The Impact of the Mexican Conditional Cash Transfer Program on Immunization Rates

Final Report

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Executive Summary

In this paper, we evaluate the impact of the Mexican conditional cash transfer (CCT) program, Progresa, on children's vaccination coverage for TB and measles. Unfortunately, due to data quality issues, we cannot estimate the impact on any of the multiple dose vaccines. We take advantage of an experiment where 320 out of 506 rural villages were randomized into treatment and control villages. Despite the randomization, we find that the treatment group has slightly lower immunization levels compared to the control group at baseline. To take these differences into account we use a double difference estimator.

Increasing vaccination rates in Mexico is a challenging goal owing to rates of vaccination above 90 percent prior to Progresa. Not surprisingly then, we find modest results. By 12 months post baseline, the program effect resulted in an equalization of vaccination rates between the treatment and control groups. While this increase in vaccination rate is not significantly different from zero for TB, it is for measles. The treatment group experienced an almost 3 percent increase in their measles immunization rates. This increase was experienced mainly among children living in localities at least 5.5 kilometers from a permanent health care clinic and whose mother did not complete primary school. This is an important finding as it shows that, at least in Mexico, CCT programs may be able to help those who have traditionally lower coverage rates.

We faced a number of important data issues giving some concern regarding the reliability of these estimates. These issues included the inability to control for changes in supply as well as

data quality problems partially resulting from the design of the survey instrument. It is not possible to determine the direction of the bias.

1 Introduction

Every year more than 2 million children die from diseases that could have been prevented by inexpensive vaccines (UNICEF, 2005). While many countries have increased immunization rates over time, coverage is still low in some regions. In 2003, average coverage for measles and diphtheria, pertussis and tetanus toxoids (DPT) vaccines was below 90 percent in Latin American and middle-income countries and below 80 percent for low-income countries (The World Bank, 2003). Vaccination coverage is even lower for traditionally under-utilized vaccines such as haemophilus influenzae type b (at one in five children worldwide) despite the dramatic reductions in the cost of this vaccine in the late 1990s (WHO, 2002). The current challenge to increasing vaccination coverage in these underserved regions is two fold: 1) to understand the impediments that limit the demand and supply of vaccines, and 2) to evaluate policies and program that may increase vaccination coverage. A better understanding of these issues will not only provide insights into how to improve immunization rates today, but may be instrumental for successful, rapid dissemination of future life-saving vaccines. In this paper, we focus on the latter question, evaluating the impact of a randomized conditional cash transfer program in Mexico – Progresa – on children's vaccination coverage for tuberculosis (TB) and measles.

Conditional cash transfers (CCT) are a policy tool that has recently been used to reduce poverty by building the human capital of children. CCTs are a departure from pure income transfer programs since transfers are provided conditional on the beneficiary household engaging in a set of behaviors designed to improve their health, nutrition and education status. One of the health conditionalities in Progresa requires all children age five and under to receive a schedule of vaccinations (Table 1). Mothers are also required to take children for regular growth

monitoring visits and attend health education talks. Both of these activities provide mothers an opportunity to talk with health personnel and other mothers about their child's health, and may encourage them to vaccinate their child. While improving vaccination coverage is not an explicit goal of Progresa, the health conditionalities attached to it may prove to be an effective way of improving immunization rates.

Our analysis takes advantage of an experiment designed to evaluate the impact of Progresa. In 1997, the Mexican government randomized 506 Progresa villages into treatment and control villages. Eligible households in treatment communities received the conditional transfers starting in the spring of 1998, and control villages were brought on approximately a year and a half later. We utilize data from the May 1998, October 1998 and May 1999 evaluation surveys to determine the impact of the program on vaccination rates for measles and tuberculosis 6 and 12 months post baseline (PBL) using a double difference estimator.[1]

Past research on the Nicaraguan CCT program did not reveal significant impacts on full vaccination coverage of children aged 12-23 months (Maluccio and Flores, 2004). In this study, 12-23 month olds were considered fully vaccinated after receiving 3 doses of polio and DPT and one dose of measles and TB vaccines. We examine the effect of the Mexican CCT program on child immunization rates to determine if the results are similar in a different context.[2] We also provide a more in-depth analysis than Maluccio and Flores (2004) by investigating the impact by age group and type of vaccine. Since the recommended age for vaccination varies by type of vaccine (Table 1)[3], disaggregating by age and vaccine type is critical. While we would like to

[1] While data was collected on DPT and polio, data inconsistencies strongly imply that these data were improperly recorded making the data unreliable for these vaccines. A discussion of these problems is provided in Appendix B.
[2] The Mexican population benefits from both better access to health care and lower poverty rates.
[3] For example, children should receive their TB inoculation at birth and measles at 12 months of age.

investigate if CCTs help children who were not vaccinated at the recommended age prior to the program catch-up with their vaccinations, vaccination rates were too close to 100 percent in the Progresa surveys to permit such differentiation. Lastly, since some subpopulations may have been affected differentially by the program, we examine the heterogeneity of the impact with respect to the gender of the child, mother's education, household head's ethnicity and health supply characteristics.

2 The Progresa Intervention

2.1 Background

Adopted in 1997, Progresa aims to break the intergenerational transmission of poverty by improving the human capital of poor children in Mexico. The program targets the rural poor, reaching nearly 2.5 million rural households by 2000. The CCT programs are extremely popular throughout the Latin American region; Argentina, Colombia, Honduras, Jamaica, and Nicaragua have all enacted similar programs to Progresa.

Progresa differs from other poverty reduction programs in that it combines two traditional methods of poverty alleviation: cash transfers and free provision of health and education services. These elements are linked by conditioning the cash transfers on children attending school and family members obtaining sufficient preventative health care. Therefore, the income transfer not only relaxes the household budget constraint, but also provides an incentive to increase utilization of health and education services. The program was first introduced in rural areas; it was later expanded into urban areas. In this study we focus only on the rural Progresa.

Eligible households in each community were identified on the basis of a welfare index. The welfare index was developed using data from a census taken of all households in each Progresa community. The census collected information on household income and other socio-economic characteristics that capture the multidimensional nature of poverty. Using these data, the welfare index was established and households were classified as poor or non-poor.[4] Only poor households became eligible for benefits. Households did not have to apply, but were informed of their eligibility either at community meetings or in household visits. Enrollment in Progresa was high at 94% of those eligible (Gertler, 2000).

The health component of Progresa was designed to promote family health and nutrition. However, the majority of the conditionalities focused on infants, children, and pregnant and lactating women in an effort to ensure a healthier start to life. While not the only health conditionality, children must receive a regular schedule of vaccinations to participate in the program (Table 1). Other health conditionalities included:

1. growth monitoring from conception to age 5;
2. regular preventative health check-ups for all family members, including prenatal care, and well-baby care;
3. mother's attendance at health, hygiene and nutrition education programs; and
4. children age 0-2 years and pregnant and lactating women taking nutritional supplements.

Anticipating the need to evaluate the impact of Progresa, the Mexican government designed a randomized experiment on a subset of eligible communities. In 1998, 506 of the approximately 50,000 eligible villages were chosen to participate in the experiment. The government randomly assigned 320 communities into treatment areas and 186 into control areas.

[4] See Skoufias, Davis and Behrman (1999) for more details.

