for the case of child height, control infants catch up to treatment infants, even though they receive the program eighteen months later.

Two studies estimate longer-term health impacts on children by comparing treatment
beneficiaries to those never receiving benefits in the C2003 comparison communities chosen through matching in 2003 (described in the Methodology section). Ozer et al. (2009) compare maternal reported behavior problems for children aged four to six in T1998 communities versus children who were never beneficiaries in C2003 comparison communities and report a reduction in 10 percent of behavior problems, in particular, those defined as aggressive/oppositionist. Fernald and Gunnar (2009), for children aged two to six, report lower levels of stress among children, as measured by cortisol levels.

Barham (2011) uses municipal-level administrative data from rural areas to estimate program impacts on infant mortality by the year 2001, four years after the program began. She identifies impact by using the proportion of rural beneficiary households by municipality and by year and reports large reductions (17 percent) in under-five infant mortality, primarily due to reductions in intestinal and respiratory diseases and to alleviation of nutritional deficiencies.

Finally, Mexico now has one of the highest levels of both child and adult obesity in the world, as well as skyrocketing levels of diabetes. In one of the few studies of impacts on youth/adolescents, Andalon (2011) studies the impact of the program on obesity rates of youth aged fifteen to twenty-one. She uses a regression discontinuity approach for youth in the experimental treatment group, comparing those just above the cutoff for program eligibility with those just above in 2003. Her findings are sensitive to the model specification, but in most specifications show a significant reduction in the probability of being overweight for girls and no effects for boys.

5.2.2 Impacts on Adult Health

Several short-term studies focus on female and reproductive health. During the experimental period of analysis, Urquieta et al. (2009) show that the program increases the probability of having a doctor or nurse attending at the time of childbirth, and Barber (2010) finds that pregnant female beneficiaries are significantly more likely to have caesarean deliveries. Lamadrid-Figueroa et al. (2010), comparing treatment and control women during the experimental period, find that the program increases, on average, the use of contraceptives, although they argue through an alternative regression discontinuity analysis that these impacts are concentrated among the poorest beneficiaries and that those close to the program eligibility threshold actually show negative impacts.

Turning to medium-term studies, Fernald, Gertler, and Hou (2008) study impacts on obesity, blood pressure, and self-reported health of adults aged thirty to sixty-five, comparing original treatment (T1998) adults to adults in communities never receiving benefits in 2003. Their results show a reduction of 4 percentage points in the probability of being obese and no significant effect on the probability of being overweight. Both systolic and diastolic blood pressure show some significant and small reductions, while impacts on activities of daily living were mixed. Ozer et al. (2011) use a similar estimator to analyze impacts on depression and find significant reductions in the incidence of depression symptoms among female beneficiaries. Behrman and Parker (2013), using the same impact estimator study effects of the program on health consultations and several indicators of self-reported health for the population above the age of fifty. They find the program increases use of health services for both aging men and women, but improves self-reported health and symptoms only for women (reducing sick days and reducing the proportion with high blood pressure). Finally, Barham and Rowberry (2013), again using municipal-level information and proportion of beneficiaries by municipality to measure program impacts, find significant reductions in average municipal-level mortality for those
over age sixty-five on the order of 4 percent. The reduction in mortality is primarily due to decreases in infectious diseases, but also due to reductions in diabetes-related death, a leading cause of death in Mexico.

5.2.3 Income Effects of CCTs on Health

The above studies estimate the overall effect of the program and do not attempt to identify separate impacts of the different program components, e.g., conditionality, income effect, the impact of health talks, and nutritional supplements. A couple of health studies, however, attempt to disentangle the income effect of the program separately from other components.\textsuperscript{21} The main approach taken compares households that received more total income from Progresa/Oportunidades higher grants with those receiving lower total grant amounts. Part of the variation in total program income received is due to the experiment (e.g., the control group began receiving benefits later than the treatment group, and thus has lower overall transfers received), but part of the variation also derives from differences in the number of children enrolled in school and in their grades across households.

Fernald, Gertler, and Neufeld (2008, 2009) argue that higher transfers received are associated with a number of large and positive growth outcomes for children including greater height, lower stunting, and a reduction in the probability of being overweight. Higher program transfers, however, might be endogenous to outcomes such as children’s cognitive development, given that children who do better and progress faster in school (perhaps because of higher ability) will generate higher cash grants for the household. They instrument for actual transfers using the maximum possible benefit amounts that the household could receive. They find similar results as obtained without instrumenting, although, in an exchange in Lancet, Attanasio, Meghir, and Schady (2010) criticize this instrument as “a deterministic function of treatment status and family composition” e.g., the maximum possible benefits are dependent on previous schooling attainment and are therefore arguably not exogenous with respect to omitted variables such as ability in school. Other than these studies, there is little research on the relative importance of the differing program components in generating observed health effects.\textsuperscript{22}

Overall, the quality of the evidence garnered from the health-impact studies is somewhat less robust than the education studies, reflecting less availability of baseline data on outcome measures and smaller and more selected samples. Nevertheless, the accumulated evidence supports significant and positive health impacts for children, adolescents, and adults. Effects are apparent in both short-term and longer-term studies.

5.3 Household Income, Consumption, Investment, Savings

In this section, we examine the evidence of the program’s impact on household measures of well-being including consumption and income, both in the short and longer run. Education and health impacts of the program on children may provide insight on impacts of Progresa/Oportunidades on the poverty and well-being of those who grow up with the program. But it is also of interest the extent to which the program improves well-being among its current beneficiary households and the extent to which

\textsuperscript{21} Unlike the case for education, no study to our knowledge attempts to isolate the importance of conditionality of benefits on health outcomes.

