Chapter Four

The Impact of CCTs on Consumption Poverty and Employment

We saw in Chapter 3 that CCT programs generally have done well in targeting their transfers to the poor. That does not mean, however, that they necessarily have a large impact on poverty. A number of factors, including behavioral and political economy responses to targeted programs, intervene in determining the ultimate impacts on poverty. For example, a study of China’s Di Bao program—the largest cash transfer program in the developing world, though not a conventional CCT—found that the cities of China where the program was better targeted to the poor generally were not the ones where the scheme had the highest impact on poverty or where the program was the most cost effective in reducing poverty (Ravallion 2008).

This chapter directly assesses the performance of existing CCT programs in reducing consumption poverty. The chapter is divided into three sections and a conclusion. In the first section, we consider the impact of CCTs on short-term consumption and consumption poverty. This is done both for the target populations of CCT programs and, for a few countries, for the country population as a whole. We also discuss evidence showing that transfer income is used differently from other sources of income.

In principle, the impact of CCTs on poverty could be smaller than would be suggested by simple back-of-the-envelope calculations based on the size of the transfer because of both intended and unintended effects of the program. The second section of the chapter discusses the evidence on behavioral changes that could offset the impact of transfers. As will be shown in chapter 5, there is solid evidence that CCTs have increased school enrollment levels. If schooling and child work are substitutes, at least in part, then we would expect that CCTs might reduce child labor—and therefore reduce the contribution that
children make to household income. We thus begin that section of the chapter with a discussion of CCT impacts on child labor. CCTs also could reduce adult labor supply for a variety of reasons: leisure is likely to be a normal good so households will tend to consume more of it as their incomes rise, and households could adjust their labor supply in an attempt to stay “poor enough” to continue being eligible for transfers. For that reason we next review the evidence on program effects on work by adults. Finally, we discuss whether CCTs appear to have crowded out transfers from other sources, had unintended impacts on fertility, or have had (local-level) general equilibrium effects.

If part of the transfer is invested, or if the transfer enables households to better smooth consumption, then CCT programs also can have impacts on consumption in the long run, above and beyond the changes arising from human capital accumulation. The third section of the chapter provides some evidence that this indeed has been the case. (Impacts on human capital accumulation will be discussed in chapter 5.)

Impact of CCTs on Household Consumption and Poverty

Impacts on Consumption among Program Beneficiaries

The impact of CCTs on immediate consumption is an important determinant of poverty alleviation in the short run, especially because most beneficiaries belong to the poorest part of the population. In this section we assess the impact of CCTs on short-term consumption or income for seven programs in which such data were collected as part of their evaluations and in which robust methods can be applied in the estimation of impact, namely, Bolsa Alimentação in Brazil,1 Familias en Acción in Colombia, PRAF in Honduras, Oportunidades in Mexico, the RPS in Nicaragua, the BDH in Ecuador, and the CESSP scholarship program in Cambodia.2 In all programs, consumption or income data were obtained through field surveys that interviewed both beneficiary and control households. Except for Brazil’s Bolsa Alimentação, Mexico’s Oportunidades, and the CESSP, all evaluations had baseline surveys that can be used to measure averages before the programs were implemented.3

Table 4.1 shows that preprogram median per capita consumption levels for the target population were low in all programs. This finding
Table 4.1  Impact of CCTs on per Capita Consumption, Various Years

<table>
<thead>
<tr>
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<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Median daily per capita consumption of control households (current US$)</td>
<td>0.83</td>
<td>0.89</td>
<td>0.85</td>
<td>1.19</td>
<td>1.12</td>
<td>1.13</td>
<td>0.79</td>
<td>0.68</td>
<td>0.59</td>
<td>0.58</td>
<td>0.59</td>
<td>0.63</td>
<td>0.53</td>
<td>0.52</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Daily per capita transfer (current US$)</td>
<td>0.06</td>
<td>0.02</td>
<td>0.12</td>
<td>0.13</td>
<td>0.08</td>
<td>0.08</td>
<td>0.06</td>
<td>0.06</td>
<td>0.12</td>
<td>0.14</td>
<td>0.13</td>
<td>0.16</td>
<td>0.15</td>
<td>0.15</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ratio of transfer to consumption (%)</td>
<td>8</td>
<td>2–3</td>
<td>17</td>
<td>13</td>
<td>8</td>
<td>7</td>
<td>9</td>
<td>11</td>
<td>21</td>
<td>20</td>
<td>19</td>
<td>29</td>
<td>31</td>
<td>30</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Impact on per capita consumption for the median household (%)</td>
<td>7.0**</td>
<td>B</td>
<td>A</td>
<td>10.0**</td>
<td>A</td>
<td>B</td>
<td>A</td>
<td>7.0*</td>
<td>B</td>
<td>7.8**</td>
<td>8.3**</td>
<td>A</td>
<td>29.3**</td>
<td>20.6**</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Source: Authors’ calculations for all countries in the table except Colombia. For Colombia, see Institute for Fiscal Studies, Econometría, and Sistemas Especializados de Información (2006).

Note: The estimated impacts presented here are not always equal to the unconditional double difference estimates because some regressions control for other correlates. The impact for Honduras was obtained from 2002 regression only. The impacts for Mexico are all for single equation cross-sectional regressions for each year. The lack of impact in 1998 is likely the result of the fact that this survey was carried out just a few months after the start of the program. Figures are in US$ obtained through the official exchange rates observed at the time of the surveys. In the case of Oportunidades in Mexico, the 1998 figures are for a few months after the start of the program.

a. The transfer amounts as a proportion of per capita expenditures (or consumption) are not the same across all tables in the report because of differences in the surveys used, including their coverage and year.

A. Baseline, before households in CCT treatment group received transfers.
B. No significant impact on consumption.
* Significant at the 10 percent level.
** Significant at the 5 percent level.
corroborates the findings of chapter 3 that CCTs were well targeted. Per capita consumption varied between $0.52 per day in Nicaragua and $1.19 per day in Colombia.

