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Eradicating diseases: The effect of conditional cash transfers on vaccination coverage in rural Nicaragua

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ABSTRACT

Despite significant global efforts to improve vaccination coverage against major childhood diseases, vaccination rates are below 90%. To eradicate diseases such as measles, however, vaccination rates close to 95% are needed. We use a randomized experiment to investigate the effect of a demand incentive, a conditional cash transfer program, in improving vaccination coverage in rural Nicaragua. Double-difference estimates show the program led to large increases in vaccination coverage, and these resulted in vaccination levels greater than 95% for some vaccines. Effects were especially large for children who are typically harder to reach with traditional supply-side interventions.

1. Introduction

Reducing the burden of illness and mortality from preventable diseases through vaccination is a key component of public health policy. Impressive achievements have been made worldwide; for example, smallpox was eradicated in 1977 and global vaccination rates reached 75% against the major childhood diseases in the mid-1990s.[1] Presently, efforts are under way to eradicate polio and many countries are working to eliminate measles (Quadros et al., 2003; Miller et al., 2006).[2] Though eradication is costly, the long-term financial gain can be large. For example, the payoff for eradicating polio is estimated to be as high as $ 1 billion per annum, since it eliminates the need for future prevention and treatment of the afflicted and avoids their potentially lost economic contributions to society (GPEI, 2003; Khan and Ehreth, 2003).

Despite these successes, two million children die each year from vaccine preventable diseases (WHO, 2008). This is in part because global vaccination levels for major childhood diseases have been static for a decade (Foster et al., 2006), leaving approximately 26 million children worldwide inadequately protected (UNICEF, 2008). Even the better performing regions are expected to plateau below 90% coverage rates for the third dose of the diphtheria–pertussis–tetanus vaccine (DPT3), a standard indicator for overall vaccination program effectiveness (WHO, 2006). Moreover, high average coverage rates hide large disparities both across and within countries (WHO/UNICEF, 2007).

To eradicate diseases such as measles, vaccination coverage rates close to 95% are needed (Barrett and Hoel, 2003). With the experience of the past decade in mind, however, it would appear that new strategies may be required to reach such levels. Geoffard and Philipson (1997) argue that the "demand side" is critical for eradication because as the prevalence of a disease declines so, too, does the demand by individuals to be vaccinated against that disease. This potentially allows the disease to resurge. Their theoretical model demonstrates that even traditional price subsidies (such as free vaccination at health facilities) and mandatory vaccination programs may be limited in their ability to eradicate a disease. Xie and Dow (2005) explore the supply and demand sides of vaccination empirically and find that both supply-side factors (e.g., the price of vaccine

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[1] These include tuberculosis, measles, polio, diphtheria, pertussis and tetanus.

[2] In the public health literature, eradication of a disease refers to complete global eradication while elimination refers to the elimination of a disease within a particular country or region.

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services), as well as demand-side factors (e.g., maternal education) are important household-level determinants of vaccination. Most national vaccination strategies, however, focus on the supply of vaccinations, including taking services directly to the household during mass vaccination campaigns. Demand-side strategies tend to be limited to awareness raising or social mobilization campaigns, which may miss some important groups such as children of poorly educated mothers. As a result, stronger demand-side incentives may be needed to increase vaccination coverage to the levels needed for eradication.

We examine how a conditional cash transfer (CCT) program, which includes a substantial demand-side component, affects vaccination rates in rural Nicaragua. Prior to the program's implementation, Nicaragua, with the lowest GDP per capita in Central America, had vaccination rates below 90%. It thus provides a low-income setting to assess whether CCTs can boost coverage to 95%. The analysis uses a randomized experimental evaluation of a pilot CCT program, the *Red de Protección Social* (*RPS*), to provide double-difference estimates of the program's effects. After 2 years of program operation (2000–2002), there were large effects, especially for typically hard to reach populations such as children whose mothers were less educated or who lived further away from a health facility. The program pushed coverage rates for children 12–23 months old above 95% for DPT3 in the treatment group, compared with 85% in the control group. In contrast, coverage for the same age group for DPT3 for the country as a whole was 83% when *RPS* began in 2000 and had reached only 86% by 2005 (WHO/UNICEF, 2007).

2. Background

2.1. Government vaccination programs in Nicaragua

The Ministry of Health in Nicaragua has a two-pronged vaccination strategy: to provide vaccinations at government health facilities and to hold vaccination campaigns which typically take vaccines to a child's house. Each year between 1999 and 2002, two, month-long vaccination campaigns against all the major childhood diseases were conducted. The government does not rely only on the provision of vaccines at health facilities because there is incomplete coverage of the population and because such a strategy relies on individuals bringing their children to the facility—a preventive health care decision that may depend on location and quality of the facilities, as well as on individual and household characteristics which alter the perceived costs and benefits of vaccination (Philipson, 1996; Mullahy, 1999; Xie and Dow, 2005). Using these two methods of vaccination, officially reported vaccination rates in Nicaragua as a whole prior to the program in 2000 were 96% for the vaccine against tuberculosis, Bacille Calmette-Guérin (BCG), 86% for the measles containing vaccine (MCV), 85% for the third dose of oral polio vaccine (OPV3), and 83% for DPT3 (WHO/UNICEF, 2007).

2.2. CCTs and vaccinations

CCT programs are social programs designed to break the intergenerational continuity of poverty. Their basic premise is that a major cause of the transmission of poverty is the inability of poor households to invest in the human capital of their children. To provide an incentive to invest in human capital enhancing activities and to alleviate possible credit constraints inhibiting such investments, cash transfers are given to families conditional upon family members engaging in behaviors to improve their health, education and nutrition status. This effectively transforms cash transfers into human capital subsidies for poor households.

Most CCT programs which aim to improve the health status of poor populations make concerted efforts to improve the supply of health services as well as providing demand-side incentives. Improvements in supply may be necessary, since conditionalities cannot be met if there are no services, and cannot be met by all if there are inadequate services. Even in places with adequate current supply, the increased utilization brought about by CCT programs could lead to crowding and a decline in quality if the supply does not adjust for the increased demand.

To date, vaccinations have not typically been an explicit conditionality in CCT programs, however they are almost always included among the services provided under a general health care conditionality, such as required preventive health care visits. Therefore, these programs differ from the traditional vaccination strategies in that they give individuals a monetary incentive to travel to the health facility to receive a package of services, including vaccinations. This is in contrast to a mass vaccination campaign, during which the government provides vaccinations only, but closer to the child's house. Along with the cash transfers, this embedding of vaccination within a larger provision of health services may increase the demand for vaccinations above that of the traditional vaccination programs. For example, if there are complementarities among different inputs into the production of child health, the marginal effect of vaccination may be higher when coupled with these other inputs, increasing the demand for vaccination (Behrman and Wolfe, 1987; Strauss and Thomas, 1995).

