Eradicating Diseases: The Effect of Conditional Cash Transfers on Vaccination Coverage in Rural Nicaragua. [1]

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Abstract: Despite significant global efforts to improve vaccination coverage against major childhood diseases, vaccination rates, even in better performing regions, are expected to plateau below 90 percent. Yet, to eradicate diseases such as measles, vaccination rates close to 95 percent are needed. In this paper, we take advantage of a randomized experiment to investigate the effect of a demand-side approach, which uses conditional cash transfers, to improve vaccination coverage in rural Nicaragua. We investigate possible measurement error which is inherent in vaccination data from household survey and spillover effects using a rich set of administrative data. The findings show the program led to large increases in vaccination coverage resulting in vaccination levels greater than 95 percent for some vaccines. The results were especially large and significant for those children who are harder to reach—those with less educated mothers or who live further from a health facility.

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1. Introduction

Reducing the burden of illness from preventable diseases through vaccination is a key component of global public health policy and human capital formation. Globally, impressive achievements have been made, including the eradication of smallpox in 1977 and reaching global vaccination rates of approximately 75 percent against the major childhood diseases by the mid-1990s.[1] Efforts are under way to eradicate polio and many countries are working to eliminate measles (Miller et al., 2006; Quadros et al., 2003).[2] Though eradication is difficult to achieve, the long-term financial gain can be large. The payoff for eradicating polio is estimated by some to be as high as $1 billion per annum, since it eliminates the need for expensive future prevention, treatment of the afflicted and their lost economic contributions (GPEI, 2003).

Despite these successes, two million children die each year from vaccine preventable diseases (UNICEF, 2005). This is in part because global vaccination levels for major childhood diseases have been static for a decade (Foster et al., 2006), leaving approximately 28 million children worldwide inadequately protected (WHO, 2006). Even the better performing regions, which include Latin America and the Caribbean, are expected to plateau below 90 percent coverage rates for the third dose against Diphtheria-Pertussis-Tetanus (DPT3), the standard indicator for overall immunization program effectiveness (WHO, 2006). Moreover, high average coverage rates in some countries mask important disparities within countries (WHO/UNICEF, 2007). To address this situation, new partnerships have been developed, such as the GAVI Alliance in 1999, to find innovative ways to raise and disburse funds for immunization efforts.

To eradicate diseases such as measles, vaccination coverage rates close to 95 percent are needed (Barrett, 2003). With the experience of the past decade in mind, it would appear that to reach such levels, new strategies may be required. 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 to be vaccinated against that disease, potentially allowing the disease to reappear. Their theoretical model demonstrates that even price subsidies and mandatory vaccination programs may be limited in their ability to eradicate a disease, due to this negative correlation between disease prevalence and the demand for vaccines. Xie and Dow (2005) explore the demand-side empirically, in their assessment of the determinants of child immunization in China. They find that both supply-side factors, such as price, as well as demand-side factors, in particular maternal education, are important determinants at the household level. Most global vaccination strategies, however, focus heavily on improving the supply of vaccinations, including taking services directly to the household during mass vaccination campaigns. The demand-side strategies that are used tend to be limited to awareness raising or social mobilization campaigns, which may miss the hardest to reach populations. Stronger demand incentives may therefore be needed to increase vaccination coverage above 90 percent.

In this paper, we examine how demand-side incentives from conditional cash transfers (CCTs) affect vaccination rates in rural Nicaragua. With vaccination rates greater than 80 percent prior to the program for the country as a whole, it is likely the program will have a greater effect on harder the reach populations. Thus, we explore the average effect for the sample and for important subgroups. The analysis takes advantage of a randomized experimental evaluation of the CCT program in Nicaragua, the *Red de Protección Social* (*RPS*), between 2000 and 2002 to provide rigorous double difference estimates of the program's effects. After two years of

[1] These include tuberculosis, measles, polio, and DPT (diphtheria, pertussis, and tetanus).
[2] In the public health literature, eradication of a disease refers to complete global eradication and elimination refers only to elimination within a particular country.

program operation, there are large program effects especially for typically harder to reach populations such as children who lived far away from a health facility or whose mothers were less educated. In fact, the program pushed coverage rates for on-time vaccination above 95 percent for DPT3 in treatment areas, while they remained at 85 percent in control areas. In contrast, vaccination coverage for the same age group for DPT3 in Nicaragua as a whole when *RPS* began in 2000 was 83 percent and by 2005 had reached only 86 percent (WHO/UNICEF, 2007).

2. Background

2.1 Government vaccination programs in Nicaragua

The Ministry of Health in Nicaragua, as in many countries, has a two-pronged approach to vaccination. First, vaccinations are provided at government health facilities. Because of incomplete coverage of the population by those facilities, as well as the reality that not all families go to the health facility for preventative services, vaccination campaigns are also an important part of the arsenal against the major childhood diseases, typically supported by the World Health Organization (WHO). Month-long vaccination campaigns occurred twice annually during the 1999–2002 period. These two strategies are quite different in their approach. Health facilities rely on individuals bringing their children to the center—which can depend on location and quality, for example as measured by drug and vaccine availability (Xie and Dow, 2005). In contrast, campaigns use volunteers to go house to house to bring the vaccines to the children. Using these two methods, vaccination rates in Nicaragua as a whole prior to the program in 2000 were 83 percent for DPT3, 86 percent for the measles containing vaccine (MCV), 85 percent for the third dose of oral polio vaccine (OPV3), and 96 percent for the vaccine against tetanus, BCG (UNICEF/WHO, 2007).

2.2 CCTs and vaccinations

CCT programs are innovative demand-side interventions that aim to break the inter-generational continuity of poverty by providing cash transfers to poor families, some of which are conditional on the family using preventive health care services. While vaccinations may not be an explicit conditionality in these types of programs, they are almost always required as part of a general preventative health care conditionality. These programs differ from the traditional approaches to health care described above in that individuals are provided with a subsidy to travel to the health facility to receive the vaccination rather than using campaign days to provide vaccinations at an individual's house. In addition, the individual may be more likely to receive other prescribed preventative health care during the vaccination visit in order to meet all the requirements of the health conditionality. It is this embedding of the vaccination incentive within a larger provision of health services as well as the subsidy that may increase the demand for vaccinations above that of the tradition vaccination programs. The first large scale CCT program started in Mexico in 1997. Since then, similar programs have been implemented in more than a dozen developing countries, and others are being planned in locations as dissimilar as Yemen and New York City.

To date, there has been limited investigation of the impact of CCTs on vaccination coverage. Barham et al. (2007) examine the effect of another CCT program in rural Mexico, *Oportunidades,* using data from a randomized experiment. Due to measurement problems for some vaccines, they were limited to examining coverage only against measles and tuberculosis.

As a result of high coverage in Mexico prior to the program, they find limited average program effects, on the order of 3 percentage points. However, they also find heterogeneous effects with respect to distance to a health facility and mother's education. Morris et al. (2004) examine the impact of a conditional voucher program (*PRAF*) in rural Honduras again taking advantage of a randomized experiment. They find significant increases for the first dose of DPT (DPT1) and no effect for measles for the on-time coverage group, but do not investigate DPT3, the catch-up group, or subgroup effects. We expand on the current literature by: 1) investigating the effects of a CCT on all the vaccines against the major childhood diseases; 2) examining the heterogeneity of those effects for a variety of subgroups; and 3) carefully assessing the robustness of the results to measurement error using high-quality administrative data on nearly all children in the randomly assigned treatment and control areas to complement the survey data. This last step is important since vaccination data based on mother reports from household surveys is likely to suffer from systematic measurement error. All of this is done in the context of the second poorest country in Latin America, in which vaccination rates, in contrast to Mexico, have not reached 90 percent.

2.2 The RPS

RPS began in Nicaragua as a pilot program in six rural municipalities in the central region of Nicaragua in late 2000.[3] The purpose of *RPS* was to address both current and future poverty via cash transfers targeted to poor households, conditional on child visits to preventive health care providers and primary school attendance through fourth grade. The program supplemented household income for up to three years with the stated goals of: 1) increasing food expenditures; 2) increasing the health care and nutritional status of children less than five years of age; and 3) reducing primary school drop-out rates. The mother in eligible household received a cash transfer, contingent on bringing children under age five for scheduled well-child health care appointments, attendance by the caregiver at health educational workshops, and sending older children to school. The required health and nutrition services visits included growth monitoring; well-baby care; vaccinations; supplementation for anemia; and provision of anti-parasite medicine. Children less than two years of age were seen monthly and those over two were seen bimonthly. Thus, while vaccination coverage was not an explicit requirement for receipt of the cash transfer, children age five and under were required to attend preventive health care visits during which they received all scheduled vaccinations.