These communities were located in 7 states within Mexico (Guerrero, Hidalgo, Michoacan, Puebla, Queretero, San Luis Potosi, and Veracruz). [5]

Eligible households in treatment communities received benefits starting in the spring of 1998 while households in control areas were incorporated in November 1999 or later. The delay in implementation of the program in control villages was justified since the government lacked sufficient funds to provide the program nationally from the outset. The transfers were fairly large, estimated as, on average, 20 percent of household expenditures (Skoukias, 2001).

2.2 Vaccination Coverage in Mexico and Possible Impediments to Program Success

The Mexican Universal Vaccination Program (UVP) was established by presidential decree in January of 1991. This program was given 20 months in which to show measurable results. It was successful, and by October 1992 coverage rates for DPT, TB and measles were 91%, 95% and 90% respectively (see Figure 1 for trends in DPT and measles). Polio coverage was high (95%) before the UVP was introduced. Despite an economic crisis in 1994, the Mexican government sustained its support for the program and vaccination rates remained above 90 percent throughout the 1990s (Rossetti and Gauri, 2004).

The high rates of vaccination achieved prior to Progresa make increasing rates further a challenging goal. Furthermore, Progresa's demand-side treatment assumes demand-side impediments hinder the improvement of vaccination rates. However, immunization coverage is

[5] See Skoufias, Davis and Behrman (1999) for a detailed description of the targeting procedures for choosing Progresa village.

the result of a complex interaction between supply- and demand-side factors.[6] Thus, Progresa's effectiveness in raising vaccination rates relies on adequate supply to meet the potential increases in demand. Unfortunately, data on vaccine supply in Mexico could not be obtained. This fact should be taken into consideration when examining the findings.

3 The Data

The Progresa evaluation dataset comprises a panel of approximately 24,000 households covering the period October 1997 to October 2000. Baseline household surveys were taken in October 1997 and May 1998, and five follow-up surveys were implemented at approximately six-month intervals. Both the poor and non-poor living in evaluation communities were included. Owing to limited availability of vaccine utilization data in the household surveys, only the May 1998 baseline and two follow-up surveys are used. The first follow-up is 6 months PBL – October 1998 – and the second is 12 months PBL – May 1999. We use an unbalanced panel of poor children under the age of three. This provides us with 19,663 observations, 12, 260 of which are from treatment villages. Table 2 provides the exact number of observations by the number of surveys in which a child participates. Community surveys were also taken during the Progresa evaluation and are used as a source of health clinic data for this study.

[6] Factors that affect vaccination coverage vary by country, but may include among others: capabilities of the national immunization organizations, political support, health care supply and quality, mother's education, local beliefs, mother's empowerment, and socio-economic level (Garui and Khaleghain, 2002). Free-riding in response to positive externalities associated with other children receiving vaccinations may also affect coverage.

3.1 Dependent Variables

Information on TB, DPT, polio, and measles immunization is available at the child level. Owing to issues of data accuracy, this study focuses on TB and measles only.[7] (A discussion of the data quality issues are provided in Appendix B.) We use the Mexican Schedule of Vaccinations (Table 1), to determine if a child's vaccinations are up-to-date. A binary dependent variable is created that takes on the value 1 if a child received all the recommended doses of a vaccine by the time of the survey and zero otherwise. (A more thorough description of how the dependent variable is calculated can be found in Appendix B.) Since both measles and TB vaccines only require one dose, there is no need to investigate partial coverage.

As shown in the vaccination schedule, it is recommended that TB be given at birth. If Progresa is effective, increases in vaccination rates for TB should be experienced by children less than 12 months of age. Unlike TB, the measles vaccine should be given to a child at 12 months of age. Program effects are therefore likely to occur between the ages of 12 and 23 months. While we would like to examine if the program helps older children make-up missed vaccinations, vaccination levels are too high prior to the program to study this effect.

4 Methods and Empirical Model

The objective of this research is to identify the average effect of Progresa on vaccination coverage. Specifically, we would like to compare immunization rates when Progresa was available with the counterfactual—i.e., when Progresa is not available in the treatment areas at

[7] The Progresa surveys ask respondent about their measles utilization,. Rossetti and Gauri (2004) explain that the measles, mumps and rubella (MMR) vaccination was available starting in 1998 in Mexico. It is unclear whether the children only received a measles vaccine or if their received MMR.

the same point in time. Since the counterfactual is never observed, we must estimate it. The Progresa evaluation employed random assignment to create a control group as the counterfactual. When randomization is done well, the treated and untreated have on average the same observed and unobserved characteristics, removing selection bias and causality concerns regarding the choice of the counterfactual (control) group.

Table 3 compares the difference in means between treatment and control groups at baseline for vaccination rates by age group and Table 4 for other individual, household, and community characteristics. These tables reveal that the randomization was successful for the covariates, but that the vaccination rates are statistically different between the treated and untreated for some age groups.[8] To take these baseline differences into account, we use a double difference estimator. A description of the double difference estimator is provided in Appendix A.

The double difference estimator assumes that the variation between groups accounted for at baseline does not change over time. It is never possible to test such an assumption and data limitations do not allow us to verify if the change in mean vaccination rates over time is the same for both groups prior to Progresa. However, this assumption seems reasonable since the time period between the baseline and the final survey is short. This assumption requires that vaccination supply is either constant or changing in the same way between treatment and control groups.

[8] These finding are consistent with Behrman and Todd (1999).

4.1 Empirical Specification

We separately estimate the intent to treat for measles and TB using an OLS model.[9,10] The regression equation is:

$$V_{ihvt} = \alpha_t + \beta_1 T_v + \gamma_1 (T_v * PBL6_t) + \gamma_2 (T_v * PBL12_t) + X'\lambda + \varepsilon_{ihvt} \quad (1)$$

where:

V_{ihvt} = 1 if child *i* in household *h* from village *v* in time period *t* is vaccinated and zero otherwise;
α_t = time fixed effects;
T_v = 1 if child *i* is from a treatment village and zero otherwise;
$PBL6_t$ = 1 if 6 months PBL and zero otherwise;
$PBL12_t$ = 1 if 12 months PBL and zero otherwise;
X = baseline socio-economic characteristics of child, households, and health clinic and personnel characteristics;[11] and
ε_{ihvt} = error term.

The coefficients, γ_1 and γ_2, are the double difference estimates of the average impact of the program on vaccinations rates 6 and 12 months PBL, respectively. We include time fixed effects, α_t, to control for time-varying factors that are common to both control and treatment areas and a program dummy, T_v, to account for differences in mean vaccination rates between the treatment and control groups at baseline. Since heteroskedasticity is present in a linear

[9] While a village fixed-effect model is feasible, lack of within-village variation results in low levels of significance. This is a consequence of high pre-program vaccination rates and too few children of the appropriate age in each village; forty-five percent of villages had less than three children age one or less at baseline, and 76 percent of the villages had vaccination rates of 100 percent for the same age group.

[10] Non-linear models such as probits or logits that use maximum likelihood methods are often employed when the dependent variable is binary. Since 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.