\textsuperscript{22} Using a similar approach, Fernald, Gertler, and Hou (2008) argue that the income component of the program leads to higher obesity among its beneficiaries, although in a separate paper (Fernald, Hou, and Gertler 2008), the same authors show that the overall effect of the program was to reduce obesity among adults.
### TABLE 8
**HOUSEHOLD WELL-BEING: CONSUMPTION, INCOME, SAVINGS, TRANSFERS, AND AGRICULTURE**

<table>
<thead>
<tr>
<th>Study authors, year</th>
<th>Method: Data and estimation techniques</th>
<th>Impacts</th>
</tr>
</thead>
<tbody>
<tr>
<td>Attanasio and Rios-Rull (2000)</td>
<td>Experimental evaluation, treatment and control communities, eligible and noneligibles HH included. After program difference estimates.</td>
<td>Agriculture/income/transfers: Weak evidence transfers from outside the household are reduced with the program.</td>
</tr>
<tr>
<td>Alburran and Attanasio (2003)</td>
<td>Experimental evaluation, treatment and control beneficiaries, 6 months post-program. After program difference estimates.</td>
<td>Reduction in probability of receiving a transfer from outside the household, reduction in total amount of transfer, conditional on receiving a transfer.</td>
</tr>
<tr>
<td>Hoddinott and Skoufias (2004)</td>
<td>Experimental sample, comparison of treatment and control groups 18 months after program began. Estimate of total effect of program on consumption. Controls for total HH consumption to estimate effect of other aspects of the program (nutrition talks) on consumption.</td>
<td>Treated households increase calorie consumption by 7.1 percent. Increases of 20.4 percent in fruit/vegetable calories consumed. Controlling for total HH consumption explains about half of total impact.</td>
</tr>
<tr>
<td>Angelucci and De Giorgi (2009)</td>
<td>Study effects on eligible and on ineligibles in experimental sample, ineligible households in both treatment and control communities 18 months post-program.</td>
<td>Increase for ineligibles of probability of receiving a loan and of receiving transfers from outside the household in 1.4 percentage points, no significant effects on loans/transfer for eligible. No effect on labor market earnings for eligible and ineligibles. Increased probability of having agricultural-related expense in 5 percentage points for eligible households, no impact for ineligibles. No significant effect on net sales of agricultural products and animals for ineligibles. Reduction in 0.6 pesos for ineligibles. Increase in food consumption of 30 pesos per adult equivalent for eligible, 17 pesos for ineligibles. Average increase in calorie consumption of 340 calories daily for eligible households and 170 for ineligibles.</td>
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<thead>
<tr>
<th>Study authors, year</th>
<th>Method: Data and estimation techniques</th>
<th>Agriculture/income/transfers</th>
<th>Consumption/savings/investment</th>
</tr>
</thead>
<tbody>
<tr>
<td>Bianchi and Bobba (2010)</td>
<td>Rural experimental evaluation, eligible treatment and control individuals, impacts between 1998 and 2003.</td>
<td></td>
<td>Treat individuals salaried or unemployed in baseline are 25 percent more likely to become an entrepreneur (self-employed). (Impact=0.9 percentage points).</td>
</tr>
<tr>
<td>Todd, Winters, and Hertz (2010)</td>
<td>Data from rural experimental evaluation, first 12 months of the program. Compares treatment with control households.</td>
<td>Increase in likelihood of using land by 4 to 6 pp. Increase in probability of owning livestock in 3.1 percentage points, significant increase in agriculture spending.</td>
<td>Increase in probability of consuming from own production by 4 pp (an increase of 5.4 percent and 6.2 percent, respectively).</td>
</tr>
<tr>
<td>Gertler, Martinez, and Rubio-Codina (2012)</td>
<td>Experimental evaluation, treatment and control households 18 months post program. Longer-term effects estimated following up treatment and control households in 2003 (5.5 years of benefits versus 4 years of benefits).</td>
<td>Treatment households 17.1 percent more likely to own draft animals and 5.1 percent more likely to own production animals compared to control households. Treatment households increase the value of draft animals owned by 21.4 percent and the value of production animals owned by 16.6 percent. Increase in participation in nonagricultural enterprises in 3.3 percentage points. 10–12 percent increase in agricultural income.</td>
<td>In 2003, per capita adult equivalent consumption 11 pesos higher monthly for treatment households for treatment households receiving 5.5 years versus 4 years for controls. Corresponds to 5.6 percent increase.</td>
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</table>
these changes are observed in the longer run. Increased household consumption levels can reflect not only the direct impact of receiving the transfers, but potentially other program-induced changes affecting the household budget constraints, e.g., labor income, transfers, agricultural and nonagricultural investment, and savings (Angelucci, Attanasio, and Di Maro 2012). For example, households may increase their savings or invest part of the transfers in other types of assets and activities that will affect their future income. Alternatively, households might invest in productive activities that enhance the effect of the transfers on their budget constraints.

Beginning with results from the initial experimental evaluation during the eighteen-month period before the control group also began to receive benefits, Hoddinott and Skoufias (2004) analyze program impacts on food consumption and, in particular, calories consumed. By November 1999, beneficiary households in treatment localities obtained 3.4 percent more calories than did eligible households in control localities. Much larger proportional increases were observed in calories consumed of fruits and vegetables (15.7 percent) and animal products (12.8 percent), suggestive that the program helped to improve the diversity of the diet, although overall calories consumed from these products remained quite low, less than 5 percent of total calories in both groups even in the treatment group.

With respect to program impacts in the short run on income and its components, as previously noted, the Progresa/Oportunidades program did not affect labor-force participation rates or hours worked of adult men or women. However, Albarran and Attanasio (2003) and Attanasio and Rios-Rull (2000), using the experimental evaluation data during the first year of program operation, find that there is moderate crowding out of inter-household transfers as a result of the program, particularly transfers from relatives in the United States.

Gertler, Martinez, and Rubio-Codina (2012) estimate program impacts on consumption in the longer term, comparing original treatment and control households (T1998 versus T2000) after 5.5 years, when the control group had received benefits for four years. They find that consumption levels for the original treatment households were 5.6 percent higher than for controls. If consumption impacts reflected only the effect of current transfers, arguably there would be no difference in impacts, given that both groups in 2003 were beneficiaries subject to the same monetary program benefits. The authors argue that these apparently permanently higher consumption rates of the original treatment group reflect that beneficiaries invested part of their payments in productive activities and that these investments increased income streams, thereby raising long-term living standards as measured by consumption. For example, original treated households in the rural experiment show significant increases compared with the original controls in a number of measures of agricultural investment.

Bianchi and Bobba (2013) find that the program increases micro-entrepreneurship, as measured by self-employment, and argue that this reflects a greater willingness to bear risk due to receiving a stable income source. They show that the program increases the probability of entering self-employment for those who were unemployed or employed in salaried work pre-program. They use variation in the timing of program transfers and provide evidence that these choices are more responsive to expected future transfers than current income, which they interpret as the program increasing risk taking rather than alleviating liquidity constraints. They do not, however, estimate the impacts of the program or the impacts of increased self-employment on income or consumption. Also, the effects
on entering entrepreneurship are rather small, at less than 1 percentage point.

Angelucci and De Giorgi (2009) study impacts of the program on the consumption of ineligible households living in the same communities as treated households. Although ineligible households do not receive program transfers, they argue that if risk sharing exists within the community, ineligible households may benefit from the transfers received by eligible households by receiving increased transfers and loans, which can be used to finance additional consumption. In fact, the results suggest significant increases in food consumption among ineligibles, the size of which are about half of the size of impacts among eligibles. They also report increases for ineligibles in the probability of receiving a loan and of receiving transfers from outside the household.

