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Improving the Health Coverage of the Rural Poor: Does Contracting Out Medical Mobile Teams Work?

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Abstract
Low population density in rural developing country coupled with deficient infrastructure, weak state capacity and limited budgets makes increasing health care coverage difficult. Contracting-out mobile medical teams can be an adequate solution in this context. This paper examines the impact of a large-scale program of this type in Guatemala. Results indicate large impacts on immunization rates for children and prenatal care provider choices. The program increased substantially the role of physician and nurses at the expense of traditional midwives. These results suggest that mobile medical teams can increase coverage of health care services substantially in rural areas in developing countries.

JEL Codes: I18, I12

Key words: Health coverage, rural, contracting-out, medical mobile

1. INTRODUCTION

A third of the welfare gains in developing countries in the last four decades can be attributed to improvements in longevity and health (Becker, Philipson, and Soares, 2005) and reductions in child mortality have had a key role in these changes. Notwithstanding these advances, the 2003 Lancet Series on Child Mortality drew significant attention by noting the sad and astounding fact that each year more than six-million children worldwide die of specific diseases, such as diarrhea, malaria, and pneumonia that can be prevented or treated successfully by inexpensive and simple measures (Jones et al., 2003). Although medical and epidemiological studies have produced clear evidence about what strategies can be used to combat deadly diseases, it is less clear how best to *deliver* these services to the poor in developing countries (Jones et al., 2003).

Finding effective delivery models in rural areas is particularly crucial because the majority of children are born there, where health coverage levels and outcomes are significantly lower compared with urban areas.[1] Reproducing the traditional urban model in which patients seek services in health-care facilities may be infeasible because of low population density, poor transportation systems, and limited budgets. Moreover, shortages in skilled health professionals are significant in low-income countries and especially acute in rural areas (Strasser, 2003). An additional challenge arises because of the weak public capacity in monitoring the provision of services in remote locations.

Within this complex environment, governments around the world have tried medical mobile team approaches. Medical mobile teams have been set up to regularly visit communities and provide basic health and preventive services. This intervention can be implemented directly under the public-health system or they can be contracted to NGOs that have flexibility to adapt to local contexts to ensure better implementations. It is an empirically important question whether medical mobile team can provide significant improvements in medical coverage of and health outcomes. To date, there have been few large-scale evaluations that provide evidence about the effectiveness of these interventions.[2]

To shed some light on this literature, this paper aims to evaluate an extensive health-coverage expansion program that targeted rural areas in Guatemala through contracted out medical mobile teams. The *Programa de Extension de Cobertura* (Coverage Extension Program, or PEC) was launched in 1997 after Peace Accords ended a civil war that lasted more than three decades. The government contracted NGOs to provide health-care services to a significant fraction of the population, mostly rural, poor, and indigenous, but underserved by the existing public-health network. The set of health services covered stressed preventive actions and primarily focused on improving maternal and child health. To provide services to the mostly rural target population, the government chose an outreach model in which NGOs set up medical teams that made monthly visits to the targeted communities. Community support was sought by involving local leaders and a network of volunteers. The program expanded rapidly covering about 3 million individuals by 2000. Enrollments were stable until 2003 when it entered in a second expansion wave adding about 0.9 million people in newly covered communities by 2005 (see Figure 1).

[Insert Figure 1]

We exploit the recent expansion and combine information on program coverage with the 2000 and 2006 Guatemalan Living Standards Measurement Surveys (LSMS) data to estimate effects on intermediate health utilization indicators such as prenatal care, childhood immunizations, and family planning methods. We estimate a difference-in-difference model that compares trends in outcomes between communities newly covered by the 2005 expansions (treatment group) and never covered areas (comparison group). We document that outcomes and covariates are well balanced between the treatment and comparison groups at baseline. Additionally, we show that covariates have evolved similarly in both groups before any intervention. These two pieces of evidence suggest that we have identified a good comparison sample and, therefore, the estimated impacts correspond to causal effects.