Per capita transfers for the median household varied more widely across countries. They were as low as $0.02 per day in Cambodia, and as high as $0.16 per day in Nicaragua. This heterogeneity reflects the different weights that each program assigned to reducing short-term versus long-term poverty. Reducing current consumption poverty was a central objective of Oportunidades and the RPS. By contrast, the CESSP program had no redistributive or poverty alleviation goals.

Because the size of the transfer varies a great deal across countries, so does the ratio of the transfer to median consumption. This difference can be seen in the third row of the table: for households in Nicaragua, the transfer represented about 30 percent of consumption, whereas in Cambodia that number is only about 2 percent. Other programs fall somewhere in between, with Familias en Acción and Oportunidades making relatively large transfers, compared with the smaller transfers for the BDH program in Ecuador, PRAF in Honduras, Bolsa Alimentação in Brazil, and especially the CESSP program in Cambodia.

The fourth row of the table summarizes program effects on consumption. The largest impacts are found for the RPS, the program that made the largest transfers.

Other programs included in the table (including Familias en Acción in Colombia, Oportunidades in Mexico, PRAF in Honduras, and Bolsa Alimentação in Brazil) also had significant impacts on per capita consumption, ranging from 7 to 10 percent. By contrast, neither the BDH program in Ecuador nor the CESSP program in Cambodia appears to have increased consumption levels. The results for the CESSP program are not unexpected, given the small size of the transfer and the fact that short-term poverty alleviation was not a program goal. The results for Ecuador are more surprising, and they appear to be related to the large reduction in child labor among BDH program beneficiaries (a point that we discuss in more detail below).

The estimated impact on consumption for the median households tells us very little about the potential distributional effects of CCTs. Therefore we next consider impacts on various poverty measures, including those that are distributionally sensitive.
The Impact of CCTs on Poverty at the Program Level

We estimate program impacts on three poverty measures of the Foster-Greer-Thorbecke (FGT) family: the headcount index, which is the number of people below the poverty line; the poverty gap, which measures the average distance between the consumption of poor people and the poverty line; and the squared poverty gap, which takes into account the distribution of resources among the poor. The analysis in this section focuses on Colombia, Honduras, Mexico, and Nicaragua. We exclude Cambodia and Ecuador; in those countries, the CCT did not have an effect on median consumption and, as would be expected, did not reduce poverty. We also exclude the Brazilian Bolsa Alimentação program because the evaluation sample is not representative of the program’s target population, which makes the analysis of the impact on poverty less informative.

The results from these calculations are summarized in table 4.2. Consistent with table 4.1, programs that had large effects on consumption also had large effects on poverty. In Nicaragua, the RPS reduced the headcount index among beneficiaries by 5–7 percentage points, the poverty gap by 9–13 points, and the squared poverty gap by 9–12 points. In Colombia, Familias en Acción also had sizable effects on poverty, especially on the poverty gap, which was reduced by almost 7 percentage points. PRAF in Honduras and Oportunidades in Mexico had more modest impacts on poverty.5

Another way to measure the impact of CCTs on welfare is to compare the cumulative distribution of consumption per capita between the treatment and control populations. This method has the advantage of not relying on the selection of a poverty line, which can be somewhat arbitrary. If the cumulative distribution for treated households lies completely to the right of the distribution for control households—so-called first-order stochastic dominance—current welfare is improved unambiguously by CCTs. This is clearly the case for RPS beneficiaries in Nicaragua, as shown in panel A of figure 4.1. Panel B shows an improvement that is much smaller for Honduras, a result that is not surprising given the smaller magnitude of the transfer.

The Impact of CCTs on Poverty at the National Level

The welfare effects of CCT programs discussed so far are based on the sample of households in the impact evaluation surveys. That is, we have assessed impacts on those households and individuals directly
Table 4.2 Impact of CCTs on Poverty Measures, Various Years

<table>
<thead>
<tr>
<th>Poverty measure</th>
<th>Colombia</th>
<th>Honduras</th>
<th>Mexico</th>
<th>Nicaragua</th>
</tr>
</thead>
<tbody>
<tr>
<td>Headcount index</td>
<td>Control</td>
<td>Impact</td>
<td>Control</td>
<td>Impact</td>
</tr>
<tr>
<td></td>
<td>0.95</td>
<td>-0.03*</td>
<td>0.88</td>
<td>0.91</td>
</tr>
<tr>
<td>Poverty gap</td>
<td>Control</td>
<td>Impact</td>
<td>Control</td>
<td>Impact</td>
</tr>
<tr>
<td></td>
<td>0.58</td>
<td>-0.07**</td>
<td>0.49</td>
<td>0.54</td>
</tr>
<tr>
<td>Squared poverty gap</td>
<td>Control</td>
<td>Impact</td>
<td>Control</td>
<td>Impact</td>
</tr>
<tr>
<td></td>
<td>0.53</td>
<td>-0.02**</td>
<td>0.30</td>
<td>0.36</td>
</tr>
</tbody>
</table>

Source: Authors’ calculations.