In the urban context, Angelucci and Attanasio (2013) analyze impacts on consumption, savings, and loans after two years of program participation. They find an impact of the program on food consumption of about half to two-thirds of the size of the transfer in 2003 and 2004. Although they find no significant effects on savings, they find reductions in net loans and argue that beneficiaries use transfers received to reduce their debt.

Angelucci and Attanasio (2012) argue that most of the increase in consumption is on food and the share of food expenditures does not decrease with total expenditure, as might be expected if food were a necessity. They argue this likely reflects changes in preferences within the household, due to women receiving the cash transfers, and that the program may orient spending to be more in accordance with women’s preferences (Attanasio and Lechene 2014; Martinelli and Parker 2003).

In summary, the studies are generally supportive of some changes in household well-being attributable to the program including increases in consumption, self-employment, and reductions in debt. The overall results are small but significant both in the short run and medium run, for instance, on the order of 5 percent increases in household consumption levels and in calories consumed. Gertler, Martinez, and Rubio-Codina (2012) summarize: “If removed from the program, beneficiary households would most likely not revert to pre-program poverty levels.” This is likely correct, however the sizes of these potentially permanent impacts are, thus far, modest.

5.4 Gender and Demography

5.4.1 Gender Effects

The Progresa/Oportunidades program design places a great deal of emphasis on gender. All monetary benefits are given to women, the health component focuses attention on pre- and postnatal care for pregnant women, and grants linked to educational attainment are larger for girls at the secondary and high-school levels than for boys. A number of studies explore the potential effects of the program on outcomes related to women’s status including female ownership of assets and violence against women. Directing the monetary benefits to the mother or female head of the household is hypothesized to improve the status of women relative to men in the household.

By at least some indicators, women’s status in the communities of the rural experimental sample preprogram was low. Adato et al. (2000) report that prior to the program, approximately 90 percent of women reported they needed their husband’s permission to visit relatives or neighbors. Husbands were generally more likely to be the ones who decide on such expenditures as children’s clothing and housing, although husbands and wives were equally likely to make decisions on food expenditures. Adato et al. (2000) report that the program has some small significant impacts on some of these self-reported variables, in particular
<table>
<thead>
<tr>
<th>Study authors, year</th>
<th>Method: Data and estimation techniques</th>
<th>Impacts</th>
<th>Demography: Marriage, divorce, fertility, migration</th>
</tr>
</thead>
<tbody>
<tr>
<td>Adato et al. (2000)</td>
<td>Experimental evaluation, after program difference estimators using multinomial logit. Compares eligible treatment versus eligible controls in first 18 months of program.</td>
<td>Reduction in probability that only husband gives permission for children to go out, reduction in probability that only husband decides on child clothing expenditures. Reduction in wife only deciding how to spend extra income.</td>
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<tr>
<td>Angelucci (2015)</td>
<td>Experimental evaluation, after program difference estimators, compared labor migration to United States for individuals in treatment households versus control households only 6 months after program begins.</td>
<td>Increase of 0.4 percentage points in probability of migrating to the United States, about a 40 percent increase.</td>
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<tr>
<td>Bobonis, González-Brenes, and Castro (2013)</td>
<td>Nonexperimental estimators using after program cross-section. Rural areas. Within-village regression estimates comparing beneficiary and non-beneficiary women. Data: Survey on Relationships within the Household (ENDIREH 2003).</td>
<td>Women in beneficiary households 5–7 percentage points (40 percent) less likely to be victims of physical abuse than non-beneficiary women, 3 to 5 percentage points more likely to be victims of emotional violence.</td>
<td></td>
</tr>
<tr>
<td>Bobonis (2011)</td>
<td>Data from experimental evaluation, compare eligible treatment with eligible control mothers, controls for selective attrition with Lee bound method.</td>
<td>No significant effects on proportion married/cohabitating. For unions at baseline, increase in probability of separating by 0.32 percentage points. Increase in 2 to 3 percentage points for separated/divorced women at baseline on probability of cohabitation after the program. For single women at baseline, increase in probability of marriage in 8–13 percentage points, but no significant effect on probability of being either married/cohabitating.</td>
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<tr>
<td>Stecklov et al. (2005)</td>
<td>Rural experimental data, difference in difference before and after regression comparing treatment and control households in first 18 months of the program.</td>
<td>Negative effect on migration of at least one individual in the household to the United States (reduction in 0.2 percentage points), reduction of over 50 percent compared with control group.</td>
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</tr>
<tr>
<td>Stecklov et al. (2007)</td>
<td>Rural experimental data, difference in difference before and after regression comparing fertility of treatment and control women age 12 to 47 in first 18 months of the program.</td>
<td>No significant effects on fertility after 18 months of the program.</td>
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</table>
it reduces the probability that only the husband decides whether children can go out of the house (versus only the wife or both deciding), and reduces the probability that only the husband decides on some categories of expenditures.

Attanasio and Lechene (2002) also analyze the impact of the program on some of the same variables and report significant impacts on decision making with fewer decisions made by men only and more decisions made jointly by both spouses in the household. However, they note that the differences, although statistically significant, are relatively small, in large part because the control group also experiences changes suggesting improvement over time in the decision-making power of the woman. They conclude that a better test of whether the program affects decision making in the household would derive from changes in expenditure patterns.

Attanasio and Lechene (2002) argue that the randomization of the program provides an exogenous source of variation in women’s share of income that can be used to test for changes in expenditure patterns. Of course, the program also increased total household income and changed relative prices of schooling and health. Nevertheless, Attanasio and Lechene make use of the randomization to analyze the effect of changes in the intrahousehold allocation of resources on household decisions. They also test for global pooling of resources, which is a test of the unitary model. Their results reject a unitary model and, in particular, find that women’s share of the income, instrumented by the randomization, positively affects spending on food and boys’ and girls’ clothing expenditures.23

Rubalcava, Teruel, and Thomas (2009) also argue that the program shifted expenditure patterns more in accordance with women’s preferences by providing resources to women that led to improved status within the household. In particular, they show that in married-couple households, the amount of Progresa/Oportunidades income, controlling for total income, is positively associated with spending on livestock, children’s clothing, and calorie consumption, whereas in households headed by single females or single males, there is no effect of Progresa/Oportunidades income on these variables.

