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Teenage School Attendance and Cash Transfers: An Impact Evaluation of PANES

Three years after a severe economic crisis, the Uruguayan government launched a temporary antipoverty plan, the National Plan for Social Emergency Assistance (PANES), which took place from April 2005 to December 2007, lasting for thirty-four months. This program included a cash transfer component, Citizen Income, which was, in fact, the main intervention of the plan. In the short run, the primary objective of this intervention was to provide cash assistance to indigent households. In the long run, it aimed at promoting social integration in many ways, such as carrying out educational and labor interventions for adults, encouraging households to foster child schooling, and promoting health checks. To pursue this objective, in the original design, program participation was conditional on school enrollment for those households with children and health checks for all beneficiaries. However, in practice, conditionalities were not controlled, owing to lack of administrative coordination among the involved institutions. Although promoting schooling was not

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the main objective of the plan, the cash transfer might have affected school attendance and child labor through a direct income effect and also through the influence of the announced conditionalities.

The provision of conditional cash transfers as a means to break the intergenerational transmission of poverty has entailed a redesign of Latin American social protection systems over the past two decades. The abundant literature on impact evaluations of conditional cash transfers indicates that many programs have successfully increased child schooling, particularly in the age group six to thirteen, and, in some cases, have reduced child labor, with no major undesirable effects on adult labor supply (ECLAC 2006; Fiszbein and Schady 2009; Bouillon and Tejerina 2007; Saavedra and García 2012). Yet the design and implementation of these programs varies a great deal across different instances, and baseline situations also differ across countries. According to the existing literature, baseline enrollment rates, design issues (such as payments scheme and timing, type of conditionalities), complementary supply-side interventions, and the generosity of the transfer are strongly associated with the magnitude of the effects on schooling (Fiszbein and Schady 2009; Bouillon and Tejerina 2007; Saavedra and García 2012).

This paper seeks to contribute to the accumulating literature about the impacts of cash transfers on teenage schooling and labor by providing evidence from a middle-income country in which the initial enrollment rates were above the average of the countries where these programs have been previously launched. We analyze program effects on school attendance and child labor for children aged fourteen to seventeen. This group concentrates the main problems of the Uruguayan educational system, as it exhibits persistently high drop-out rates. Owing to this circumstance, it is important to find out to what extent cash transfers can contribute to overcome this problem. We also explore the role of some of the potential channels highlighted in the cash transfer literature: household income, adult labor supply and labor income, and conditionalities.

This evaluation is based on two data sets: the official administrative records from PANES applicants (baseline data) and two waves of a follow-up survey that was specifically designed to carry out the impact evaluation of the program, the first gathered eighteen months after the program started, and the second two months after the program ended.

We provide evidence using two identification strategies: a regression discontinuity design (RDD) using separately the data from each wave of the evaluation survey, and a difference-in-differences (DD) approach that exploits the longitudinal nature of the collected data. The questionnaire from the first wave of the follow-up survey allows us to partially explore the role of conditionalities.

Cash Transfers, Child Labor, and School Attendance: Previous Findings

The Determinants of Child Labor and School Attendance

Traditionally, the economic literature has considered school attendance and child labor as a single decision in developing countries (Cardoso and Verner 2007; Nielsen 1998). In our framework, which considers schooling and labor as alternatives, human capital theory identifies two main reasons why children leave school to start working. One is that net returns for human capital investments are lower than for other assets, owing to either high indirect and direct costs of schooling or low education quality. Another explanation focuses on capital market imperfections that may prevent investment in human capital.[1]

Child labor has been formalized in an overlapping generations framework (see Basu 1999; Rosati and Rossi 2003; Deb and Rosati 2004; Basu and Pham 1998). These models are based on the unitary household decision model that assumes child consumption to be solely determined by parental transfers, regardless of the gender of the child.[2] In turn, parental income depends on parents' previous human capital accumulation, implying intergenerational transmission of child labor. In this setting, parents control their children's time and allocate it either to work or to study. While work increases present consumption, school attendance yields increased future income. The parental decision for allocation of their children's time, given their level of available resources, is based on the relative cost of present and future consumption. Higher education costs and higher remuneration to child labor increase the relative cost of future consumption, whereas the latter decreases as returns to human capital accumulation increase.

Since in these models the determinants of child labor and school attendance are the same, optimal behavior leads to one of two corner solutions (a child either works or studies) or to a solution in which the child both

1. For developed countries, the literature has identified other factors relating to school dropout, such as drug use, alcohol consumption, and parents' psychiatric disorders, in all cases controlling for socioeconomic and personal characteristics (Cardoso and Verner 2007).

2. Bargaining models that depart from the unitary household decision model allow different outcomes for girls and boys, as household members can allocate resources according to their individual preferences.

studies and works. However, if child labor and school attendance have different determinants, policies that promote the eradication of child labor may actually not promote school attendance and could thus even produce an increase in the number of idle children. Along these lines, Deb and Rosati (2004) develop a model that considers a third status in addition to these two options: allowing for children to be idle. The authors argue that this third optimal solution can be observed if the value of a child's leisure is positive or if work or schooling entails fixed costs.

The main results obtained in the empirical literature on child school attendance, child labor, and idleness indicate that older children and males are more likely to both attend school and work, elder siblings are less likely than their siblings to attend school, and children with lower ability are more likely to drop out from school to specialize in labor or to become idle (Cardoso and Verner 2007). In terms of household characteristics, typical findings indicate intergenerational persistence of child labor and a positive relationship between household economic well-being and school participation, as well as a negative one between household economic well-being and child labor. Finally, income poverty is found to result in specialization in labor or in inactivity, while negative shocks increase the probability of dropping out of school and entrance into the labor market. Cardoso and Verner (2007) also note the presence of an unexplained negative correlation between school attendance and child labor.

In general terms, the empirical literature has also confirmed that income, wealth, and credit availability are not strong explanatory factors of child time allocation (Deb and Rosati 2004). Moreover, causation cannot be attributed, as child time allocation and economic status are joint outcomes of a single decisionmaking process. Country-specific studies show that a nonnegligible share of children remain idle. These findings are important for three main reasons: First, unobserved household characteristics could play a key role if they explain unobserved heterogeneity in access to credit and income. Second, it may be possible to reduce child labor without relying exclusively on income growth. Finally, the phenomenon of children who neither work nor attend school (idle children) needs to be tackled to a greater extent in both theoretical and empirical works.[3]

3. If some training occurs on the job, idle children could become worse off in terms of human capital accumulation. Similarly, if labor market participation or school attendance involves access to networks, the state of being idle may lower social capital during the life cycle.

In this context, we aim to analyze the potential impact of a cash transfer on child school attendance and child labor decisions, as this intervention may change direct determinants of these decisions.

The Impact of Cash Transfer Programs on Child Labor and School Attendance

On the basis of the arguments presented in the previous section, household income can be considered one of the factors that explain how a cash transfer program may affect school attendance and child labor. If the transferred amount is above a certain threshold, then the household might modify the child's time allocation in favor of schooling (a formal model is presented in Skoufias and Parker 2001). Hence the incentive for sending children to school will vary with initial household income.[4] For the cash transfer to affect child labor and school attendance, an increase in net household income is required. This means that adults should not compensate the additional income from the transfer with a reduction in their labor effort. Given this potential disincentive effect, we also analyze potential channels that may result in a net negative income effect via labor market outcomes.

Economic theory suggests that the income effect associated with transfers may alter beneficiaries' labor supply (see, for example, Moffit 2002 and Tabor 2002). Specifically, assuming that leisure is a normal good, income transfers could lead to a fall in labor participation or the number of hours worked. Additionally, means-tested programs (such as the Uruguayan PANES) create an additional incentive to reduce labor supply, as means testing is in practice equivalent to an implicit tax on labor earnings. This would create an additional substitution effect, reducing labor supply. These adverse effects have led to important changes in the design of some welfare programs in the United States, and evaluations suggest the existence of important disincentive effects (Moffit 2002).

In the specific case of conditional cash transfers, these conditions may also influence outcomes. As Fiszbein and Schady (2009) argue, conditionalities are based on the idea that households' misguided beliefs about the process of investment in human capital may affect decisions relating to their children's education or parents' inclination to discount the future more heavily than they should ("incomplete altruism"). Such factors would result in a lower level of

4. For instance, if many eligible households were already sending their children to school, the incentive would lead children to allocate more time to studying rather than to increase enrollment rates.

human capital investment in children, and conditionalities are seen as a way to address these inefficiencies.[5] Skoufias and Parker (2001) therefore point out that school attendance conditionalities reduce the shadow price of schooling, which may reinforce the potential income effect of the transfer as long as school and work are substitutes. However, since this is not necessarily the case, schooling can be promoted at the expense of child leisure.

Theoretically, conditionalities may also affect adult labor supply because the time devoted to fulfilling conditionalities means that adults have less time for work. If these conditionalities include activities aimed at enhancing the human and social capital of adults in the household, the opposite effect could emerge in the medium run if program participation were to increase employability.[6]

Two recent systematic reviews address the effects of conditional cash transfer programs on schooling (Bouillon and Tejerina 2007; Saavedra and García 2012). The first one reports that most conditional cash transfer programs have increased enrollment and fostered the transition to secondary schooling. The authors also point out that differences in impact between countries may arise from the amount of the transfer, complementary supply-side interventions, and enrollment rate levels in the baseline, a point also addressed by Fiszbein and Schady (2009).