The cash transfers were made bimonthly (every other month) to beneficiaries who met the conditionalities.[4] During the first two years of the program, the average annual family transfer was $272 dollars, or approximately 17 percent of total annual household expenditures before the program. All beneficiary families, regardless of whether there were school-age children in the household, received a food security transfer of $224 a year if the health conditionalities were met. Families who had children between the ages of 7–13 who had not yet completed fourth grade also received a school attendance transfer of $112 per household per year and a per child school transfer for school supplies of $21 per year. The nominal value of the transfer

[3] The Central Rural Region is the poorest in Nicaragua with a poverty rate of 80 percent in 1998 (World Bank, 2001, Annex 19). The municipalities were chosen on the on the basis of information from the health and education ministries, as well as correlates of poverty, such as illiteracy of the adult population. In the six program municipalities 36–54 percent of the rural population in each of the chosen municipalities was extremely poor and 75–90 percent poor in 1998 (Maluccio, 2008).

[4] Maluccio and Flores (2005) describe how compliance is enforced.

denominated in Córdobas remained constant over time, with the consequence that the real value of the transfer declined by about 8 percent due to inflation over the two year period.

Due to weaker public health capacity in the program areas, and the concern that the Ministry of Health could not expand its services that quickly, *RPS* contracted and trained private health providers, including NGOs, to deliver health care services from mobile units (Regalia and Castro, 2006). Beneficiaries were required to use these contracted service providers. Providers visited program areas on pre-planned dates and delivered services in existing health care clinics, community centers, or private homes. As such, those services were at least as close to the beneficiary households as the nearest health care clinic, and often much closer. They were not as close to the house as a typical vaccination campaign, however, which deliver services to the household itself. There was a delay in organizing provision of these services to beneficiaries, so they only became available starting in June 2001. As a result, there was no enforced conditionality relating to the food security transfer during the first 8 months of program operation. All services were provided free of charge to beneficiary households.

3. Evaluation design and data

A 2000–02 evaluation for *RPS* was implemented based on a randomized, locality-based intervention with three annual household-level panel surveys taken in both treatment and control areas, both before (2000) and after (2001 and 2002) the program began. One-half of the 42 localities eligible were randomly selected into the program; thus, there are 21 localities in the treatment area and 21 distinct localities in the control area.[5] Given the geography of these areas, however, control and intervention localities are in some cases adjacent to one another, a feature of the design we incorporate into our analysis. Eligible households in treatment areas received conditional transfers beginning in November 2000, and control areas became program beneficiaries two and a half years later. The delay in implementation in control areas was justified as *RPS* was a pilot program requiring evaluation and lacked the administrative capacity to begin operations in all the designated areas at once.

3.1 Evaluation surveys

The first primary data source we use is the *RPS household census*, carried out in both treatment and control areas before the start of the program, in May 2000. The census collected basic information necessary to incorporate a household in the program as well as basic characteristics of the household and its members including demographics, educational background, housing characteristics, and ownership of assets, but no health or vaccination information. The second primary data source we use is the accompanying *RPS evaluation survey,* the annual household panel survey mentioned above which was carried out for a representative sample of the *RPS* treatment and control areas.[6] The *RPS* evaluation survey sample was a clustered (at the locality level[7]) random sample of all of the 42 localities described above, so that not all beneficiaries in those areas (and in the *RPS* household census) were included. Instead, 42 households were randomly selected from each of the 42 localities using as

[5] See Maluccio (2008) and Maluccio and Flores (2005) for more details on the randomization and targeting of the program.

[6] These surveys were directed by the International Food Policy Research Institute, in collaboration with *RPS*. The data are publicly available, see www.ifpri.org.

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

the sample frame the *RPS* household census, for an initial target sample of 1,764 households. In addition, a locality-level questionnaire was implemented. Overall, 90 percent (1,581) of the sample was interviewed in the first round from late August to early September 2000, with slightly lower completion rates in control localities. The two follow-up surveys were implemented in October 2001 and 2002.[8,9] As a result, the follow-up surveys provide information to estimate the impact of the program on vaccination coverage approximately 5 and 17 months after the May 2000 start of the program's health component. In this paper, we focus on those children less than age three during any one of the annual survey rounds, yielding a total of 2,229 observations of children. The sample is divided fairly evenly between treatment and control areas, although there are approximately 2 percent more children under three years of age in the control areas.

3.2 Dependent variables

The household survey recorded the number of doses each child had received of each of the following vaccines: 1) a Bacille Calmette-Guérin (BCG) vaccine (against tuberculosis); 2) an oral polio vaccine (OPV); a Diphtheria-Pertussis-Tetanus (DPT) vaccine;[10] and 4) a measles containing vaccine (MCV).[11] The interview protocol was to ask first for the child's health card (on which vaccinations are recorded) and when that was unavailable rely on the mother's report; the source of information was also recorded. Following the schedule of vaccinations presented in Table 1, 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 by the time of the survey, and zero otherwise. A child is not considered to be vaccinated against DPT or polio unless they have received their third dose of the DPT vaccine (DPT3) or the oral polio vaccine (OPV3), respectively. A summary measure also was created to determine whether the child was fully vaccinated (FVC) with all four of the vaccines (i.e., was covered with BCG, OPV3, DPT3, and MCV).

The World Health Organization and other public health institutions typically use <12 month and 12–23 month age groups to evaluate up-to-date vaccination coverage for a child depending on the vaccine schedule (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 "on-time", with BCG, MCV, OPV, DPT, and if the child was fully vaccinated (FVC) with all of these vaccines. As shown in Table 1, BCG vaccinations should be given at birth; therefore we use children <12 months of age as the population group for measuring on-time BCG vaccination rates. For MCV, OPV, and DPT vaccination, the 12–23 month age group is used to assess on-time vaccination, because MCV vaccination is scheduled to be given at 12 months of age, and a large proportion of children under 12 months of age will not have received all three doses of OPV or DPT vaccines under the prescribed application schedule at 2, 4, and 6 months of age.

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[8] In 2001, 1,453 (91.9 percent) of baseline households were re-interviewed and in 2002, 1,397 (88.4 percent), on a par with surveys of similar magnitude in other developing countries (Alderman et al., 2001; Thomas, Frankenberg, and Smith, 2001). Attrition rates for children are similar between treatment and control areas in 2001 and vary by 9 percent in 2002. Comparing baseline characteristics of the attriters in treatment and control areas shows that the groups are similar.

[9] An examination of possible contamination in the control sample due, for example to other development programs, finds no evidence for such concerns (Maluccio and Flores, 2005).

[10] Vaccination against DPT may have come from a DPT vaccine or the pentavalent vaccine.

[11] Vaccination against measles may have come from a measles vaccine or a measles, mumps and rubella (MMR) vaccine.

We also explore possible effects on late application of vaccines, the "catch-up" group (e.g., Langsten and Hill, 1998). While on-time vaccination offers better protection (Bolton et al., 1998), "catch-up" still provides public health benefits and is important for eradication. With respect to catch-up, for BCG, the catch-up group comprises children 12–23 months of age and for all the other vaccines, 24–35 months.

3.3 Outcome of the randomization

To guide the empirical specification, we assess how well randomization balanced the treatment and control areas, i.e., whether the randomization was "successful." In Table 2a, we present the differences in means at baseline between treatment and control areas for vaccination rates for the on-time and catch-up groups. In 2000 (before *RPS* began), vaccination rates across treatment and control areas were not statistically significantly different for any of the indicators. Moreover, eight of the 10 measures we examine were very similar, with initial differences of 2 percentage points or less. The remaining two (BCG and OPV3 for the on-time group), however, suggest there was higher coverage, by about 5 percentage points, in the control areas. While these differences are not statistically significant, that may be due to the small sample sizes. To take into account and control for these differences in the vaccination rates at baseline, a double-difference estimator will be used.

In Table 2b, we also examine differences in means at baseline across treatment and control areas for an array of individual, parental, household, and locality characteristics associated with child health care. For the sample of all children under 3 years of age in 2000, differences in means of these important characteristics were statistically insignificant for all but one of 29 the factors examined—mother's age. All of the differences were small in magnitude relative to the overall means, including mother's age with a difference of less than one year compared with an average 27 years. Nevertheless, we control for all these variables in the analyses.