Note: We exclude Cambodia and Ecuador from this table because the CCT did not have an effect on median consumption in those countries and so it is not surprising that it did not reduce poverty. We also exclude the Brazilian Bolsa Alimentação program because the evaluation sample is not representative of the program’s target population, which makes the analysis of the impact on poverty less informative. For Honduras, Mexico, and Nicaragua, calculations were done via regression of household level Foster-Greer-Thorbecke indicator on treatment dummy and other explanatory variables. Using the evaluation sample of each program, we compute \( P(i,t,a) = \frac{z - y(i,t)}{z} \alpha \) * Poor\((i,t)\), for alpha = 0, 1, and 2; and for each household, where \( y(i,t) \) is household \( i \)’s level of consumption per capita at year \( t \), \( z \) is the country-specific poverty line, and \( \text{Poor}(i,t) \) is an indicator function that equals 1 if the household is poor and equals 0 otherwise. For Honduras, the poverty line used was Lps 24.6 per capita per day in 2000 lempiras. Expenditure values for 2002 were deflated to 2000 lempiras. For Nicaragua, we used C$13.87 per capita per day in 2000 córdobas. Expenditure values for 2001 and 2002 were deflated to 2000 córdobas. For Mexico, we used the value of the Canasta Básica of 1997, which was M$320 per capita per month. We inflated this value of the Canasta Básica for 1998 and 1999 using the Canasta Básica Price Index found at: http://www.banxico.org.mx/polmoneinflacion/estadisticas/indicesPrecios/indicesPreciosConsumidor.html. Therefore, for October 1998, we used M$320 × 1.134. For June 1999, we used M$320 × 1.280. For October 1999, we used M$320 × 1.314. For Colombia (see Institute for Fiscal Studies, Econometría, and Sistemas Especializados de Información 2006), the estimated impacts presented here are not equal to the unconditional double difference estimates because regressions control for other correlates. The impact for Honduras was obtained from 2002 regression only. The impacts for Mexico are all for single equation cross-sectional regressions for each year.

A. Baseline, before households in CCT treatment group received transfers.
B. No significant impact on poverty measure.

* Significant at the 10 percent level.
** Significant at the 5 percent level.
affected by the programs at a given stage of each program’s implementation. Given that the evaluation samples were derived either from the pilot stages of a program (as in Honduras and Nicaragua) or from the early phases of expansion of the program (as in the Bolsa Alimentação program in Brazil and the Oportunidades program in Mexico), these estimates may not be representative of the impacts of CCTs on the population of beneficiaries after coverage has been expanded to the national level. In this section, we investigate the poverty impacts of some relatively large CCT programs, using nationally representative household surveys in four countries: Brazil, Ecuador, Jamaica, and Mexico. The welfare measure used is household consumption per capita (except for Brazil, where we use household income per capita because consumption data were not available). The poverty line is set in each country at the 25th percentile of the pretransfer distribution of consumption or income.

To approximate pretransfer income or consumption for CCT beneficiaries, we simply subtract the full value of the transfer from income or consumption reported in the survey. This approach has important shortcomings: it amounts to ruling out behavioral changes, such as reductions in labor supply or remittances arising from the receipt of transfers, by assumption. Furthermore, and unlike the evaluations

Figure 4.1 Impact of CCTs on the Distribution of Consumption, Nicaragua and Honduras, 2002

Source: Authors’ calculations.
Note: CDF = cumulative distribution function.
discussed in the previous section, these calculations may be biased by purposeful program placement or self-selection. Nevertheless, and keeping these important caveats in mind, the results are useful because they enable us to approximate the impact of large-scale CCT programs on measures of poverty at the national level.

Table 4.3 suggests that CCTs generally helped reduce national poverty. In Mexico there are large effects on poverty, especially for the poverty gap and squared poverty gap measures. For example, the estimates in table 4.3 suggest that Oportunidades decreased the squared poverty gap by approximately 29 percent. In Jamaica, PATH reduced the squared poverty gap index by 13 percent from its pretransfer value. In Brazil, the impacts of the Bolsa Família program on the headcount index and the poverty gap are modest; however, the program reduces the squared poverty gap by a substantial amount, 15 percent. This finding is consistent with the findings of Paes de Barros, Foguel, and Ulyssea (2006), who suggest there is a strong link between the introduction of CCTs and the fall in inequality in Brazil.

The most puzzling findings correspond to the BDH program in Ecuador because the results in tables 4.2 and 4.3 are very different. It is likely that this difference arises, at least in part, because the estimates in

<table>
<thead>
<tr>
<th>Country</th>
<th>Headcount</th>
<th>Poverty gap</th>
<th>Squared poverty gap</th>
<th>Size of the transfer (% of PCE)a</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Pre-transfer</td>
<td>Post-transfer</td>
<td>Pre-transfer</td>
<td>Post-transfer</td>
</tr>
<tr>
<td>Brazil</td>
<td>0.2421</td>
<td>0.2369</td>
<td>0.0980</td>
<td>0.0901</td>
</tr>
<tr>
<td>Ecuador</td>
<td>0.2439</td>
<td>0.2242</td>
<td>0.0703</td>
<td>0.0607</td>
</tr>
<tr>
<td>Jamaica</td>
<td>0.2439</td>
<td>0.2329</td>
<td>0.0659</td>
<td>0.0602</td>
</tr>
<tr>
<td>Mexico</td>
<td>0.2406</td>
<td>0.2222</td>
<td>0.0847</td>
<td>0.0683</td>
</tr>
</tbody>
</table>

Source: Authors’ calculations.

Note: PCE = per capita expenditure. The poverty line used in each country is the 25th percentile of the pretransfer national distribution (prior to the symmetrical trimming of the distribution for extreme outliers; that is, values of less than the 1st percentile and above the 99th percentile of the distribution). For Brazil, the measure of welfare used is per capita income (PCI). In the other three countries, the measure of welfare is PCE. Pretransfer welfare is derived by subtracting the full value of the per capita cash transfer reported by a beneficiary household in each country from its welfare measure, inclusive of the transfer (PCE or PCI in Brazil). For Brazil, we use the Pesquisa Nacional por Amostra de Domicílios 2006. For Ecuador, we use the Encuesta de Condiciones de Vida 2006. For Jamaica, we use the Survey of Living Conditions 2004. For Mexico, we use the Encuesta Nacional de Ingresos y Gastos de los Hogares 2004.