[11] Controls include: sex of the child; mother and father's age and education level; whether the mother speaks an indigenous language, whether the father speaks an indigenous language, presence of a permanent health clinic in the locality; distance to the nearest health clinic; presence of a mobile health clinic in the locality; number of doctors per 1000 population, number of nurses per 1000 population; number of medical residents per 1000 population; number of other health care professions per 1000 population; and other controls presented in Table 4.

probability model and there is possible spatial and temporal correlation among the error terms, standard errors are robust and clustered at the village level.[12]

It is important to examine whether Progresa helps some groups increase their vaccination coverage more than others. The specified equation is presented in Appendix C. We examine the heterogeneity of the impact with respect to six different binary variables:

1. if the child is female;
2. if the child's mother finished primary school;
3. if the household head only speaks an indigenous language;
4. if the village has a permanent health clinic;
5. if the village has a mobile health clinic; and,
6. if the nearest health care clinic is more than 5.5 kilometers from the village.[13]

While establishing a relationship between vaccination coverage and the amount of the transfer could be of interest to policy makers, it is not possible using the Progresa experiment. This is because receipt of vaccinations at the scheduled time is a conditionality of the program for all children and does not vary by transfer amount.

4.3 Non-Random Attrition and Additions to the Sample

The double difference estimator will be biased if there is non-random attrition or introduction of new children into the sample between surveys. A non-random change in the sample would lead the treatment and control groups to be incomparable over time. Such a change might occur if the program had an impact on outcomes such as infant mortality, migration or fertility. Past research shows that the program had little or no effect on fertility and out-migration patterns (Skoufias, 2001; Raymond, 2002). The program did lead to a decrease in infant mortality rates of 2 deaths per 1000 live births among the treated (Barham, 2005). With less than

[12] Spatially- and temporally- correlated errors may be present due to the nature of the panel data.
[13] We choose the distance 5.5 kilometers because it is the mean distance to the nearest permanent health care clinic.

1600 treated children under the age of one in the sample, infant mortality has a negligible impact on attrition.

We test for non-random changes over time in the sample by comparing differences in the baseline characteristics between treatment and control groups in each of the surveys. As discussed previously, non-vaccination characteristics do not significantly differ at the five percent level between the treated and untreated at baseline (May 1998). If attrition is a problem, we may find differences in later surveys between these same groups. With the exception of one variable in the October 1998 survey, baseline characteristics between the treated and untreated do not differ in the October 1998 or May 1999 surveys (Tables 5 and 6). At a significance level of five percent, it is expected that one variable is significantly different by chance.

5 Results

5.1 Summary Statistics

Table 7 presents the differences in the percent of children inoculated against TB and measles between the treatment and control groups at baseline and each post baseline survey by age group. Because the TB vaccine is scheduled for delivery at birth, one would expect the program to have the highest impact among children less than 12 months of age. Baseline data for this group indicate that differences in vaccination rates existed between treatment and control groups prior to program interventions. Coverage for the treated is 3 percentage points lower than the control group (88 percent as compared to 91 percent). At six months PBL, vaccination rates

rose slightly to 89 percent for the treated but declined to 87 percent among the controls.[14] Using the double difference estimator, this drop in coverage in the control group resulted in a five percentage point increase in vaccination rates due to the program. This impact decreases to 1.6 percent by 12 months PBL and is not significantly different from zero. The change in the impact is a result of the control group recovering from the decline in vaccination coverage and the treatment group's rate increasing to above the level of the controls at baseline.

We further disaggregate TB coverage rates with respect to the sex of the child, mother's education,[15] ethnicity of the household head, presence of a permanent or mobile clinic, and distance to a permanent clinic. We do no present these results since we find no evidence of heterogeneity for TB.

The program could have affected TB coverage among older children who were not inoculated before the age of one. For children aged 12-23 months and 24-35 months, vaccination rates are so close to 100 percent prior to the program that there is little room for Progresa to have an effect. In fact, the double difference estimator may be misleading because coverage rates reach their maximum of 100 percent and simply reflect differences at baseline between the treated and untreated. For this reason, program impacts are not reported for these two groups in Table 7 and regression results will not be provided for these age groups.

Vaccination against measles should take place at 12 months of age. At baseline, 92 percent of treated children aged 12-23 months were vaccinated as compared to 95 percent of

[14] It is unknown what led to the decline in immunization coverage in the control group. It is possible that a shortage of vaccines during this year led to a shift in supply to Progresa areas from control areas. An alternative explanation is that there was a free rider problem and controls decreased their coverage in response to the positive externalities accrued from immunization in treatment areas. This reason seems unlikely given that coverage rates only increased by one percent in the treatment areas. Finally, the reduction may be due to mean reverse.

[15] We are unable to identify the mother of children who entered the sample after the baseline. To keep these children in the sample, we proxy for mother's education with the education of the head of the household or spouse of the head of the household, depending on which one is female.

control children in the same age group (Table 7). By six months PBL, the immunization rate increased four percentage points to 96 percent for the treated, equalizing the coverage rates between the two groups and leading to a program impact of three percentage points. The program impact is sustained through 12 months PBL. Immunization levels among treatment and control areas remain the same, though levels declined to 91 percent in this period.[16]

Examining the heterogeneity of the results reveals that the program effect on measles is only experienced among children whose mother had less the primary school education, and children that reside in localities that are at least 5.5 kilometers away from a permanent health care clinic (Table 8).[17] For both these groups the findings resemble the full sample; the program equalizes vaccination rates between the treated and the untreated.

Similar to TB, measles coverage is too high for the 24-26 month olds to calculate a reliable double difference estimate (Tables 7) and will not be included in the regression analysis.

5.2 Regression Results

Controlling for baseline levels of health care supply and parents' and household characteristics, the findings presented in the summary statistics are confirmed (Table 9).[18] The impact of the program is not significant for TB when results are pooled for 6 and 12 months PBL (Table 9, columns 1 and 2). However, they are significant 6 months PBL once the impact is separated by survey year (Table 9, columns 3 and 4). Among children 0-11 months old,

[16] As with TB, it is not clear why there was a decline in vaccination rates 12 months PBL for the treatment and controls. There may be a problem of mean reversion that is affecting the results.
[17] We do not perform this analysis on villages with low vaccination coverage since sample sizes are too small. We also do not present results for gender and health supply since they are not significant. Results for all the heterogeneity tests are provided in the regression results.
[18] We also investigated the impact of the program on low vaccination coverage localities (those with less than 90 percent coverage for TB and measles). Since the findings were the same as with the full sample, we do not include these results in this report.

Progresa led to a five percentage point increase in TB coverage. Recalling the findings in the summary statistics, this effect is primarily due to a reduction in vaccination rates in the control group rather than an increase in the treatment group. After the control group recovered from the drop in vaccination coverage, there is no longer a significant impact of the program on TB 12 months PBL. However, the vaccination rates are equalized between the treatment and control group.

For measles, the program has a statistically significant impact of 3 percentage point for 12-23 month olds at 6 and 12 months PBL (Table 9, column 8). Such small effects are not surprising given the high levels of immunization in Mexico prior to Progresa. An alternative view is that the program led to a 50 percent reduction in the coverage gap for children who were not vaccinated, and approximately equalized the coverage rates between the treatment and the control groups.

5.3 Heterogeneity of the Treatment Effect

As discussed in the summary statistics, there is no heterogeneity for TB (Tables 10 and 11, columns 1-3), but there is for measles 12 months PBL (Tables 10 and 11, columns 4-6). The intent to treat effect for measles is higher for children whose mother has less than primary school education and who live further away from a permanent health care clinic. These results are encouraging as they indicate that Progresa may have aided disadvantaged groups who had lower vaccination rates at baseline.