3.4 Administrative data

The expansion of *RPS* into original control areas after two years, combined with the collection for administrative purposes of historical information for beneficiaries (necessary for program operations) yielded vaccination data for nearly all of the original treatment and control children for 2000–02, the same period covered by the household survey. These *RPS* administrative data permit an alternative (to the household survey) assessment of program effects.

A substantial advantage to these data is that they provide information on all children who participated in the program between 2000 and 2004. This includes the entire original treatment group, and all those who were originally in the control group but then eventually participated in the program after they became eligible in 2003. In contrast, the survey was a random sample comprises only approximately 15% of the population. Thus, the administrative data sample size is much larger (9,986) than for the household survey data (2,229).

A second advantage of the administrative data is that they are almost certainly more accurate than the survey data. The historical data were collected by trained health professionals during the first basic examination of a child for program participation. Mothers were instructed to bring their health cards, and a vaccination history was taken to be used in scheduling future health care appointments with that child. Use of vaccination cards to record immunizations was common prior to the program in both treatment and control areas. This data collection process is in contrast to the household survey which necessarily asked about a variety of other things. From

that first point of contact onward, the type and date of application for all vaccines administered to that child were recorded by the health provider and submitted to RPS (and then entered in the administrative data).

At the same time, there are some limitations to using the *RPS* administrative data, which is why we do not incorporate them as our primary results. The most important of these is that it is not possible to replicate the main analyses for catch-up vaccination, because there is no baseline information for 24–35 month olds in 2000, before the program began. Second, unlike the randomly selected household survey, the sample is selected—only individuals who were both eligible and (eventually) participated are included. Assuming the selection process for participation remained similar over time (which seems reasonable since participation rates were on the order of 90 percent for both groups), however, this is unlikely to have a large effect on the estimates. In some measure, estimates of program effects using these data are more accurately considered treatment-on-the-treated estimates and, as a result, our expectation is that they would be slightly larger than the intent-to-treat estimates based on the household surveys.

A comparison with the *RPS* census data of both treatment and control areas from early 2000 indicates that virtually 100 percent of the households merge perfectly between the census and the *RPS* administrative database.[12] This allows us to combine census information (in particular, characteristics at the household level) with the child level administrative data.

3.5 Matched survey-administrative data

The final data set we work with merges children data for a given child in the household survey with the administrative data. This allows an assessment of differences in reported vaccination coverage from the two sources. While the household-level merge between the two data sources is perfect, this is not the case at the individual child level. Nevertheless, 75 percent of the children in the household survey data in 2000 can be matched to children in the census. It is likely that the remaining 25 percent are in fact in the administrative data, but for various programmatic reasons their person-level identification numbers changed between the time of the census and the time they entered the program. (In contrast, household identification numbers were fixed throughout the process.) There is disagreement on vaccination status in about 10–20 percent of the cases, with multiple dose and FCV measures at the higher end of that range. Disagreement is lower, however, when one examines the more recent measure in 2002. This is consistent with a reduction in the proportion of reports based on mother reports only.

4. Methods and Empirical Model

4.1 Analysis groups and methods

The objective of our analysis is to estimate the intent-to-treat effect of *RPS* on vaccination coverage for children under the age of three. We evaluate whether a child was vaccinated by the appropriate age, or "on-time", or a year late "catch-up" with BCG, MCV, OPV, DPT, and if the child was fully vaccinated (FVC) with all of these vaccines. Random assignment was employed

[12] This match is lower (about two thirds) for individual children, but is likely because children's identification numbers may have changed across the databases, a hypothesis supported by the nearly equal numbers of individuals that appear to be in the census but not the administrative data, and vice versa. As a result, in what follows, we use the complete sample from the *RPS* administrative database; limiting the sample to only those that merges does not change the results substantively.

to create a control group as the counterfactual. The advantage of using randomization is that, when successful, treatment and control areas will have the same observed and, more importantly (since they are difficult to control for), unobserved characteristics, on average, which removes selection bias in program participation. In section 3.3, we demonstrated that the randomization was indeed successful for the *RPS* evaluation. Despite that success, there were some small (and statistically insignificant) differences in baseline vaccination rates. To take these baseline differences into account fully, a double-difference estimator will be employed. This estimator compares the change in the mean coverage rates for a specific vaccine in the treatment group before and after the intervention, to the change in mean coverage rates in the control group over the same period. By comparing changes, the estimator controls for 1) characteristics that do not change over time within treatment and control groups, as well as 2) characteristics that do change over time, but in the same way in each of the groups (the so-called common trends assumption).

4.2 Empirical specification

We use the double-difference estimator to determine the average intent-to-treat effect of the program. Using ordinary least squares (OLS) to estimate a linear probability model,[13] the final regression equation is:

(1) $V_{ict} = \mu_c + \beta_1\, 2001_t + \beta_2\, 2002_t + \beta_3 T_c + \delta_1\, T_c * 2001_t + \delta_2\, T_c * 2002_t + \boldsymbol{X}'\lambda + \varepsilon_{ict}$, where

μ_c = locality-level fixed effect
V_{ict} = 1 if child i from locality c 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_c = 1 if program intervention in locality c and zero otherwise,
$\boldsymbol{X}$ = vector of baseline individual, parental, and household variables, and
ε_{ict} = unobserved idiosyncratic error (assumed to be uncorrelated with all other variables).

The parameters of interest are δ_1 and δ_2; δ_1 is the double-difference estimator of the effect for 2001 (relative to 2000) and δ_2 for 2002 (relative to 2000). The program effects are identified by the randomized design. Because we do not condition on actual program participation, but only on whether the household resides in a treatment locality, the estimates reflect the "intent-to-treat" average effect of the program (Burtless, 1995). Due to the randomization of T_c, it (and any interactions involving it) should be uncorrelated with all observed or unobserved individual-, parental-, or household-level variables, so that the δs are consistently estimated. Given the randomization was successful, it is not necessary to include other variables in this regression for the consistency of the estimator for δ_1 and δ_2, though doing so provides a further check on the randomization, and may increase the precision of the estimates. The baseline controls (the vector $\boldsymbol{X}$) include all the individual, parental, and household variables summarized in Table 2b.

In all of the analyses, we include children for whom data are complete in any of the survey rounds. Since we focus on one-year age groups and the surveys are approximately a year apart,

[13] Non-linear models such as probits or logits that use maximum likelihood methods are often employed if the dependent variable is binary. When vaccination rates are close to or equal to one for certain groups, these models provide unreliable estimates because the probability is perfectly or almost perfectly predicted. Also with such models, observations are dropped if a certain subgroup is completely vaccinated. Since many of the vaccination rates are close to 100 percent, we use a linear probability model. Where possible we have compared results with logit models and do not find substantive changes in the results.

each child can only show up once in any given on-time analysis (e.g., BCG for 0–12 month olds), but can show up again for a given catch-up analysis (e.g., BCG 12–23 months). So while the children are drawn from the household panel survey, we do not estimate using a panel of children (since they age out of the analysis groups under consideration). Standard errors are calculated allowing for heteroskedasticity and for clustering at the locality level.[14]

5. The impact of *RPS* on vaccination

Table 3 presents the mean vaccination rates by survey year for treatment and controls areas, as well as the double-difference estimated impacts of *RPS* for on-time and catch-up vaccination for each of the vaccines. Due to the delay in the health component of *RPS*, the estimated effects of the program in 2001 and 2002 measure the impact of the program approximately 5 and 17 months after implementation.

5.1 On-Time Effects

In the left-hand-side panel of Table 3, we present the mean vaccination rate (expressed as a proportion), associated standard error, and number of observations separately for the treatment and control areas for each survey year. On-time vaccination coverage across the various vaccines rose dramatically in treatment areas from a starting level of 54–77 percent in 2000 to 86–97 in 2002. At the same time, however, there was a substantial rise in vaccination rates in control areas. For example, coverage for BCG at rose from 82 percent at baseline to 91 percent in 2001. Nevertheless by 2002, only in the treatment areas did vaccination rates for all vaccines except MCV reach levels at or near 95 percent, the rate considered needed for eradication of some vaccines.