There is limited research on the impact of CCTs on vaccination coverage. Barham et al. (2007) examine the effect of the Mexican CCT program, *Oportunidades*, in rural areas using data from a randomized experiment. Due to measurement problems for some vaccines, their analysis is limited to examining coverage of MCV and BCG. They find small average program effects, on the order of 3 percentage points, and argue that this is due to high coverage rates (above 90%) before the program. They also find heterogeneous effects for sub-populations, with larger effects for those children whose mothers were less educated or who lived further away from a health facility. Morris et al. (2004) examine the impact of a conditional voucher program in rural Honduras, also using a randomized experiment. They find small significant increases for the first dose of DPT and no effect for MCV, but do not investigate DPT3 or sub-population effects. We extend the current literature by (1) investigating the effects of a CCT on the vaccines against all the major childhood diseases for different age groups; (2) examining the heterogeneity of effects for important sub-populations such as children whose mothers were less educated; and (3) assessing the robustness of the results to measurement error using two data sources, one of which is high-quality administrative data including nearly all children in the randomly assigned treatment and control groups. This last extension to previous work is important since vaccination data from household surveys are likely to be measured with error (Valadez and Weld, 1992).

2.3. The Red de Protección Social

RPS began in 2000 as a pilot program in 42 localities[3] in six poor rural municipalities of central Nicaragua[4]; take-up was approximately 90%. The purpose of *RPS* was to reduce both current and future poverty through conditional cash transfers for health and education attainment. Beneficiary households in the treatment

[3] Localities included between one and five small communities averaging approximately 100 households each.

[4] See Maluccio (2009) for more detail on the municipality- and locality-level targeting.

group began the program and received conditional transfers in November 2000, while the control group households became beneficiaries two and a half years later. Separate transfers were made for, and different conditions applied to, the health and education components of the program. The mother in the beneficiary household received the cash transfer for health contingent on her (1) bringing her children to scheduled preventive health care appointments—once a month for children under 2 years of age, and bimonthly (every other month) for those between two and five; (2) attending bimonthly health educational workshops and; (3) ensuring adequate weight gain for her children. Health services at the scheduled visits included growth monitoring, vaccination, supplementation for anemia and provision of anti-parasite medicine. While vaccination coverage for an individual child was not an explicit requirement for receipt of the cash transfer, providers were paid to deliver vaccinations during scheduled visits. In the event a vaccine was not available, beneficiaries received the vaccination in a subsequent visit. This may result in lower levels of vaccination utilization than if vaccination receipt had been an explicit condition. Receipt of the separate education transfer was contingent on all children in the household aged 7–13 who had not yet completed 4th grade enrolling in school and attending at least 85% of the time.

The cash transfers were made bimonthly to beneficiary households[5] who met the conditionalities during the prior 2 months.[6] During the first 2 years of the program, the average annual family transfer was $ 272 dollars, or approximately 17% of pre-program total annual household expenditures. All beneficiary families with children received a health transfer of approximately $ 37 every 2 months (for a total of $ 224 a year) if every health conditionality was met. The education transfer included $ 112 per household per year and a per beneficiary child transfer for school supplies of $ 21 per year. Receipt of the education transfer was not tied to meeting the health conditionalities or vice versa.

Due to low coverage in the program area and the concern that the Ministry of Health could not expand its services in time to meet an increased demand for services, *RPS* contracted and trained private health providers (Regalia and Castro, 2007). Beneficiaries were required to use those providers for fulfillment of the conditions and all services were free of charge. Providers visited program areas on scheduled dates and delivered services in existing health facilities, community centers, or private homes. As such, these services were generally at least as close to the beneficiary households as the nearest health facility, and often much closer. They were likely not as close to the house, however, as the typical vaccination campaign services would have been. There was a delay in organizing provision of these health services to beneficiaries, so they only became available starting in May 2001. As a result, there were neither health services nor enforced conditionalities relating to the health transfer during the first 8 months of transfers.

3. Evaluation design and data

3.1. Evaluation design

A 2000–2002 evaluation for *RPS* was implemented based on a randomized, locality-based intervention in 42 localities. The randomization took place at a public event in which representatives from the localities, the Government of Nicaragua, the Inter-American Development Bank, the International Food Policy Research Institute and the media were present. To ensure that the selection of localities into treatment and control groups was well stratified by wealth, a poverty index was created for each locality and seven groups of six localities with similar wealth rankings were created. From each of these sets of six, three localities were randomly selected for inclusion in the treatment group and three in the control group.

3.2. Data

3.2.1. RPS household survey data

The first data source we use is a household panel survey collected for the *RPS* evaluation (hereafter, survey data). These data include the number of doses received since birth for all vaccinations against the four major childhood diseases. The interview protocol was to ask for the child's health card (on which vaccinations should be recorded) and, if that was unavailable or incomplete, to rely on the mother's report; the source of vaccination information also was recorded. In addition, a locality-level survey was conducted each round to collect information including the availability of government and private health facilities.

The baseline survey was carried out on 1581 households in August/September 2000 (before the program began) and was a clustered random sample of approximately 15% of the population in the 42 localities. Two follow-up rounds took place in October 2001 and 2002, 5 and 17 months after the May 2001 start of the program's health and nutrition services. Household-level attrition rates in the second and third rounds are similar to comparable surveys (Alderman et al., 2001; Thomas et al., 2001) and across treatment and control groups (Maluccio and Flores, 2005).[7]

We limit the sample to children ages 0–35 months and for whom vaccination data is available in at least one of the three survey rounds. This yields a total sample of 2229 observations with 866 at baseline, 751 in 2001 and 612 in 2002. The sample is divided evenly between the treatment (49%) and control (51%) groups.

3.2.2. RPS administrative data

The second data source we use is administrative data which *RPS* collected for operational purposes. The program collected data on basic individual- and household-level characteristics, as well as on vaccinations. The individual and household data were collected in a census of all households in the 42 treatment and control localities in May 2000, before the program began. They included information on demographic and educational background, housing characteristics and ownership of assets. The vaccination data included vaccination histories, which were collected during the first preventive health care visit by the health care providers, and all on-going vaccination information in subsequent visits. Mothers were instructed to bring their health cards to all visits (and were issued them as necessary) and the data were verified and recorded by trained health professionals. These data provide the type and date of application for all vaccines administered to the child since birth.