Saavedra and García (2012) carry out a meta-analysis considering forty-two evaluations of conditional cash transfer programs in fifteen developing countries. Twelve of these evaluations correspond to Latin American countries. The authors find that program effects are larger in secondary school than in primary school. According to their meta-analysis, the average effect on secondary attendance is 12 percent relative to the average baseline (68 percent), and Latin American programs are less effective in increasing this outcome compared with the other developing countries. Primary and secondary schooling effects are larger in those contexts that show worse initial conditions. Finally, the

5. The other well-known argument for conditionalities refers to the political economy of redistribution programs, as citizens tend to support conditional programs. Nevertheless, the imposition of conditions is a debated issue. Some authors consider conditionalities to be costly, inequitable, inefficient, and offensive to basic egalitarian principles (Standing 2008), whereas others highlight their benefits (de Brauw and Hoddinott 2008).

6. It should be noted that since the impact of cash transfers on children's outcomes depends on intrahousehold resource decisions, specifying the transfer recipient affects the policy's impact because it strengthens the beneficiary's internal bargaining power. There is evidence that cash transfers targeted at women have a stronger impact on children's outcomes, particularly for girls (see Barrientos and DeJong 2006). This may indicate a stronger preference of mothers for their children's consumption or investment.

authors point out that attendance effects are significantly larger in published impact evaluations, suggesting potential publication biases.

Impact evaluations have also shown that effects are concentrated in specific groups such as ethnic minorities, girls, and children living in rural areas (see, for example, ECLAC 2006; Coady 2001; Coady and Parker 2002; Skoufias and Parker 2001; Attanasio, Meghir, and Santiago 2002; Schultz 2004). Evidence regarding the effects on how far children ultimately go in school is thinner, and mostly comes from the experiences of a single program (Oportunidades in Mexico). It appears that conditional cash transfers modestly impacted the number of years of schooling completed by adults, but they do not seem to have affected children's school performance (ECLAC 2006; Fiszbein and Schady 2009).

In relation to child labor, the review carried out by Fiszbein and Schady (2009) indicates that conditional cash transfers have been successful in reducing child labor and that the favorable results are higher among older children. However, other studies show that child paid labor was reduced, albeit not to the extent originally expected by policy designers. This led to the hypothesis that schooling was, in part, increased by reducing child leisure time. Ravallion and Wodon (2000) show that a conditional in-kind transfer in Bangladesh increased school attendance and did not reduce child paid labor, so it presumably reduced children's leisure time. Skoufias and Parker (2001) find that the PROGRESA program in Mexico significantly increased school attendance and simultaneously reduced child labor, but in the case of girls, the increase in school attendance was much larger than the reduction in labor. Again, this may indicate that the increase in schooling came at the expense of leisure time. In fact, using data from a time-use module, they find that PROGRESA had no significant impact on the leisure time of boys but had a significant and negative impact for girls.

In their analysis of the Bono de Desarrollo Humano program in Ecuador, Edmonds and Schady (2012) find a significant negative effect of a lottery-induced (unconditional) cash transfer on paid employment and unpaid activity, with larger effects for those who were students at the baseline. Children in paid employment were a considerably large fraction, so there was a larger margin for decline in Ecuador than in other countries in the region. This decline in economic activity was accompanied by an increase in time devoted to unpaid household services, but overall, time spent working declined.

There is accumulated evidence on the possible channels explaining the effects of transfers on school attendance and child labor. First, as previously noted, adults may compensate for the income transfer with reduced labor participation. Fiszbein and Schady (2009) argue that most evaluations of conditional cash transfers found no significant disincentive effect on adult work. The exception is the Red de Protección Social program in Nicaragua, for which a significant negative impact on hours worked by adult men was found.[7]

Second, many studies suggest that conditionalities may explain some of the positive results found for school attendance. Evidence from several countries (Mexico, Ecuador, Cambodia, and Brazil) suggests that the impact on school attendance would have been smaller if the cash transfers had not included explicit conditions (Fiszbein and Schady 2009; Skoufias and Parker 2001). De Brauw and Hoddinott (2008) test the importance of conditions in relation to the increase in school enrollment found for PROGRESA. The authors exploit the fact that some program beneficiaries did not receive the form used for monitoring conditionalities, owing to an administrative error. They find that children from households that did not receive the form were less likely to attend school, especially if their children were transitioning from primary to lower secondary school. Similarly, Schady and Araujo (2008) compare the impact of Bono de Desarrollo Humano on school enrollment among those who erroneously believed there was a school conditionality with that of the remaining beneficiaries. They find that the program's effects on enrollment are only significant for conditioned households, defined as those who declared in the follow-up survey that they were aware of the enrollment requirement (which actually did not hold).

Baird, McIntosh, and Ozler (2011) provide an exploration of the role of conditionalities based on a randomized trial in Malawi, where the authors obtain positive impacts on school outcomes but negative ones on other outcomes, yielding the conclusion that the convenience of conditionalities relates to the outcome to be considered and to the specific context of the intervention. The authors compare results from a group randomly assigned to an unconditional cash transfer with those of a group that received a cash transfer conditioned on school enrollment and attendance. Regarding schooling outcomes, the authors find that conditional transfers had a large gain in enrollment and a modest but significant advantage in learning when compared with unconditional ones.[8]

7. The disincentive effect on hours worked was determined in relation to the control group because the labor supply of beneficiaries actually increased during the evaluation period (Maluccio and Flores 2005).

8. Teenage pregnancy and marriage rates, on the other hand, were substantially lower in the unconditional cash transfer arm than in the conditional one, casting doubts on their convenience.

According to the recent literature, design aspects are also key issues in the magnitude of the effects. More generous programs and those that vary their payments according to household size and educational level, pay benefits less frequently than monthly, and include conditionalities on achievement (as, for example, not failing grades) tend to produce better results (Saavedra and García 2012).

Barrera-Osorio and others (2011) also find that design features are a key determinant of program effectiveness. Specifically, they present experimental evidence from Colombia, showing that program effects increase when a fraction of the payment is delivered at the time children reenroll and when conditionalities depend on students' graduation and tertiary enrollment.

The Intervention: PANES

Program Characteristics

In March 2005, a center-left party (Frente Amplio) took power for the first time in Uruguay. The government created a new Ministry for Social Development and designed and implemented PANES. The program was a temporary antipoverty effort that lasted for almost three years. The program had two main purposes: to provide direct assistance to households that fell into poverty owing to the 2001–02 financial crisis and to reduce the long-run vulnerability of the poor by strengthening their human and social capital.

The target population consisted of households in the bottom quintile of the households below the national poverty line (approximately 8 percent of the population). Participating households included children in 95 percent of cases. In all, 102,353 households eventually became program beneficiaries, approximately 10 percent of Uruguayan households (and 14 percent of the population). Targeting was successful compared with most Latin American cash transfer programs (World Bank 2007).[9]

The program included several components. The main one was a monthly cash transfer whose value per household was set at US$56 (UY$1,360 at the 2005 exchange rate) regardless of household size. Households with children or pregnant women were also entitled to a food card, an in-kind transfer that operated through an electronic debit card whose monthly value varied

9. The program was entirely funded with government resources. Its total cost was US$247,657,026, which represents 0.41 percent of GDP and 1.95 percent of government social expenditures.

from US$13 to US$30. Approximately 70 percent of PANES beneficiary households received the food card. Other smaller components included a workfare program, job training, adult educational interventions, and health care subsidies.

As originally planned, the program was dismantled in December 2007 and replaced with a new system of family allowances that aimed to cover children in the bottom income quintile.[10] In practice, all PANES applicants who had children and were below a new proxy means test were automatically transferred to the new regime.

Enrollment and Eligibility

Enrollment occurred in two phases. All low-income households were publicly invited to apply. The application form recorded the name, sex, age, nationality, and a national ID number of all household members and self-reported per capita income. The government also made a large outreach effort, sending enumerators to poor communities in an attempt to boost applications and to ensure program uptake among the most deprived households. Accepted applicants received benefits for the duration of the program, and rejected households could reapply.

The program was means tested, and only households with per capita income below approximately US$50 a month were eligible and were subsequently visited by personnel from the Ministry for Social Development.[11] The income condition disqualified around 10 percent of the initial applicants.

Eventually, 188,671 applicant households were visited by ministry personnel, who administered a detailed baseline survey. This questionnaire resembles a typical household survey, with information collected on individual demographic and socioeconomic characteristics (age, sex, access to health insurance, education and schooling, labor market participation, income) along with data on possession of durables and housing conditions.

10. The family allowance, Asignaciones Familiares, is part of a wider program, named Plan de Equidad.

11. Per capita income was computed as the higher of social security income (excluding noncontributory benefits, that is, child allowances and noncontributory pensions) and self-declared income.

Among visited households, assignment to PANES was determined using a predicted poverty score that depended on household socioeconomic characteristics collected in the baseline survey.[12] Since a higher score denotes higher predicted poverty, only households with income above a predetermined poverty score were assigned to the program.[13]

Although the program was in principle conditional on children's school attendance and health checkups, conditionalities were not enforced, as was publicly acknowledged by Ministry for Social Development authorities after the program ended. Nevertheless, this fact was not known by beneficiaries while the program was taking place, and they may have assumed that they were being monitored.

Information gathered in the first follow-up survey indicates that beneficiaries were not fully aware of the existence of conditionalities. In effect, 58 percent of beneficiary households were aware that some conditions were attached to the program and only 20 percent declared that compliance with child school attendance was required. This fact raises questions about the nature of the program. Although PANES is usually considered a conditional cash transfer program, conditions were not monitored; more important, data suggest that a considerable share of beneficiaries were unaware of any conditions, particularly with respect to school attendance. In our analysis, we try to shed some light on this point.