In the right-hand-side portion of Table 3, we present the intent-to-treat double-difference estimates, starting with the unconditional (i.e., without any controls) estimate and its associated standard error. Next, we condition the estimate on an array of individual, parental, household, and locality characteristics,[15] all measured at baseline, as well as municipal-level fixed effects. The latter are included to control for all observed or unobserved time-invariant municipal variables. Comparing these two sets of estimates, the impacts of the program on vaccination coverage for each of the vaccines for 2001 and 2002 remain fairly constant (all but a few are within 2 percentage points) when controls are included. This provides further evidence that the randomization was successful. Lastly, the final set of estimates replaces the municipality-level fixed effects with locality-level fixed effects (and drops the locality-level variables), thus also controlling for all time-invariant characteristics of each locality. We base our discussion on this final specification. While they have the significant advantage of controlling for observed and unobserved time-invariant factors at the locality level, in many instances, these locality-level fixed-effects regressions are less significant, likely due to somewhat small samples sizes within localities that are providing the "within" variation needed for estimation. In this sense, we highlight the most conservative of our estimates.

[14] We do not report as our main results weighted regressions that account for sample design (Section 3.1), since they are inconsistent and less efficient than OLS (Deaton, 1997). Results are similar, however, when regressions are weighted by sample probabilities.

[15] Control variables include all variables presented in Table 2b.

By the 2001 survey, the double-difference estimates of the effects of *RPS* on on-time vaccinations were 10 percentage points for BCG, 11 percentage points for MCV and OPV3, 1 percentage point for DPT3, and 14 percentage points for FVC. While all but one of these program impacts are fairly large, only the summary measure, FVC is statistically significant at a 10 percent level.

A year later, *RPS* had led to 7–13 percentage point increases in vaccination coverage for all individual vaccines, except for MCV. The impact on MCV was only 4 percentage points. However, none of these findings is significantly different from zero. Using FVC as a summary indicator, the results show a statistically insignificant program effect of 14 percentage points in 2002, a 26 percent improvement as measured against the initial coverage of 54 percent. So, while statistically insignificant, these are substantial effects, particularly for such a short period of time.

One pattern underlying the FVC results in 2002 is the decline in program effects for MCV between the first and second survey rounds from 11 percentage points to 4 percentage points. This drop is related to a reduction in MCV coverage in treatment areas from 91 to 87 percent, and a simultaneous increase in MCV coverage in treatment areas from 75 to 83 percent in control areas. We are unable to explain these changes in coverage rates for MCV based on the household survey; it is possible that they are due to measurement error. We reconsider them in Section 5.4 when analyzing the administrative data.

5.2 Catch-up effects

With regard to catch-up, Table 3 shows that, with the exception of MCV, the impact of the program on the catch-up group was similar between the two survey rounds. The double-difference estimator of the effect of *RPS* on BCG was marginal and statistically insignificant in 2001 and 2002, likely due to high initial coverage rates in treatment areas, allowing little room for improvement. Results for OPV3 and DPT3 show statistically insignificant impacts of approximately 8 percentage points. Between the two survey periods, the pattern of program effects for FVC is driven by MCV. For these two vaccines the program impact increases from an insignificant 3 and 7 percentage points in 2001, to a statistically significant 13 and 17 percentage points in 2002 for MCV and FVC. The lack of an effect in 2001 for MCV was due to a large increase in coverage for MCV among the controls from 86 (in 2000) to 95 percent (in 2001). However, the coverage rate in the control areas dropped to 87 percent in 2002, which resulted in a program impact of 12 percentage points. (As explained above, we are unable to explain the patterns in coverage for MCV in control areas.) Given an initial coverage rate of 68 percent for FVC in treatment areas, the program impact in 2002 represents a statistically significant 25 percent increase in full vaccination coverage. As with the effects for on-time vaccination, these are substantial effects in a short period of time.

5.3 Heterogeneity effects for subpopulations

Average effects for the whole population may mask important heterogeneous effects. In the case where effects are large for one subgroup but relatively small for another, they also may result in insignificant overall average estimated effects. The differential impact of the program on subgroups of the population is examined using equation (1) for each subgroup. In particular, we investigate how the program effects differ by pre-program household per capita expenditures,[16] maternal education levels, presence of a health facility[17] in the locality, distance

[16] As is common in the literature, we use expenditures as a proxy for income.

from the population center of the locality to the nearest health care facility,[18] and whether the locality was accessible by a road (about 20 percent were not). Results for those vaccinations and subgroups for which there were statistically significant results are presented in Table 4. These 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).

Double-difference estimates for on-time vaccinations for children living more than 5 km away from a health facility show substantial program effects by 2001: a statistically significant 21 percentage points for OPV3 and 26 percentage points for FVC. The results for FVC mean that *RPS* led to a 68 percent increase in the percentage of children 12–23 months who were fully vaccinated, from an initial level of 38 percent in treatment areas. By 2002, program effects are even larger for OPV3 (30 percentage points) and FVC (39 percentage points), and also are seen for DPT3 (36 percentage points). The result for FVC represents a 92 percent increase over initial levels in vaccination coverage. In addition, the program appears to have had an equalizing effect between children living near or far from a health facility. This is illustrated by comparing 2000 and 2002 FVC rates for the full sample in Table 3 (54 and 86 percent) to those children who live far from a health care clinic in Table 4 (38 and 85 percent). Finally, 2002 vaccination rates for OPV3 and DPT3 in treatment areas were greater than 95 percent, despite having started in 2000 at levels below 65 percent. These dramatic gains highlight the potential for CCTs to assist countries in reaching vaccination rates over 95 percent in a short period of time.

In 2001, the findings for those who live further than 5 km from a health facility for the catch-up vaccination are similar to those for on-time vaccination. Program impacts were large and statistically significant. In particular, they were 25 percentage points for OPV3, 30 percentage points for DPT3, and 30 percentage points for FVC. By 2002, effects had increased further for each vaccine type, and become significant for MCV coverage (32 percentage points) as well. The large increase in the impact for MCV, however, is related to the decline in vaccination rates in the control areas between 2001 and 2002, rather than a large increase in coverage for the treatment areas. Using FVC as a summary indictor, the *RPS* program led to a nearly 100 percent increase in vaccination coverage over two years, and a program impact of 50 percentage points, for those living far from a health facility.

Lastly, on-time program effects were fairly large and ranged from 5 to 17 percentage points in 2002 for children whose mother has less than a fourth grade education. However, only the 17 percentage point increase for OPV3 is statistically significant (Table 4). For the catch-up group, there are statistically significant findings for MCV (21 percentage points) and FVC (24 percentage points) by 2002. The latter result represents a 38 percent increase in FVC among 24–35 month olds. Similarly to distance to a health facility, comparison of rates between the full sample (Table 3) and those whose mothers were less educated (Table 4) illustrates that coverage rates were largely equalized between children living with more or less educated mothers.

5.4 Estimates using administrative data

In addition to heterogeneity of effects, another reason the main program effects in Table 3 lack greater statistical significance may be due to small sample sizes and large and unexplained

[17] A health facility could be a health post or health clinic, hospital, or a place to visit a doctor.

[18] Distance is a measure of the approximate distance in km if one were to walk from the center of the most populous community or residential area in the locality to the nearest health clinic.

changes in MVC coverage described above. To explore this possibility we replicate the on-time analyses presented in Table 3 using the *RPS* administrative data (Section 3.4) and present the results in Table 5.[19]

These results corroborate the patterns found and conclusions made using the household survey data. Comparing the mean vaccination rates (in treatment and control areas) between the household survey and the administrative data samples, only six of 30 possible comparisons are not within a 95 percent confidence interval of each other (using standard errors from the administrative data, with much larger samples). In particular, on-time MCV coverage in 2002 is 92 percent in the administrative data in contrast to 87 percent in the survey data, and catch-up MCV coverage in 2001 is 86 in the administrative data and 95 in the survey data. This leads us to believe that the wide swings seen in MCV rates in the survey data that lead to lower double-difference estimates for MCV and consequently FVC are a result of measurement error in the survey data. In addition, reported coverage for DPT3 is consistently higher in the administrative data, particularly at baseline, also suggesting more difficulty in measuring coverage for that vaccine.[20]

Double-difference estimates of program effects for on-time vaccination are also similar between the two databases. All of the 15 double-difference estimates reported in Table 5 fall within the 95 percent confidence interval of the corresponding household survey estimate of the same effect (using standard errors from the administrative data). While the point estimates are similar, the significance of the double-difference estimates are improved and are significant at the 5 percent level or higher for all vaccines in 2001 and for the FVC summary measure in both post-intervention years.