The vaccination data were collected on all beneficiaries (under 5 years of age) in the treatment group beginning in mid-2001. In the control group, however, the collection of vaccination data began later, in mid-2003, when *RPS* extended the program to the controls. As a result, the administrative data can be used to determine the vaccination status for nearly all of the original treatment and control children less than 24 months old at the time of the household

[5] Households determined to have significant economic resources were not eligible for the program (this affected 7% of households in the treatment group). Approximately 4% of eligible households chose not to participate in the program (Maluccio, 2009).

[6] Maluccio and Flores (2005) describe how compliance and randomization was enforced and monitored.

[7] Of the 1581 households interviewed at baseline, 91.9% were re-interviewed in 2001, and 88.4% in 2002.

surveys (in 2000, 2001, and 2002). Unlike the household survey, however, there is no information on children aged 24–35 months in the control group in 2000 because those children were older than five in mid-2003 and therefore were not monitored by the program. The total sample includes 9986 observations with 3922 at baseline, 3382 in 2001 and 2682 in 2002.

There are two advantages of the administrative data over the survey data. First, the sample size is much larger, with 9986 children under 24 months as compared to 1417 in the survey data for the same age range. Second, the administrative data are almost certainly more accurate because the health professionals collecting them were well trained and were responsible for a narrower range of topics than the enumerators collecting the survey data. Such health care provider data are often treated as the gold standard in studies investigating the validity of reported vaccinations from other types of surveys (Suarez et al., 1997; Murray et al., 2003).

In contrast to the random sample for the survey data, the administrative sample is selected since only children living in households that (eventually) participated in the program are included. Participation rates were on the order of 90% for both groups and selection rules and outreach efforts were the same. Therefore we argue that the selection process was the same and is unlikely to have a large effect on the estimates.

3.3. Dependent variables

Both the survey and administrative data recorded the number of doses each child had received since birth of each of the following: (1) a tuberculosis vaccine (BCG); (2) a measles containing vaccine (MCV); (3) an oral polio vaccine (OPV); and (4) a diphtheria–pertussis–tetanus vaccine (DPT). A binary dependent variable was created to measure coverage for each vaccine—it takes the value one if a child received all of the recommended doses of that vaccine at the time of each survey, and zero otherwise. A child is not considered to be vaccinated against DPT or polio unless he or she has received their third dose of each vaccine (DPT3 or OPV3).

International public health organizations typically use <12-month and 12–23-month age groups to evaluate up-to-date vaccination coverage for a child, depending on when the vaccine is recommended to be administered (Bolton et al., 1998; WHO/UNICEF, 2007). We use these same age groups to evaluate whether a child was vaccinated by the appropriate age, or what we refer to as "on-time," with BCG, MCV, OPV and DPT. Table 1 shows the vaccination schedule published by the Ministry of Health in Nicaragua. The schedule prescribes that the BCG vaccine should be given at birth; therefore we use the <12-month age group for measuring on-time BCG vaccination rates. For MCV, OPV and DPT vaccines, the 12–23-month age group is used to assess on-time vaccination. This is because MCV is scheduled to be given at 12 months of age, and a large proportion of children <12 months of age would not yet have received all three doses of OPV or DPT vaccines by the time there were 12 months old.

Table 1
Basic vaccination schedule for up-to-date vaccinations in Nicaragua.

Disease	Vaccine	Dose	Ages given
Tuberculosis	BCG	1	At birth
Measles	MCV[a]	1	12 months
Polio	OPV	3	2, 4, 6 months
Diptheria–pertussis–tetanus	DPT[b]	3	2, 4, 6 months

[a] Beneficiaries could have received a dose of the measles vaccine or the MMR (measles, mumps, rubella) vaccine to be immunized against measles.

[b] Beneficiaries could have received DPT or the pentavalent vaccine (or a combination of both).

In addition to on-time vaccination, we look at older age groups to assess possible program effects on late application of vaccines, or what we refer to as "catch-up." While on-time vaccination offers better protection, catch-up still provides public health benefits (Bolton et al., 1998) and therefore is important for eradication. For BCG, we examine catch-up vaccination for both 12–23- and 24–35-month olds. For all other vaccines, catch-up is for the 24–35-month olds.

There are several reasons why the effect of *RPS* may differ across vaccine types, leading us to examine each vaccine type individually. First, each vaccine combats a different disease with its own prevalence and potential consequences. As a result, the real and perceived benefits of immunization may vary across diseases. Second, the real and perceived costs may vary across vaccine types, since vaccines can differ in at least three ways: (1) number of doses; (2) age at which they are prescribed; and (3) application modality (oral versus injection). Another reason for which analyzing program effects by vaccine type is important is that public health efforts aimed at elimination or eradication typically focus on one disease type at a time, as with the Global Polio Eradication Initiative. Therefore, for both on-time and catch-up, we examine the effect of *RPS* on vaccination for each individual vaccine, as well as for a summary measure of whether the child was fully vaccinated (FVC) for all four vaccines. The child with all four vaccines by age 12–23 (24–35) months is considered fully vaccinated on-time (in the catch-up period).

4. Methods and empirical model

We estimate the effect of *RPS* on vaccination coverage for children under three. We evaluate whether a child was vaccinated with BCG, MCV, OPV3, DPT3, and if the child was fully vaccinated with all four vaccines (FVC) and exploit the random assignment to create a control group to examine the counterfactual. In this section, we provide evidence that the randomization led to well-balanced treatment and control groups and then present the main empirical methods.

4.1. Outcome of the randomization

While the selection of treatment and control localities was random (Section 3.1), it remains important to examine if the observable characteristics were well balanced by the randomization (Roberts and Torgerson, 1999). Using the survey data, we examine differences in means at baseline between treatment and control groups for vaccination rates for each age group (Table 2). Nine of the 11 measures we examine are very similar, with initial differences of 2 percentage points or less. The remaining two (BCG for 12–23-month olds and OPV3 for 24–35-month olds), suggest their coverage was higher, by about 5 percentage points, in the control group. However, none of the differences are statistically different.[8]

In Table 3, we examine differences in means at baseline between treatment and control groups for an array of individual, parental, household and locality characteristics associated with child health care. For the sample of children under age 3 in 2000, differences in means of these important characteristics are also small (relative to the overall means) and statistically insignificant for all but one of the 29 factors examined—mother's age. Even mother's age, however, differed by <1 year compared with an average of 27 years.

[8] An anonymous referee observed that the *p*-values from such tests may be problematic since any observed differences between the groups are random, having resulted from a random selection.