6 Discussion

Using a double difference estimator, we find that a year after its introduction, Progresa led to a statistically insignificant increase in immunization rates for TB, and a statistically significant (nearly three percent) increase in measles coverage for 12-23 month olds. While this is a significant impact, it is unclear how this small increase in coverage will affect the incidence of measles in Mexico. The improvements in immunization rates were experienced among children living in localities at least 5.5 kilometers from a permanent health care clinic and whose mother did not complete primary school. In all cases, the program effect resulted in an equalization of vaccination rates between the treatment and control from baseline levels which were lower for the treated.

The limited impact of Progresa on immunization coverage is not surprising given vaccination levels were already at or over 90 percent for both TB and measles prior to the program.[19] This is a difficult context for Progresa to definitively improve immunization rates. It is encouraging, then, that Progresa helped groups whose measles immunization levels lagged owing to distance to health care and lack of education. This is an important finding as it shows that, at least in Mexico, CCT programs may be able to help those who have traditionally lower coverage rates. It is also important because high measles coverage (95% or above) is necessary for measles elimination. In analyzing CCT programs in other countries, it will be relevant to investigate if this result is robust to a change in context.

We faced a number of data issues which may have implications regarding the reliability of the results. First, we were unable to control for the supply of vaccines. With vaccination rates

[19] Due to these high vaccination rates, we are unable to examine the success of Progresa in encouraging older children who missed receiving their vaccinations at the recommended age.

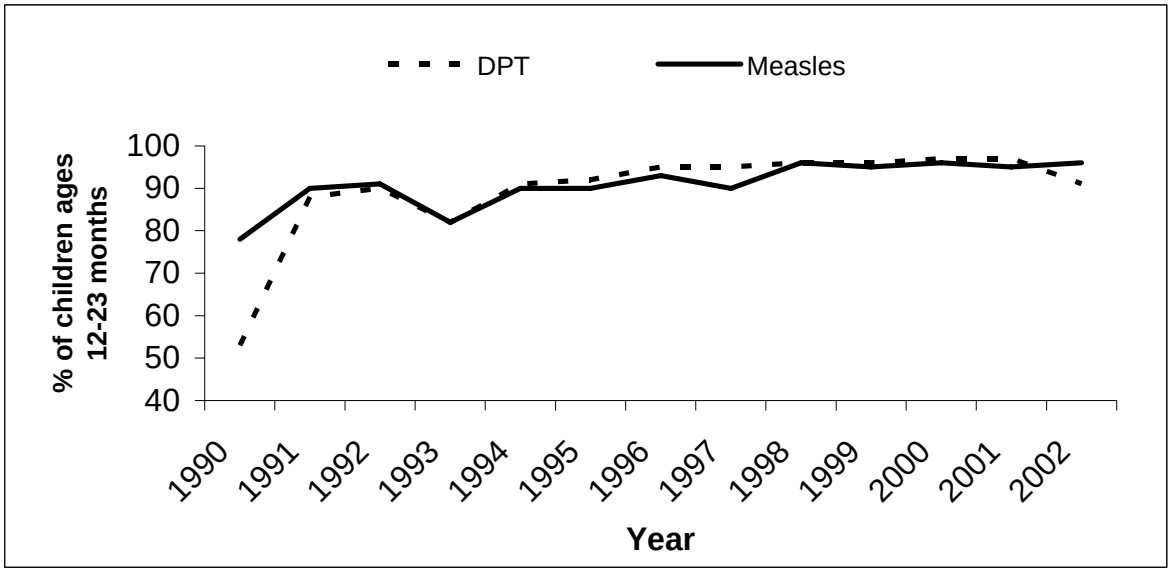
greater than 90 percent since 1992 in Mexico, the vaccination supply is probably not a constraint. Our research design relies on the vaccine supply being constant or changing in the same way between the treatment and controls in order for the results to be unbiased. In future work on CCTs in other countries, it would be useful to collect data on the number of doses delivered and investigate if there were any major fluctuations during the treatment period. Information on when the national campaign days (or weeks) took place would also aid in understanding fluctuations in vaccination rates between surveys.

Data quality issues arose due to surveyors recording the data incorrectly and the survey instrument not being designed appropriately for an unbalanced panel.[20] In particular, in the post baseline surveys, respondents were asked to provide information on the number of vaccines they had received since the last survey, rather than the total number of vaccines ever received. This created measurement error in the data on children who joined the survey after the baseline. This problem may have caused mean reversion in the data which lead to the findings.[21] To avoid the problems of data inaccuracy experienced in this study, it is recommended that future panel data questionnaires gather information on the total doses ever received in each of the surveys. This is especially important in studies where an unbalanced panel will need to be used.

[20] See appendix B for a more detailed description of the problems.
[21] It was not possible to use a regression discontinuity design to check for mean reversion because the variation in the data was inadequate given the smaller sample size needed for this design.

1. Figures and Tables

Figure 1: Trends in DPT and Measles Coverage in Mexico.



Source: World Bank Development Indicators, The World Bank 2003.

Table 1: Basic Vaccination Schedule for Mexico.

Vaccine	Dose	Recommended Age for Vaccine
Tuberculosis (TB)	1	At birth
Polio	1	2 months
	2	4 months
	3	6 months
Diphtheria, pertussis and tetanus toxoids (DPT)	1	2 months
	2	4 months
	3	6 months
	Booster 1	2 years
	Booster 2	4 years
Measles[a]	1	12 months

Source: Provided by The Mexican National Institute of Public Health.

[a] Mexico introduced the measles, mumps, and rubella (MMR) vaccine in 1998.

Table 2: Observations by Number of Surveys a Child is Present.

	May-98	**Oct-98**	**Nov. 99**
Children in 3 surveys	2,750	2,750	2,750
Children in 2 surveys	2,037	2,037	
	119		119
		1,943	1,943
Children in 1 survey	741		
		1,105	
			1,369
Total Observed	5,647	7,835	6,181

Note: The non-poor are not included in these numbers.

Table 3: Baseline Vaccination Rates.

	Treatment			Control			Difference	
	Mean	**SE**	**Obs**	**Mean**	**SE**	**Obs**	**Difference**	**T-Stat**
< 12 months								
TB	0.88	(0.013)	1320	0.91	(0.013)	788	-0.03	-1.71
12-23 months								
TB	0.97	(0.005)	1397	0.98	(0.006)	850	-0.01	-1.05
Measles	0.92	(0.009)	1383	0.95	(0.009)	841	-0.03	-2.27
23-35 months								
TB	0.98	(0.006)	758	0.97	(0.008)	484	0.01	0.72
Measles	0.96	(0.009)	751	0.95	(0.012)	481	0.01	0.72

Table 4: Difference in Means Between Treatment and Control Groups, May 1998.