We find that the PEC program was particularly effective in re-directing care towards trained professionals and at increasing immunization rates. Results indicate that the program did not change the fraction of women having prenatal checkups but altered significantly the participation of skilled professionals in the provision of these services. Our estimates indicate that the fraction of women that received prenatal care from a doctor or nurse increased by 29 percentage points (p-value<0.1) from a baseline level of 26 percent and the fraction of women receiving three or more prenatal care visits from these providers increased by 38 percentage points (p-value<0.05) from a baseline of only 19 percent. These changes indicate a substantial reduction in pregnancies cared for by traditional midwives. These estimated effects are remarkable as the program in a short timespan has been able to surmount strong cultural hurdles against Western medicine within the rural, mostly indigenous population.

Focusing on immunizations, results indicate that PEC coverage increased vaccinations against tuberculosis, measles, polio and diphtheria / perstussis / tetanus (DPT) of the order of 18 to 31 percentage points. Estimated effects on coverage of DPT and polio boosters are 24 and 37 percentage points, respectively. In contrast, we do not find statistically significant effects on knowledge or use of family planning methods.

Unfortunately, given the existing data collection procedures in Guatemala, we cannot examine health outcomes such as infant mortality. Consistent with previous reports, we found significant measurement problems in the infant mortality variable, primarily because children delivered without the aid of trained medical professionals that died early are typically not registered in the National Vital Statistics records, understating mortality rates for births in historically underserved areas.

The principal contribution of the paper lies in evaluating, with a credible empirical strategy, one of the largest examples to date of extending coverage to rural populations in developing countries through medical mobile teams administered by contracted-out NGOs. The contracting out program evaluated here involved providing services to about 4.2 million people in Guatemala by 2006, about a third of the country population. The findings suggest that large expansions of preventive health services are possible in a short period through the contracted-out medical mobile teams.

2. GUATEMALA: HEALTH SYSTEM AND COVERAGE EXPANSION PROGRAM

In 2005, Guatemala's PPP GDP per capita in current dollars was $5,015 and was categorized as a middle low-income country (World Development Indicators, 2007). The country fares poorly against the rest of the Central American countries in health indicators although it has similar GDP levels to its neighbors. For example, infant and child mortality rate (25/1000 and 41/1000, respectively) are significantly higher in Guatemala compared with the rest of Central America (21/1000 and 31/1000, respectively).

The health-care system can be characterized as fragmented and with low levels of coordination. The Ministry of Health and the Guatemalan Social Security Institute (Instituto Guatemalteco de Seguridad Social, IGSS) provide health services along a number of private suppliers. The health services supplied by the Ministry of Health are stratified by three levels according to the complexity of the care provided. In 2005, the first level included 926 health posts geographically distributed countrywide but typically in somewhat densely populated areas.[3] The second level is composed by 335 health centers that have different capability levels but are all staffed with at least a physician. The health centers are typically in county capitals.[4] Finally, the third level of care is provided by 43 hospitals in the most populated cities. The health infrastructure has remained virtually unchanged in the last fifteen years except for changes brought about by the PEC program.

The government of Guatemala launched the Coverage Extension Program (*Programa de Extension de Cobertura,* PEC) to rapidly scale-up the provision of primary health services in rural, underserved areas in 1997. Under this program, NGOs were contracted to provide a basic package of health services in a set of assigned communities. NGOs established mobile medical teams composed of a physician or a nurse and a health assistant responsible to provide the services in a set of assigned communities. During the visits, immunizations, checkups and other (mostly preventive) services were delivered and occasionally education sessions were held. [5] NGOs were paid on a capitation basis and had to reach specific pre-established targets in their assigned geographical areas or risk the cancelation of their contracts.

The first phase of the program (1997-1999), can be characterized as one of rapid expansion under a weak management environment for planning, supervision and monitoring. The program experienced only a slight increase in enrollment and suffered some deep budget cuts between 2000 and 2004. The new president elected in 2004 envisioned PEC as one of its key programs. As a result, population coverage started to increase again, reaching 3.8 million in 2005. Payments per capita increased substantially and returned to the typical 1997-1999 levels (around $8 per capita annually). Supervision was strengthened and targets occasionally increased. Our evaluation exploits the geographical expansion of the program that took place in 2004-2005. Therefore, the estimated effects correspond to the strengthened version of the program that was prevailing at the post-treatment period (2006). For this expansion, groups of uncovered rural communities, mostly indigenous communities with difficult access to the public infrastructure network were identified and selected for coverage. Public officials report that there was no strict procedure followed to determine communities selected, instead decisions were taken in an ad hoc manner.[6]

There were 21 health service indicators that the NGOs needed to target to ensure contract renewal (see Table A.I of the appendix for the complete list). Four indicators focused on prenatal and postnatal care. Twelve indicators were related to children aged younger than five and included vaccines, provision of Vitamin A and iron, regular checkups, and appropriate treatment for pneumonia and diarrhea. Finally, there were five indicators of services for women aged between15 and 49 and they focused on the provision of family planning services, the Diphtheria, Tetanus, and Pertussis vaccine, and pap smears.