Table 2
Difference in baseline (2000) means of dependent variables by treatment status.

	Treatment (T)		Control (C)		Difference (T – C)	
	Mean	*N*	Mean	*N*	Mean	S.E.
<12 months						
BCG	0.77	125	0.82	134	−0.06	(0.07)
12–23 months						
BCG	0.95	164	0.93	142	0.02	(0.03)
MCV	0.70	164	0.69	142	0.01	(0.08)
OPV3	0.76	164	0.80	142	−0.05	(0.07)
DPT3	0.68	164	0.67	142	0.01	(0.08)
FVC	0.54	164	0.55	142	−0.01	(0.08)
24–35 months						
BCG	0.91	146	0.92	155	−0.01	(0.03)
MCV	0.85	146	0.86	155	−0.01	(0.05)
OPV3	0.82	146	0.85	155	−0.02	(0.07)
DPT3	0.75	146	0.75	155	−0.01	(0.08)
FVC	0.68	146	0.66	155	0.01	(0.08)

Notes: the standard errors (S.E.) are clustered at the locality level. None of the differences in means (T – C) are statistically different.

Finally, we examine if the differences in baseline coverage rates and observable characteristics are less than 0.25 standard deviations apart (Imbens and Wooldridge, 2008), and find there is only one variable with a difference larger than 0.25. Taken together, these findings strongly suggest that the randomization adequately balanced the treatment and control groups on a range of measures.

4.2. Empirical specification

We use a double-difference estimator to determine the average effect of the program since it controls for the baseline differences in the vaccination rates. This estimator also controls for all characteristics that do not change over time within treatment and control groups and all characteristics that do change over time, but in the same way in each of the groups.

The main regression equation is

$$V_{icmt} = \beta_0 + \beta_1 2001_t + \beta_2 2002_t + \beta_3 T_{cm} + \delta_1 T_{cm} 2001_t + \delta_2 T_{cm} 2002_t + \varepsilon_{icmt}, \quad (1)$$

where $V_{icmt} = 1$ if child i from locality c in municipality m in time period t is vaccinated and zero otherwise, $2001_t = 1$ if year is 2001 and zero otherwise, $2002_t = 1$ if year is 2002 and zero otherwise, $T_{cm} = 1$ if in treatment group, i.e., program in locality c in municipality m and zero otherwise, ε_{icmt} = unobserved idiosyncratic error (assumed to be uncorrelated with all other variables).

We estimate (1) using ordinary least squares (OLS).[9] Standard errors are calculated allowing for heteroskedasticity and for clustering at the level of the locality.[10] The parameters of interest are δ_1 and δ_2, where δ_1 is the double-difference estimate of the program effect for 2001 (relative to 2000) and δ_2 is that for 2002 (relative to 2000). The program effects are identified by the randomized design. Because we do not condition on actual program participation (which was approximately 90%) when using the survey data, but only on whether the household resides in a treatment locality, the estimates reflect the "intent-to-treat" average effect of the program (Ravallion, 2008). Given the randomized design, it is not necessary to include other variables in this regression for the consistency of δ_1 and δ_2. These estimates are our main results.

The program was randomized at the locality level and Section 4.1 provided evidence that this randomization was successful. Nevertheless, we conduct some further robustness checks. Given that government health service delivery is organized at the municipality- and not the locality-level, it remains a concern that some municipality-level characteristics were not well balanced between the treatment and control groups. Therefore, we also estimate models in which we include municipality fixed-effects, as well as the set of baseline locality-level characteristics shown in Table 3. If we find substantial changes in the estimated program effects, it would be cause for concern that municipality-level variables were not well balanced. Finally, because they may enhance precision we also include the array of baseline individual- and household-level characteristics in Table 3. The inclusion of these latter controls did not improve the precision or appreciably change the estimated effects, so they are not shown.

5. The effect of *RPS* on vaccination

First, we present results for children under three based on the survey data and then replicate those results using the administrative data for all but the 24–35-month olds. We then incorporate information on the distance between treatment and control localities to explore possible spillover effects, considering each of the two datasets in turn. Finally, in Section 6, we merge the two datasets together at the individual level, to explore potential measurement error biases.

Table 4 reports the mean vaccination rates by survey year for treatment and control groups, as well as the double-difference estimated impacts of *RPS* for three age groups: (1) <12 month olds; (2) 12–23-month olds; and (3) 24–35-month olds. For the discussion, we organize the different age groups into on-time and catch-up groups.

5.1. On-time effects

Table 4 shows that on-time vaccination coverage rose dramatically in the treatment group, from 68–77% in 2000 to 87–97% in 2002. Over the same period, however, there was also a substantial rise in vaccination rates in the control group. For example, coverage for BCG for <12-month olds rose from 82% in 2000 to 91% in 2001. Nevertheless by 2002, only in the treatment group did vaccination rates for all vaccines except MCV reach levels at or near 95%, the rate considered necessary for eradication of some diseases.

Table 4 also reports the intent-to-treat double-difference estimates with and without the municipality- and locality-level controls. The results without controls represent our main findings and will be the focus of our discussion.[11] The effects of the program on vaccination coverage for each of the vaccines for 2001 and 2002 are similar with and without controls (all within 2 percentage points). This suggests our results are not biased by our concern that municipality-level variables may not have been well balanced after randomizing at the locality level.

For the 2001 survey, the double-difference estimates of the effects of *RPS* on on-time vaccinations are 9 percentage points for BCG, 15 percentage points for MCV, 14 percentage points for OPV3,

[9] Non-linear models that use maximum likelihood methods are often employed if the dependent variable is binary. When vaccination rates are 100% for certain subgroups (such as BCG for 12–23-month olds in the treatment group in 2002), such models cannot be estimated. Where possible, we compare the linear probability estimates to logit and probit estimates and find no substantial differences in the estimated marginal effects or significance levels.

[10] Results are similar when regressions are weighted by sample probabilities.

[11] Estimated effects based on logit regression for those vaccines and age groups, where estimation was possible, were on average 2 percentage points lower (ranging between 0 and 3 percentage points).

Table 3
Difference in baseline (2000) means of individual, household and locality variables by treatment status.