Data

This research is based on official PANES records (the baseline data) and two waves of a follow-up survey that was specially designed for the impact evaluation of the program. The official records and the follow-up surveys contain information on individual demographic and socioeconomic characteristics (age, sex, access to health insurance, education and schooling, labor market

12. The score is based on a probit model of the likelihood of being below a critical level of per capita income relative to the poverty line (details can be found in Amarante, Arim, and Vigorito 2005).

13. Eligibility thresholds were allowed to vary across the country's five main administrative regions. The regional thresholds were set such that a similar share of poor households was entitled to the program in each area.

participation, income), household possession of durable goods, and housing conditions.

The impact evaluation involved collection of data through a special panel survey of a sample of beneficiaries and nonbeneficiaries. The main strategy used to evaluate the program was a regression discontinuity analysis based on the program admission criteria.

The survey sample was restricted to a group of households whose applications for benefits were evaluated between 23 September 2005 and 30 April 2006. Using data from this sample also ensures that the poverty score used for PANES eligibility is the same as that used for all applicants in our analysis.

The first wave of the follow-up survey was carried out between October 2006 and March 2007, roughly eighteen months after the beginning of the program. To exploit the potential of the discontinuity design, the original survey sample contained data on 3,000 households, including both eligible and ineligible applicants with scores in a 2 percent interval around the program eligibility threshold. There was an interest in overrepresenting eligible households so that the sample could be split between eligible and ineligible households in a 2:1 ratio.[14] The initial nonresponse rate was moderate, at 30 percent, so replacement households with approximately the same score as the nonresponding households were subsequently interviewed. Besides providing information about our outcomes of interest, this first wave of the follow-up survey allowed us to explore the role of conditionalities, as beneficiary households were asked about their awareness of a set of requirements.[15]

A second follow-up household survey was administered between March and June 2008, shortly after the end of the program. Attrition is a minor concern, as 92 percent of households from the first follow-up round were successfully resurveyed.

14. This main sample was supplemented with data on 500 eligible households that were further from the eligibility threshold, although we do not use these data in the present paper.

15. In addition to information on housing, household composition, possession of durables, labor, income, and schooling (as in the baseline survey), the follow-up survey collected information on health, economic expectations, knowledge of political, labor, and civil rights, trust in a wide set of institutions, participation in social groups, people or institutions the beneficiary asks for help when in trouble, opinions about the PANES program, and political attitudes, including support for the government.

To limit strategic responses, surveyed households were not informed about the exact purpose of the surveys during data collection. Information provided to respondents was referred solely to the university department in charge of fieldwork and did not specifically mention PANES or the ministry.

Methodology

We used two identification strategies for the impact evaluation of PANES. The first was a discontinuity regression approach, as the follow-up survey was specifically designed for this method. We also used a difference-in-differences approach to exploit the longitudinal nature of our data.

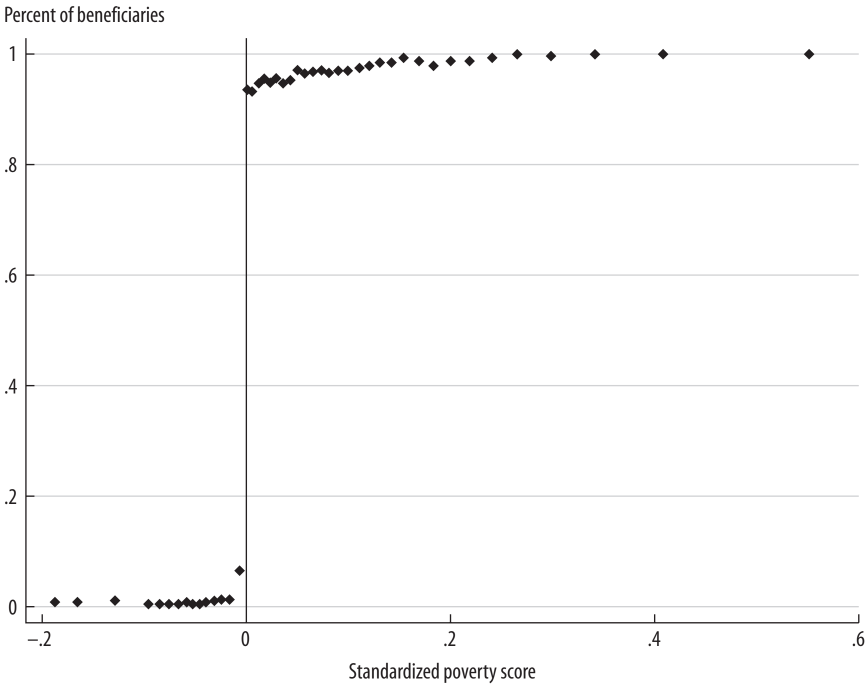
Discontinuity Regression

As described earlier, assignment of applicant households to the PANES program was done on the basis of a predicted poverty score that depended only on household socioeconomic characteristics collected in a baseline survey and an income threshold that was drawn from social security records. Households that were eligible on the basis of income were visited, and only those with a predicted poverty score above a predetermined threshold were assigned to the program.

Evidence from previous work on PANES (Manacorda, Miguel, and Vigorito 2011) shows almost perfect compliance with the intended assignment rule. Figure 1 reports the proportion of households that had ever enrolled in PANES as a function of the standardized score (based on official PANES data), making it clear that implementation of PANES was remarkably well targeted. Using the McCrary (2008) methodology, Manacorda, Miguel, and Vigorito (2011) show that the score is smoothly distributed in the vicinity of the discontinuity threshold, which suggests a general absence of manipulation. This design provides a credible quasi-experimental variation in assignment to the program that lends itself naturally to a sharp regression discontinuity approach.

To operationalize the regression discontinuity design approach, let S_i be the predicted poverty score assigned to household i (where a higher score denotes higher predicted poverty) and let E denote the eligibility threshold, such that in principle only households with scores above E are eligible for treatment. Let $N_i = S_i - E$ be the normalized poverty score. Following Lee and Card (2008), we propose to regress the variable of interest for household i, y_i,

FIGURE 1. PANES Eligibility and Participation, Uruguay, 2005–06



Source: Data from official PANES administrative records.

on a constant, an indicator for households above the threshold $1(N_i > 0)$, and two parametric polynomials, $f(N_i)$ and $g(N_i)$, for the normalized score. This is done on each side of the threshold, such that $f(0) = g(0) = 0$:

$$(1) \qquad y_i = b_0 + b_1 1(N_i > 0) + f(N_i) + 1(N_i > 0)\, g(N_i) + X'g + u_i,$$

where X represents additional covariates. The identification assumption for RDD requires that outcome variables be monotonic functions of the predicted poverty score with the exception that the treatment has an additional effect (see, for example, Imbens and Lemieux 2008). The analysis of baseline data for households included in the follow-up survey indicates that no discontinuity was present before treatment, which means that the RDD assumptions hold.

In a case where the RDD assumptions hold, the potential discontinuity in the outcome variables in the vicinity of the discontinuity point can thus be legitimately interpreted as a program effect. The impact of the program will be then captured by b_1, the change in y at the eligibility threshold.

One drawback of RDD is that its determination of local average treatment effects in the vicinity of the discontinuity point cannot necessarily be generalized across program beneficiaries as a whole in cases where heterogeneous effects are present.

Difference in Differences

For all the outcomes included in this study, we have data for treatment and control groups before the program was implemented as well as on the two follow-up survey waves. As mentioned earlier, these data overrepresent households around a 2 percent interval of the eligibility threshold.

The availability of panel data allows us to use a difference-in-differences estimation, also known as the double-difference method. This method essentially compares changes in the situations of treatment and control groups relative to their observed outcome at a preintervention baseline. The method assumes that unobserved heterogeneity does not vary over time, so any potential biases from unobserved heterogeneity cancel each other out when looking at the difference in the change between groups. This is known as the parallel trend assumption, which means that unobserved characteristics that affect program participation do not vary over time with treatment status.