3. DATA

The empirical strategy uses a difference-in-difference specification and compares newly enrolled areas as the treatment group and uncovered areas as a comparison sample. To provide before and after outcomes measures for both groups, the paper uses data from the 2000 and 2006 Living Standard Measurement Surveys (LSMS). The surveys contain information on a range of socio-economic dimensions such as housing conditions, family composition, income, consumption, employment, and education. The LSMS also includes a module on maternal and child health that collects information from mothers about their last pregnancy including data on prenatal care, birth delivery methods and postnatal care. It includes vaccination records for children ages 0 to 5 and, for all women ages 15 to 49, knowledge, and the use of family planning services. All results in the paper are generated using survey weights to be representative.

To determine coverage of the program over time, we obtained administrative records containing the list of covered communities in 2003 and 2005.[7] These lists were matched to the census of communities in 2002, which in turn was matched to the LSMS 2000 and 2006. [8]

Program administrators advised us that between 2000 and 2003 there were minor changes in geographic coverage.

Because the program was targeted to rural, indigenous population, we restrict the empirical sample to include only individuals in these groups. To compare newly covered areas with those that remained uncovered over time, we further restrict the sample to communities that had not been covered by 2003. Finally, we construct three distinct subsamples to analyze the impacts on services provided with prenatal care, vaccination, and family planning, respectively. We selected these services because of the overlap between variables present in the survey and those services that should be provided by the mobile medical teams.

[Insert Table 1 here]

Table 1 presents summary statistics for the three samples. Column 1 shows statistics for the sample used to estimate effects in prenatal care, which includes women ages 15 to 49 who gave birth in the 12 months before the surveys.[9] About 60% of these women did not have any formal education and access to housing services is low (for example, only 6% had flush toilet). With prenatal care, 73% of them receive some type of prenatal care service though a physician or nurse attends roughly a quarter of them, while traditional midwives or other individuals such as relatives attend the rest. On average, women make 3.4 prenatal care visits and only 23% of them have three or more prenatal care visits with a physician or nurse.

To examine the program impacts on vaccination rates, we focus on children younger than 18 months old. This age restriction ensures all children in the treatment groups were born after their communities were added to the program. Column 2 of Table 1 presents statistics for this group. About 82% of survey respondents report that these children have vaccination cards though about only half can show the cards to the enumerators. If the adult respondent does not have a vaccination card for a child, information was collected through respondent self-reports. Outcomes are constructed for four initial dose vaccinations and two boosters. Bacillus Calmette-Guérin (BCG) is the vaccine against tuberculosis, which should be administered right after birth in Guatemala. Diphtheria, Pertussis (whooping cough) and Tetanus (DPT) is a vaccine against infectious diseases. Three doses are scheduled to be given at 2, 4, and 6 months of the birth.[10] Polio is the vaccine against the disease of the same name, which can cause paralysis or even death and four doses should be administered starting two months after birth. Measles is an infectious disease that attacks the respiratory system and its vaccine is scheduled to be administered when the child turns one year old. Vaccination rates in the analyzed sample hover between 65% and 80% for DPT, Polio, and BCG although coverage for Measles and boosters for DPT and Polio present significantly lower rates.

Finally, we analyze impacts on family planning by focusing on all women aged between 15 and 49 who gave birth in the last five years. We apply this restriction for both LSMS 2000 and 2006 rounds because questions on contraception knowledge and use were only asked to this group in the earlier wave. Column 3 presents statistics for this group. Demographic characteristics are similar to the prenatal care sample (low education and access to housing services). Family planning coverage levels are low with about half the women knowing about birth control methods and about 28% using them.