	Treatment ($N=435$)		Control ($N=431$)		Difference	
	Mean	S.D.	Mean	S.D.	Mean	*t*-statistic
Individual characteristics						
Age in months	18.39	(9.89)	18.59	(10.35)	−0.20	−0.29
Male (=1)	0.50	(0.50)	0.50	(0.50)	0.00	0.13
Household characteristics						
Block wall (=1)	0.14	(0.35)	0.13	(0.34)	0.01	0.17
Dirt floor (=1)	0.84	(0.37)	0.82	(0.38)	0.02	0.34
Zinc roof (=1)	0.53	(0.50)	0.53	(0.50)	0.00	0.02
Tile roof (=1)	0.28	(0.45)	0.32	(0.47)	−0.04	−0.42
Number of rooms in house	1.40	(0.79)	1.49	(0.82)	−0.09	−1.06
Owns house (=1)	0.74	(0.44)	0.76	(0.43)	−0.03	−0.41
Latrine in house (=1)	0.54	(0.50)	0.47	(0.50)	0.07	1.02
House has electricity (=1)	0.20	(0.40)	0.18	(0.38)	0.03	0.47
Piped water into house (=1)	0.01	(0.12)	0.03	(0.16)	−0.01	−0.76
Value of durable assets	368	(1487)	306	(788)	–	0.45
Land owned (square meters)	14159	(15081)	15231	(20537)	−1072	−0.56
At least one animal (=1)	0.12	(0.32)	0.13	(0.33)	−0.01	−0.37
Per capita expenditures	3081	(1941)	2975	(1998)	107	0.44
Father's years of education	1.77	(1.97)	1.74	(1.94)	0.03	0.16
Mother's years of education	2.05	(2.28)	2.15	(2.55)	−0.10	−0.39
Father's age	33.54	(9.26)	34.43	(9.77)	−0.89	−1.32
Mother's age	26.80	(7.29)	27.66	(7.71)	−0.86	−2.18
Household size	7.24	(3.48)	6.91	(3.13)	0.32	1.09
Locality characteristics						
Doctor (=1)	0.15	(0.36)	0.16	(0.37)	−0.01	−0.09
Nurse (=1)	0.41	(0.49)	0.33	(0.47)	0.08	0.53
Pharmacy (=1)	0.04	(0.20)	0.06	(0.25)	−0.02	−0.28
Health facility (=1)	0.47	(0.50)	0.35	(0.48)	0.11	0.71
Distance to health facility (km)	8.20	(7.55)	6.05	(5.13)	2.15	1.01
Road access (=1)	0.78	(0.41)	0.80	(0.40)	−0.02	−0.16
Km to public transport	5.14	(5.79)	4.06	(4.02)	1.08	0.69

Notes: S.D. = standard deviation. The standard errors used to calculate the *t*-statistics are clustered at the locality level. Per capita expenditures and value of durable assets are in year 2000 córdobas.

Table 4
Mean vaccination rates by treatment status and double-difference estimates.

	Year	Mean		Double-difference estimate				Mean		Double-difference estimate			
		T	C	OLS	S.E.	OLS	S.E.	T	C	OLS	S.E.	OLS	S.E.
		<12 months ($N=658$)											
BCG	2000	0.77	0.82										
	2001	0.95	0.91	0.09	(0.07)	0.08	(0.07)						
	2002	0.93	0.91	0.08	(0.07)	0.06	(0.08)						
		12–23 months ($N=759$)						24–35 months ($N=812$)					
BCG	2000	0.95	0.93					0.91	0.92				
	2001	0.99	0.96	0.01	(0.03)	0.00	(0.03)	0.99	0.95	0.06	(0.04)	0.06+	(0.04)
	2002	1.00	0.97	0.01	(0.04)	0.01	(0.04)	1.00	0.98	0.04	(0.04)	0.03	(0.04)
MCV	2000	0.70	0.69					0.85	0.86				
	2001	0.91	0.75	0.15+	(0.09)	0.13	(0.08)	0.94	0.95	0.00	(0.05)	0.00	(0.05)
	2002	0.87	0.83	0.03	(0.09)	0.04	(0.09)	0.98	0.87	0.12*	(0.06)	0.11+	(0.06)
OPV3	2000	0.76	0.80					0.82	0.85				
	2001	0.96	0.87	0.14*	(0.07)	0.13+	(0.07)	0.99	0.94	0.07	(0.07)	0.08	(0.06)
	2002	0.97	0.90	0.11	(0.08)	0.13+	(0.08)	1.00	0.93	0.09	(0.07)	0.08	(0.06)
DPT3	2000	0.68	0.67					0.75	0.75				
	2001	0.91	0.85	0.05	(0.07)	0.04	(0.07)	0.98	0.91	0.08	(0.07)	0.07	(0.07)
	2002	0.97	0.85	0.10	(0.08)	0.11	(0.08)	0.98	0.89	0.10	(0.08)	0.08	(0.08)
FVC	2000	0.54	0.55					0.68	0.66				
	2001	0.84	0.65	0.20*	(0.08)	0.19*	(0.08)	0.91	0.87	0.03	(0.08)	0.03	(0.08)
	2002	0.86	0.75	0.12	(0.09)	0.14	(0.09)	0.96	0.80	0.15+	(0.08)	0.14	(0.08)
Municipality fixed-effects				No		Yes				No		Yes	
Locality controls				No		Yes				No		Yes	

Notes: T = treatment; C = control; OLS = ordinary least square estimate; S.E. = standard error. A "+" indicates the OLS estimate is significant at the 10% level and "*" at the 5% level. The standard errors are in parentheses and are clustered at the locality level. Locality controls include all locality characteristics listed in Table 3.

Table 5
Mean vaccination rates by treatment status and double-difference estimates by distance to health facility and mother's level of education.