Little evidence exists on the extent of violence against women preprogram in Mexico. Bobonis, González-Breñes, and Castro (2013) analyze data from the National Survey on Relationships within the Household (ENDIREH 2003), which has self-reported information on the prevalence of male-to-female spousal abuse and threats of violence against women, comparing program participants and nonparticipants. For married women above the age of twenty-five in rural areas, Bobonis, González-Breñes, and Castro (2013) report that spousal physical abuse is significantly reduced by 5 to 7 percentage points for Progresa/Oportunidades participants. There is no significant effect of the program on verbal threats against women, although the coefficients are generally positive, leading the authors to suggest that the program possibly induces a substitution of emotional violence for physical violence.

Overall, these varied studies provide a mixed picture of the impact of the program on improving women’s status in these poor rural communities where women’s status appears to be quite low preprogram. The evidence based on self-reported indicators of decision making and physical/emotional violence is not strong. However, it may also be that self-reported information on these decision variables and particularly on

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23 Two related papers, Attanasio and Lechene (2014) and Bobonis (2009), use the Progresa/Oportunidades experimental data to test for the efficiency of collective decision making, in both cases finding that efficiency of household decision making cannot be rejected.
self-reported violence is not very precise. The evidence based, however, on household expenditure patterns indicates strong impacts of the program on changing household expenditures shares to be better aligned with women’s preferences.

5.4.2 Demographic Outcomes

The Progresa/Oportunidades program might also affect patterns of marriage and fertility. Young adolescent women might delay marriage and childbearing due to increased time spent in school. For adult women, program transfers might increase economic independence and reduce marriage/increase divorce. On the other hand, the transfers may increase the benefits from marriage. The program might increase fertility through an income effect or through increasing benefits of having additional children.

Using the experimental data from the first eighteen months of the program, Bobonis (2011) shows that although there is no overall effect of the program on the proportion of women in a union (married or cohabitating), this masks changes in marriage and divorce. In particular, the program increases the probability of separation/divorce for those intact unions at baseline (although the effect is only 0.32 percentage points, as few households separate over the two year period). The program increases significantly the probability of cohabitating for separated/divorced women at baseline and the probability of marriage for single women at baseline.

With regard to program impacts on fertility, using the experimental data from the first eighteen months, Todd and Wolpin (2006), using structural methods, and Stecklov et al. (2007), using difference-in-difference models, find that the program has no impact on overall fertility. Gulemetova (2009) examines the effect of the Oportunidades program in urban areas on young women’s decision-making during the transition to adulthood using the urban evaluation data. She estimates a multistate duration model of time to first sexual experience, time to marriage, and timing and spacing of first and second births. Her findings show that young women who were exposed to the program delay marriage and first and second births. The time to premarital sex was also delayed. These effects are greater for girls who were younger when the program was introduced, and for those who were exposed to it for longer lengths of time.

Finally, two articles address the topic of migration to the United States using the short-term experimental data, reaching opposite conclusions. Angelucci (2015) argues that transfers from Progresa/Oportunidades relieve financial constraints to finance migration, both by transfers and by the increased take up of loans. Her evidence using the experimental sample shows that the program increases migration to the United States by about 0.4 percentage points, an increase of almost 50 percent compared to the control group during the first six months post-program, but still low in the sense that the impacts imply an increase in the proportion of individuals migrating to the United States from 0.007 to 0.01. She argues that the impacts derive primarily from the lower part of the skill distribution (measured by the predicted wage distribution) and that Oportunidades not only increased migration to the United States, but worsened the skill distribution of migrants by enabling the lowest-income earners to finance migration. One concern with the results is that they do not hold up after eighteen months, by which time there is no significant difference in migration to the United States between the experimental treatment and control groups.

Stecklov et al. (2005), using the same datasets, argue that the program reduced US migration (measured by whether any individual in the household migrated to the United States) by 0.2 percentage points, which they estimate represents a reduction
in migration to the United States of more than 50 percent. Angelucci notes that the studies are not comparable, because Stecklov et al. use as their independent variable all migrations, whereas Angelucci uses labor migrations. Another distinction is that Stecklov uses difference-in-difference models to control for baseline differences in the levels of migration between treatments and controls, whereas Angelucci uses only post-program difference estimators. Subtracting pre-baseline differences reduces the size of the estimated impacts.

Although the two studies have opposite results in terms of the sign of the estimated program impact, the overall very low level of migration to the United States in these communities (less than 1 percent, preprogram) suggests the program hardly leads to a transformation of migration patterns. Nevertheless, given that both studies analyze only the impacts after one year for rural communities, there is a clear need for additional studies of program impacts on migration in the medium and longer term for both rural and urban areas.

5.5 Other Effects: Political and Environmental

The provision of social programs in Mexico has long been suspected to be connected with vote buying of citizens and related corruption (Levy 2006). The Progresa/Oportunidades program included some features to minimize the potential intervention of local officials in program aspects, including the selection of beneficiaries (by selecting beneficiaries at the federal level based on analysis of socioeconomic questionnaires), and the provision of benefits (by sending transfers directly from the federal level to the community of the beneficiary, thereby reducing local officials’ access to these benefits). Additionally, new households cannot become beneficiaries during election years. De la O (2012) studies the issue of how the program might affect voting outcomes, including whether beneficiaries are more likely to vote for incumbents in the 2000 elections, by comparing electoral outcomes in the original treatment and control communities (both of whom were receiving benefits). She shows that early program enrollment is associated with substantially higher voter turnout and a higher incumbent’s vote share in the 2000 presidential election. Given that both groups were receiving benefits at the time of the 2000 election, the results are unlikely to suggest vote buying, but rather to reflect that the treatment group voters were more sympathetic toward the incumbent having benefitted a longer time from the program.

An innovative topic, and one of the few studies to find negative program impacts, comes from Alix-Garcia et al. (2013), which studies the environmental impacts of the program. Using a regression-discontinuity approach based on the community margin index, which affects the order of which communities receive the program, they report that the program doubles the probability that a community experiences deforestation (from a base of 4.9 percent) and that the average increase in deforestation is between 15 and 33 percent. Although these results are based on nationwide data, they use the experimental sample to try to understand the household-level mechanisms affecting deforestation. The authors show that the program significantly increased consumption of beef and milk, which they interpret as changing allocations towards consumption of land-intensive goods contributing to deforestation. However, no program effects on room construction, a proxy for the use of timber, or on number of cows owned were observed. They suggest that antipoverty design might include the simultaneous development of programs to prevent adverse environmental outcomes.
5.6 Multiple-Hypothesis Testing

As seen in the previous sections, there are a large number of existing studies of the impacts of Progresa/Oportunidades, with a substantial proportion reporting significant impacts. A natural concern, however, in drawing conclusions from these different studies is controlling for multiple-hypothesis testing, as some of the estimated program impacts could be statistically significant at conventional levels simply by chance. The individual studies described in this review generally do not adjust significance levels for multiple hypotheses, even when there are multiple hypotheses being tested. We examine the question of how inference would be affected, taking into account multiple-hypothesis testing.