Finally, it is also possible with the RPS administrative data to analyze effects for subgroups of interest. The finding in the bottom two panels of Table 5 are consistent with the survey data and demonstrate that the program was more effective in areas where a health facility was more than 5 km away or for children whose mothers had less than a fourth grade education. The mean vaccination rates tend to be higher in the administrative data especially in 2002. The double difference estimates are for the most part similar or lower (which is probably more reasonable) in the administrative data, but the vast majority of effects for these subgroups are statistically significant using the larger administrative data sample. Using FVC as the summary indicator, in 2002 the program effect for children who live more than 5km from a health facility is 15 percentage points in the administrative data as compared to 39 percentage points in the survey data. Both of these estimates are statistically significant at the 10 percent level or higher. For children whose mothers had less education, the program impact is almost the same in 2002 between the two databases (17 versus 16 percentage points), however, the effect is only statistically significant using the larger administrative data.

[19] Three control variables not available for inclusion as controls in this analysis are: whether any animals were owned; the value of durable assets; and per capita expenditures. For the latter, we instead include a predicted measure of per capita expenditures based on household characteristics, described in Maluccio (2008).

[20] For both OPV and DPT, the questionnaire asks if the child has been vaccinated against the disease(s) and then asks the number of doses, conditional on answering yes. For DPT, however, the word "triple" is included in parentheses after the question about whether the child had been vaccinated. Despite training the two questions in the same fashion, it is possible that in the field some enumerators incorrectly indicated as the number of doses a "1" (meaning one triple) rather than "3", leading to an underestimate of those completely covered.

6. Empirical Issues

There are two important concerns with the main findings. One is that vaccination data collected based on recall is known to suffer from measurement error. Research shows that coverage rates are usually under-reported in these cases (Langsten and Hill, 1998; Suarez et al., 1997). Indeed, the erratic movement of MCV rates in 2002 for on-time vaccinations in the treatment areas and for the catch-up group in the control area may be examples of measurement error in the household data. The other concern is that spillover effects into control areas may bias our results. Indeed, the increase in coverage in control areas may be a result of an informational or demonstration spillover. We again take advantage of the administrative data, as well as information from a GIS database, to determine whether measurement error or spillover effects are biasing our results.

6.1 Measurement error

It is widely agreed that the most reliable method for assessing national coverage of vaccines is via representative household surveys rather than officially reported statistics based on records of vaccination doses supplied (Murray et al. 2003). At the same time, several studies have shown that household surveys tend to underestimate vaccination coverage, to the extent that the data is based on mother's recall rather than actual vaccination cards. Given that 24 percent of households in the baseline survey did not show a vaccination card, measurement error may result in underestimated levels of vaccination rates reported in Table 3. In addition, this type of measurement error may lead to an over-estimation of the double differences results if the measurement error was reduced over time in treatment areas relative to control areas as a result of the program. Given that the program ensured that participant children had an up-to-date vaccination card, it is not surprising that the percent of responses not based on vaccination cards was lower in treatment areas (4 percent) than in control areas (15 percent) in 2002. Several robustness tests are preformed below in an effort to determine if the results suffer from these biases.

To examine whether the vaccination levels in Table 3 are underestimated we compare the vaccination status of children appearing in the matched *RPS* administrative and household survey data set (section 3.5). As outlined in section 3.4, the *RPS* administrative data are more accurate than the survey data because they were collected by trained health professionals or from health provider records. The use of data collected from medical provider records is the gold standard in studies investigating the validity of reported vaccinations in other types of surveys (Suarez et al. 1997).

Using children in the matched survey-administrative data, we compare the survey and administrative data estimates of average coverage at baseline for on-time vaccination. Consistent with the usual bias found in the presence of measurement error, we find that coverage rates for FCV are 6 percentage points higher using the administrative data than the survey data report. The gap is even larger for DPT3 (12 percentage points), one of the vaccines about whose measurement at baseline we are most concerned. There are only minor differences when these comparisons are made at the treatment versus control area level. It would seem that, if anything, our estimates of coverage from the household survey are biased downward, consistent with other literature and making our results regarding reaching high levels of coverage with RPS conservative.

To assess whether the double-difference estimates are overestimated we compare the survey data and the administrative data for those children that matched in the two data bases. The

double-difference estimates are lower, but not statistically different, in the survey data than in the administrative data.[21] For example, the estimates using locality fixed-effects for FVC for 2001 and 2002 in the administrative data are 27 and 24 percentage points as compared to 17 and 21 percentage points in the survey data. While measurement error is likely to be present in the survey data, we conclude that it is unlikely to be driving any of the double-difference findings presented above.

6.2 Spillover effects

There was a substantial increase in vaccination coverage in both treatment and control areas post-intervention (Table 3). This raises the possibility that there were spillover effects of the program to control areas. For example, the emphasis of the program in treatment areas on preventive health care for children might have led to demonstration effect or informational spillovers. This is plausible not only because of their proximity, but also because approximately one-third of the control households were aware of the broad outlines of the program. A second possible mechanism via which positive spillovers might have occurred is through the governmental health care system. Because *RPS* directly hired private providers to administer the health care components of the program, it is likely that utilization of (other) government health care facilities by beneficiaries decreased with the program. Control localities that shared health clinics with residents of treatment localities, may have indirectly benefited (via shorter wait-times or greater availability of medical provisions in those clinics).

Both of these types of possible spillovers suggest that control areas that were nearer to treatment areas would benefit more than those that were further way. Exploiting the geographic nature of the evaluation, in which localities were randomly allocated to treatment and control areas within municipalities, and are therefore at times nearer or farther away from a locality of the opposite type, we can examine this hypothesis. Using GIS locator information for the localities, we first calculate the distance from the geographic center of each control locality to the nearest treatment locality. Our hypothesis is that if there were spillover effects of the program to control areas, then they are likely to be larger for control areas that were nearer treatment areas than those that were further away. One way to explore this is to examine whether trends over time in the control groups were different for those control localities nearer to a treatment locality. To do this, we estimate a regression parallel to the second double-difference specification in Table 3, but using households in control localities only and including as additional regressors an indicator of whether the locality was near a treatment locality, and interactions of that indicator with each of the year dummy variables. We find no consistent tendency of changes in vaccination over time in control areas near treatment areas, relative to those further away, using either the household survey data or the administrative data (results not shown).[22]

7. Discussion

We find positive, fairly substantial, and significant program impacts of the Nicaraguan conditional cash transfer program, *RPS*, on vaccination coverage for selected vaccines and

[21] We also examine how the survey results in Table 3 differ if we limit the sample to only those who showed a vaccination card. While the point estimates are similar, there is much greater significance when the sample is restricted to those with vaccination cards.

[22] We define "near" using various cut-offs, including the 33rd and 66th percentiles of the distance to nearest treatment locality variable, both separately and in the same regression.

specific sub-populations. Effects were particularly large for those groups that have been traditionally harder to reach—children who live further away from a health facility or whose mothers were less educated. Results using a household survey are corroborated with analysis of program administrative data. In terms of eradication, what is striking about the *RPS* program is that on-time vaccination coverage in the treatments areas was close to or greater than 95 for BCG, OPV3 and DPT3 by 2002, while OPV3 and DPT3 remained below 90 percent for the country as a whole (UNICEF/WHO, 2007).

Using a double-difference estimator, the study shows that five months after the introduction of the health component of the program (by the 2001 survey), the *RPS* led to a significant 14 percentage point (26 percent) increase in on-time coverage rates for fully vaccinated children. Seventeen months after the introduction of the health component (by the 2002 survey), the program impacts are lower but still fairly large, ranging from 4 to 13 percentage points depending on the vaccine. These effects, however, are not statistically significant. The decline in impact between 2001 and 2002 is mainly due to an increase in coverage rates in control areas. The lack of statistical significance of the main program effects is likely in part due to the small sample size. A similar analysis using a larger administrative database rather than the evaluation survey data, found statistically significant estimates for FVC of over 18 percentage points (30 percent) by 2002.

The effects of the program on catch-up were insignificant after the first five months of program operations, but significant after 17 months for MCV and FVC. Double-difference estimates show an approximately 13 percentage point (15 percent) increase in MCV coverage, and a 17 percentage point (25 percent) increase in FVC by 2002.

These average effects, however, mask important heterogeneous impacts for certain populations. In particular, by 2002 children of mothers with less than four years of education experienced an increase in on-time vaccination coverage of 17 percentage points (23 percent) for OPV3, and in catch-up vaccination coverage of 21 and 24 percentage points (23 and 33 percent) for MCV and FVC. In addition, the program impact is at least 28 percentage points (at least 39 percent) for children who lived more than five kilometers from the nearest health post or clinic for on-time OPV3, DPT3 and FVC coverage, and for all vaccinations for the catch-up group. It is encouraging that the results show vaccination coverage improves more for children who may be most disadvantaged in the sense that their mothers are less educated and they live further away from clinics. These appear to be the populations most affected by demand-side incentives.