Considering two periods, $t = 0$ before the program and $t = 1$ after the program begins, and outcomes Y_t^T and Y_t^C for the treatment and control groups, the double-difference method (DD) estimates the average program impact as

$$DD = E\left(Y_1^T - Y_0^T \middle| T_1 = 1\right) - E\left(Y_1^C - Y_0^C \middle| T_1 = 0\right),$$

where $T_1 = 1$ indicates that the program was active at time $t = 1$ and $T_1 = 0$ denotes lack of treatment at time $t = 1$. In this formulation, the effect of the program is calculated as the difference between the differences in the observed outcomes for the treatment and control groups before and after the intervention. The DD estimate can also be calculated using a regression framework. In this case the equation can be specified as

$$Y_{it} = \alpha + \beta T_{i1} t + \rho T_{i1} + \gamma t + \varepsilon_{it},$$

where $T_1 = 1$ indicates that the program was active at time $t = 1$ and $T_1 = 0$ denotes lack of treatment at time $t = 1$. The coefficient β, corresponding to the interaction between the treatment variable and the time variable, gives the average DD effect of the program. For the DD estimator to be interpreted correctly, the error term must be uncorrelated with the other variables in the equation, and specifically it must hold that

$$Cov(\varepsilon_{it}, T_{i1}t) = 0.$$

The regression version of the DD estimator can include covariates (X), but two factors must be taken into account. Although the only helpful strategy would be to include time-varying Xs, these may be affected by the treatment, introducing endogeneity. These aspects must be taken into account when introducing the covariates, as seen in the equation

$$Y_{it} = \alpha + \beta T_{i1}t + \rho T_{i1} + \gamma t + \phi X_i + \varepsilon_{it}.$$

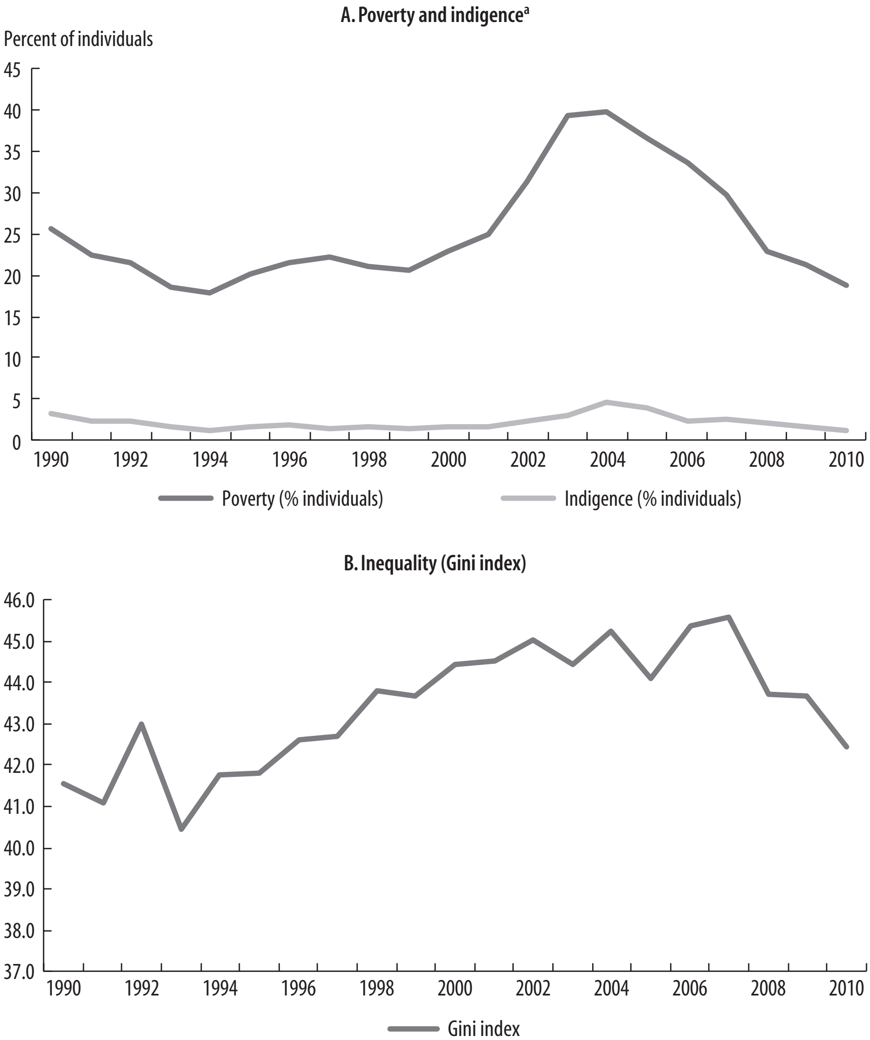
Finally, we combine the two methodologies by using the regression discontinuity polynomials interacted with time as a set of control variables in the difference-in-differences regression:

$$Y_{it} = \alpha + \beta T_{i1}t + \rho T_{i1} + f(Ni)t + 1(Ni > 0)g(Ni)t + \gamma t + \phi X_i + \varepsilon_{it}.$$

Poverty, Inequality, School Attendance, and Child Labor in Uruguay

Uruguay is a small, middle-income Latin American country. Its poverty and inequality indexes are among the lowest in the region, and the annual per capita income, adjusted for purchasing power parity, is currently just below US$10,000. Nevertheless, an increasing trend in the incidence of indigence and poverty between 1994 and 2005 has been documented in many studies (Amarante, Arim, and Vigorito 2005 and UNDP 2008, among others), as has the trend of increasing income inequality till 2007 (UNDP 2008; Alves and others 2011). These trends are shown in the two panels of figure 2. The underlying causes of this erosion of household well-being mostly have to do with changing labor market performance, the severe 2002 economic crisis, the small amount of public transfers to poor households, and the fact that, until 2004, the social security system was largely focused on transfers to the

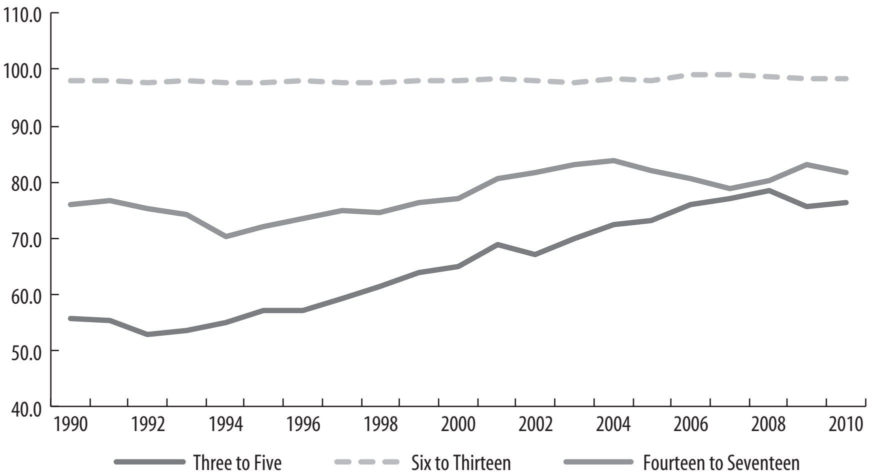
FIGURE 2. Income Poverty, Indigence, and Inequality, Uruguay, 1990–2010[a]



Source: Data from household surveys.

a. Per capita household income adjusted by retail price index (December 2006 = 100). Poverty level defined by the Uruguay National Institute of Statistics, 2006.

FIGURE 3. School Attendance, by Age and Household Income, Uruguay, 1990–2009[a]



Source: Data from official PANES administrative records.
a. Per capita household income adjusted by retail price index (December 2006 = 100).

elderly. At the beginning of PANES in 2005, the poverty incidence reached 29 percent of the total population and was 49.4 percent for children from birth to seventeen.[16]

The Uruguayan educational system is organized into three main levels: three years of preprimary school starting at the age of three followed by six years of primary education and six years of secondary education. In 2005 attendance was compulsory for children from the age of five until completion of the third year of secondary school.[17] Attendance rates for four- and five-year-old children have increased significantly over the past decade as a result of the education system reform carried out in the mid-1990s. Meanwhile, primary school attendance has been almost universal since the early decades of the twentieth century and has long held steady (figure 3). The main problems at the primary level are grade repetition and absenteeism (UNDP 2008).

The main failure of the Uruguayan educational system is located at the secondary level, where drop-out rates have held steady since the 1980s. As

16. See table A.1 in the appendix.

17. Since January 2009 a new education law has set schooling as compulsory from the age of four until completion of the sixth year of secondary school. During PANES, compulsory education started at five years old and extended through the first three years of secondary school.

TABLE 1. School Attendance and Child Labor among Children Aged Fourteen to Seventeen, by Income Group, Uruguay, 2006
Percent

	School and work participation				
Income group	*Attends school*	*Attends school and works*	*Only works*	*Neither attends school nor works*	*Total*
Indigent households	52	5	9	34	100
Poor households	62	5	9	24	100
All households	75	3	6	16	100

Source: Data from household surveys.

a result, the average number of years of schooling among adults has grown slowly in recent decades (reaching 8.6 years in 2008), but Uruguay's early achievements in this respect have been surpassed by other Latin American countries (UNDP 2008). Dropouts are mainly concentrated in the lower income strata, and boys both number heavily among this group and have a higher labor market participation rate (Bucheli and Casacuberta 2000; UNDP 2008).[18] The reasons for these high school drop-out rates have not been clearly established in the existing literature, which shows a high correlation between dropping out of school and experiencing income shortages and poor socio-economic conditions.

The quality of education provided at secondary school is also an issue of present concern. Although Uruguay performed regionally well on the standard PISA (Program for International Student Assessment) assessments (2003, 2006, 2009), a significant proportion of teenagers do not meet minimum competency requirements.

Regarding child labor, Uruguay has ratified international agreements (the International Labor Organization's conventions 138 and 182). The minimum legal age to work is fifteen, and children aged fifteen to eighteen who want to work must have special approval from the authorities. Working conditions are the main criteria considered to get this permission. Information from household surveys indicates that the share of idle children (not working or attending school) among fourteen- to seventeen-year-olds is surprisingly high: it reaches 24 percent among poor households and 34 percent among indigent ones (table 1). The percentage of children who are only working at this age is relatively low. Some of these apparently idle children may in fact be engaged in domestic chores.