	Treatment			Control			Difference	
	Mean	SE	Obs	Mean	SE	Obs	Difference	T-Stat
Female (=1)	0.49	(0.009)	3507	0.50	(0.009)	2140	-0.01	-0.85
Age of father	36.81	(0.270)	3507	36.96	(0.432)	2140	-0.14	-0.28
Age of mother	32.98	(0.280)	3507	32.89	(0.381)	2140	0.09	0.18
Years of father's education	3.53	(0.090)	3507	3.42	(0.104)	2140	0.11	0.80
Years of mother's education	3.13	(0.098)	3507	3.18	(0.110)	2140	-0.05	-0.34
Household head speaks an indigenous language (=1)	0.40	(0.034)	3504	0.39	(0.046)	2139	0.01	0.10
Health center (=1)	0.10	(0.025)	3507	0.06	(0.019)	2140	0.04	1.15
Mobile health clinic (=1)	0.72	(0.033)	3507	0.68	(0.043)	2140	0.04	0.81
Any clinic (=1)	0.79	(0.030)	3507	0.71	(0.043)	2140	0.08	1.45
Health worker in village (=1)	0.74	(0.030)	3507	0.79	(0.038)	2140	-0.05	-1.06
Distant to health center (km)	6.85	(0.419)	3507	6.51	(0.567)	2140	0.34	0.48
Cost to reach clinic (pesos)	9.42	(1.208)	3507	11.78	(1.978)	2140	-2.36	-1.02
Size of household (=1)	6.47	(0.069)	3507	6.45	(0.107)	2140	0.02	0.16
Number of hectars of land	1.71	(0.112)	3506	1.72	(0.125)	2139	-0.01	-0.09
Number of draft animals	1.66	(0.028)	3499	1.64	(0.042)	2132	0.02	0.40
Number or rooms in house								
Household has:	0.56	(0.015)	3498	0.58	(0.018)	2136	-0.02	-1.03
Radio (=1)	0.37	(0.019)	3502	0.41	(0.024)	2135	-0.04	-1.38
TV (=1)	0.02	(0.004)	3502	0.02	(0.007)	2136	0.00	-0.05
Vehicle (=1)	0.34	(0.016)	3507	0.31	(0.018)	2140	0.03	1.03
Dirt floor (=1)	0.67	(0.019)	3493	0.68	(0.025)	2132	-0.01	-0.35
Piped water on land (=1)	0.36	(0.027)	3500	0.29	(0.033)	2135	0.07	1.70
Water piped to home (=1)	0.06	(0.007)	3501	0.04	(0.007)	2129	0.01	1.34
Bathroom (=1)	0.53	(0.023)	3499	0.53	(0.026)	2125	0.00	0.02
Water piped to bathroom (=1)	0.03	(0.004)	3493	0.03	(0.004)	2129	0.00	-0.47
Electricity (=1)	0.66	(0.027)	3501	0.69	(0.029)	2137	-0.03	-0.86
Fridge (=1)	0.07	(0.007)	3502	0.09	(0.011)	2135	-0.01	-1.04
Gas heater (=1)	0.21	(0.017)	3502	0.23	(0.027)	2136	-0.02	-0.51

Table 5: Difference in Means Taken at Baseline Between Treatment and Controls, October 1998.

	Treatment			Control			Difference	
	Mean	SE	Obs	Mean	SE	Obs	Difference	T-Stat
Female (=1)	0.49	(0.008)	4891	0.50	(0.008)	2930	-0.02	-1.60
Age of father	37.65	(0.260)	4899	38.02	(0.401)	2936	-0.37	-0.78
Age of mother	34.06	(0.288)	4899	34.23	(0.388)	2936	-0.17	-0.35
Years of father's education	3.41	(0.091)	4899	3.29	(0.104)	2936	0.12	0.86
Years of mother's education	3.01	(0.092)	4899	3.05	(0.107)	2936	-0.04	-0.32
Household head speaks an indigenous language (=1)	0.41	(0.034)	4600	0.40	(0.046)	2754	0.00	0.05
Health center (=1)	0.09	(0.026)	4899	0.06	(0.018)	2936	0.04	1.16
Mobile health clinic (=1)	0.74	(0.031)	4899	0.67	(0.045)	2936	0.07	1.27
Any clinic (=1)	0.79	(0.028)	4899	0.70	(0.044)	2936	0.09	1.79
Health worker in village (=1)	0.74	(0.030)	4899	0.77	(0.040)	2936	-0.03	-0.56
Distant to health center (km)	7.02	(0.479)	4899	6.83	(0.613)	2936	0.18	0.24
Cost to reach clinic (pesos)	10.07	(1.361)	4899	11.46	(1.918)	2936	-1.38	-0.59
Size of household (=1)	6.43	(0.065)	4605	6.47	(0.108)	2755	-0.05	-0.36
Number of hectars of land	1.70	(0.115)	4604	1.85	(0.137)	2755	-0.16	-0.87
Number of draft animals	0.35	(0.016)	4605	0.33	(0.018)	2755	0.02	0.67
Number or rooms in house	1.64	(0.027)	4595	1.64	(0.041)	2746	0.00	-0.08
Household has:								
Radio (=1)	0.56	(0.015)	4595	0.58	(0.020)	2753	-0.03	-1.16
TV (=1)	0.36	(0.019)	4599	0.41	(0.024)	2752	-0.06	-1.83
Vehicle (=1)	0.02	(0.004)	4599	0.02	(0.006)	2753	0.00	0.00
Dirt floor (=1)	0.68	(0.019)	4589	0.69	(0.026)	2746	-0.01	-0.29
Piped water on land (=1)	0.36	(0.027)	4595	0.28	(0.032)	2751	0.08	1.79
Water piped to home (=1)	0.06	(0.007)	4597	0.04	(0.007)	2742	0.01	1.45
Bathroom (=1)	0.52	(0.024)	4593	0.52	(0.027)	2744	0.01	0.16
Water piped to bathroom (=1)	0.03	(0.003)	4586	0.02	(0.004)	2746	0.00	0.38
Electricity (=1)	0.65	(0.027)	4598	0.69	(0.031)	2753	-0.04	-0.87
Fridge (=1)	0.07	(0.007)	4599	0.08	(0.010)	2752	-0.01	-1.06
Gas heater (=1)	0.20	(0.017)	4598	0.23	(0.027)	2753	-0.03	-0.82

Table 6: Difference in Means at Baseline Between Treatment and Control Groups, May 1999.

	Treatment			Control			Difference	
	Mean	SE	Obs	Mean	SE	Obs	Diff.	T-stat
Female (=1)	0.49	(0.009)	3826	0.50	(0.009)	2308	-0.02	-1.47
Age of father	37.55	(0.296)	3854	38.50	(0.476)	2327	-0.95	-1.70
Age of mother	34.13	(0.329)	3854	34.72	(0.445)	2327	-0.59	-1.06
Years of father's education	3.47	(0.096)	3854	3.25	(0.107)	2327	0.22	1.51
Years of mother's education	3.10	(0.102)	3854	3.04	(0.108)	2327	0.06	0.41
Household head speaks an indigenous language (=1)	0.40	(0.035)	3630	0.42	(0.048)	2183	-0.02	-0.39
Health center (=1)	0.10	(0.026)	3854	0.06	(0.018)	2327	0.04	1.26
Mobile health clinic (=1)	0.74	(0.032)	3854	0.65	(0.047)	2327	0.09	1.55
Any clinic (=1)	0.80	(0.029)	3854	0.68	(0.047)	2327	0.12	2.20
Health worker in village (=1)	0.75	(0.029)	3854	0.79	(0.039)	2327	-0.04	-0.77
Distant to health center (km)	6.88	(0.462)	3854	6.57	(0.616)	2327	0.31	0.40
Cost to reach clinic (pesos)	8.85	(1.009)	3854	11.41	(2.036)	2327	-2.56	-1.13
Size of household (=1)	6.14	(0.063)	3634	6.32	(0.123)	2184	-0.18	-1.27
Number of hectars of land	1.65	(0.118)	3634	1.80	(0.143)	2182	-0.15	-0.82
Number of draft animals	0.34	(0.016)	3634	0.33	(0.019)	2184	0.01	0.40
Number or rooms in house	1.60	(0.029)	3628	1.60	(0.036)	2176	-0.01	-0.18
Household has:								
Radio (=1)	0.56	(0.015)	3627	0.59	(0.020)	2180	-0.03	-1.03
TV (=1)	0.36	(0.019)	3629	0.39	(0.024)	2180	-0.03	-1.07
Vehicle (=1)	0.02	(0.003)	3629	0.02	(0.004)	2180	0.00	0.51
Dirt floor (=1)	0.67	(0.019)	3622	0.71	(0.025)	2176	-0.03	-1.08
Piped water on land (=1)	0.36	(0.027)	3626	0.28	(0.033)	2179	0.08	1.77
Water piped to home (=1)	0.06	(0.007)	3628	0.04	(0.007)	2177	0.01	1.30
Bathroom (=1)	0.53	(0.025)	3627	0.52	(0.026)	2175	0.01	0.29
Water piped to bathroom (=1)	0.03	(0.003)	3618	0.02	(0.004)	2173	0.00	0.05
Electricity (=1)	0.67	(0.028)	3629	0.67	(0.033)	2181	0.00	-0.03
Fridge (=1)	0.07	(0.007)	3629	0.08	(0.011)	2179	-0.01	-0.72
Gas heater (=1)	0.20	(0.017)	3628	0.20	(0.025)	2180	0.01	0.18