a. The transfer amounts as a proportion of per capita expenditures (or consumption) are not the same across all tables in the report because of differences in the surveys used, including their coverage and year.
table 4.3 disregard the very large reduction in child labor, which offsets the impact of the transfer (Edmonds and Schady 2008). In addition, differences in the coverages of the surveys may be important: the survey used for table 4.3 is nationally representative, whereas the data collected for the impact evaluation of the BDH was limited to four provinces and to poor households within those provinces.8

**Impacts on the Composition of Consumption**

In addition to impacts on aggregate consumption, CCTs may affect disproportionately the consumption of particular items, such as food. This is of interest for a variety of reasons, including the link between food consumption and such measures of nutritional status as height-for-age and weight-for-height in children and body mass index for adults.9 Also, in analyzing consumption patterns among CCT recipients, it is possible to test whether households use transfer income differently from other sources of income. This could happen for a variety of reasons. Transfers are made to women, and there is a large body of evidence suggesting that women have different preferences over consumption than do men (Thomas 1990; Hoddinott and Haddad 1995; Lundberg, Pollak, and Wales 1997; Doss 2006; Ward-Batts 2008); that the conditions attached to transfers or the social marketing of programs may affect how transfer income is used;10 and that transfer income may be perceived as temporary, in which case households may save rather than consume the bulk of it (as suggested by the permanent income hypothesis).

A number of authors have analyzed CCT effects on the food Engel curve—the share of consumption that is devoted to food at various levels of total consumption. The intuition behind this is as follows: CCTs transfer cash, which increases total consumption, as shown above. If households perceive CCTs as any other source of income, we would expect that transfers move them along the food Engel curve. On the other hand, if transfer income is treated differently from other sources of income, CCTs may result in shifts of the food (and other) Engel curves.

To see whether that is the case, figure 4.2 graphs food Engel curves for treated and control households in Ecuador and Nicaragua. The figure shows that Engel curves in both countries have the familiar downward-sloping shape, with the share of food decreasing as total expenditures rise. This phenomenon is known as “Engel’s law.” However, both panels of the figure show that, at the time of follow-up,
the food Engel curves of CCT beneficiaries are everywhere above those for control households—clear evidence that transfer income was used differently from other sources of income.

Similar results are reported elsewhere. Using nonexperimental data for the Familias en Acción program in Colombia and the urban Oportunidades program in Mexico, Attanasio, Battistin, and Mesnard (2008) and Angelucci and Attanasio (2008) report upward shifts of the food Engel curves among program beneficiaries. The regression results presented in table 4.4 also corroborate these findings. Food share regression results indicate that for a given level of total household expenditure, treated households tend to consume a larger proportion of food. For example, the food share is about 4 percentage points higher among program beneficiaries in Colombia, Ecuador, and Nicaragua than among non-beneficiaries. Moreover, insofar as CCT programs affect total consumption, the effect on the level of food expenditures (as opposed to the share, measured by the Engel curve) can be considerable. In Mexico, for example, the median value of food consumption was 11 percent higher for beneficiary households than for comparable control households, and the median caloric consumption had increased by 8 percent (Hoddinott, Skoufias, and Washburn 2000).
The increase in expenditures on food generally is directed toward increasing quality. Households that benefited from Familias en Acción in Colombia significantly increased items rich in protein, such as milk, meat, and eggs (Attanasio and Mesnard 2006); and the increases in food expenditures in Mexico and Nicaragua were driven largely by increased consumption of meat, fruits, and vegetables (Hoddinott, Skoufias, and Washburn 2000; Maluccio and Flores 2005). Oportunidades also increased caloric diversity as measured by the number of different food-stuffs consumed. At similar overall food expenditure levels in Nicaragua, Macours, Schady, and Vakis (2008) show that households that receive transfers from the Atención a Crisis program spend significantly less on staples (primarily rice, beans, and tortillas) and significantly more on animal protein (chicken, meat, milk, and eggs), as well as on fruits and vegetables. Angelucci and Attanasio (2008) report similar results using data for urban Oportunidades in Mexico. Not only did households diversify their diets; they also shifted toward higher-quality sources of calories.

What causes these Engel curve shifts? Schady and Rosero (2007, 2008) hypothesize that CCTs increase the bargaining power of women within the household, and that this results in increased food expenditures. They use data on the BDH program in Ecuador to test

<table>
<thead>
<tr>
<th>Table 4.4</th>
<th>Impact of CCTs on Food Shares</th>
</tr>
</thead>
<tbody>
<tr>
<td>Brazil</td>
<td>Colombia</td>
</tr>
<tr>
<td>Daily per capita food consumption</td>
<td>Control</td>
</tr>
<tr>
<td>Impact (%)</td>
<td>12**</td>
</tr>
<tr>
<td>Food shares</td>
<td>Control (%)</td>
</tr>
<tr>
<td>Impact (percentage points)</td>
<td>0.02**</td>
</tr>
</tbody>
</table>

Sources: For Brazil, Ecuador, Honduras, and Nicaragua, authors’ calculations. For Colombia, see Institute for Fiscal Studies, Econometría, and Sistemas Especializados de Información (2006).

Note: Daily per capita food consumption is presented in US$ converted by the official current exchange rates at the time of the surveys. Food share is the percentage of total per capita consumption dedicated to food. The estimated impacts presented here are not always equal to the unconditional double-difference estimates because some regressions control for other correlates. In the case of Honduras, impacts were estimated with 2002 data only (via cross-sectional regression).

A. Baseline, before households in CCT treatment group received transfers.
B. No significant impact on poverty measure.
** Significant at the 5 percent level.
that hypothesis. Specifically, they argue that if changes in bargaining power are important, one would expect to see program effects on the food Engel curve among households that included prime-age men and prime-age women at baseline (where bargaining between males and females is an issue), but not among households with only prime-age women (where there is no bargaining of this sort). The results of their analysis are consistent with this prediction.