There is now an extensive statistics literature on the problem of simultaneously testing a finite number of null hypotheses (see Heckman et al. 2010a for a summary). The methodologies can be subdivided into those that control for the family wise error rate (FWER) defined as the probability of one false rejection, versus those which control for the false discovery proportion (FDP), the number of false rejections divided by the total number of rejections, or the false discovery rate (FDR), which is the expected value of the FDP.

Our interest is measuring the extent to which the general conclusions of the different overall program effects hold up even if some false rejections of the null hypotheses are found. Because of the large number of total hypotheses in our case (over 700), we consider that methods based on controlling for the FWER to be too restrictive. Under FDR- and FDP-based tests, the number of false rejections that are “acceptable” is higher when many null hypotheses are rejected versus when fewer are rejected. Thus, we use FDR and FDP methods, which are generally agreed to be more appropriate for a large number of hypotheses (Heckman et al. 2010a).

In particular, we carry out three adjustments. First, we use arguably the first and most commonly used adjustment, developed in Benjamini and Hochberg (1995), which controls for the expected proportion of errors among the rejected hypotheses or the false discovery rate. A possible limitation, however, of this method is the assumption of independence of the test statistics. However, Benjamini and Yekutieli (2001) study the false-discovery rate under dependence and prove that the Benjamini and Hochberg adjustment also controls the false-discovery rate when the test statistics have positive-regression dependency on each of the test statistics corresponding to the true null hypotheses, which seems likely to apply to our context, as most of the studies are based on the same data.

We also implement two other methods, which control for all possible forms of dependence at the cost of being more conservative, arguably erring on the side of being too conservative (as pointed out by Benjamini and Yekutieli 2001). First, we apply Benjamini and Yekutieli’s (2001) procedure, which controls the FDR for any joint distribution of the test statistics. Second, we implement the test proposed by Romano and Shaikh (2006) that adjusts p-values for the false discovery proportion but does not impose assumptions on the joint distribution of the p-values. We include in the set of impacts studied those where sufficient information is provided by the authors to calculate the p-values.25 We

24 Publication bias, e.g., that many alternative hypotheses may have been tested and not published due to insignificant results, is also a related concern.

25 If the author did not report p-values, then we used the t-values and degrees of freedom, which we then used to calculate the p-values. When the t-values were not reported, they were computed with the impact coefficients and standard deviations. If the authors did not report the number of observations that we needed for the degrees of
Concentrate on the principal results from each paper. A full list of the papers and the impacts considered is presented in the appendix (online publication).

Table 10 reports the results overall and by topic where we first report the number of significant findings by conventional significance levels without adjusting for multiple-hypothesis testing and then the number of significant findings under each adjustment. We carry out testing by category of impacts, e.g., separately carrying out the corrections for each of the four sets of impacts (education, health, consumption/income, and gender/demography). Table 10 is based on the testing of 787 hypotheses in the diverse studies.

<table>
<thead>
<tr>
<th>Corrections for multiple hypothesis testing</th>
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<tbody>
<tr>
<td><strong>Benjamini and Hochberg (1995)</strong></td>
</tr>
<tr>
<td>10 percent</td>
</tr>
<tr>
<td>5 percent</td>
</tr>
<tr>
<td>1 percent</td>
</tr>
<tr>
<td><strong>Benjamini and Yekutieli (2001)</strong></td>
</tr>
<tr>
<td>10 percent</td>
</tr>
<tr>
<td>5 percent</td>
</tr>
<tr>
<td>1 percent</td>
</tr>
<tr>
<td><strong>Romano and Shaikh (2006)</strong></td>
</tr>
<tr>
<td>10 percent</td>
</tr>
<tr>
<td>5 percent</td>
</tr>
<tr>
<td>1 percent</td>
</tr>
</tbody>
</table>

Source: Authors' calculations, all multiple-hypothesis corrections are carried out by category.
As expected, for all topics the number of significant results decreases with controls for the false-detection rate and is even further reduced for methods that make no assumptions on dependence among the test statistics. In particular, applying Benjamini and Hochberg (1995) reduces the overall number (out of a total of 787) of significant hypotheses from 251 to 155 under a 5 percent significance level. However, the proportion of significant results continues, in general, to be far above what would be expected by chance for the cases of education and health, as well as income/consumption measures. For the cases of education and health, the proportion of findings that are statistically significant at the 5 percent level are 29 and 50 percent respectively and falls to 20 and 34 percent with the correction of Benjamini and Hochberg (1995). For income/consumption measures, the proportion of significant results at the 5 percent level falls from 32 percent to 12 percent.

Only for the case of gender/demography studies does the Benjamini and Hochberg (1995) multiple-hypothesis correction significantly lower the proportion of significant results to roughly the fraction that would be expected from chance. Whereas 20 percent of hypotheses tested under gender/demographics are significant at the 5 percent level without multiple-hypothesis testing, only 5 percent are significant with the correction.

Turning to the two more conservative procedures that control for the FDR and FDP respectively under dependence, the number of significant hypotheses diminishes; the results under both Benjamini and Yekutieli (2001) and Romano and Shaikh (2006) are relatively similar. Under Romano and Shaikh correction, using a 5 percent significance level implies that 14 percent of the education, 30 percent of the health, and 8 percent of the consumption/income impacts estimated remain statistically significant. Under Benjamini and Yekutieli (2001), using a 5 percent significance level, 12 percent of the education impacts, 26 percent of the health studies, and 4 percent of the consumption/income studies remain statistically significant. None of the findings of the gender/demographic studies hold up under either one of these methods.

Overall, we consider that corrections for multiple-hypothesis testing do not substantially alter the main inferences of the significant effects of the program on education, health, and the income/consumption studies that we have described in this article. Even under the quite conservative multiple-hypothesis tests that allow for any form of dependence, the proportion of significant hypotheses continues to be much larger than would be expected for education and health under any significance level and generally larger than would be expected for income/consumption, although the reduction in the proportion of significant hypotheses is larger for the income/consumption studies than for education and health. With the exception of studies in gender and demography, the multiple-hypothesis corrections, while diminishing the proportion of significant studies, do not substantially alter the main conclusions from the evaluation studies.