Table 7: Summary Statistics for Vaccination Rates.

	Treatment			Control			Simple Difference		Double Difference	
	Mean	SE	Obs	Mean	SE	Obs	Difference	T-Stat	Difference	T-Stat
Tuberculosis										
< 12 months										
Baseline	0.88	(0.013)	1320	0.91	(0.013)	788	-0.03	-1.71		
6 months PBL	0.89	(0.010)	1312	0.87	(0.015)	737	0.02	1.16	0.052	2.07
12 months PBL	0.92	(0.010)	952	0.93	(0.013)	592	-0.02	-0.94	0.016	0.66
12-23 months										
Baseline	0.97	(0.005)	1397	0.98	(0.006)	850	-0.01	-1.05		
6 months PBL	1.00	(0.001)	1615	0.99	(0.002)	981	0.00	1.02	--	
12 months PBL	0.98	(0.003)	1354	0.98	(0.004)	821	0.00	0.18	--	
23-35 months										
Baseline	0.98	(0.006)	758	0.97	(0.008)	484	0.01	0.72		
6 months PBL	1.00	(0.001)	1790	1.00	(0.001)	1096	0.00	-1.50	--	
12 months PBL	1.00	(0.001)	1440	1.00	(0.000)	848	0.00	-1.42	--	
Measles										
12-23 months										
Baseline	0.92	(0.009)	1383	0.95	(0.009)	841	-0.03	-2.27		
6 months PBL	0.96	(0.005)	1543	0.96	(0.007)	935	0.00	0.17	0.030	2.03
12 months PBL	0.91	(0.009)	1299	0.91	(0.010)	790	0.00	-0.07	0.028	1.00
23-35 months										
Baseline	0.96	(0.009)	751	0.95	(0.012)	481	0.01	0.72		
6 months PBL	0.99	(0.003)	1753	0.99	(0.002)	1078	-0.01	-1.61	--	
12 months PBL	1.00	(0.002)	1425	1.00	(0.002)	840	0.00	-0.96	--	

Table 8: Summary Results for Measles by Mother's Education and Distance to Clinic.

	Treatment			Control			Simple Difference		Double Difference	
	Mean	SE	Obs	Mean	SE	Obs	Difference	T-Stat	Difference	T-Stat
Mother has less Than Primary School Education										
Baseline	0.91	(0.012)	989	0.95	(0.011)	588	-0.04	-2.30		
6 months PBL	0.96	(0.007)	1123	0.96	(0.007)	685	-0.01	-0.89	0.028	1.53
12 months PBL	0.91	(0.010)	957	0.90	(0.012)	574	0.02	0.95	0.052	2.52
Mother Completed Primary School Education										
Baseline	0.94	(0.012)	394	0.95	(0.013)	253	-0.01	-0.55		
6 months PBL	0.97	(0.008)	420	0.94	(0.015)	250	0.03	1.68	0.039	1.60
12 months PBL	0.90	(0.016)	342	0.94	(0.016)	216	-0.04	-1.97	-0.034	1.20
Distance to Permanent Health Clinic is Less than 5.5 km										
Baseline	0.93	(0.013)	672	0.94	(0.013)	426	-0.01	-0.45		
6 months PBL	0.96	(0.008)	763	0.96	(0.009)	491	0.00	0.17	0.010	0.51
12 months PBL	0.91	(0.013)	610	0.92	(0.012)	426	-0.01	-0.59	-0.002	0.10
Distance to Permaent Health Clinic is 5.5 km or Greater										
Baseline	0.90	(0.013)	711	0.95	(0.011)	415	-0.05	-2.86		
6 months PBL	0.96	(0.008)	780	0.95	(0.011)	444	0.00	0.11	0.050	2.29
12 months PBL	0.91	(0.012)	689	0.90	(0.018)	364	0.01	0.47	0.059	2.57

Table 9: OLS Results of the Impact of Progresa on TB and Measles Coverage.

	TB (12-23 month olds)				Measles (24-35 month olds)			
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Program (=1)	-0.031*	-0.029	-0.031*	-0.029	-0.029**	-0.030**	-0.029**	-0.030**
	[0.018]	[0.018]	[0.018]	[0.018]	[0.013]	[0.013]	[0.013]	[0.013]
Program * PBL (=1)	0.036	0.035			0.029**	0.032**		
	[0.022]	[0.023]			[0.015]	[0.015]		
Program * 6 months PBL (=1)			0.052**	0.050*			0.030**	0.033**
			[0.025]	[0.026]			[0.015]	[0.016]
Program * 12 month PBL (=1)			0.016	0.015			0.028	0.031*
			[0.024]	[0.025]			[0.018]	[0.018]
Controls	N	Y	N	Y	N	Y	N	Y
Observations	5701	5290	5701	5290	6791	6490	6791	6490
Adjusted R-square	0	0	0	0	0.01	0.01	0.01	0
Mean of Dependent Variable	0.9	0.89	0.9	0.89	0.93	0.93	0.93	0.93

Notes:

1. Standard errors are in brackets and clustered at the village level.
2. * significant at 10%; ** significant at 5%; *** significant at 1%.
3. Controls include sex of child, and parent and health supply characteristics.

Table 10: Heterogeneity of the Impact by Individual and Household Characteristics.