4. IDENTIFICATION STRATEGY AND ECONOMETRIC MODEL

For each of the analytical samples, we have information from two groups: those that will be exposed to PEC because of the 2004-2005 expansion and those that never have PEC (treatment and comparison groups, respectively). We also have data for only two periods, before and after the expansion. Data with this structure lends itself well to a simple difference-in-difference specification. The basic model is straightforward. Let y_{it} be the outcome for person i in period t. Define $\overline{y}^{tb}$ and $\overline{y}^{ta}$ as the mean outcomes for the treatment group before and after the intervention respectively. Likewise, let $\bar{y}^{cb}$ and $\bar{y}^{ca}$ be the same values for the comparison sample, (before and after respectively). A simple difference-in-difference estimate is calculated with these four means as simply

(1) $$\hat{\beta} = (\bar{y}^{ta} - \bar{y}^{tb}) - (\bar{y}^{ca} - \bar{y}^{cb})$$

where the first difference measures the change over time in the treated group while the second difference measures the amount of the change that can be attributed to secular changes in the economy. Econometrically, the estimate for (1) can also be captured in a regression model of the form

(2) $$y_{it} = \alpha + Treated_{it}\delta + Post_{it}\gamma + Treated_{it} * Post_{it}\beta + \varepsilon_{it}$$

where $Treated_{it}$ is an indicator for whether the respondent lives in a community that will receive PEC by 2006, $Post_{it}$ an indicator for year 2006. The scalar β is the parameter of interest, which estimates the average treatment effect for the selected sample. Note that because this data set is constructed by pooling two cross-sectional data sets, individuals and communities do not always show up in the data set in both periods.

We will obtain unbiased estimate of β using this specification as long as the comparison sample provides an estimate of the secular change in outcomes that would have occurred without the intervention. This assumption may be more plausible if the groups are similar at baseline. Table 2 explores this concern by presenting estimated differences between the treatment and comparison groups for demographic and outcome variables in the pre-treatment period. Results indicate that the variables are well balanced in the two groups for the three analytical samples. In the sample of women that gave birth in the previous year, we list 15 variables that will be either outcomes or covariates in our regressions. In all cases, we cannot reject the null that the pre-treatment means are the same across the two samples. We obtain similar results for the third sample, which contains women who gave birth sometime over the past five years. In the children under 18 months sample, we cannot reject the null that the means are the same in all cases except two: we can reject the null that pre-treatment means in age are and the pre-treatment measles vaccination rate the same at the 10% level.

The results in Table 2 may seem surprising given that communities were selected for PEC because of their lack of care. However, because we have reduced the comparison sample to include only indigenous populations living in rural areas, there does not appear to be a large difference in outcomes between the two groups in the pre-treatment period. The one exception would be in immunization rates where we find qualitatively large but statistically insignificant differences across groups.

[Insert Table 2 here]

Although the levels in outcomes are similar across the two groups in the pre-treatment period, there is a concern that the pre-treatment trends may be varying. We will return to this point later in Table 6 and provide some evidence that the growth in outcomes before treatment grew at similar rates for treatment and comparison samples.

A limitation of the specification above is that it does not control for any differences across people in observed characteristics. This can easily be incorporated into the model by estimating an equation of the form

$$(3) \qquad y_{it} = \alpha + Treated_{it}\delta + Post_{it}\gamma + Treated_{it} * Post_{it}\beta + x_{it}\theta + \varepsilon_{it}$$

where the vector x_{it} captures characteristics of the individual. In the prenatal care and family planning models, we add variables that measure the woman′s age in years, dummy variables for whether they were married, indigenous, currently employed, and two dummy variables for years of education (1-3 years and ≥4 years with no education as the reference group). We also control for some measures of wealth by adding separate dummy variables for whether the family home has running water, a flush toilet, electricity or a concrete floor. For the vaccination sample, we use a restricted set of controls because some variables are not relevant to this age-group (for example, education).