	Year	Health facility > 5 km away								Mother's education < 4 years							
		Mean		D–D estimate		Mean		D–D estimate		Mean		D–D estimate		Mean		D–D estimate	
		T	C	OLS	S.E.	T	C	OLS	S.E	T	C	OLS	S.E.	T	C	OLS	S.E.
		<12 months (N = 301)								<12 months (N = 464)							
BCG	2000	0.71	0.80							0.74	0.79						
	2001	0.95	0.87	0.16	(0.11)					0.92	0.90	0.07	(0.09)				
	2002	0.91	0.90	0.10	(0.11)					0.90	0.92	0.04	(0.10)				
		12–23 months (N = 318)				24–35 months (N = 354)				12–23 months (N = 537)				24–35 months (N = 602)			
BCG	2000	0.91	0.94			0.85	0.96			0.95	0.90			0.88	0.92		
	2001	0.98	0.93	0.07	(0.06)	0.99	0.95	0.15*	(0.07)	0.99	0.95	-0.01	(0.04)	0.99	0.92	0.11*	(0.05)
	2002	1.00	0.94	0.09	(0.07)	1.00	0.98	0.14*	(0.06)	1.00	0.96	-0.01	(0.04)	1.00	0.97	0.06	(0.05)
MCV	2000	0.59	0.60			0.77	0.92			0.71	0.67			0.82	0.87		
	2001	0.85	0.65	0.21	(0.14)	0.93	0.95	0.12+	(0.07)	0.90	0.75	0.12	(0.09)	0.95	0.96	0.03	(0.06)
	2002	0.85	0.80	0.05	(0.15)	0.96	0.83	0.29**	(0.08)	0.88	0.81	0.04	(0.09)	0.99	0.85	0.18**	(0.07)
OPV3	2000	0.63	0.72			0.72	0.91			0.73	0.77			0.79	0.83		
	2001	0.95	0.77	0.27**	(0.09)	0.99	0.91	0.26**	(0.09)	0.95	0.86	0.13	(0.08)	1.00	0.92	0.12	(0.09)
	2002	0.96	0.83	0.22	(0.15)	1.00	0.90	0.28*	(0.11)	0.98	0.87	0.15+	(0.08)	1.00	0.92	0.12	(0.08)
DPT3	2000	0.58	0.64			0.59	0.79			0.65	0.62			0.71	0.77		
	2001	0.87	0.75	0.17*	(0.08)	0.96	0.84	0.32*	(0.12)	0.88	0.83	0.03	(0.09)	0.98	0.87	0.17+	(0.10)
	2002	0.96	0.74	0.27*	(0.13)	0.96	0.80	0.36**	(0.12)	0.98	0.82	0.14	(0.09)	0.97	0.87	0.16	(0.10)
FVC	2000	0.38	0.50			0.51	0.70			0.51	0.51			0.63	0.68		
	2001	0.75	0.50	0.37**	(0.09)	0.89	0.80	0.28*	(0.13)	0.82	0.64	0.18+	(0.10)	0.92	0.86	0.11	(0.10)
	2002	0.85	0.66	0.31*	(0.13)	0.93	0.68	0.44**	(0.12)	0.88	0.69	0.19+	(0.10)	0.96	0.76	0.24*	(0.10)

Notes: D–D = double-difference; T = treatment; C = control; OLS = ordinary least square estimate; S.E. = standard error. A "+" indicates the OLS estimate is significant at the 10% level, "*" at the 5% level and "**" at the 1% level. The standard errors are in parentheses and are clustered at the locality level. No control variables are included in regressions.

5 percentage points for DPT3, and 20 percentage points for FVC. The effect for MCV is significant at the 10% level, and those for OPV3 and FVC at the 5% level.

By 2002, *RPS* had led to double-difference estimated 8–11 percentage points increases in vaccination coverage since baseline for all individual vaccines, except for MCV, which was only 3 percentage points. However, none of these findings are significantly different from zero. Using FVC as a summary indicator, the results show a statistically insignificant program effect of 12 percentage points in 2002, given an initial coverage of 54% in the treatment group. So, while statistically insignificant, the point estimates do suggest substantial effects, particularly for such a short period of time.

One pattern underlying the FVC results in 2002 is the decline in the program effect for MCV between the first and second survey rounds from 15 percentage points to 3 percentage points. This drop is related to a reduction in MCV coverage in the treatment group from 91% to 87%, and a simultaneous increase in MCV coverage in the control group from 75% to 83%. We are unable to explain these changes in MCV coverage rates estimated with the survey data; as shown in Table 5, these patterns are not replicated in the administrative data and are thus likely due to measurement error in the much smaller survey data where a small number of incorrectly reported vaccines for MCV could lead to such patterns.

5.2. Catch-up effects

For the catch-up group, Table 4 shows that, except for MCV, the impact of the program was similar in 2001 compared with 2002. The double-difference estimated effect of *RPS* on BCG was small and statistically insignificant in 2001 and 2002, likely due to high initial coverage rates in the treatment group which allowed little room for improvement. The program impact increases from an insignificant 0 to a statistically significant 12 percentage points for MCV (at the 5% level). Effects for OPV3 and DPT3 were insignificant and 7–10 percentage points in both years. The pattern for FVC follows the pattern for MCV, increasing from an insignificant 3 percentage points to a statistically significant (at the 10% level) 15 percentage points in 2002. Similar to the effects for on-time vaccination, the 2002 findings show the substantial effects of the *RPS* on vaccination coverage in a short period of time.

The lack of an effect in 2001 for MCV was due to a large increase in the control group coverage for MCV from 86% (in 2000) to 95% (in 2001). However, the coverage rate in the control group dropped to 87% in 2002. We are unable to explain the apparent patterns in coverage for MCV in control localities, though they appear to be due to measurement error.

For both the on-time and catch-up groups there were no consistent differences in results for vaccinations that require one versus three doses.

5.3. Heterogeneity effects

The costs and benefits of vaccination are likely to differ not only across the different vaccines but also for different sub-populations. For example, Xie and Dow (2005) show that supply-side factors (such as price or distance to the health facility), as well as demand-side factors (in particular, maternal education) are important determinants of vaccination at the household level. As a result, the average effects for all children may mask important heterogeneous effects.

We investigate the effects for different sub-groups of children chosen based on common determinants of child health status or demand for health care (Moss et al., 2002; Wagstaff et al., 2004) such as pre-program household per capita expenditures, maternal education levels, presence of a health facility in the locality, distance from the population center of the locality to the nearest health facility, and whether the locality was accessible by a road (about 20% were not). For each of these variables, the sample was divided into two groups. For the continuous variables, we examined results splitting the groups using the 25th, 50th or 75th percentiles as cut-off points. The heterogeneity results are presented in Table 5 only for those sub-populations for which there were statistically signif-

Table 6
Mean vaccination rates by treatment status and double-difference estimates using the administrative data.