	TB (<12 Month Olds)			Measles (12-23 Month Olds)		
	[1]	[2]	[3]	[4]	[5]	[6]
Program	-0.023	-0.064**	-0.038*	-0.038**	-0.004	-0.036**
	[0.021]	[0.030]	[0.022]	[0.017]	[0.023]	[0.016]
*Female	-0.014			0.02		
	[0.025]			[0.022]		
*Indigenous language, HH head		0.057			-0.039	
		[0.037]			[0.027]	
*Primary education, mother			0.03			
			[0.030]			0.026
						[0.023]
Program * 6 months PBL (=1)	0.047	0.095**	0.066**	0.046**	0.003	0.029
	[0.034]	[0.040]	[0.031]	[0.022]	[0.027]	[0.019]
*Female	0.011			-0.028		
	[0.041]			[0.029]	0.049	
*HH head only speaks Spanish		-0.073			[0.032]	
		[0.054]				0.011
*Primary education, mother			-0.049			[0.031]
			[0.045]			
Program * 12 months PBL (=1)	0.011	0.018	0.023	0.048*	0.037	0.053**
	[0.028]	[0.035]	[0.027]	[0.025]	[0.028]	[0.021]
*Female	0.011			-0.04		
	[0.038]			[0.032]		
*HH head only speaks Spanish		-0.003			-0.017	
		[0.050]			[0.035]	
*Primary education, mother			-0.022			-0.090***
			[0.044]			[0.034]
Female (=1)	0.009	0.004	0.004	-0.012	0.006	0.007
	[0.019]	[0.009]	[0.009]	[0.017]	[0.006]	[0.006]
Primary education, mother (=1)	0.007	0.007	0.001	0.007	0.008	0
	[0.012]	[0.012]	[0.022]	[0.008]	[0.008]	[0.017]
Indigenous language, HH head (=1)	0.003	0.017	0.003	0.004	-0.048**	-0.003
	[0.012]	[0.024]	[0.012]	[0.008]	[0.019]	[0.008]
Other controls	Y	Y	Y	Y	Y	Y
Observations	5667	5667	5667	6783	6783	6783
Adjusted R-square	0	0.01	0	0.01	0.01	0.01
Mean of Dependent Variable	0.9	0.89	0.9	0.93	0.93	0.93

Notes:

1. Standard errors are in brackets and clustered at the village level.
2. * significant at 10%; ** significant at 5%; *** significant at 1%.
3. Controls include sex of child, and parent and health supply characteristics.

Table 11: Heterogeneity of the Impact by Health Supply.

	TB (<12 Month Olds)			Measles (12-23 Month Olds)		
	[1]	[2]	[3]	[4]	[5]	[6]
Program	-0.039**	0.002	-0.04	-0.027*	-0.01	-0.008
	[0.018]	[0.029]	[0.028]	[0.014]	[0.026]	[0.019]
*Health clinic	0.118			-0.021		
	[0.102]	-0.043		[0.040]		
*Mobile clinic		[0.036]			-0.027	
			0.023		[0.029]	
*Distance to health clinic			[0.037]			
						-0.041
						[0.025]
Program * 6 months PBL (=1)	0.058**	0.043	0.069*	0.032**	0.011	0.011
	[0.027]	[0.042]	[0.038]	[0.016]	[0.029]	[0.023]
*Health clinic	-0.064			0.01		
	[0.131]			[0.064]	0.032	
*Mobile clinic		0.01			[0.034]	
		[0.054]				0.042
*Distance to health clinic			-0.035			[0.031]
			[0.052]			
Program * 12 months PBL (=1)	0.019	0.011	0.039	0.022	0.031	-0.004
	[0.025]	[0.046]	[0.038]	[0.019]	[0.033]	[0.023]
*Health clinic	-0.029			0.069		
	[0.132]			[0.060]		
*Mobile clinic		0			-0.005	
		[0.054]			[0.039]	
*Distance to health clinic			-0.051			0.063*
			[0.049]			[0.036]
Health clinic	-0.078	0.008	0.011	-0.002	-0.013	-0.012
	[0.098]	[0.019]	[0.020]	[0.030]	[0.012]	[0.012]
Mobile clinic	0.006	0.017	0.005	0.001	0.023	0.001
	[0.012]	[0.025]	[0.012]	[0.008]	[0.020]	[0.008]
Distance to health clinic	-0.007	-0.007	-0.025	-0.012	-0.012	0.01
	[0.012]	[0.012]	[0.024]	[0.008]	[0.008]	[0.017]
Other controls	Y	Y	Y	Y	Y	Y
Observations	5667	5667	5667	6783	6783	6783
Adjusted R-square	0	0	0	0.01	0.01	0.01
Mean of Dependent Variable	0.9	0.9	0.9	0.93	0.93	0.93

Notes:

1. Standard errors are in brackets and clustered at the village level.
2. * significant at 10%; ** significant at 5%; *** significant at 1%
3. Controls include sex of child, and parent and health supply characteristics.

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APPENDIX A
Double Difference Estimator

The double difference estimator compares the change in outcomes in the treatment group before and after the intervention to the change in outcomes in the control group. By comparing changes, the estimator controls for characteristics that do not change over time within treatment and control groups, as well as characteristics that change over time in the same way between the two groups. The change in the control group is an estimate of the true counterfactual, that is, what would have happened to the treatment group if Progresa had not been implemented.

The first difference is achieved by comparing the treatment and control group at baseline, thereby accounting for any inherent differences in means between the groups. The second difference measures the change over time between the treatment and control. This can be represented by the following equations, where $I_{i,t}$ is the mean immunization rate for group i at time t.

First Difference:

$D1 = (I_{1,0} - I_{2,0})$ 1= Progresa (treatment village), 2=control village; 0= Baseline;

Second Difference:

$D2 = (I_{1,t} - I_{2,t})$ 1= Progresa (treatment village), 2=control village; t= 6, or 12 months PBL;

Double Difference Estimation:

$D2 - D1 = (I_{1,t} - I_{2,t}) - (I_{1,0} - I_{2,0})$, measures the impact of Progresa 6 or 12 months PBL.

APPENDIX B
Survey Instrument, and Calculation and Quality of the Dependent Variable

Survey Instrument

The survey instrument used to collect the vaccination data was different in each of the surveys. The baseline survey (May 1998) asked for information on the total number of doses ever received by children age five and under for each vaccine type. The October 1998 survey, provides data on the number of doses a child aged five and under received of each vaccine in the past 6 months (i.e. in the period since the last survey). Similar to the October 1998 survey, data in the May 1999 survey is collected on the number of doses of each vaccine that the child received in the prior six months. However, immunization information for this survey is only provided for those children aged two and under. We therefore limit our analysis to this age group.

It is unclear from the survey and the survey manual if vaccination data was collected just on the basis of mother's recall or using vaccination cards and mother's recall ("Card plus history"). In each of the surveys, the respondents are asked if they have a vaccination card for the child. At least 95% of all respondents said they had a vaccination card for the child. The two post baseline surveys also indicate if the respondents showed the vaccination card to the surveyor. Of those respondents that said they had a vaccination card, 18 and 16 percent could not show it to the surveyor 6 and 12 months PBL respectively.

Whether the data was collected via mother's recall or by "Card plus history" research in developing countries and the United States has shown that immunization rates tend to be

underestimated (Valedez and Weld, 1992; Lagsten and Hill, Gauri and Khaleghian, 2002; Saurez et. al., 1997). Due to the randomization, the recall or recording biases associated with collecting the immunization data is mostly likely similar between the treatment and control communities and the treatment effect should not be biased. This assumes though that the biases remain the same across survey rounds.

Calculation of up-to-date vaccinations

To determine whether a child is up-to-date with vaccinations, the total number of doses of each vaccine received by each survey date must be calculated. This is calculated in October 1998 by adding the number of doses received by baseline (provided in the May 1998 survey), to the number of doses received in the last six months (recorded in the October 1998 survey). A similar process is used to calculate the cumulative number of doses received by May 1999.