One reason *RPS* did not have a larger and more significant impact on vaccinations for the aggregate effects (particularly in the second year) is the substantial increase in vaccination rates in control areas during the study period. Unfortunately, there is no clear explanation for this increase. While it is possible that the control group areas heard of the program and there were positive spillover effects of the program on the control group, our examination of such possible effects provided no evidence of them. Instead, the general strengthening of the Ministry of Health and coordination of its efforts in the areas, which was an explicit objective of *RPS,* may have benefited control areas 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. We believe that, to the extent that control communities also benefited from the *RPS*'s efforts to improve the functioning of the Ministry of Health, the estimated impact on vaccinations would be downward biased, making the relatively large results reported here conservative.

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 are necessary for the credibility of the CCT programs, since

conditionalities cannot be met if there are no services, and cannot be met by all if there are inadequate services, as is common in the underserved areas these programs typically target. Even in places with currently adequate supply, the increased utilization brought about by CCT programs could lead to crowding out and a decline in quality if that supply does not adjust for the increased demand.

It was not possible in our analysis to control for potential supply-side effects, such as vaccine supply, on immunization coverage rates. Therefore all the estimated impacts above represent changes in both demand and supply. We believe, though cannot confirm empirically (other than comparing with the rest of Nicaragua where no such increases were seen), that the increase in immunization coverage in control areas is a result of the improvements in government health services to those areas. Under the assumption that the large rise in vaccination rates in the control areas represents the supply-side program effect, however, one could argue that the estimates presented above do in fact represent demand-side effects. More likely, however, they are due at least in part to improvements in supply, as well. Disentangling the demand- and supply-side effects when they are not built into the design of the experiment is complicated by both the lack of data on the full range of supply-side issues and the difficulty in controlling for the strengthening of institutions which accompanies these programs. Where possible, future experiments using CCTs should make efforts to account for supply-side changes.

Tables

Table 1: Basic Vaccination Schedule for Vaccinations in Nicaragua

Vaccine	Dose	Ages Given
BCG [1]	1	At birth
OPV	3	2 - 6 months
DPT[2]	3	2 - 6 months
MCV[3]	1	12 months

Notes:

1. BCG is the vaccine given against Tuberculosis.

2. Participants need to have received 3 doses of DPT or the Pentavalent vaccine (or a combination of both) to be immunized against DPT. The DPT vaccine was the main vaccine being provided prior to 2000. After 2000, the Nicaraguan government switched to pentavalent.

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

Table 2a: Difference in Baseline Vaccination Rates by Treatment Status

	Treatment Area			Control Area			Difference		
	Mean	SE	Obs	Mean	SE	Obs	Diff.	SE	T-Stat
On-Time Vaccinations									
BCG	0.77	(0.06)	125	0.82	(0.04)	130	-0.06	(0.07)	-0.75
MCV	0.70	(0.05)	164	0.69	(0.06)	142	0.01	(0.08)	0.15
OPV3	0.76	(0.05)	164	0.80	(0.06)	142	-0.05	(0.07)	-0.64
DPT3	0.68	(0.05)	164	0.67	(0.06)	142	0.01	(0.08)	0.19
FVC	0.54	(0.06)	164	0.55	(0.05)	142	-0.01	(0.08)	-0.16
Catch-Up Vaccinations									
BCG	0.95	(0.02)	164	0.93	(0.02)	142	0.02	(0.03)	0.73
MCV	0.85	(0.04)	146	0.86	(0.04)	155	-0.01	(0.05)	-0.16
OPV3	0.82	(0.05)	146	0.85	(0.05)	155	-0.02	(0.07)	-0.32
DPT3	0.75	(0.06)	146	0.75	(0.05)	155	-0.01	(0.08)	-0.11
FVC	0.68	(0.06)	146	0.66	(0.05)	155	0.01	(0.08)	0.16

Notes: The standard errors (SE) are clustered at the community level. Obs = observations, T-Stat = t-statistics, Diff = difference.

Table 2b: Difference in Baseline Means of Control Variables by Treatment Status

	Treatment Area			Control Area			Difference		
	Mean	**SE**	**Obs**	**Mean**	**SE**	**Obs**	**Diff.**	**SE**	**T-Stat**
Individual Characteristics									
Age in months	18.39	(0.44)	435	18.59	(0.50)	431	-0.20	(0.66)	-0.29
Male (=1)	0.50	(0.02)	435	0.50	(0.03)	431	0.00	(0.04)	0.13
Household Characteristics									
Block Wall (=1)	0.14	(0.04)	435	0.13	(0.03)	431	0.01	(0.05)	0.17
Dirt Floor (=1)	0.84	(0.04)	435	0.82	(0.03)	431	0.02	(0.05)	0.34
Zinc Roof (=1)	0.53	(0.06)	435	0.53	(0.06)	431	0.00	(0.08)	0.02
Tile Roof (=1)	0.28	(0.08)	435	0.32	(0.07)	431	-0.04	(0.10)	-0.42
Number of rooms in house	1.40	(0.06)	435	1.49	(0.06)	431	-0.09	(0.08)	-1.06
Owns house (=1)	0.74	(0.05)	435	0.76	(0.04)	431	-0.03	(0.06)	-0.41
Latrine in house (=1)	0.54	(0.05)	435	0.47	(0.04)	431	0.07	(0.07)	1.02
House has electricity (=1)	0.20	(0.04)	435	0.18	(0.04)	431	0.03	(0.06)	0.47
Piped water into house (=1)	0.01	(0.01)	435	0.03	(0.01)	431	-0.01	(0.02)	-0.76
Value of durable assets	368	(122)	435	306	(65)	431	(61)	(138)	0.45
Land owned (square meters)	14,159	(967)	435	15,231	(1648)	431	-1072	(1911)	-0.56
At least one animal (=1)	0.12	(0.02)	435	0.13	(0.02)	431	-0.01	(0.03)	-0.37
Per capita expenditures	3081	(199)	435	2975	(140)	431	107	(244)	0.44
Father literate (=1)	2.22	(0.07)	435	2.23	(0.04)	431	-0.01	(0.08)	-0.17
Mother literate (=1)	2.09	(0.09)	435	2.04	(0.06)	431	0.05	(0.11)	0.46
Years of education father	1.77	(0.16)	435	1.74	(0.10)	431	0.03	(0.19)	0.16
Years of education mother	2.05	(0.20)	435	2.15	(0.17)	431	-0.10	(0.26)	-0.39
Age father	33.54	(0.59)	435	34.43	(0.34)	431	-0.89	(0.68)	-1.32
Age mother	26.80	(0.27)	435	27.66	(0.29)	431	-0.86	(0.40)	-2.18
Household size	7.24	(0.26)	435	6.91	(0.14)	431	0.32	(0.30)	1.09
Locality Characteristics									
Doctor (=1)	0.15	(0.08)	435	0.16	(0.09)	431	-0.01	(0.12)	-0.09
Nurse (=1)	0.41	(0.11)	435	0.33	(0.11)	431	0.08	(0.16)	0.53
Pharmacy (=1)	0.04	(0.04)	435	0.06	(0.06)	431	-0.02	(0.08)	-0.28
Health clinic (=1)	0.47	(0.12)	435	0.35	(0.11)	431	0.11	(0.16)	0.71
Distance to health clinic (=1)	8.20	(1.79)	435	6.05	(1.15)	431	2.15	(2.13)	1.01
Road access (=1)	0.78	(0.10)	435	0.80	(0.09)	431	-0.02	(0.14)	-0.16
KM to public transport (=1)	5.14	(1.29)	435	4.06	(0.89)	431	1.08	(1.57)	0.69

Notes: The standard errors (SE) are clustered at the community level. Obs = observations, T-Stat = t-statistic, and Diff = difference. Per capita expenditures and value of durable assets are in 2000 cordobas.