	Year	All				Health facility > 5 km away				Mother's education < 4 years			
		Mean		D–D estimate		Mean		D–D estimate		Mean		D–D estimate	
		T	C	OLS	S.E.	T	C	OLS	S.E.	T	C	OLS	S.E.
		<12 months (N = 4596)				<12 months (N = 2163)				<12 months (N = 3407)			
BCG	2000	0.77	0.74			0.72	0.64			0.73	0.70		
	2001	0.92	0.83	0.06+	(0.03)	0.91	0.79	0.04	(0.05)	0.91	0.81	0.07+	(0.04)
	2002	0.93	0.85	0.05	(0.04)	0.92	0.84	0.00	(0.06)	0.92	0.86	0.03	(0.04)
		12–23 months (N = 5390)				12–23 months (N = 2513)				12–23 months (N = 3952)			
BCG	2000	0.90	0.90			0.88	0.88			0.90	0.88		
	2001	0.98	0.90	0.09**	(0.02)	0.98	0.85	0.13**	(0.03)	0.98	0.88	0.09**	(0.02)
	2002	1.00	0.95	0.06**	(0.02)	1.00	0.93	0.07*	(0.02)	0.99	0.94	0.04+	(0.02)
MCV	2000	0.69	0.67			0.61	0.57			0.67	0.65		
	2001	0.93	0.76	0.15**	(0.04)	0.90	0.69	0.17**	(0.06)	0.92	0.75	0.15**	(0.04)
	2002	0.92	0.84	0.06	(0.05)	0.92	0.85	0.02	(0.07)	0.92	0.84	0.05	(0.06)
OPV3	2000	0.82	0.79			0.75	0.68			0.80	0.74		
	2001	0.96	0.85	0.09*	(0.04)	0.95	0.76	0.11*	(0.05)	0.96	0.82	0.08+	(0.04)
	2002	0.98	0.91	0.03	(0.05)	0.98	0.87	0.04	(0.07)	0.97	0.90	0.01	(0.05)
DPT3	2000	0.82	0.77			0.73	0.65			0.79	0.72		
	2001	0.96	0.83	0.09*	(0.04)	0.94	0.75	0.12*	(0.05)	0.96	0.80	0.10*	(0.05)
	2002	0.97	0.90	0.03	(0.05)	0.97	0.84	0.05	(0.07)	0.97	0.89	0.02	(0.05)
FVC	2000	0.60	0.59			0.52	0.46			0.57	0.55		
	2001	0.91	0.68	0.23**	(0.04)	0.88	0.56	0.27**	(0.06)	0.90	0.64	0.23**	(0.05)
	2002	0.92	0.76	0.15**	(0.05)	0.92	0.73	0.13+	(0.08)	0.92	0.76	0.14*	(0.06)

Notes: D–D = double difference; T = treatment; C = control; OLS = ordinary least square estimate; S.E. = standard error. A "+" indicates the OLS estimate is significant at the 10% level, "*" at the 5% level and "**" at the 1% level. The standard errors are in parentheses and are clustered at the locality level. No control variables are included in regressions.

icant differences between those above and below the cutoff for at least two of the vaccines for any of the age groups. They include double-difference estimates for on-time and catch-up vaccination coverage for children living in localities more than 5 km (the median for the sample) from a health facility, and for those children whose mother had less than a fourth grade education (the level at which they should achieve functional literacy). *F*-tests on the fully interacted model showed that the slopes of the coefficients varied by sub-population for some of the vaccines, so results in Table 5 are determined using Eq. (1) estimated for each sub-population.

Double-difference estimates for on-time vaccinations for children living further from a health facility show substantial program effects by 2001: a statistically significant (at the 5% level or lower) 27 percentage points for OPV3, 17 percentage points for DPT3, and 37 percentage points for FVC. By 2002, program effects are larger for DPT3 (27 percentage points), though slightly smaller for OPV3 (22 percentage points) and FVC (31 percentage points). The effect for OPV3 is also no longer significant.

Of particular importance, is that the program largely equalized FVC coverage rates between children living near versus far from a health facility. Before the program, coverage was 54% for the full sample but only 38% for the restricted sample of children living far from a health facility. In 2002, however, coverage rates for the both the full and restricted samples were the same, approximately 85%. Finally, 2002 vaccination rates for those living far from a health facility for OPV3 and DPT3 in the treatment group were greater than 95%, despite having started in 2000 at levels below 65%. These dramatic gains highlight the potential for CCTs to assist countries in reaching vaccination rates over 95% in a short period of time.

For those living further from a health facility within the catch-up group, we also find large and generally statistically significant effects for all the vaccines in 2001 and 2002. Specifically for 2002, these effects were an insignificant 9 percentage points for BCG, and statistically significant (at the 5% level or lower) 29 percentage points for MCV, 28 percentage points for OPV3 and 36 percentage points for DPT3. Using FVC as a summary indicator, *RPS* led to a statistically significant (at the 1% level) program impact of 44 percentage points.[12]

With respect to mother's education, on-time program effects are fairly large and range from 4 to 19 percentage points in 2002 for children whose mother had less than a fourth grade education. However, only the 15 percentage points increase for OPV3 and 19 percentage points increase for FVC are statistically significant (at the 10% level). For the catch-up group, there are statistically significant (at the 5% level) findings for MCV (18 percentage points) and FVC (24 percentage points) by 2002. As with distance to a health facility, comparisons of mean vaccination rates between the full sample (Table 4) and those whose mothers were less educated (Table 5) demonstrate that coverage rates were largely equalized between children with more or less educated mothers.

5.4. Replication of on-time effects using RPS administrative data

It is possible that some of the main program effects in Table 4 lack greater statistical significance due to the relatively small sample sizes in the survey data. To explore this possibility we replicate the analyses using the larger administrative data for children <12 and 12–23 months old and present the results in Table 6.[13]

The results using the administrative data corroborate the patterns found and conclusions made using the survey data. Comparing the mean vaccination rates (in treatment and control groups) between the survey and the administrative data samples, only seven of 36 possible comparisons are statistically different at the 5% level.

[12] Similarly large effects were found using logit models for FVC and for the individual vaccines for which it was possible to use logit models.

[13] Estimates of program effects using the administrative data are more accurately considered treatment-on-the-treated estimates since only beneficiaries are included in those data.

Double-difference estimates of program effects for on-time vaccination are also similar between the two datasets. None of the double-difference estimates reported in Table 6 (left-hand panel) are statistically different at the 5% level from the corresponding survey data estimate of the same effect. While the point estimates are similar, the standard errors are approximately half as large, leading to greater statistical significance for most estimates. The estimates are significant at the 10% level or lower for all vaccines in 2001 and at the 1% level for the FVC summary measure in both 2001 and 2002.

It is also possible with the administrative data to analyze effects for the same sub-populations of interest examined using the survey data. The findings in the two right-hand side panels of Table 6 are consistent with the survey data and demonstrate that the program was generally more effective for children who lived further from a health facility or, separately, whose mother was less educated. Moreover, the vast majority of effects for these sub-groups estimated using the larger administrative data sample are statistically significant at the 10% level or lower.

5.5. Spillover effects

There was a substantial increase in vaccination coverage in both treatment and control groups after *RPS* began (Table 4). This suggests the possibility that there were positive spillover effects of the program to the control group. Such spillovers, while positive benefits of the program, would bias downward the estimated effects presented above.