Analyzing Offsetting Behavioral Responses to CCTs

The size of the transfer and the fraction of poor households that receive it are major determinants of CCT impacts on consumption poverty. However, table 4.1 shows that, for most countries, the impact of the transfer is generally somewhat smaller than the magnitude of the transfer (when both are normalized as a fraction of the consumption or income of households in the control group). The difference between these two values may be a result of behavioral changes by CCT beneficiaries, which partly offset the value of the transfer itself. We now turn to a discussion of the evidence on these possible offsetting effects, focusing on impacts on child labor, adult labor, remittances, fertility, and spillovers and other general equilibrium effects.

Child Labor

Whether CCT programs help reduce the prevalence and amount of child work is of interest not only because of the resulting difference between the amount of the transfer and the change in household consumption. Rather, a reduction in child work is often seen as a good in its own right: working under poor conditions can adversely affect both the physical and mental health of children, and income-generating activities for children often take place at the cost of reductions in educational attainment and future earnings.

There are two main channels through which CCTs could reduce the prevalence and amount of work among school-age children. The first channel works though the conditional nature of the programs. Given the requirement of school enrollment and regular attendance, children have less time available for participation in income-generating activities.
Conditions also may increase parents’ awareness of the importance of schooling and thereby decrease child work. The second channel is a pure income effect: households that receive the transfer are less likely to be dependent on the income of their children, and therefore may reduce child work, as suggested by a number of theoretical models (Basu and Van 1998; Baland and Robinson 2000).

Several CCTs have been successful in reducing child work. Frequently, these impacts have been concentrated among older children. Table 4.5 shows that Oportunidades reduced child work among older children, aged 12–17, especially among boys (for whom baseline levels of child work also were substantially higher). Skoufias and Parker (2001) also show that domestic work decreased substantially, especially for girls.

In Ecuador, Edmonds and Schady (2008) show that the Bono de Desarrollo Humano program had very large effects on child work among those children most vulnerable to transitioning from schooling to work. Those effects are concentrated in work for pay away from the child’s home. On the other hand, BDH transfers had small effects on child time allocation at peak school attendance ages and among children already out of school at baseline. In Cambodia, the CESSP program, which gives transfers to children in transition from primary to lower-secondary school, reduced work for pay by 11 percentage points (Filmer and Schady 2009c).

### Table 4.5  Impact of Oportunidades on the Probability of Children Working

<table>
<thead>
<tr>
<th>Age group</th>
<th>Pre-program level</th>
<th>November 1999 Coefficient</th>
<th>t-statistic</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Boys</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>8–11 years</td>
<td>0.0620</td>
<td>–0.011</td>
<td>–1.3</td>
</tr>
<tr>
<td>12–17 years</td>
<td>0.3775</td>
<td>–0.047</td>
<td>–2.1</td>
</tr>
<tr>
<td><strong>Girls</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>8–11 years</td>
<td>0.0353</td>
<td>0.000</td>
<td>–0.5</td>
</tr>
<tr>
<td>12–17 years</td>
<td>0.1317</td>
<td>–0.023</td>
<td>–1.8</td>
</tr>
</tbody>
</table>

*Source: Skoufias and Parker 2001, table 5.*
Other CCT programs also appear to have reduced child work. In Nicaragua, the RPS reduced child work by 3–5 percentage points among children aged 7–13 (Maluccio and Flores 2005). Furthermore, the fraction of children who only studied (as opposed to worked and studied, only worked, or neither worked nor studied) increased significantly (from 59 percent to 84 percent) as a result of the RPS (Maluccio 2005). Yap, Sedlacek, and Orazem (2008) estimate the effects of the Brazilian PETI, another precursor of the Bolsa Família program. PETI gave out conditional transfers to secondary school-age children enrolled in school. Stipends were given directly to students, not to the families, conditional on school attendance and participation in special training workshops. PETI beneficiaries reduced substantially their probability of working. Attanasio et al. (2006), however, find no effect of the Familias en Acción program on child work in Colombia (although the program does appear to have reduced the amount of time dedicated to domestic chores); and Glewwe and Olinto (2004) find no effects of the PRAF program on child work in Honduras.

Two recent papers consider the impact of CCTs on child work when the transfer is conditional on school attendance for only one child in the household, and that child has siblings. Potentially, programs of this nature could have positive or negative spillovers for other siblings—positive if the income effect reduces child work for all children, if transfers increase the bargaining capacity of women within the household, or if the social marketing by the program leads parents to reduce child work even for children whose school attendance is not monitored; negative if parents compensate for the reduction in work of one child by increasing the work of other siblings. Barrera-Osorio et al. (2008) analyze Subsidio Condicionado a la Asistencia Escolar, a pilot CCT program in Bogotá, Colombia. This program randomized assignment to individual children rather than households, and made transfers directly to students rather than to their parents. Barrera-Osorio et al. show that, within the same household, a student selected into the program is 2 percentage points more likely to attend school and works about 1 hour less than a sibling who has not been selected. However, the beneficiary’s sibling (particularly if this sibling is a girl) is less likely to attend school than are children in households that received no cash transfer at all. On the other hand, Filmer and Schady (2009c) find that the CESSP program in Cambodia had no effect on the school enrollment of a beneficiary’s ineligible siblings. More research is needed to understand this difference.
between the two programs, especially if the number of CCTs that attempt to target individual children increases. (For a discussion of time spent in school as a substitute for child labor, see box 4.1.)

**Adult Labor Supply**

Of greater concern than changes in the amount of child labor are any possible reductions in adult work that result from CCTs. Such reductions could happen for a variety of reasons. If leisure is a normal good, then the income effect associated with the transfer might result in more leisure and less work. There also may be a price effect: beneficiaries of CCTs may believe (correctly or incorrectly) that they need to supply less labor to become or continue to be “poor” and eligible for a means-tested program. Adults also may have to take time away from work—for instance, to take children to school or health clinics. All of these could result in a reduction in adult work effort. Indeed, concern with possible work disincentives was one of the main reasons for the reform of transfer and other welfare programs in the United States in the 1990s (see box 4.2).