6. **Public-Policy Implications of Progresa/Oportunidades**

In this section, we turn to the policy implications of the evaluation studies. First, how do costs of the program compare to the benefits and how do these cost/benefit ratios compare with some comparable programs in which costs/benefits have been measured? Secondly, what do the studies suggest about alternative program designs and how the program might be improved? Finally, as an important case study of an influential cash...
transfer program, is there evidence that these numerous impact studies have had an impact on policy making in Mexico and beyond?

6.1 Cost–Benefit and Effectiveness Analysis

The relatively large estimates of impacts on human capital and other related indicators raise the question of how program benefits of the Progresa/Oportunidades compare to costs. As has been made clear in this review, the program impacted a diverse set of outcomes and quantifying all of the program benefits is a difficult task. Unlike many education or health programs, which have predominantly a single objective (such as providing free textbooks), Progresa/Oportunidades has multiple objectives, including goals both related to stimulating investment in health, nutrition, and education, as well as alleviating current poverty by giving money directly to the poor and achieving some redistribution.

Behrman, Parker, and Todd (2011) carry out a cost–benefit analysis of the program as if the only objective of the program were investment in human capital and the only benefits the subsequent impact on future earnings of the increased schooling attainment of students affected by Progresa/Oportunidades. Costs include the resource costs of the program, administrative costs of the program (costs of transferring benefits, conditionality, and targeting), and private costs associated with program participation. Under a range of plausible scenarios on returns to education and levels of discount rates, program benefits are substantially higher than costs (see table 11, below) with benefit/cost ratios ranging from 3 to 7 under all scenarios other than very high discount rates and very low returns to schooling.

How does the program compare with other human-capital investment programs from a cost–benefit perspective? Most existing evidence focuses on cost–benefit analysis of early childhood education programs. For example, Heckman et al. (2010b) analyze long-run benefits and costs of the Perry Preschool program, which take into account social benefits of program impacts such as reductions in crime and welfare payments. They find benefit/cost ratios for the entire population ranging from 2.2 to 31.5 depending on the discount rate (0, 3, 5, or 7 percent) and the assumed value of avoiding a murder (high, which accounts for the statistical value of life, versus low, which does not). The benefit–cost ratios of Progresa/Oportunidades, which only include private benefits on education, compare favorably with these estimates.

The Copenhagen Consensus (2004, 2008, and 2012) has commissioned papers that review benefits and costs of programs in diverse areas (ranging from malnutrition and education to climate change) aimed at obtaining a ranking of programs with best investments. Orazem, Glewwe, and Patrinos (2009) carry out a review of cost–benefit analysis for some education programs, including early nutrition programs, vouchers, scholarship, and CCT programs, and conclude that the highest benefit/cost ratio programs in terms of affecting education are those investing in early nutrition, although all those presented show benefit/cost ratios substantially greater than 1.27

With regard to cost effectiveness, Dhaliwal et al. (2014) present evidence on the cost effectiveness of a number of randomized education interventions around the developing world and suggests that Progresa/Oportunidades is expensive compared with some other education programs. However, this cost-effectiveness evidence is problematic for the

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27It is worth noting that the Copenhagen Consensus (Lomborg 2014) lists CCTs on a list of sixteen recommended interventions. The only other programs ranked higher in terms of improving education are deworming and micronutrient programs.
following reasons: (1) most interventions presented are carried out in much poorer contexts than Mexico, (2) most contexts have much lower initial education levels than Mexico, and (3) most of the pilot programs studied have the single objective of improving education versus the clearly multi-objective program of Progresa/Oportunidades and the evidence of its impacts on a large number of other variables. Cost-effectiveness analysis comparisons of the education impact alone effectively place a weight of zero on the impact of all other (numerous, in the case of CCTs) program outcomes.

There is also the important question of whether to regard income transfers as pure-cost components of the program in the same way as a capital investment (e.g., bed nets or school supplies). Households presumably place greater value on monetary transfers than in-kind transfers, because a monetary transfer can be used for any desired purchases or saved for the future. A monetary transfer retains its full value after the transfer is made and arguably should not be considered simply as a cost of the program. Rather than cost-effectiveness analysis, cost–benefit analysis where the numerous benefits of the program could potentially be valued would seem a better tool for comparing Progresa/Oportunidades with other human-capital investment programs. A full cost–benefit analysis has yet to be made.

6.2 Alternative Program Designs

Related to a cost–benefit analysis is whether the Progresa/Oportunidades program design is optimal in the sense of maximizing potential impacts given the cost. Todd and Wolpin (2006, 2008) and Attanasio, Meghir, and Santiago (2012) use behavioral models to simulate the effect of alternative subsidy schedules. Todd and Wolpin (2006) solve a dynamic-optimization problem for

<table>
<thead>
<tr>
<th>Discount rate</th>
<th>Initial earnings</th>
<th>Return to schooling</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>W/o program</td>
<td>With program</td>
</tr>
<tr>
<td>3%</td>
<td>1,855</td>
<td>1,966</td>
</tr>
<tr>
<td>5%</td>
<td>1,855</td>
<td>2,003</td>
</tr>
<tr>
<td>10%</td>
<td>1,855</td>
<td>2,040</td>
</tr>
</tbody>
</table>

Source: Behrman, Parker, and Todd (2011).
each household over their entire life cycle. In the model, the household makes decisions about whether to send their children to school, or to work, or to keep them at home, and about fertility. They use the estimated model to simulate the effect of alternative policies on completed schooling of children ever born. The results of implementing alternative policies from their paper are shown in table 12 below.

The model predicts that in the absence of the program, average years of completed schooling would be 6.3 for girls and 6.4 for boys and that 19.8 percent of girls and 22.8 percent of boys would have completed ninth grade. The hypothetical policy of perfectly enforcing compulsory schooling laws gives a maximal potential program impact, given failure rates. This policy leads to an average completed schooling of 8.37 years for girls and 8.29 years for boys. The cost of enforcing a compulsory schooling law, however, is unknown. The model predicts that the subsidy schedule as implemented increases school about one-half year for both girls and boys.

_Progresa/Oportunidades_ rewards school attendance starting at grade three. However, attendance in grades three–five is nearly universal, making the transfers in these years nearly equivalent to income transfers. Todd and Wolpin (2006) calculated that the per family cost of the program could be held roughly constant if the subsidy in grades three–five were eliminated and the subsidy in grades six–nine increased by about 45 percent. Under this alternative schedule, the proportion of girls completing ninth grade would increase by 3.4 percentage points and the proportion of boys by 3.8 percentage points, although average completed schooling would drop slightly for both girls and boys.