18. During the crisis, the secondary school attendance rate grew.

TABLE 2. Main Outcomes among PANES Applicants, Baseline Basic Statistics, 2005–06
Percent

	Outcome	*Beneficiaries*	*Unsuccessful applicants*	*All*
Children[a]	School attendance	78.97	79.09	79.05
	Child labor	11.14	15.34	12.07
Adults[b]	Labor force participation	50.65	46.46	49.2
	Employment	37.67	36.22	37.17
	Unemployment	12.98	10.24	12.03
	Inactivity	49.35	53.54	50.8
	Total	100.0	100.0	100.0
Households	Average real labor income per household[c]	1,240	1,320	1,267
	Average real per capita household income[c]	603	742	648

Source: Data from PANES administrative records.
a. Aged fourteen to seventeen.
b. Aged twenty and above.
c. Uruguayan pesos, constant prices (April 2007 = 100).

Our data show that school enrollment was initially lower among PANES beneficiaries than among unsuccessful applicants for the group aged fourteen to seventeen. At the baseline, the labor market participation rate among PANES beneficiaries was higher than among nonbeneficiaries, and, conversely, inactivity was slightly higher among nonbeneficiaries (table 2). The differences mainly arise from unemployment. In these calculations, as in the rest of this paper, employment and unemployment rates are calculated in terms of the whole population considered (in this case, adults aged twenty and older), so they add up to the participation rate. As expected, labor income and per capita household income were lower among beneficiaries.

Main Results

In what follows we explore the effect of PANES on school attendance and child labor. After that, we focus on two explanatory channels: adult labor supply and household income. We also analyze whether awareness of the conditionalities played a role in fostering school attendance.

Child Outcomes: Schooling and Child Labor

As previously stated, we use RDD and DD analysis to evaluate whether the program affected school attendance. We also explored impacts on years of

TABLE 3. Effects on School Attendance and Child Labor among Children Aged Fourteen to Seventeen, Uruguay[a]

	Regression discontinuity design (RDD)			*Difference in differences (DD)*	
Wave and outcome	*Linear specification*	*Quadratic specification*	*Quadratic specification with control variables*[b]	*Individual fixed effects*	*DD with RDD polynomial*[c]
First-wave survey					
School attendance	0.0543	0.0543	0.161	0.0536	0.05176
	(0.0824)	(0.0824)	(0.133)	(0.0438)	(0.0727)
N	726	726	726	726	726
Child labor	0.0441	0.0061	−0.0189	−0.0081	−0.0041
	(0.0527)	(0.0769)	(0.0943)	(0.0411)	(0.0811)
N	726	726	726	726	726
Second-wave survey					
School attendance	0.115	−0.0145	−0.004	0.0585	0.0175
	(0.0883)	(0.0802)	(0.0696)	(0.0440)	(0.1100)
N	768	768	768	768	768
Child labor	−0.0109	0.0444	0.0316	0.0140	0.1440
	(0.0513)	(0.0840)	(0.0775)	(0.0426)	(0.1021)
N	768	768	768	768	768

Source: Data from PANES administrative records and follow-up surveys.

a. Marginal effects coefficient and standard errors of the treatment variable. Robust standard errors in parentheses.

b Control variables: age, sex, region, attending primary school dummy, attending secondary school dummy.

c. Control variables: age, sex.

***Significant at 1 percent level.

**Significant at 5 percent level.

*Significant at 10 percent level.

schooling under the RDD strategy, as well as effects on child labor. Under RDD, we consider three alternative specifications. The first includes the treatment variable and is a linear function of the normalized score, the second includes a quadratic polynomial, and the third augments the second specification with a set of covariates: sex and age of the child, region of residence, housing characteristics (flooring and ceiling materials), and household head attributes (sex, age, and education). These three specifications are reported in the first three columns of table 3. In the DD specifications, we report results for individual fixed effects and estimations using a polynomial on the poverty score interacted with time as a group of control variables (the last two columns in table 3). Covariates that vary with time are excluded to avoid endogeneity.

Although the two identification strategies are concentrated in the 2 percent interval of the eligibility score, in each of them we choose different sets of children. Whereas RDD selects children who were fourteen to seventeen years old at the time of the follow-up surveys, the difference-in-differences approach selects children who were fourteen to seventeen years old at the baseline, so they were approximately sixteen to nineteen years old when the second wave of the survey was gathered.

We were unable to identify any impact on school attendance in the age group under study under either RDD or DD. The coefficients of the treatment variables (marginal effects) for the two waves are reported in table 3. An alternative measure of education, namely, years of education, also reflects no impact of the intervention.[19] Similar results on school attendance, based on slightly different equation specifications, are found in related work by Ferrando (2012). Merging program data with the administrative records of the educational system, Ferrando considers whether the intervention had any impact on school outcomes for those who were enrolled at school. There is evidence of a reduction in unjustified absences as a result of the program and no impact on justified absences.[20] This reduction in unjustified absences does not imply a reduction in repetition rates (as might have been the case if it implied higher compliance with minimum requirements on attendance for promotion) nor any change on average qualification per grade.

We also estimated PANES's impact on child labor for those aged fourteen to seventeen. Again, no significant effect is found in any specification (table 3) or in the graphical analysis.[21]

Heterogeneous Effects

We explored whether the absence of significant results at the aggregate level hid different effects for different groups. Owing to sample size we were not able to open the estimations by single ages, so we split our group of interest into two subgroups. We found that the lack of impact on school attendance persists when children are disaggregated by age or sex (table 4).[22] Unfortunately, our sample size does not allow us to carry out the same analysis in the case of child labor.

19. Table A.2 in the appendix.

20. Unjustified absences were reduced by roughly 0.5 days a month. This magnitude is reported to be similar to the one found for PATH in Jamaica by Levy and Ohls (2007).

21. Graphs showing RDD results for all tables are available from the authors on request.

22. We tried different age groupings, but the results were no different. We have also analyzed impacts by household size. Detailed results are available on request.

TABLE 4. Effect on School Attendance among Children Aged Fourteen to Seventeen, Uruguay[a]

	Regression discontinuity design (RDD)			*Difference in differences (DD)*	
	Linear specification	*Quadratic specification*	*Quadratic specification with control variables*[b]	*Individual fixed effects*	*DD with RDD polynomial*[c]
First-wave survey					
Fourteen- to fifteen-year-olds	0.0376	−0.0378	−0.0485	0.0096	0.0882
	(0.104)	(0.158)	(0.161)	(0.048)	(0.050)*
N	395	395	395	395	395
Sixteen- to seventeen-year-olds	0.0741	0.0328	0.0266	−0.0092	−0.0079
	(0.1100)	(0.1670)	(0.0154)*	(0.0601)	(0.0610)
N	381	381	381	381	381
Girls	0.115	0.355	0.040	−0.030	−0.033
	(0.1200)	(0.1860)*	(0.1790)	(0.0560)	(0.0592)
N	374	374	374	374	374
Boys	0.0379	0.0995	−0.0467	0.0161	0.0015
	(0.1180)	(0.1920)	(0.1520)	(0.0550)	(0.0056)*
N	347	347	347	347	347
Second-wave survey					
Fourteen- to fifteen-year-olds	0.0141	0.1100	0.1070	0.0122	0.0113
	(0.8043)	(0.1500)	(0.1320)	(0.0558)*	(0.0567)*
N	395	395	395	395	395
Sixteen- to seventeen-year-olds	0.0356	0.0806	0.0113	0.0248	0.0240
	(0.1220)	(0.2020)	(0.1800)	(0.0641)	(0.0655)
N	381	381	381	381	381
Girls	0.1310	0.1770	0.1690	−0.0297	−0.0315
	(0.1030)	(0.1670)	(0.1510)	(0.0611)	(0.0637)
N	435	435	435	435	435
Boys	0.0291	−0.0685	−0.0469	0.0161	0.0151
	(0.1010)	(0.1680)	(0.1420)	(0.0630)	(0.0636)
N	392	392	392	392	392

Source: Data from PANES administrative records and follow-up surveys.

a. Marginal effects coefficient and standard errors of the treatment variable. Robust standard errors in parentheses.

b. Control variables: age, sex, region, attending primary school dummy, attending secondary school dummy.

c. Control variables: age, sex.

***Significant at 1 percent level.

**Significant at 5 percent level.

*Significant at 10 percent level.

TABLE 5. Effects on School Attendance among Children Aged Fourteen to Seventeen, by Household Size, Uruguay[a]

	Regression discontinuity design (RDD)			*Difference in differences (DD)*	
	Linear specification	*Quadratic specification*	*Quadratic specification with control variables[b]*	*Individual fixed effects*	*DD with RD polynomial[c]*
First-wave survey					
One child	0.1790	0.2240	0.1980	0.0944	0.2970
	(0.1390)	(0.2140)	(0.1960)	(0.0668)	(0.1350)**
N	180	180	180	180	180
Two children	0.01360	0.3000	0.2180	−0.0335	−0.1100
	(0.1370)	(0.2150)	(0.2200)	(0.0596)	(0.1660)
N	251	251	251	251	251
Three children and more	0.0120	0.0497	0.1190	−0.0474	−0.1560
	(0.1430)	(0.2340)	(0.2700)	(0.0564)	(0.1110)
N	386	386	386	386	386
Second-wave survey					
One child	−0.0247	−0.0651	0.0684	−0.1130	−0.4600
	(0.1440)	(0.2850)	(0.2050)	(0.0816)	(0.1970)**
N	412	412	412	412	412
Two children	0.1380	0.0581	0.0144	0.1080	0.2870
	(0.1220)	(0.2130)	(0.1530)	(0.0726)	(0.2470)
N	468	468	468	468	468
Three children and more	0.1020	0.0932	0.0956	0.1620	0.1820
	(0.1080)	(0.1600)	(0.1500)	(0.0711)**	(0.1690)
N	568	568	568	568	568

Source: Data from PANES administrative records and follow-up surveys.
a. Marginal effects coefficient and standard errors of the treatment variable. Robust standard errors in parentheses.
b. Control variables: age, sex, region.
c. Control variables: age, sex.
***Significant at 1 percent level.
**Significant at 5 percent level.
*Significant at 10 percent level.