In practice, CCTs appear to have had, at most, modest disincentives for adult work. Two studies (Parker and Skoufias 2000; Skoufias and

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**Box 4.1  Is Time Spent in School a Perfect Substitute for Time Working?**

An important question is whether time spent in school and time spent at work are fully substitutable. The answer seems to be rarely so. In Colombia, Attanasio et al. (2006) provide evidence of partial substitution between school and work, with at most 25 percent of each extra hour spent on schooling coming from time otherwise spent at work. Because most of the substitution arises from a decrease in hours of domestic work activities, time spent on income-generating activities largely is unaffected and leisure is somewhat reduced. The substitution effects are largest among the children ages 10 to 13 in rural areas and ages 14 to 17 in urban areas.

In their analysis of the FFE program in Bangladesh, Ravallion and Wodon (2000) show that the decrease in the prevalence of child work for boys was only about a quarter of the increase in the percentage of boys enrolled in school. For girls, it was only about one eighth of the increase in enrollment.

On the other hand, using data from Oportunidades and a broad definition of work (including market, farm, and domestic work), Skoufias and Parker (2001) show that the reduction in time spent at work largely equals the increase in schooling for boys, but not for girls. Rather, leisure time is reduced significantly for girls. The pattern also varied by gender in other ways, with boys reducing both market and domestic work and girls reducing mainly domestic work.
Box 4.2 Work Disincentive Effects of Social Assistance Programs in Developed Countries

The literature on work disincentive effects of social assistance programs in developed countries is vast (for surveys, see Atkinson 1987; Krueger and Meyer 2002; Moffitt 2002). Moffitt, for instance, finds that because of its implicit tax on income, the U.S. federal assistance program, Aid to Families with Dependent Children (AFDC), reduced labor force participation of beneficiaries by 10 to 50 percent, when compared with similar nonbeneficiary households.\(^a\)

To address the issue of a strong built-in disincentive to work, in 1996 the U.S. government replaced AFDC with a new program, Temporary Assistance for Needy Families (TANF). TANF is different from AFDC in many ways. First, there is no entitlement. The fact that a household’s income is below a certain level does not entitle it to a transfer. Second, TANF introduced time limits. Individuals cannot receive cash benefits for more than five years (with few exceptions); and after two years in the program, recipients must work at least 30 hours per week to continue to be eligible for the transfers. Third, at least 50 percent of single-mother recipients and 90 percent of two-parent families must be working or in a job training program offered by the state. Finally, states now may decide on their program’s benefit reduction rates (or implicit tax on income). That is, instead of being required to reduce the benefits by one dollar for each dollar earned, states can decide whether this implicit tax rate will be zero to one, one to one, or any other rate in between.

A study by Grogger (2003) indicates that up to 12 percent of the welfare caseload decline observed between 1993 and 1999 (from 33 percent to 15 percent), and up to 7 percent of the observed increase in employment rates of families headed by single mothers (from 69 percent to 83 percent) during the same period were the result of time limits introduced by TANF. The study finds no effects of time limits on hours worked by recipient single mothers. Bloom and Michalopoulos (2001) show results from randomized experiments that indicate time limits seem to increase employment of welfare recipients by 4–11 percentage points (from a base that varied from 40 percent to 55 percent employment rates).

According to the research surveyed by Blank (2002) and Moffitt (2002), however, the marginal tax rate changes embodied in TANF do not seem to have had any effect on work effort. It seems that most of the changes in labor supply induced by TANF came from its time limits and work requirement.

\(^a\) Between 1935 and 1996, the main government cash transfer program in the United States was AFDC. Federal law required that the AFDC grant to an individual be reduced by one dollar for each dollar earned as income. This requirement represented a 100 percent implicit tax on income, and many policy makers and academics alike worried that such design features created strong work disincentives.

di Maro 2006) examine the effects of Oportunidades on adult labor supply; neither finds evidence of disincentive effects. The data used by Edmonds and Schady (2008) suggest that the BDH program in Ecuador had no effects on adult labor supply; in a similar vein, Filmer and Schady (2009c) report that adult labor supply was largely unaffected by the CESSP program in Cambodia. Only in Nicaragua is there some evidence of significant negative effects on adult work: Maluccio and Flores (2005) show that the RPS resulted in a significant reduction
in hours worked by adult men in the preceding week (by about 6 hours), with no effect among adult women.

Why did CCT programs not lead to larger reductions in adult labor supply, as had been a concern of many policy makers and academics? There are various possible explanations. First, the beneficiaries of CCT programs generally are very poor, and the income elasticity of leisure may be quite low for households that are this poor. Moreover, for some households the reduction in income from child work and the increase in school expenditures associated with the additional school enrollment offset the amount of the transfer. Obviously, that is particularly true for programs that made small transfers but had large effects on school enrollment and child work, as with the CESSP program in Cambodia; but Edmonds and Schady (2008) show that it was also the case for beneficiaries of the BDH program in Ecuador. Under these circumstances, increasing adult labor supply (or at least not reducing it) is one way to keep income and consumption at a level comparable with what it would have been had a household not taken up the program. Not coincidentally, perhaps, disincentive effects on adult labor supply are found only for the program that made the most generous transfers, the RPS in Nicaragua.

There are other reasons that might help explain why there have not been large disincentives to adult labor associated with CCTs. First, there are issues of timing. If households perceive transfers to be “temporary” rather than a permanent new “entitlement,” they would treat them as a windfall, and generally would not change the labor supply of adults. Moreover, the data used to estimate the CCT impacts on labor supply generally reflect household responses shortly after they have become eligible for the program for the first time. In the longer run, as households have more time to adjust their behavior, disincentive effects on adult labor may become more of an issue. Nevertheless, recent research on the South African old-age pension (OAP) scheme, which makes transfers that dwarf those of even the most generous CCTs and which is likely to be seen as “permanent” by beneficiaries, is encouraging. As box 4.3 shows, the OAP does not appear to have reduced work effort by prime-age adults.