Another alternative subsidy scheme might reward grade completion, rather than attendance. The impact of a ninth-grade graduation bonus of 30,000 pesos would have a relatively small impact on average schooling, 0.21 years for boys and 0.26 years for girls. The impact on average schooling is not as large as the effect of the original subsidy, even though the cost is higher. Todd and Wolpin (2006) also simulate the effect of building a grades seven–nine school in each village by setting the distance to secondary schools to zero. The school-building program would increase mean schooling by 0.10 years for boys and 0.13 years for girls. Lastly, they simulate the effect of a pure-income transfer of 5,000 pesos per year to families. The increase in schooling is only about 20 percent as large as the original attendance-based subsidy and the cost per family is much larger.

Attanasio, Meghir, and Santiago (2012) also simulate the impact of eliminating the subsidy to primary school and redistributing the savings to increase the subsidies at later grades and they simulate the impact of building schools. With regard to changing the subsidy schedule, they find that the modified program would increase school attendance rates of boys from 0.065 to 0.106, an increase of about 70 percent. They find a modest effect of building schools, as did Todd and Wolpin (2006). The findings of the Todd and Wolpin (2006) and Attanasio, Meghir, and Santiago (2012) studies are remarkably similar, despite the differences in model features. The Attanasio, Meghir, and Santiago (2012) model incorporates general-equilibrium effects on child wages and they find that the child wage increased 6 percent due to the withdrawal of child labor induced by the _Progresa/Oportunidades_ school attendance subsidy. Todd and Wolpin (2006) do not estimate general-equilibrium effects, but they simulate schoolgoing under an assumption that the program induces a 25 percent increase in child wage offers, a much greater increase than was estimated by Attanasio, Meghir, and Santiago (2012).
Todd and Wolpin (2006) find the increase in completed school would be 85 percent of the partial-equilibrium effect of the original subsidy for girls and 69 percent for boys.

Both Attanasio, Meghir, and Santiago (2012) and Todd and Wolpin (2006, 2009) perform counterfactual experiments where they double the subsidy at all grades. As seen in table 13, the three predictions are quite close. The result reported in Todd and Wolpin (2009) (based on the nonparametric single child matching estimator described earlier) implies that doubling the subsidy would increase the attendance rate impact for boys by a factor of 2.08; the discrete choice dynamic programming (DCDP) model of Todd and Wolpin (2006) implies an increase by a factor of 2.06; the DCDP model of Attanasio, Meghir, and Santiago (2012) an increase by a factor of 1.87. The matching estimate based on the multiple-child model implies the lowest increase, by a factor of 1.32.

These results indicate that quite substantial increases in impacts would occur with increasing subsidy amounts (and conversely much lower impacts would have been observed with smaller education-grant levels). Few other studies, either structural, experimental, or nonexperimental, where the size of transfers in CCT programs, is varied, exist. An exception is Filmer and Schady (2011), who study the effect of a Cambodian transfer program using a regression-discontinuity design. Although their results show that both groups of scholarships with different amounts ($60 versus $45) have large effects on improving school enrollment (25 percentage points), there are no significant differences on enrollment between the $60 and $45 scholarship groups. Baird, McIntosh, and Ozler (2011) also randomly vary the amounts received under a CCT program and under an unconditional transfer program in Malawi. Their results show little sensitivity of enrollment impacts to the size of benefits under the conditional group, but more significant responsiveness to transfer amounts for the unconditional transfer group. Compared with these other contexts, impacts on education outcomes in Progresa/Oportunidades seem much more sensitive to transfer amounts.

Conditionality also seems quite important in the Progresa/Oportunidades context. Todd and Wolpin (2006, 2009) and de Brauw and Hoddinott (2011) show that conditionality explains most of the education impacts, e.g., unconditional transfers would have

---

**TABLE 12**

<table>
<thead>
<tr>
<th>Mean completed schooling</th>
<th>Baseline</th>
<th>Compulsory attendance</th>
<th>Original subsidy</th>
<th>Modified subsidy</th>
<th>Bonus for completing grade 9</th>
<th>Build schools</th>
<th>Income transfer (5,000 pesos per year)</th>
<th>Subsidy + 25% increase in child wage</th>
</tr>
</thead>
<tbody>
<tr>
<td>Boys</td>
<td>6.42</td>
<td>8.29</td>
<td>6.96</td>
<td>7.07</td>
<td>6.58</td>
<td>6.55</td>
<td>6.53</td>
<td>6.79</td>
</tr>
</tbody>
</table>

Annual cost per family (pesos)

|                          | 0        | 26,968               | 25,193            | 36,976           | 237,000                  | 25,250       |

led to much smaller impacts in education. There is also beginning to be more evidence in other contexts of the importance of conditionality, e.g., Baird, McIntosh, and Ozler (2011) in Malawi and Akresh, de Walque, and Kazianga (2013) in Burkina Faso.

These studies suggest that reallocating the cost of primary grants to providing higher secondary and above grants would substantially increase program impacts on completed schooling levels. A concern of this policy change might be that this would substantially reduce the amount of transfers received by families with children only in primary school who would receive higher benefits later on only if their children enrolled in secondary school. If poorer eligible households are less likely, even with the program, to enroll their children in secondary school than less poor but still eligible households, then such a change might reduce overall benefits that poorer households receive.

De Janvry and Sadoulet (2006) argue that education impacts could be substantially improved by redesigning program targeting so as to reduce the percentage of education transfers that go to children who would have gone to school even without the program and offering transfer levels sufficient to affect behavior. They suggest incorporating variables such as parental education, birth order, and distance to school into the determination of transfers linked to going to school so that transfers would be more effective and less leakage would occur to students who would attend school even in the absence of the program.

### 6.3 Impact of the Evaluation in Mexico and Internationally

A number of authors have argued that the evaluation results played a key role in insuring the survival of Progresa/Oportunidades into the next presidential administration. Levy (2006), one of the principal program architects, in his book on the history of the program, argues that the evaluation results, and particularly their distribution internationally, played a role in not only ensuring the program was continued by the next administration but expanded to urban areas. Similar arguments are made in Lustig (2014) and Behrman (2010). Since then, the program has continued to expand both in rural and urban areas, but it remains a fundamentally rural program. With respect to specific policy changes as a result of the evaluation, in addition to its expansion, the main policy change in the