Given that the transfer was a plain amount, independent of household size, we also explored the potential presence of heterogeneous effects by household size. For those households that had children in our age group of interest, we ran separate regressions considering children aged fourteen to seventeen in households with one child, with two children aged birth to eighteen, and with three or more children aged birth to eighteen. Again, we were not able to identify any significant result (table 5).

Although the program had some potential to increase school enrollment given the initial conditions, this result was not achieved. This lack of improvement can be linked to the paucity of incentives, given the size of the plain transfer relative to household income, potential substitution effects that may have inhibited an increase in household income, or lack of monitoring of the conditionalities. In the following section, we investigate the last two potential channels.

Adult Labor Market Participation and Household Income

To carry out the analysis of the income channel, we analyzed impacts on labor market decisions of adults, considering five outcome variables: participation rates, unemployment, employment, hours of work, and labor income. Participation, unemployment, and employment were defined in relation to the working-age population (aged fourteen and older). Hence unemployment and employment add up to participation rates.

We restricted our sample to adults aged twenty and older in those households that had children in the age group under study. Again, we ran three RDD specifications: the first is linear and uses the treatment variable and the normalized poverty score, the second combines the score with a quadratic term, and the third includes a set of individual control variables (age, sex, and region of residence) and housing characteristics (flooring and ceiling materials). In the DD specifications, we again report results for individual fixed effects and with a polynomial on the poverty score interacted with time as a group of control variables.

No significant effects were found for labor force participation, unemployment, or the number of hours worked, and this holds whether considering all adults aged twenty and older within a beneficiary household (table 6) or PANES applicants only (table 7). Program applicants are in most cases the parents of the children under study. Similar results are found when the whole sample is considered and observations are not restricted to households with children aged fourteen to seventeen.[23]

Our results indicate that the program had no effect on personal labor income (considering adults aged twenty and older): no significant difference was found between beneficiaries and nonbeneficiaries in the vicinity of the threshold (table 8). At the same time, no discontinuity was found when total household income (in per capita terms) of beneficiaries and nonbeneficiaries

23. Tables A.3 and A.4 in the appendix.

TABLE 6. Effects on Adult Labor-Market Participation among PANES Beneficiaries in Households with Children Aged Fourteen to Seventeen, Uruguay[a]

	Regression discontinuity design (RDD)			*Difference in differences (DD)*	
Population and variable	*Linear specification*	*Quadratic specification*	*Quadratic specification with control variables[b]*	*Individual fixed effects*	*DD with RD polynomial[c]*
First-wave survey					
Labor market participation	−0.0074	−0.0152	−0.0326	0.0127	0.0062
	(0.0447)	(0.0695)	(0.0796)	(0.0291)	(0.0277)
N	2,628	2,628	2,628	2,628	2,628
Unemployment	−0.0504	−0.0476	−0.0323	−0.0214	−0.00795
	(0.0304)*	(0.0535)	(0.0597)	(0.0303)	(0.0233)
N	2,628	2,628	2,628	2,628	2,628
Employment	0.0430	0.0323	−0.000317	0.0342	0.0141
	(0.0391)	(0.0540)	(0.0707)	(0.0359)	(0.0293)
N	2,628	2,628	2,628	2,628	2,628
Hours of work	−1.861	−2.892	−1.715	−1.892	−1.411
	(1.689)	(2.637)	(2.851)	(1.205)	(1.073)
N	686	686	686	686	686
Second-wave survey					
Labor market participation	0.0953	0.0877	0.0954	0.0308	0.00446
	(0.0613)	(0.0962)	(0.0854)	(0.0338)	(0.0333)
N	2,818	2,818	2,818	2,818	2,818
Unemployment	0.0405	0.0516	0.0672	−0.0384	−0.0341
	(0.0319)	(0.0463)	(0.0525)	(0.0311)	(0.0251)
N	2,818	2,818	2,818	2,818	2,818
Employment	0.0548	0.0361	0.0282	0.0693	0.0386
	(0.0626)	(0.100)	(0.0997)	(0.0371)*	(0.0331)
N	2,818	2,818	2,818	2,818	2,818
Hours of work	0.631	−3.744	−4.340	−1.687	−3.208
	(3.340)	(5.344)	(5.727)	(2.344)	(2.251)
N	694	694	694	694	694

Source: Data from PANES administrative records and follow-up surveys.

a. Marginal effects coefficient and standard errors of the treatment variable. Robust standard errors in parentheses.

b. Control variables: age, sex, region, years of education, flooring and ceiling materials.

c. Control variables: age, sex.

***Significant at 1 percent level.

**Significant at 5 percent level.

*Significant at 10 percent level.

TABLE 7. Effects on Adult Labor-Market Participation among PANES Applicants in Households with Children aged Fourteen to Seventeen, Uruguay[a]

	Regression discontinuity design (RDD)			*Difference in differences (DD)*	
Population and variable	*Linear specification*	*Quadratic specification*	*Quadratic specification with control variables[b]*	*Individual fixed effects*	*DD with RD polynomial[c]*
First-wave survey					
Labor market participation	0.0873	–0.0403	–0.0292	0.0268	0.0294
	(0.0785)	(0.1260)	(0.1210)	(0.0384)	(0.0383)
N	736	736	736	736	736
Unemployment	–0.0111	–0.0104	–0.0105	–0.0497	–0.0454
	(0.0053)**	(0.0782)	(0.0780)	(0.0404)	(0.0408)
N	736	736	736	736	736
Employment	0.0984	–0.0299	–0.0188	0.0766	0.0748
	(0.0841)	(0.1320)	(0.1310)	(0.0474)	(0.0476)
N	736	736	736	736	736
Hours of work	1.442	–4.379	–2.776	–1.074	0.734
	(4.130)	(6.283)	(6.235)	(3.137)	(2.862)
N	374	374	374	374	374
Second-wave survey					
Labor market participation	0.0159	0.1010	0.0873	–0.0088	–0.0067
	(0.0778)	(0.1220)	(0.1180)	(0.0430)	(0.0430)
N	673	673	673	673	673
Unemployment	0.0554	0.0387	0.0300	–0.0676	–0.0660
	(0.0476)	(0.0708)	(0.0699)	(0.0413)	(0.0415)
N	673	673	673	673	673
Employment	0.1030	0.0620	0.0573	0.0588	0.0593
	(0.0826)	(0.1290)	(0.1270)	(0.0482)	(0.0481)
N	673	673	673	673	673
Hours of work	–0.225	–6.621	–2.972	–1.674	–0.693
	(2.924)	(4.563)	(5.043)	(2.452)	(2.238)
N	403	403	403	403	403

Source: Data from PANES administrative records and follow-up surveys.

a. Marginal effects coefficient and standard errors of the treatment variable. Robust standard errors in parentheses.

b. Control variables: age, sex, region, years of education, flooring and ceiling materials.

c. Control variables: age, sex.

***Significant at 1 percent level.

**Significant at 5 percent level.

*Significant at 10 percent level.

TABLE 8. Effects on Personal Labor Income and Total Household Income among Adults in Households with Children Aged Fourteen to Seventeen, Uruguay[a]

	Regression discontinuity design (RDD)			*Difference in differences (DD)*	
Population	*Linear specification*	*Quadratic specification*	*Quadratic specification with control variable*	*Individual fixed effects*	*DD with RD polynomial*
First-wave survey					
Personal labor income	−113.50	−356.70	−530.10[b]	−94.42	−91.05[c]
	(126.80)	(218.00)	(273.30)*	(52.96)*	(55.96)
N	1,139	1,139	1,139	1,139	1,139
Household income (per capita)	−164.10	−341.70	−309.30[d]	764.20	754.50[e]
	(123.20)	(207.30)*	(189.30)	(23.05)***	(50.61)***
N	3,543	3,543	3,543	3,543	3,543
Second-wave survey					
Personal labor income	−155.20	−391.60	−331.50[b]	−49.67	−45.02[c]
	(271.10)	(450.80)	(435.60)	(36.45)	(34.31)
N	1,460	1,460	1,460	1,460	1,460
Household income (per capita)	−21.47	−277.10	−337.80[d]	−103.30	−72.67[e]
	(130.20)	(202.00)	(199.70)*	(22.47)***	(36.84)**
N	3,326	3,326	3,326	3,326	3,326

Source: Data from PANES administrative records and follow-up surveys.
a. Marginal effects coefficient and standard errors of the treatment variable. Robust standard errors in parentheses.
b. Control variables: age, sex, region, years of education.
c. Control variables: age, sex, region, years of education, flooring and ceiling materials.
d. Control variables: region, household size, age of household head.
e. Control variables: household size, age of household head.
***Significant at 1 percent level.
**Significant at 5 percent level.
*Significant at 10 percent level.

was compared. This result allows us to reject the presence of a substitution effect among beneficiary households, although a related study that uses social security and program microdata (Amarante and others 2011), shows that PANES reduced formal employment and earnings, probably owing to the income eligibility threshold. Hence the previously reported lack of effect on schooling cannot be attributed to beneficiary households having substituted some share of the transfer for their previous income once they began to receive the transfer.