Table 3: Vaccination Rates and Double Difference Results by Year

	Year	Treatment Area			Control Area			Double-Difference Estimates					
		Mean	SE	Obs	Mean	SE	Obs	OLS	SE	OLS	SE	OLS	SE
On-Time Vaccination													
BCG	2000	0.77	(0.06)	125	0.82	(0.04)	130						
	2001	0.95	(0.02)	111	0.91	(0.03)	123	0.09	(0.07)	0.10	(0.07)	0.10	(0.08)
	2002	0.93	(0.03)	76	0.91	(0.02)	89	0.08	(0.07)	0.06	(0.07)	0.07	(0.08)
MCV	2000	0.70	(0.05)	164	0.69	(0.06)	142						
	2001	0.91	(0.03)	116	0.75	(0.05)	121	0.15+	(0.09)	0.14+	(0.08)	0.11	(0.09)
	2002	0.87	(0.04)	92	0.83	(0.03)	124	0.03	(0.09)	0.05	(0.08)	0.04	(0.09)
OPV3	2000	0.76	(0.05)	164	0.80	(0.06)	142						
	2001	0.96	(0.02)	116	0.87	(0.05)	121	0.14*	(0.07)	0.12+	(0.07)	0.11	(0.07)
	2002	0.97	(0.02)	92	0.90	(0.03)	124	0.11	(0.08)	0.14*	(0.08)	0.13	(0.08)
DPT3	2000	0.68	(0.05)	164	0.67	(0.06)	142						
	2001	0.91	(0.03)	116	0.85	(0.05)	121	0.05	(0.07)	0.03	(0.07)	0.01	(0.07)
	2002	0.97	(0.02)	92	0.85	(0.03)	124	0.10	(0.08)	0.12	(0.08)	0.11	(0.08)
FVC	2000	0.54	(0.06)	164	0.55	(0.05)	142						
	2001	0.84	(0.03)	116	0.65	(0.06)	121	0.21*	(0.08)	0.17*	(0.08)	0.14+	(0.08)
	2002	0.86	(0.04)	92	0.75	(0.04)	124	0.12	(0.09)	0.15	(0.09)	0.13	(0.09)
Catch-Up Vaccinations													
BCG	2000	0.95	(0.02)	164	0.93	(0.02)	142						
	2001	0.99	(0.01)	116	0.96	(0.02)	121	0.01	(0.03)	0.00	(0.03)	-0.01	(0.04)
	2002	1.00	(0.00)	92	0.97	(0.02)	124	0.01	(0.04)	0.01	(0.04)	0.01	(0.04)
MCV	2000	0.85	(0.04)	146	0.86	(0.04)	155						
	2001	0.94	(0.02)	150	0.95	(0.02)	130	0.00	(0.05)	0.01	(0.05)	0.03	(0.06)
	2002	0.98	(0.01)	108	0.87	(0.04)	123	0.12*	(0.06)	0.11+	(0.06)	0.13+	(0.07)
OPV3	2000	0.82	(0.05)	146	0.85	(0.05)	155						
	2001	0.99	(0.01)	150	0.94	(0.02)	130	0.07	(0.07)	0.07	(0.07)	0.08	(0.07)
	2002	1.00	(0.00)	108	0.93	(0.03)	123	0.09	(0.07)	0.08	(0.07)	0.08	(0.07)
DPT3	2000	0.75	(0.06)	146	0.75	(0.05)	155						
	2001	0.98	(0.01)	150	0.91	(0.03)	130	0.08	(0.07)	0.08	(0.07)	0.09	(0.08)
	2002	0.98	(0.01)	108	0.89	(0.03)	123	0.10	(0.08)	0.07	(0.08)	0.08	(0.09)
FVC	2000	0.68	(0.06)	146	0.66	(0.05)	155						
	2001	0.91	(0.02)	150	0.87	(0.03)	130	0.03	(0.08)	0.04	(0.09)	0.07	(0.09)
	2002	0.96	(0.02)	108	0.80	(0.05)	123	0.15+	(0.08)	0.14	(0.09)	0.17+	(0.09)
Controls								N		Y		Y	
Municipality Fixed-Effects								N		Y		N	
Locality Fixed-Effects								N		N		Y	

Notes: The standard errors (SE) are in parentheses and are clustered at the community level. Two and one asterisks and + indicates that the differences are significant at the 1, 5 and 10 percent level. The number of observations (Obs) for the regressions is the same as for the means. Controls are the same as in Table 2b.

Table 4a: Vaccination Rates and Double Difference Results by Distance to Health Facility

	Year	Treatment Area			Control Area			Double-Difference Estimates					
		Mean	SE	Obs	Mean	SE	Obs	OLS	SE	OLS	SE	OLS	SE
On-Time: Health facility more then 5 km away													
MCV	2000	0.59	(0.07)	79	0.60	(0.09)	50						
	2001	0.85	(0.05)	60	0.65	(0.08)	48	0.21	(0.14)	0.15	(0.14)	0.13	(0.15)
	2002	0.85	(0.05)	46	0.80	(0.07)	35	0.05	(0.15)	0.12	(0.16)	0.11	(0.17)
OPV3	2000	0.63	(0.05)	79	0.72	(0.12)	50						
	2001	0.95	(0.04)	60	0.77	(0.08)	48	0.27**	(0.09)	0.20^{+}	(0.10)	0.21*	(0.10)
	2002	0.96	(0.03)	46	0.83	(0.05)	35	0.22	(0.15)	0.29^{+}	(0.15)	0.30^{+}	(0.15)
DPT3	2000	0.58	(0.06)	79	0.64	(0.11)	50						
	2001	0.87	(0.05)	60	0.75	(0.08)	48	0.17*	(0.08)	0.11	(0.08)	0.10	(0.08)
	2002	0.96	(0.03)	46	0.74	(0.05)	35	0.27*	(0.13)	0.36*	(0.12)	0.36*	(0.13)
FVC	2000	0.38	(0.07)	79	0.50	(0.10)	50						
	2001	0.75	(0.05)	60	0.50	(0.08)	48	0.37**	(0.09)	0.29**	(0.09)	0.26**	(0.08)
	2002	0.85	(0.05)	46	0.66	(0.07)	35	0.31*	(0.13)	0.39*	(0.15)	0.39*	(0.16)
Catch-up: Health facility more then 5 km away													
MCV	2000	0.77	(0.06)	79	0.92	(0.03)	53						
	2001	0.93	(0.02)	80	0.95	(0.03)	44	0.12^{+}	(0.07)	0.12	(0.08)	0.14	(0.09)
	2002	0.96	(0.02)	57	0.83	(0.05)	41	0.29**	(0.08)	0.30**	(0.09)	0.32**	(0.10)
OPV3	2000	0.72	(0.08)	79	0.91	(0.05)	53						
	2001	0.99	(0.01)	80	0.91	(0.03)	44	0.26**	(0.09)	0.25*	(0.09)	0.25*	(0.10)
	2002	1.00	(0.00)	57	0.90	(0.05)	41	0.28*	(0.11)	0.29*	(0.11)	0.28*	(0.11)
DPT3	2000	0.59	(0.08)	79	0.79	(0.07)	53						
	2001	0.96	(0.02)	80	0.84	(0.05)	44	0.32*	(0.12)	0.30*	(0.12)	0.30*	(0.12)
	2002	0.96	(0.02)	57	0.80	(0.04)	41	0.36**	(0.12)	0.35*	(0.12)	0.36**	(0.12)
FVC	2000	0.51	(0.07)	79	0.70	(0.10)	53						
	2001	0.89	(0.03)	80	0.80	(0.04)	44	0.28*	(0.13)	0.27^{+}	(0.15)	0.30^{+}	(0.16)
	2002	0.93	(0.03)	57	0.68	(0.07)	41	0.44**	(0.12)	0.46**	(0.13)	0.50**	(0.14)
Controls								N		Y		Y	
Municipality Fixed-Effects								N		Y		N	
Locality Fixed-Effects								N		N		Y	

Notes: The standard errors (SE) are in parentheses and are clustered at the community level. Two and one asterisks and + indicates that the differences are significant at the 1, 5 and 10 percent level. The number of observations (Obs) for the regressions is the same as for the means. Controls are the same as in Table 2b.