<table>
<thead>
<tr>
<th></th>
<th>Boys</th>
<th></th>
<th>Girls</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1 × subsidy</td>
<td>2 × subsidy</td>
<td>1 × subsidy</td>
<td>2 × subsidy</td>
</tr>
<tr>
<td>Todd and Wolpin (2009)</td>
<td>0.056</td>
<td>0.116</td>
<td>0.060</td>
<td>0.141</td>
</tr>
<tr>
<td>Single-child model</td>
<td>0.059</td>
<td>0.078</td>
<td>0.070</td>
<td>0.089</td>
</tr>
<tr>
<td>Multiple-child model</td>
<td>0.077</td>
<td>0.159</td>
<td>0.064</td>
<td>0.146</td>
</tr>
<tr>
<td>Todd and Wolpin (2006)</td>
<td>0.070</td>
<td>0.131</td>
<td>N/A</td>
<td>N/A</td>
</tr>
<tr>
<td>Attanasio, Meghir and Santiago (2012)</td>
<td>0.070</td>
<td>0.131</td>
<td>N/A</td>
<td>N/A</td>
</tr>
</tbody>
</table>

**TABLE 13**

The Predicted Effect of Doubling the Progresa Subsidy on School Attendance Rates of Children, Aged 12–15
structure of the program was the addition of education grants at the high-school level, tenth to twelfth grade. Levy (2006), in particular, cites Schultz’s (2000) program report showing the program impacts on enrollment occurred mostly at the transition between primary and secondary school with few impacts on enrollment within primary (later published in the Journal of Development Economics in 2004) as a catalyst for implementing grants at the high-school level.

At the international level, the impacts of the Progresa/Oportunidades program and its evaluation have likely been even larger. The initial spread of similar programs throughout Latin America and the Caribbean, including programs in Colombia, Honduras, Ecuador, Jamaica, and Nicaragua that were introduced shortly after Progresa/Oportunidades, likely would not have occurred without the early evaluation results and their publication. The multilateral banks, e.g., the Inter-American Development Bank and the World Bank, were quick to begin promotion and financing of similar CCT programs and their evaluation. As in the early program years, the evidence on long run impacts of Progresa/Oportunidades may provide a guide for newer programs on possible future impacts.

7. Conclusions, Discussion, and Directions for Future Research

This review has illustrated the wide-ranging impacts of the Progresa/Oportunidades CCT program. Most evaluation studies were based on the original experimental evaluation data and demonstrated clearly positive impacts on education, health, and nutrition indicators. Some longer-term studies have shown significant, although small, increases in other indicators, such as consumption, income, and agricultural investment. There are a few studies suggesting improvements in some indicators of women’s status, including evidence that household expenditures have shifted to be more in accordance with women’s preferences.

There are some caveats to this generally positive story. First, the studies, even the more recent ones, do not generally take into account multiple-hypothesis testing and thus some of the impacts, particularly those in the areas of gender and demography, are overstated and do not hold up with corrections for multiple-hypothesis testing, as discussed at the end of section 5. Future studies would do well to adopt methods of inference appropriate to the multiple-hypotheses setting. Second, with regard to variables where impacts are not positive, Fernald, Gertler, and Hou (2008) suggest that providing income transfers might increase obesity of adults in Mexico, a country where obesity levels already rank among the highest in the world. Alix-Garcia et al. (2013) argue that the program has increased deforestation through increasing the consumption of resource intensive products and argues strongly for a welfare analysis of the program to include environmental impacts. Both these topics are of great importance in Mexico and continuous evaluation of their impacts should occur.

Finally, there are only a few studies of program impacts in urban areas. There was lower take-up of the program in urban areas, in part because the procedure for enrolling in the program differed from that in rural areas. Individuals in rural areas were informed about their eligibility, whereas individuals in urban areas had to apply to the program during a time window of opportunity. Education impacts for program participants in urban areas appear to be similar to those observed in rural areas, but impacts on consumption are somewhat lower. More evidence is needed on the effectiveness of the program in urban areas.

On balance, the available evidence suggests that Progresa/Oportunidades is an effective program that alleviates poverty and creates few negative incentive effects. Much
of the existing evidence, however, uses the data from the initial rural experiment and focuses on impacts during the first few years of the program. *Progresa/Oportunidades* has now been operating for a decade and a half. Future research should thus be devoted to studying the longer-term impacts of the program and its intergenerational impacts.

We conclude with some general suggestions for this research. One question that remains largely unanswered is the effect of the program on the intergenerational transmission of poverty, that is, the effects on the children whose households receive benefits and effectively grow up with the program. In particular, an important question is whether the increase in their health, education, and other dimensions of human capital will significantly impact employment, income, and poverty of child and youth beneficiaries when they become adults. Although there is generally a consensus among the evaluation studies that *Progresa/Oportunidades* increased overall schooling and health among children, there is little direct evidence on how these increases will affect adult outcomes.28 One exception is Behrman, Parker, and Todd (2011) who suggest that the program, after six years, increased the labor-force participation of young female adults and shifted participation away from agricultural activities to nonagricultural activities. New work by Parker and Vogl (2017) uses the 2010 Mexican Population Census merged to program participation at the municipality level to estimate longer-term program impacts on education and labor-market outcomes for young adults. Their findings suggest the significant effects of additional education from the program lead to significant increases in labor-force participation and income for women but less clear impacts for men. Further studies and follow up are needed.

Another important issue relating to the observed increases in schooling and health, and indeed the overall objectives of the program, are the quality of education and health services. Overall, the quality of public education in Mexico, as measured by performance on international achievement tests such as the Programme for International Student Assessment (PISA), is poor. If school quality is at a low level, it might not be optimal to extend the period during which children go to school, as expected returns to the additional grades of schooling achieved might not materialize. More evidence is needed on how participation in the program and increase in school-going affect cognitive test scores for children of various ages. The government provided some additional resources to schools to be able to cope with the higher enrollments expected under the program. However, how quality of school services interacts with the impacts of the *Progresa/Oportunidades* program remains an unexplored area.

Finally, a program as large in terms of coverage and benefits as *Progresa/Oportunidades* is likely to have important general-equilibrium effects on the local economy. Very little evidence exists on these effects.29 What are the long-term impacts of the program on prices within the economy? For instance, given changes in educational attainment, will the program induce changes in the skill premium at different schooling levels? What about effects on employment, inflation, and taxes paid? What are the implications for wages and the distribution of wages, particularly for the poor population, which is the focus of the

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28 McKee and Todd (2011) offer some evidence based on a synthetic cohort approach.

29 Coady and Harris (2004) develop a general-equilibrium model for evaluating domestically financed transfer programs and use preprogram household-level data to estimate welfare effects due to redistribution, reallocate, and distortionary effects. They particularly focus on the financing mechanism of the program, which in the case of *Progresa/Oportunidades* was financed in part from reducing universal food subsidies. As previously described, Attanasio, Meghir, and Santiago (2012) incorporate equilibrium effects on child wages into their analysis.
program? Understanding such effects will be critical for better understanding the program’s long-term effects on welfare.

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