The above analysis shows that households did not engage in strategic behavior as a consequence of receiving the transfer. Total household income and labor income do not show any discontinuity or any difference in the change between beneficiaries and nonbeneficiaries. If no potential substitution effect took place, then the lack of impact on schooling must be explainable by other factors.

The Role of Conditionalities

One explanation of how cash transfer programs positively impact outcomes such as school attendance considers the conditions that may compel households to behave differently, particularly in relation to their demand for education and health services (Skoufias and Parker 2001; De Brauw and Hoddinott 2008; Schady and Araujo 2008). The imposition of conditionalities is, however, by no means uncontroversial: some authors argue that the conditions are inherently paternalistic and assume that parents either do not know what is in the best interests of their children or are irrational. It has also proved difficult to monitor conditionalities; moreover, effective monitoring may carry regressive effects as dropout rates are higher among more vulnerable households. It has also been argued that conditionalities imply direct costs that are typically assumed by mothers (Molyneux 2008). On the positive side, conditionalities may favor middle- and upper-class opinion about cash transfer programs. Evidence about the effects of imposing conditions on beneficiaries is still scarce, largely owing to methodological challenges involved in isolating their effects, as discussed earlier in this paper.

In the first follow-up survey, beneficiaries were asked to specify conditions (if any) that must be met for them to receive the transfer. This information indicates whether the respondent (usually the PANES beneficiary) knew about the existence of conditionalities and allows us to test whether benefits that are conditioned have a different impact on children's school attendance from that of unconditioned benefits. In this case, the analysis is restricted to beneficiary households that were asked about their knowledge of conditionalities.

Twenty percent of survey respondents were aware of the school enrollment requirement for children aged six to seventeen. A simple probit estimation shows that knowledge of conditionalities is significantly related to the educational level of the household head.[24]

Beneficiary children aged fourteen to seventeen who belonged to households in which the respondent was aware of conditionalities have a higher

24. Table A.5 in the appendix.

TABLE 9. Effect of Awareness of Conditionality on School Enrollment among Beneficiary Households with Children Aged Fourteen to Seventeen, with and without Controls, Uruguay[a]

	First-wave survey		*Second-wave survey*	
Control	*Without controls*	*With controls*	*Without controls*	*With controls*
Awareness of conditionality	0.0706	0.0625	0.0962	0.0340
	(0.0460)	(0.0474)	(0.0509)*	(0.0570)
Poverty score	6.757	12.880	23.010	27.660
	(13.800)	(14.090)	(15.210)	(18.520)
Poverty score (cuad)	–417.2	–725.2	–1,086.0	–1,286.0
	(656.9)	(668.7)	(722.5)	(863.7)
Age		–0.0922		–0.1320
		(0.0186)***		(0.0334)***
Sex		0.0807		0.1460
		(0.0402)**		(0.0489)***
Education		0.0097		0.0288
		(0.00592)*		(0.00842)***
Region		–0.0275		–0.0181
		(0.0564)		(0.0653)
N	587	585	542	464

Source: Data from first- and second-wave surveys.
a. Marginal effects. Robust standard errors in parentheses.
***Significant at 1 percent level.
**Significant at 5 percent level.
*Significant at 10 percent level.

probability of attending school than beneficiary children of the same age in other households, for both waves of the follow-up survey, but the effect is not significant (columns 1 and 3, table 9). If a set of control variables is introduced, including education of the household head, this positive correlation between awareness of conditionalities and school attendance persists, but again it lacks statistical significance (columns 2 and 4). Our results thus do not indicate that awareness of conditionalities robustly affects school attendance among beneficiaries. There is a positive association, but it is very imprecisely estimated.

A second strategy to explore the role of conditionalities consists in comparing the control group with those households among the treatment group that declared awareness of conditionalities (table 10). Regression discontinuity estimations on these groups show a similar picture: the treatment variable is positive but never significant. There are no differences in school attendance between PANES beneficiaries who were aware of conditionalities and the control group.

TABLE 10. Effects of Awareness of Conditionality on School Enrollment among Children Aged Fourteen to Seventeen, Aware Treatment Group versus Control Group, Uruguay[a]

	First-wave survey		*Second-wave survey*	
	Quadratic specification	*Quadratic specification with control variables*	*Quadratic specification*	*Quadratic specification with control variables*
Treatment	0.1550 (0.0958)	0.1510 (0.0994)	0.0382 (0.0842)	0.0448 (0.0825)
N	411	411	403	403

Source: Data from first and second follow-up survey.
a. Marginal effects of treatment variable. Robust standard errors in parentheses.

The results presented in this section are illustrative but present severe caveats that need to be resolved in future research. The main one is the lack of a clear identification strategy. At the same time, sample sizes do not allow for precise estimations of these effects for the age group under study.

Discussion and Final Comments

In this paper we analyze the effects of a cash transfer program on teenage labor and school attendance and three potential channels that have been established in the existing literature: labor market outcomes, income, and conditionalities. We were not able to identify any effect on school attendance or child labor for children aged fourteen to seventeen as a whole or when disaggregating by specific subgroups. The absence of effects is not related to substitution effects led by variations in labor market participation, personal labor income, or household labor income.

In the literature on cash transfers and schooling, results vary considerably across countries, depending basically on the baseline enrollment rates, design issues, and the amount of the transfer. With regard to enrollment conditions, high school dropout rates at the secondary school level in Uruguay indicate that there was a scope for improvement, as intended by the intervention, although our results indicate that this could not be reached. However, given that the initial attendance rates were higher than the average in other countries (79 versus 68 percent), finding large effects was less likely than in other contexts.

The amount of PANES transfer was significant, especially when compared with other interventions. Conditional cash transfers represented 6.1 percent of pretransfer consumption in Brazil, 17.0 percent in Colombia, 6.0 percent in Ecuador, 7.0 percent in Honduras, 8.2 percent in Jamaica, 21.8 percent in Mexico, and 29.3 percent in Nicaragua (Fiszbein and Schady 2009). In all these cases, positive effects on enrollment rates were found (Bouillon and Tejerina 2007). In the Uruguayan case, the transfer represented around 25 percent of preintervention reported income, according to the household surveys data. So the amount of the transfer was not negligible, but still we were not able to identify significant effects.

The literature has noted that the effectiveness of transfers is increased when supply-side interventions are carried out. Although this was not the case in PANES, school infrastructure can be considered adequate to respond to a potentially higher demand for services, so these factors are again not likely to explain our results.

We believe that our results can be explained, to some extent, by design factors, as the transfer was a lump sum independent of the number of children in the household. This design was probably not suitable for the needs of secondary school-age students who had dropped out of school. Most of the examples of conditional cash transfers that impacted positively on school enrollment were based on transfers dependent on the number of children in the household and, in some cases, on their educational level. That the transfer was a flat sum across all the educational levels may also explain our results. As an example, in PROGRESA the transfer increases according to school grade and gender, whereas in the Colombian Familias en Acción, it varies for primary and secondary cycles. In both cases positive schooling results are found. Other potentially nonsuitable aspects of the design of the program may be related to the schedule of payments. Recent research points out that monthly payments, such as the one undertaken by PANES, are less effective than other schemes (Barrera-Osorio and others 2011; Saavedra and García 2012).

The existence and implementation of conditionalities is another relevant issue that may help explain our results. Saavedra and García (2012), in their systematic review for developing countries, show that the existence of conditionalities has been found to increase the effects of the transfers and that setting conditionalities on performance rather than on attendance leads to better results. In the case of PANES, conditionalities were not controlled. We present weak evidence that those households that were aware of their existence did not perform better. But we cannot rule out the possibility that our results might be also associated with the lack of enforcement of conditionalities.

Finally, a relevant aspect that should be taken into account when discussing our results is that, as highlighted by the recent literature of the determinants of schooling, there may be very high costs of reentering school once dropout takes place, which are probably affected by income. If income is not the main variable affecting schooling decisions among teenagers, complementary interventions are needed to foster school attendance in this age group. Cash transfers on their own, with unenforced conditionalities and independent of household size and educational grades, might not be enough to generate favorable side effects such as increases in school attendance among teenagers. These aspects are relevant for the design of future policies in Uruguay and in those middle-income countries in which efforts to foster educational attainment in the population face constraints that are clearly located in the expansion of attendance in secondary schooling. A final aspect is that the extent to which money given to parents is translated into school attendance might be related to the decision process within households, an issue that needs to be further explored.

Appendix

TABLE A.1. Poverty Incidence, by Age Group, 1990–2008[a]
Percent

	Age						
	Child				Adult		
Year	Birth to five	Six to twelve	Thirteen to seventeen	All children	Eighteen to sixty-four	Sixty-five and older	All adults
1990	49.6	46.7	41.6	46.0	24.3	15.0	29.6
1991	41.1	39.8	33.0	38.0	19.1	9.7	23.3
1992	37.8	36.6	29.5	34.6	16.1	6.7	20.2
1993	32.5	31.2	26.7	30.1	13.4	5.5	16.9
1994	30.5	28.6	24.0	27.7	11.9	4.1	15.1
1995	34.3	32.1	25.9	30.9	14.0	5.0	17.3
1996	35.3	31.8	25.6	31.0	13.6	4.8	17.0
1997	36.1	30.3	25.6	30.7	14.1	4.8	17.1
1998	34.7	29.2	26.7	30.1	13.1	4.1	16.7
1999	32.9	29.2	23.4	28.4	12.4	3.4	15.7
2000	37.7	32.0	25.9	31.7	14.4	3.8	17.8
2001	38.3	35.4	27.7	34.0	15.3	3.9	18.8
2002	46.5	41.9	34.6	41.1	20.3	5.4	23.6
2003	56.5	50.2	42.8	49.8	27.8	9.7	30.9
2004	56.5	53.7	45.0	51.9	28.7	10.8	32.1
2005	54.1	51.0	42.8	49.4	25.8	9.2	29.4
2006	48.6	47.6	40.0	45.6	22.6	7.7	26.8
2007	46.4	46.5	39.7	44.5	21.3	6.9	25.8
2008	38.4	36.8	32.1	35.8	17.1	6.0	20.6

Source: Based on household surveys.
a. Authors' calculation of income poverty, based on the official poverty line from Instituto Nacional de Estadística de Uruguay.