Table 4b: Vaccination Rates and Double Difference Results by Mother's Level of Education

	Year	Treatment Area			Control Area			Double-Difference Estimates					
		Mean	SE	Obs	Mean	SE	Obs	OLS	SE	OLS	SE	OLS	SE
On-Time: Mothers with less grade 4 education													
MCV	2000	0.71	(0.05)	119	0.67	(0.06)	98						
	2001	0.90	(0.03)	84	0.75	(0.06)	99	0.12	(0.09)	0.10	(0.09)	0.07	(0.10)
	2002	0.88	(0.04)	59	0.81	(0.04)	78	0.04	(0.09)	0.07	(0.09)	0.05	(0.10)
OPV3	2000	0.73	(0.05)	119	0.77	(0.06)	98						
	2001	0.95	(0.03)	84	0.86	(0.06)	99	0.13	(0.08)	0.13	(0.08)	0.11	(0.08)
	2002	0.98	(0.02)	59	0.87	(0.03)	78	0.15+	(0.08)	0.18+	(0.09)	0.17+	(0.09)
DPT3	2000	0.65	(0.06)	119	0.62	(0.06)	98						
	2001	0.88	(0.04)	84	0.83	(0.06)	99	0.03	(0.09)	0.00	(0.09)	-0.03	(0.10)
	2002	0.98	(0.02)	59	0.82	(0.04)	78	0.14	(0.09)	0.14	(0.11)	0.13	(0.11)
FVC	2000	0.51	(0.06)	119	0.51	(0.06)	98						
	2001	0.82	(0.04)	84	0.64	(0.08)	99	0.18+	(0.10)	0.14	(0.10)	0.09	(0.10)
	2002	0.88	(0.04)	59	0.69	(0.05)	78	0.19+	(0.10)	0.20+	(0.11)	0.17	(0.11)
Catch-Up: Mothers with less than grade 4 education													
MCV	2000	0.82	(0.05)	111	0.87	(0.04)	120						
	2001	0.95	(0.02)	112	0.96	(0.02)	84	0.03	(0.06)	0.04	(0.06)	0.06	(0.07)
	2002	0.99	(0.01)	75	0.85	(0.04)	100	0.18**	(0.07)	0.19**	(0.07)	0.21**	(0.07)
OPV3	2000	0.79	(0.06)	111	0.83	(0.06)	120						
	2001	1.00	(0.00)	112	0.92	(0.03)	84	0.12	(0.09)	0.10	(0.09)	0.10	(0.09)
	2002	1.00	(0.00)	75	0.92	(0.04)	100	0.12	(0.08)	0.10	(0.09)	0.09	(0.09)
DPT3	2000	0.71	(0.08)	111	0.77	(0.06)	120						
	2001	0.98	(0.01)	112	0.87	(0.04)	84	0.17+	(0.10)	0.14	(0.10)	0.14	(0.10)
	2002	0.97	(0.02)	75	0.87	(0.03)	100	0.16	(0.10)	0.13	(0.10)	0.12	(0.11)
FVC	2000	0.63	(0.08)	111	0.68	(0.07)	120						
	2001	0.92	(0.03)	112	0.86	(0.04)	84	0.11	(0.10)	0.09	(0.10)	0.11	(0.11)
	2002	0.96	(0.02)	75	0.76	(0.06)	100	0.24*	(0.10)	0.22*	(0.10)	0.24*	(0.11)
Controls								N		Y		Y	
Municipality Fixed-Effects								N		Y		N	
Locality Fixed-Effects								N		N		Y	

Notes: The standard errors (SE) are in parentheses and are clustered at the community level. Two and one asterisks and + indicates that the differences are significant at the 1, 5 and 10 percent level. The number of observations (Obs) for the regressions is the same as for the means. Controls are the same as in Table 2b.

Table 5: On-Time Vaccination Rates and Double-Difference Results using Administrative Data

	Year	Treatment Area			Control Area			Double-Difference Estimates					
		Mean	SE	Obs	Mean	SE	Obs	OLS	SE	OLS	SE	OLS	SE
Overall Effect													
BCG	2000	0.77	(0.02)	1236	0.74	(0.04)	675						
	2001	0.92	(0.01)	928	0.83	(0.03)	550	0.06+	(0.03)	0.08*	(0.04)	0.08*	(0.03)
	2002	0.93	(0.01)	740	0.85	(0.02)	467	0.05	(0.04)	0.04	(0.04)	0.04	(0.04)
MCV	2000	0.69	(0.04)	1335	0.67	(0.04)	676						
	2001	0.93	(0.01)	1224	0.76	(0.03)	680	0.15**	(0.04)	0.15**	(0.04)	0.16**	(0.04)
	2002	0.92	(0.01)	927	0.84	(0.02)	548	0.06	(0.05)	0.07	(0.05)	0.08	(0.05)
OPV3	2000	0.82	(0.04)	1335	0.79	(0.05)	676						
	2001	0.96	(0.01)	1224	0.85	(0.04)	680	0.09*	(0.04)	0.09*	(0.04)	0.09*	(0.04)
	2002	0.98	(0.01)	927	0.91	(0.02)	548	0.03	(0.05)	0.05	(0.05)	0.05	(0.04)
DPT3	2000	0.82	(0.04)	1335	0.77	(0.05)	676						
	2001	0.96	(0.01)	1224	0.83	(0.04)	680	0.09*	(0.04)	0.09*	(0.04)	0.09*	(0.04)
	2002	0.97	(0.01)	927	0.90	(0.03)	548	0.03	(0.05)	0.05	(0.05)	0.05	(0.04)
FVC	2000	0.60	(0.04)	1335	0.59	(0.06)	676						
	2001	0.91	(0.01)	1224	0.68	(0.05)	680	0.23**	(0.04)	0.23**	(0.04)	0.23**	(0.04)
	2002	0.92	(0.01)	927	0.76	(0.03)	548	0.15**	(0.05)	0.17**	(0.05)	0.18**	(0.05)
Clinic more then 5 km away													
MCV	2000	0.61	(0.05)	600	0.57	(0.06)	304						
	2001	0.90	(0.02)	603	0.69	(0.05)	302	0.17**	(0.06)	0.16**	(0.05)	0.16**	(0.05)
	2002	0.92	(0.02)	460	0.85	(0.02)	244	0.02	(0.07)	0.03	(0.07)	0.03	(0.07)
OPV3	2000	0.75	(0.06)	600	0.68	(0.07)	304						
	2001	0.95	(0.01)	603	0.76	(0.06)	302	0.11*	(0.05)	0.11+	(0.05)	0.11+	(0.05)
	2002	0.98	(0.01)	460	0.87	(0.04)	244	0.04	(0.07)	0.06	(0.07)	0.06	(0.07)
DPT3	2000	0.73	(0.06)	600	0.65	(0.07)	304						
	2001	0.94	(0.01)	603	0.75	(0.06)	302	0.12*	(0.05)	0.11+	(0.05)	0.11+	(0.05)
	2002	0.97	(0.01)	460	0.84	(0.04)	244	0.05	(0.07)	0.07	(0.07)	0.07	(0.07)
FVC	2000	0.52	(0.06)	600	0.46	(0.06)	304						
	2001	0.88	(0.02)	603	0.56	(0.07)	302	0.27**	(0.06)	0.25**	(0.06)	0.25**	(0.06)
	2002	0.92	(0.02)	460	0.73	(0.04)	244	0.13+	(0.08)	0.15+	(0.07)	0.15+	(0.07)
Mothers with less than fourth grade education													
MCV	2000	0.67	(0.04)	957	0.65	(0.05)	489						
	2001	0.92	(0.01)	863	0.75	(0.04)	530	0.15**	(0.04)	0.16**	(0.04)	0.16**	(0.04)
	2002	0.92	(0.01)	705	0.84	(0.02)	408	0.05	(0.06)	0.07	(0.05)	0.07	(0.05)
OPV	2000	0.80	(0.04)	957	0.74	(0.06)	489						
	2001	0.96	(0.01)	863	0.82	(0.04)	530	0.08+	(0.04)	0.09*	(0.04)	0.10*	(0.04)
	2002	0.97	(0.01)	705	0.90	(0.02)	408	0.01	(0.05)	0.04	(0.05)	0.04	(0.05)
DPT3	2000	0.79	(0.04)	957	0.72	(0.06)	489						
	2001	0.96	(0.01)	863	0.80	(0.04)	530	0.10*	(0.05)	0.11*	(0.04)	0.11*	(0.05)
	2002	0.97	(0.01)	705	0.89	(0.03)	408	0.02	(0.05)	0.04	(0.05)	0.04	(0.05)
FVC	2000	0.57	(0.05)	957	0.55	(0.06)	489						
	2001	0.90	(0.01)	863	0.64	(0.06)	530	0.23**	(0.05)	0.24**	(0.05)	0.25**	(0.05)
	2002	0.92	(0.01)	705	0.76	(0.03)	408	0.14*	(0.06)	0.16*	(0.06)	0.16**	(0.06)
Controls								N		Y		Y	
Municipality Fixed-Effects								N		Y		N	
Locality Fixed-Effects								N		N		Y	

Notes: The standard errors (SE) are in parentheses and are clustered at the community level. Two and one asterisks and + indicates that the differences are significant at the 1, 5 and 10 percent level. The number of observations (Obs) for the regressions is the same as for the means. Controls are the same as in Table 2b except animals owned, value of durable assets, and per capita expenditures.

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