**Crowding-Out of Remittances and Transfers**

The impact of CCTs on consumption poverty also could be offset if they crowd out transfers from other sources, such as remittances. That could happen if senders of remittances or other private transfers target
Box 4.3 Do Transfers Reduce the Supply of Adult Labor? Evidence from the South African Pension Scheme

The South African OAP scheme provides a generous benefit to retirees in that country. The value of the transfer is more than twice median per capita income for African (black) households. In principle, the program is means tested. In practice, however, all households that do not have a private pension are eligible. The program was made available to black families after the end of apartheid in 1994. By now, it is likely that the program is seen as an “entitlement” by most beneficiary households.

Early research on the OAP suggested that it had substantial negative effects on adult labor supply (Bertrand, Mullainathan, and Miller 2003). More recent research (Ardington, Case, and Hosegood 2008) disputes those findings. These new results are based on better data—specifically, panel data rather than a single cross-section, which allows the authors to control for time-invariant differences between pension recipients and nonrecipients; and data on nonresident (migrant) household members, which are important because migrant status is correlated with pension receipt. The preferred specification in Ardington, Case, and Hosegood (2008) suggests that the OAP had a positive effect on adult labor supply—the probability that prime-age adults are employed is approximately 3 percentage points higher in households with at least one pension recipient. Those authors argue that the OAP relieves financial and child care constraints, which can be short-run impediments to migrating, even when the medium-run returns to migrating are positive.

Empirical evidence on the crowding-out effects of CCTs shows mixed results. For Mexico, Albaran and Attanasio (2003) show some indication of crowding out for Oportunidades, using one round of ex post evaluation data. However, Teruel and Davis (2000) reject the crowding-out impact of Oportunidades on private transfers, using more rounds of evaluation data. Their result holds for both monetary and in-kind transfers.

More recently, Nielsen and Olinto (2008) provide evidence on crowding-out effects of the Honduran and Nicaraguan CCT programs. They find that both the prevalence and the amount of remittances in the two countries were unaffected by the programs. That finding is comforting because remittances constitute a major source of foreign income for recipient households or seek to equate marginal utility across donors and recipients. When part of the transfer is crowded out, this fraction will accrue to households outside the target group, and program recipients will benefit less than intended. However, the implied mistargeting of program resources also means that there is a positive effect of the program beyond that measured by evaluation surveys in the treatment areas.
currency for many countries in Central America. However, the evidence in Nielsen and Olinto (2008) points toward some crowding out of private food transfers and money and food transfers from NGOs in Nicaragua, which could be a concern if it represents a change in informal insurance schemes. The PRAF in Honduras does not seem to crowd out any of these private transfers, most likely because of the modest size of CCT payments in that program.

Fertility and Family Composition

Transfers made by CCT programs are often a function, in part, of the number of children, sometimes with a cap on the total amount of transfers for which a household can be eligible (see the discussion in chapter 3). One concern is that CCTs could provide incentives for increased fertility, which could result in eventual reductions in household (and national) welfare.

In practice, any effects on fertility appear to have been modest. A recent paper (Stecklov et al. 2006) finds no effects on the total fertility rate among beneficiaries of Oportunidades in Mexico or the RPS in Nicaragua; however, it appears that PRAF increased fertility among eligible households in Honduras by 2–4 percentage points. The authors argue that these differences can be explained by differences in program design: In Mexico, the transfer to households with preschool children was a lump sum, regardless of the number of children; and (in the first three years of the program) poor, childless households could not become eligible for transfers if they had children after the first wave of inscriptions. In Nicaragua, the transfer was also a lump sum, although some households became eligible for transfers once they had children. In Honduras, finally, new households could be registered if they gave birth to children, and the amount of the transfer depended on the number of young children. If borne out by results from other countries, the evidence in Stecklov et al. (2006) would suggest that the details of program design are important because they can provide incentives that result in unintended outcomes.¹³

Spillovers and General Equilibrium Effects

CCT programs often are targeted geographically to poor and remote rural areas. In some cases, transfers are substantial and a
large proportion of the population in a community receives them. Potentially, this could result in general equilibrium and spillover effects in the local economy. For instance, CCTs could increase the prices of consumption goods through higher demand, or could increase prevailing local wages because of the reduction in the labor supply of children.

To assess whether such spillover effects occurred in Mexico, Angelucci and de Giorgi (2008) analyze the evaluation data of Oportunidades for both beneficiaries and ineligible households living in treatment communities. They find that there was no indirect negative effect on labor earnings, prices, and the receipt of other welfare payments. In fact, they observe that the real incomes of ineligible households living in treatment communities seem to have been affected positively by the program. They show that ineligible households in treatment villages consumed more by receiving more private transfers, by borrowing more (almost exclusively from family, friends, or informal moneylenders), and by reducing their stocks of grains and animals. In addition, they show that the indirect program effects on consumption and loans are larger for households hit by a negative shock.

The lack of impact on wages and prices of consumer goods is not surprising. In most countries in which CCTs have been evaluated, labor and goods markets are sufficiently developed so that both labor and goods are largely tradable. CCTs may induce larger local demand for goods and lower local supply of labor, and, in the short run, prices may change to reflect these imbalances; in the long run, however, prices should return to their initial equilibrium.