TABLE A.2. Effects on Years of Schooling, Children Aged Fourteen to Seventeen[a]

	RDD			DD	
Wave	Linear specification	Quadratic specification	Quadratic specification with control variables	Individual fixed effects	DD with RD polynomial
First	0.120	0.332	–0.450	0.0721	0.0303
	(0.328)	(0.537)	(0.423)	(0.135)	(0.127)
Second	0.340	0.389	0.265	0.125	0.0281
	(0.233)	(0.314)	(0.261)	(0.153)	(0.155)

Source: Based on *PANES* administrative records and the two waves of the follow-up survey.
a. Marginal effects coefficient and standard deviation of the treatment variable. Robust standard errors in parentheses.

TABLE A.3. Effects on Labor Market, First Wave[a]

	RDD			*DD*	
Population and variable	*Linear specification*	*Quadratic specification*	*Quadratic specification with control variables*	*Individual fixed effects*	*DD with RD polynomial*
All					
Labor market participation	0.0433	0.0652	0.0446	–0.0241	–0.0331
	(0.0274)	(0.0423)	(0.0459)	(0.0156)	(0.0152)**
Unemployment	–0.00924	–0.000246	–0.00874	–0.0115	–0.00898
	(0.0165)	(0.0270)	(0.0320)	(0.0145)	(0.0110)
Employment	0.0525	0.0654	0.0534	–0.0126	–0.0242
	(0.0259)**	(0.0387)*	(0.0452)	(0.0177)	(0.0149)
Hours of work	–1.861	–2.892	–1.715	–1.892	–1.411
	(1.689)	(2.637)	(2.851)	(1.205)	(1.073)
PANES holders or applicants					
Labor market participation	0.0408	0.0904	0.0717	–0.00855	–0.00928
	(0.0398)	(0.0619)	(0.0615)	(0.0202)	(0.0199)
Unemployment	–0.0161	0.00790	0.000934	–0.00411	–0.00260
	(0.0259)	(0.0391)	(0.0398)	(0.0191)	(0.0191)
Employment	0.0570	0.0825	0.0707	–0.00444	–0.00668
	(0.0415)	(0.0633)	(0.0630)	(0.0229)	(0.0228)
Hours of work	–3.042	–2.262	–2.405	–1.899	–1.958
	(2.230)	(3.497)	(3.515)	(1.538)	(1.343)

Source: Based on *PANES* administrative records and second follow-up survey.

a. Marginal effects coefficient and standard deviation of the treatment variable. Robust standard errors in parentheses.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

TABLE A.4. Effects on Labor Market, Second Wave[a]

Population and variable	RDD: Linear specification	RDD: Quadratic specification	RDD: Quadratic specification with control variables	DD: Individual fixed effects	DD: DD with RD polynomial
All					
Labor market participation	0.0488	0.0568	0.0488	0.0123	0.0120
	(0.0289)*	(0.0449)	(0.0289)*	(0.0160)	(0.0373)
Unemployment	0.00422	−0.0324	−0.0418	−0.00424	−0.00224
	(0.0157)	(0.0271)	(0.0312)	(0.0133)	(0.0230)
Employment	0.0446	0.0863	0.122	0.0165	0.00768
	(0.0267)*	(0.0410)**	(0.0457)***	(0.0167)	(0.0362)
Hours of work	−1.631	−5.175	−3.686	−0.978	−2.660
	(1.888)	(2.796)*	(2.943)	(1.497)	(2.348)
PANES holders or applicants					
Labor market participation	0.0462	0.113	0.0963	−0.0040	0.0025
	(0.0413)	(0.0664)*	(0.0676)	(0.0215)	(0.0548)
Unemployment	−0.00993	−0.0574	−0.0617	−0.0120	−0.0153
	(0.0276)	(0.0500)	(0.0507)	(0.0191)	(0.0452)
Employment	0.0560	0.163	0.167	0.0079	−0.0048
	(3.885)	(5.409)	(5.461)	(0.0239)	(0.0599)
Hours of work	−2.939	−7.395	−7.172	−1.277	−1.209
	(2.794)	(4.228)*	(4.270)*	(1.881)	(3.149)

Source: Based on *PANES* administrative records and second follow-up survey.
a. Marginal effects coefficient and standard deviation of the treatment variable. Robust standard errors in parentheses.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

TABLE A.5. Probability of Being Aware of Conditionalities[a]

Poverty score	−15.15
	(22.73)
Poverty score cuad	327.1
	(1,118)
Age	−0.00273
	(0.00270)
Sex	−0.00162
	(0.0840)
Household head education	0.0347
	(0.00979)***
Region	0.119
	(0.0949)
Constant	−0.837
	(0.231)***
Observations	1,818

Source: Based on first follow-up survey.
a. Robust standard errors in parentheses.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Comment

Felipe Barrera-Osorio: This paper has several strengths. First, though there are numerous evaluations of the impact of conditional cash transfers in education, it is critical to replicate results in different contexts and with different designs. This article serves that goal.

Second, the paper does not find effects of the program on educational outcomes, in contrast to the general findings of conditional cash transfers around the world (Saavedra and García 2012; Fiszbein and Schady 2009). At the risk of stating the obvious, a no result is an important result. Too often, journals ignore this fact.

Finally, the paper investigates three mechanisms by which it is possible to explain the lack of results. It is clear that one of the most important frontiers in evaluation is the quest to find mechanisms (à la Jens, Kling, and Mullainathan 2011), in addition to just finding effects. Again, this paper contributes to this agenda.

The authors do not find impacts from a typical conditional cash transfer in Uruguay on school attendance and child labor. As mentioned before, these results contrast sharply with the general empirical evidence elsewhere. In contexts with similar secondary school enrollment rates, conditional cash transfers have impacted schooling decisions (for example, my own research in Bogotá, Colombia; Barrera-Osorio and others 2011).

Moreover, it seems that the lack of results is not driven by adult labor responses. This paper corroborates an important finding of the literature on conditional cash transfers: these programs do not seem to trigger adult labor responses. Finally, the authors do not find a correlation between school enrollment and household's knowledge (or lack thereof) of the program's conditionality requirements.

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Indeed, the lack of results on school attendance is very puzzling. Given that labor supply and conditionalities cannot explain the lack of results, the authors advance two potential hypotheses. First, it may be that the program does not complement the demand intervention with supply policies. However, it is difficult to accept this explanation since the vast majority of conditional cash transfer programs around the world are not complemented with supply interventions, and they have shown effects on school enrollment and attendance. Second, the authors advance the hypothesis that the lack of results can be explained by the lump-sum nature of the cash transfer, that is, the family receives a certain amount of money independent of the number of minors in the household. Again, it is difficult to accept this hypothesis: we know that the amount of cash matters (Fernald, Gertler, and Neufeld 2009), but we also know that the elasticity of education is quite high (Fiszbein and Schady 2009). Even if the amount is a lump sum, based on the vast empirical literature, income and price effects should trigger an education response.

Let me advance another explanation. The evaluation covers, in the first follow-up, only six months of treatment; in the second follow-up, approximately 1.5 years of treatment. It is a very short period of treatment. Also, the targeted population of the program—the very poor—was getting out of a deep recession. The timing of these two events can explain the lack of responses: families received the money and were trying to come back to their long-term consumption and investment trajectory—which would explain the lack of response—and the evaluation covered a very brief period of time. It would be extremely important to see the long-term effects of the program. However, the program was terminated in December 2007, and therefore it is quite difficult to assess long-term effects.

Another interesting aspect of this particular program is that the government changed it substantially in December 2007. Actually, the program was finished and replaced by another. I would like to believe that the evaluation triggered that change, that, given the lack of results, the government decided to change the design of the program.

Let me finish by proposing two future lines of research. First, as I mentioned, the measurement of long-term effects of conditional cash transfers, especially in eliminating poverty, would be the acid test of these types of policies. For randomized controlled trials, research on long-term impacts is challenging, since the original sample may suffer drastic changes through time. Second, I believe that the next generation of conditional cash transfers should be conditioned on performance and not on attendance to services. Conditioning on performance has the challenge that the individuals best suited to reach the conditionality are presumably the more apt ones in the target population, raising questions of equity.

Sometimes the work of economists is like the work of detectives. The detective arrives at a scene and tries to follow clues to construct a plausible story of events. In economics, we have a context (scene) in which a program is implemented. We try to follow clues to reconstruct the effects of the program. With some luck, we find the smoking gun. In this specific case, the story seems very difficult to read. It is akin to a detective who knows that something happened in a place, but on arrival at the scene finds nothing that indicates any change. Moreover, the detective cannot find any clues. This paper did not find any smoking gun—or, in fact, any gun. It is quite unsatisfactory and puzzling at the same time.

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