The PEC program was implemented in many small communities throughout rural Guatemala. Because there are potential omitted local characteristics that may be correlated with both the PEC intervention and the growth in health outcomes, we want to control for local, time invariant characteristics as much as possible. Ideally the same communities would have been sampled before and after the intervention. Unfortunately, given the sampling frame for the LSMS, this did not occur and few communities were sampled in both 2000 and 2006. However, we can control for higher-level geographic areas in the model. Specifically, Guatemala is divided into 22 states. Let $u(j)_i$ be a dummy variable that equals 1 if person i lives in state j. We will add a set of 21 state dummy variables to the model and the estimating equation will take the form

$$(4) \qquad y_{it} = \alpha + Treated_{it}\delta + Post_{it}\gamma + Treated_{it} * Post_{it}\beta + x_{it}\theta + \sum_{j=1}^{21} \mu(j)_i \psi_j + \varepsilon_{it}$$

Because there is variation within the state in communities that are treated and not, we can add the variable *Treated* and the state effects to our model.

As a further effort of controlling for local time-invariant conditions, we restrict the sample to include only observations from counties that are observed both in the pre- and post-period. We repeat this procedure for each of the three analyzed samples. For these samples, we can add fixed effects at the county level. Under this specification, we cluster the standard errors at this level of geographic aggregation. The estimating equation is

$$(5) \quad y_{it} = \alpha + Treated_{it}\delta + Post_{it}\gamma + Treated_{it} * Post_{it}\beta + x_{it}\theta + \sum_{k=1}^{68} \mu(k)_i\psi_k + \varepsilon_{it}$$

Bertrand, Duflo, and Mullainathan (2004) note that many difference-in-difference models are possibly subject to high Type I error rates because of autocorrelation in the outcome of interest. This should not pose too much a problem with our estimates because our observations are six years apart and any autocorrelation should dissipate over time. They recommend clustering the standard errors over the dimension by which the covariate of interest is varying. In this case, PEC is instituted at the community level so following this suggestion, we should cluster at this level. Unfortunately, as we noted above, the sample frame for the LSMS survey is such that we do not observe many communities on both the 2000 and 2006 surveys, therefore, clustering at this level will not capture autocorrelation. However, if we move up to a higher geographic level (county or state), we can capture potential correlation in behavior in communities within this level of geography. Therefore, in all models, the standard errors are calculated allowing arbitrary correlation in errors within a state or a county over time.

5. RESULTS

In this section, we describe the principal results of the paper. Table 3 shows the results for prenatal care. The first column presents the results for the simple difference-in-difference specification formalized in equation (2) above where errors are clustered at the state level. Results suggest that the program did not affect the fraction of women receiving prenatal care services nor the number of average visits. However, results indicate that the fraction of women receiving prenatal care by physician or nurse increased by 29 percentage points (p-value < 0.10). Even larger effects, closer to 38 percentage points, are found when analyzing the fraction of women receiving three or more prenatal care by physician or nurse. To explore the robustness of the findings, in columns 2 to 4 we implement the empirical specifications described in the previous section in equations (3) to (5). Column 2 presents estimates when adding several covariates at the individual level whereas columns 3 and 4 show results when adding fixed effects at the state and county level, respectively. Estimated coefficients are similar across specifications though standard errors increase substantially in the last specification because of the reduction in sample size and therefore, the impact regarding women having three or more prenatal care visits by physician or nurse, lose statistical significance.

[Insert Table 3]

To explore these findings in more depth, Figure 2 shows the distribution of women to the prenatal care provider in 2000 and 2006 for the treatment and comparison samples, respectively. The comparison sample contained no major changes for women except from a reduction of about 13 percentage points for no prenatal care and concomitant increases in the prevalence of prenatal care by traditional midwives and physicians. In contrast, women in the treatment areas experience major changes: the number of women receiving prenatal care by a physician or nurse increase by 38 percentage points and this increase is reflected in large decreases in the fraction of women receiving no care, care by a traditional midwife and by other provider (for example, relatives).

[Insert Figure 2]

These important changes are also present when considering the distribution of women with respect to the number of prenatal care checkups performed by physician and nurse (Figure 3). Whereas in the comparison sample there is not much change between 2000 (pre-treatment) and 2006 (post-treatment), the distribution in treatment areas is significantly altered during this period. As noted, there is a dramatic reduction in the fraction of women receiving no prenatal care by physician or nurse. However, for those attended by a physician or nurse, the distribution became heavily concentrated in the three to five visits categories. This is consistent with aggressive and strategic actions by the NGOs to improve the indicator pertaining women receiving 3 or more prenatal care visits.