Recording Errors

Careful examination of the data reveals that the surveyors may not have collected the data correctly. Table 1B below displays the total number of TB vaccinations received if calculated according to the procedure described above. In May 1998, 59 one year olds had not been vaccinated and 2,188 received the recommended one dose of TB. By the second survey, 60 percent of children received 2 doses of the TB vaccine – more than the recommended number. In the May 1999 survey, 87 percent of children received more than one TB vaccination. It is highly unlikely that such a high proportion of children received more vaccinations than is necessary in the October 1998 and May 1999 surveys, but not in the baseline survey. It is especially

surprising since the TB vaccine leaves a scar indicating the child received the vaccine. The pattern is similar for polio and the other vaccine types that are not presented in Table 1B (measles and DPT). Thus, we believe that some surveyors recorded the cumulative number of doses received in the October 1998 and May 1999 surveys, rather than the number of doses received in the past six months.

Table 1B: Number of Observations by Total Number of Doses for Children 12-23 Months.

Total Doses	TB			Polio		
	May-98	Oct-98	May-99	May-98	Oct-98	May-99
0	59	31	7	41	11	12
1	2,188	722	115	590	129	51
2		1,146	420	498	377	125
3			410	526	464	184
4				287	378	174
5				184	235	147
6				0	122	85
7				0	59	58
8				0	19	34
9				0	5	16
10				0	1	4
11				0	0	1
13				0	0	1
14				0	0	1
Total Observations	2247	1899	952	2126	1800	893

This data problem does not affect the calculations of up-to-date vaccinations for TB and measles. Since both of these vaccinations require one dose to be fully covered, a child reported to have received one, two or three vaccinations all indicate full coverage. However, data for vaccinations that require more than one dose, namely DPT and polio, are unusable. To understand why, consider a child that received one dose of the polio vaccine before May 1998,

and another dose after this survey, but before the October 1998 survey. The child needs three doses of polio to be fully vaccinated. If the data was recorded correctly, it should indicate that the child was not fully vaccinated by the October 1998 survey. However, if the surveyor incorrectly recorded the cumulative number of doses received in October 1998 survey, the data would indicate that the child had received three doses of the polio vaccine and was fully vaccinated. Since it is not possible to determine whether the data was collected in accordance with the survey instrument,[1] the DPT and polio data are not examined in this analysis.

The total number of vaccinations ever received may also be inaccurate due to two data issues in the baseline survey. The first problem concerns the inability to match some children across surveys due to incorrect individual identification numbers.[2] In the baseline, 484 children could not be matched because they are missing individual identifiers.[3] An unknown amount of other children had incorrect individual identification numbers at the baseline as a consequence of the missing individual identifiers.[4] The second data problem arises because 805 children in the baseline had no vaccination data.[5]

As a result of the two missing data problems outlined above, 1,798 children greater than 11 months of age in the two follow-up surveys could not be match to the baseline survey. As

[1] Examination of the surveyor's manual did not clarify the issue.
[2] A child is uniquely identified by a family identification number and an individual identification number. The family id number is the same for each person in a particular family.
[3] Over 3000 children in the baseline were missing an individual identification number. Since the children had a family identification number, most of the children in the baseline could be matched with the subsequent surveys using the family identification number, and the age and sex of the child. Unfortunately, the child's name is not available in all of the surveys. This information is needed to correctly match the rest of the children.
[4] Typically the individual identification numbers for a family start at one with the head of the household. Number two is usually the spouse of the head of the household. Each other member of the family is numbered according to age. So the next oldest person would have an individual id number of three. In families where a child was missing a personal identification number, and this child was not the youngest, the personal identifiers were recorded incorrectly. Without going through each case individually, these are both difficult to identify and hard to correct since the child's name is not provided.
[5] 382 of these children were missing the data because the interviews were never completed or did not take place.

shown in Table 2B, this is approximately 40 percent of the children who join the sample after the baseline. This creates a data problem for the dependent variable. Since we only have information on the number of vaccinations a child received in the previous six months in the October 1998 and May 1999 surveys, we cannot accurately calculate the total number of vaccinations ever received. As a result, if the data indicates that the child is not vaccinated (and we code this observation as a zero), it is unclear if the child indeed has never received a vaccination or if they received the vaccine more than 6 months ago. We code these observations as missing.[6] Table 3B rows 5 and 10 show that the percent of missing observations is almost the same between the treatment and controls for all surveys rounds. The percent of missing observations is much higher for measles than for TB because there are more children who are coded as never having received a measles vaccine because they receive the vaccination at an older age than TB (at age 12 months as apposed to at birth).

Table 2B: Age Distribution of Children New to the Sample After May 1998.

Age	Frequency	Percent
< 12 months	2,619	59%
12-23 months	1,029	23%
24-36 months	769	17%
Total	4,417	100

As stated above, it appears that a large percent of the surveyors recorded the total number of vaccinations ever received rather than the number received in the last six months in the post baseline surveys. We therefore create a new dependent variable which recodes the missing

[6] Due to these observations being coded as missing observations, there will be fewer children who have received zero doses in the last two surveys and vaccination rates may be artificially high.

observations back to zeros for new entrants in last two surveys.[7] Table 3B row 5 shows that the present of missing observations is still the same between the treatment and control groups. They also represent less than 10 percent of all observations.

While the two methods of creating the dependent variable lead to different levels of vaccinations, the finding remain the same. For this reason, we only present the results for the variable that was recoded back to zero.

[7] This assumption may lead to an under-estimate of vaccination levels for the sample

Table 3B: Observations for Alternative Handling of the Dependent Variable, by Vaccine Type.

	Total			Treatment Group			Control Group		
	May-98	**Oct-98**	**Nov-99**	**May-98**	**Oct-98**	**Nov-99**	**May-98**	**Oct-98**	**Nov-99**
TB									
Total observations	6480	9101	7228	3999	5659	4468	2481	3442	2760
Number of missing observations	54	665	360	34	398	221	20	267	139
% missing	1	7	5	1	7	5	1	8	5
Number of missing reset to zero	0	296	152	0	173	94	0	123	58
% not changed	0	45	34	0	43	35	0	46	32
Number of missing after change	54	369	208	34	225	127	20	114	81
% missing after change	1	4	3	1	4	3	1	4	3
Measles									
Total observations	6480	9101	7228	3999	5659	4468	2481	3442	2760
Number of missing obervations	195	1937	1665	122	393	246	73	250	154
% missing	3	21	23	3	7	6	3	7	6
Number of missing reset to zero	0	1294	1265	0	798	772	0	496	493
% changed	0	67	69	0	67	68	0	66	70
Number of missing after change	195	643	400	122	393	246	73	250	154
% missing after change	3	7	6	3	7	6	3	7	6

APPENDIX C
Estimation of the Heterogeneity Effects

We estimate the heterogeneity using a modification of equation 1 in which all the program variables are interacted with the binary variable of interest, *H*. The subscript on *H* will differ depending on which of the six variables listed in section 4.1 it represents. The regression equation is specified as:

$$V_{ihvt} = \alpha_t + \beta_1 T_v + \beta_2 H_i + \beta_3 T_v * H_i + \gamma_1 (T_v * PBL6_t) + \lambda_1 (T_v * PBL6_t * H_i) + \gamma_2 (T_v * PBL12_t) + \lambda_2 (T_v * PBL12_t * H_i) + X'\lambda + \varepsilon_{ihvt} \quad (2)$$

If H_i is one for a female child, and λ_1 and λ_2 are the difference in the impact of the program between girls and boys 6 and 12 months PBL, respectively.