Another kind of spillover effect is related to changes in access to and use of the formal banking sector. A number of CCT programs, including Bolsa Familia in Brazil and the BDH in Ecuador, directly deposit benefits in bank accounts created for beneficiaries, who then can withdraw cash using an automated teller machine (ATM) card. That payment system appears to have reduced transaction costs (such as standing in line to receive transfers), and is likely to have reduced any stigma attached to the program. In addition, creating a bank account for CCT beneficiaries and giving them ATM cards may make it more likely that they use the formal banking sector in other capacities—potentially, a very important benefit of CCT programs and one that has not been evaluated to date.
Long-Term Impacts of CCTs on Consumption

As we have shown, many CCT programs have had substantial effects on consumption and poverty in the short run. A natural question is whether those positive impacts are likely to remain, at least in part, once households are no longer eligible for the CCT or the program ceases to exist altogether. Positive effects could be maintained, for example, if part of the transfer is saved and invested in productive assets, or if the stable income stream of the transfer allowed households to gain access to credit and overcome liquidity constraints. It also is possible that the transfer enables households to smooth consumption when they face negative shocks. If CCT programs do have a long-term impact on household consumption, then the estimates of program impact on short-term consumption and poverty reported above will underestimate the true (medium- and long-run) impact of CCTs on poverty.

Looking first at the investment of transfers, Gertler, Martínez, and Rubio-Codina (2006) provide extensive evidence on the Mexican experience. They find that the program had a substantial positive impact on investment in productive activities such as microenterprises and agriculture (animals and land). On average, 12 percent of transfers were invested, and households that received more transfers from Oportunidades also invested more. It seems that the CCT helped alleviate two market failures. First, the increased income allowed households to overcome credit constraints. Second, the stable stream of income may have made households willing to undertake more risky (and profitable) investments.

Another study (Maluccio 2008) assesses the impact of the RPS program in Nicaragua on various types of investments. The author finds only limited evidence that the program led to an increase in investment for agricultural equipment. His findings do not imply that the program had no long-term effects—it almost certainly did in terms of investment in child health and education, which should continue to lead to benefits for many years to come. In contrast to Mexico, however, there was only weak (albeit positive) evidence that RPS improved investment activities, possibly because of an economic downturn during the period, the strong program orientation toward increased food expenditures, and the limited opportunities in the impoverished rural areas where the program operated.

CCTs also may help households smooth their consumption and protect them from adverse shocks. If that is true, treated households may
be relatively more willing to undertake risky investments and less likely to sell assets or discontinue their children’s school enrollment during an economic downturn.

The ability of the RPS to function as a social safety net during the so-called coffee crisis provides empirical evidence that CCTs can protect households from adverse shocks. For Nicaragua during 2000 and 2001, the crisis consisted of a drop in coffee prices to a 30-year low (or a 100-year low, adjusting for inflation) due to a worldwide oversupply of coffee (Varangis et al. 2003). The fall in prices hurt farmers and laborers socially and financially. By comparing coffee-growing and non-coffee-growing areas and treatment and control households, one can measure how well the RPS performed as a social safety net. Maluccio (2005) finds that the RPS enabled beneficiary households to maintain per capita expenditures during the crisis and helped reduce labor supply increases in coffee-growing areas. The effect was larger for those who were most affected by the fall in coffee prices.

It is important to make clear that the protection of income during a shock was achieved even if the RPS in no way was designed to respond to shocks. A similar result holds for Mexico, where Oportunidades helped beneficiary households smooth their consumption in the face of income fluctuations (Skoufias 2002). Skoufias also finds that Oportunidades provided that protection without replacing existing informal insurance schemes.

**Conclusion**

This chapter has reviewed the impacts of CCTs on household consumption, poverty, the composition of consumption, behavioral responses that might offset the effect of transfers, and long-term welfare. We focus on CCT programs for which there are robust evaluation data.

Policy makers and academics alike long have been concerned with the disincentive and general equilibrium effects of government cash transfers to the poor. That fear stems from hypothesized disincentives to work, crowding out of private transfers, effects on fertility and family composition, and effects on local wages and prices. The evidence reviewed in this chapter, however, suggests that these offsetting effects generally have been modest.
First, by and large, programs have had positive impacts on consumption, especially when the transfer amount is generous (as with the RPS program in Nicaragua). In and of itself, those positive impacts on consumption are indirect evidence that the offsetting behavioral responses are unlikely to be large, and that the marginal propensity to consume out of transfer income is high. Moreover, because transfers generally are well targeted to the poor, the effects on consumption have translated into impacts on poverty.

Second, the evidence suggests that CCTs generally do not have large disincentive effects on the labor supply of adults. More research is needed to see whether those patterns are maintained as programs mature and beneficiaries have more time to adjust their behavior. However, the results to date indicate that the popular view that cash transfers encourage indolence is not supported by the evidence.

Third, unlike most social assistance programs in the developed world, CCTs do not seem to crowd out private transfers. Although there is some evidence that CCTs crowd out intracommunity transfers of in-kind goods, these usually are transfers among the poor and therefore have very little redistributional impact. Also, CCTs do not appear to have had large effects on fertility.

Fourth, CCTs seem to have no significant negative effects on local wages, local prices, and the receipt of other welfare payments. In fact, contrary to expectations, there is some evidence that the real incomes of ineligible households living in program communities have been affected positively by CCTs. In Mexico, nonpoor households in treatment villages received more private transfers, borrowed more, and reduced their stocks of grains and animals; they also consumed more.

Finally, although the evidence suggests that, as intended, CCTs do have significant impacts in reducing child labor, the resulting income losses generally are not large enough to offset the impact of transfers on per capita consumption. (Cambodia and especially Ecuador appear to be exceptions to this general pattern.)

In sum, the main conclusion of this chapter is that redistribution via direct cash transfers seems to have worked well. Most programs, especially those making sizable transfers, have had substantial impacts on consumption and on poverty. The offsetting effects that were a source of concern when CCT programs were created do not appear to have occurred on a scale large enough to offset the bulk of the transfer.
CCTs do not seem to reduce the labor supply of adults or to crowd out private transfers. They do reduce the supply of child labor, but this reduction seems to have only a modest impact on household income and consumption. Moreover, some CCTs seem to increase productive investment, which boosts the impact on poverty even farther.