There are a number of reasons why we might expect spillover effects. The emphasis of the program in the treatment group on preventive health care for children might have led to demonstration effects or information spillovers. This is plausible not only because of their proximity, but also because one-third of the control households were aware of the broad outlines of the program by 2002. A second possible mechanism for the transmission of positive spillovers is the government health care system. Because *RPS* hired private providers to administer the health care components of the program, it is likely that utilization of government health facilities by beneficiaries decreased with the program. Residents in control localities that shared health facilities with residents of treatment localities may have benefited indirectly via shorter wait-times or greater availability of medical supplies in those facilities.

Both of these types of possible spillovers suggest that those in control localities nearer to treatment localities would benefit more than those further away. We examine this possibility by exploiting the fact that localities were randomly assigned treatment and control status within municipalities, creating variation in distance between localities of the opposite type. We estimate Eq. (1) for households in control localities only, replacing the treatment variable with a variable indicating if the control locality was near a treatment locality.[14] We find no consistent differences in the control localities near treatment localities, compared to those further away, using either the survey or the administrative data (results not shown).

6. Measurement error

Several studies have shown that vaccination coverage based on mother's recall rather than actual vaccination cards is biased downwards, and the extent of the bias may be greater for multiple dose vaccines (Valadez and Weld, 1992; Suarez et al., 1997; Langsten and Hill, 1998). At baseline, 24% of households in the survey data did not show a vaccination card, so coverage rates for the sample may be underestimated. If the coverage rates are underestimated in each survey round, and the measurement error was reduced over time in the treatment relative to the control group, the double-difference estimates could be overestimated. Reporting based on vaccination cards increased in the treatment group because the program ensured beneficiaries had up-to-date vaccination cards. The data show the percent of responses not based on vaccination cards was the same across groups in 2000, but slightly lower in the treatment group (4%) than the control group (10%) in 2002.

To determine whether measurement error is biasing our results, we merge together the survey data and the higher quality administrative data at the individual level. First, we examine whether the coverage rates in Table 4 are underestimated in the survey compared to the administrative data. We find coverage rates at baseline for on-time vaccination based on the survey data are slightly higher for the single dose vaccines, but lower for the multi-dose vaccines and 6 percentage points lower for FVC than the administrative data (results not shown).

Second, we assess whether the double-difference estimates are biased upward by comparing separate estimates from the survey and administrative data for those children that matched across the two datasets.[15] We find the double-difference estimates are slightly lower in the survey data than the administrative data, by between 1 and 4 percentage points for on-time vaccination, except BCG which is 10 percentage points lower but not statistically different.[16] While measurement error is likely to be present in the survey data, we conclude that it is not driving the double-difference findings.

7. Discussion

We find positive, fairly substantial, and significant impacts of the Nicaraguan conditional cash transfer program, *RPS*, on vaccination coverage for selected vaccines, age groups and sub-populations. Effects were particularly large for those sub-populations that are traditionally harder to reach—children who live further away from a health facility or whose mothers are less educated. In terms of achieving eradication, on-time vaccination coverage in the treatment group was close to or greater than 95% for BCG, OPV3 and DPT3 by 2002, whereas it remained below 90% for the country as a whole for OPV3 and DPT3 (WHO/UNICEF, 2007).

Using household survey data collected as part of a randomized evaluation, we estimated intent-to-treat double-difference program effects. Five months after the introduction of the health component of the program (2001), *RPS* had led to a significant 20 percentage points increase in on-time coverage rates for fully vaccinated children; this effect remained large (12 percentage points), but was insignificant a year later (2002). The lack of statistical significance of the main program effects is likely due in part to the small sample size. A similar analysis using a larger and better quality administrative dataset, rather than the household survey data, showed a statistically significant estimate for on-time FVC of 15 percentage points by 2002.

We also consider the average effect of *RPS* on older children who received their vaccinations late, the catch-up group. The effect of the program on catch-up was marginally significant (at the 10% level) 15 percentage points increase in FVC in 2002. While effects

[14] We define "near" using various quartile cut-offs of the distance to nearest treatment locality variable.

[15] 71% of the children in the survey data can be matched to children in the administrative data.

[16] The point estimates of the program effects are similar and there is much greater significance when the household survey sample is restricted to those with vaccination cards. This is consistent with random measurement error decreasing the precision of the estimated effects.

differed across individual vaccines, the program had the largest and most significant effects on the FVC summary indicator, for both the on-time and catch-up groups. This provides strong evidence that the program helped protect children against all the major vaccine preventable childhood diseases.

These average effects, while large, also mask important heterogeneous effects for certain sub-populations which typically have lower coverage rates. By 2002, the effect on children of mothers with less than four years of education was 19 percentage points or more for FVC for on-time and catch-up groups. The impact was even larger for children who lived further than 5 km from a health facility (31 percentage points or more for FVC). This resulted in an equalization of vaccination rates across the sub-populations (i.e., those with less versus more educated mothers, or those who lived further away versus closer to a health facility). As a consequence, the program was equity enhancing. These results are similar to those for *Oportunidades* in rural Mexico (Barham et al., 2007), and underscore the ability of CCTs to reach sub-populations for whom supply-side oriented strategies have typically been less successful.

One reason *RPS* did not have an even larger and more significant impact on vaccination coverage is that there was a substantial increase in vaccination rates in the control group during the study period. Unfortunately, there is no clear explanation for this increase. While it is possible that there were positive spillover effects of the program on the control group, we find no evidence of such effects. An alternative explanation is that there was a general strengthening of the Ministry of Health operations in the study area as a result of the program, which may have benefited the control localities indirectly. Moreover, because *RPS* directly hired private providers to administer the health care components of the program, control localities may have benefited from freed-up resources in the region. Since vaccination coverage did not increase at the same pace elsewhere in Nicaragua, we believe, though cannot confirm rigorously, that the increase in vaccination coverage in the control group resulted from these improvements in government health services, and that the estimated impacts on vaccinations are therefore conservative.

As with most CCT programs, *RPS* had both demand- and supply-side components. Unfortunately, it was not possible in our analysis to isolate the effects of these two components. Therefore all the estimated effects for the treatment group result from program-induced changes in both demand and supply. Disentangling the demand- and supply-side effects of CCT programs when they are not built into the design of the experiment is complicated by the difficulty of controlling for supply inputs (e.g., vaccines) and for the strengthening of institutions which accompanies these programs. If possible, future evaluations of CCTs should be designed to isolate the demand- from the supply-side effects.

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