[Insert Figure 3]

As part of improving child and maternal health, the program aimed to increase vaccination rates. Table 4 presents estimated impacts for this set of outcomes. As before, we present in Column 1 results from the simple difference-in-difference specification. Results suggest that the program was effective in improving vaccination outcomes across the board. Estimates point to an increase in about 23 percentage points in the fraction of card vaccination holders and in 18 percentage points in the fraction showing it, though only the first impact is statistically significant. For initial dose vaccines, there are positive statistical significant impacts in the range of 18 percentage points (BCG) to 31 percentage points (Measles). Estimated effects for DPT and Polio boosters are large: 24 and 37 percentage points, respectively. Vaccination impacts tend to be larger for those indicators with lower baseline levels, which may explain why larger impacts are present for boosters than for initial dose vaccinations. In turn, baseline vaccination rates are lower for vaccines administered at an older age.[11] Results are robust to the various specifications though standard errors are larger when restricting the sample to observations from counties observed both in the pre and post periods (Column 4).[12]

[Insert Table 4]

Turning to family planning outcomes, Table 5 documents that impacts are small in magnitude and in no case are results statistically significant at the 10% level. One potential explanation could be that the indicators the NGO had to achieve in this area were not well aligned with the outcomes measures. In particular, the NGO targets did not include indicators related to knowledge of birth control methods.

As we noted above, we allow arbitrary correlation in the errors for observations from the same state and there are 22 states in Guatemala.[13] The asymptotic properties for these procedures are shown for fixed group size and as the number of panel increases to infinity. Research has documented that these procedures tend to perform poorly when the number of panels is small (Wooldridge, 2005). What constitutes small is, however, open to debate and some research suggest that 22 groups may be a concern. Cameron, Miller, and Gelbach (2008) have developed a 'wild bootstrap' procedure to produce p-values of the test of the null among within-group correlation in errors that in Monte Carlo simulations has shown to have low Type I error rates, even with several groups. We use the wild bootstrap procedure with 1000 replications to produce p-values for our statistically significant results from Tables 3-5 and we find that the p-values from the regular clustered standard errors typically increase but usually results remain significant at the 10% level. The p-value from the (regular clustered standard errors) and the [wild bootstrap] procedure generate the following results: three or more prenatal care visits by physician or nurse (0.020) and [0.093], BCG vaccinations (0.011) and [0.047], DPT vaccinations (0.073) and [0.155], Polio vaccinations (0.016) and [0.128], Measles vaccinations (0.001) and [0.002] and Polio booster (0.007) and [0.066].

[Insert Table 5]

The identifying assumption for the difference-in-difference framework is that, without treatment, outcomes variables would have evolved similarly in the treatment and comparison samples over the analyzed period. Although this assumption is untestable, we can, however, check whether other covariates, which should have been unaffected by the program, have evolved similarly over time in both groups. To that end, we run difference-in-difference models but using as dependent variables the covariates employed in the analysis. Table 6 presents the results. The first column shows estimates when using all observations and the second column when restricting to those from counties present in the data set in both the pre and post periods. From the 52 regressions ran, we find only one case in which we reject, at the 5% level, that the covariate did not evolve similarly in the treatment and comparison groups. Though these tests may be underpowered, we do find statistically significant effects in targeted outcomes in the principal analysis suggesting that the estimated impacts do reflect causal changes generated by the program.

[Insert Table 6]

6. CONCLUSIONS

Improving the health of rural populations in developing countries will require increasing significantly the coverage of proven effective medical services. This is a challenging endeavor and will require models of provision of health care distinct to those prevalent in urban areas. Unfortunately, there is scant evidence about the effectiveness of health programs aiming to improve health coverage in rural areas implemented at a large scale. This paper aims to contribute to this literature by evaluating a large-scale program that contracted out mobile medical units to reach previously underserved population in rural Guatemala. The paper exploits the 2004-2005 expansion of this program with 2000, 2006 LSMS data, and a difference-in-difference framework to analyze program's impacts on prenatal care, child vaccination, and family planning measures. Results indicate that the program was effective in inducing a significant increase in the use of physicians and nurses as primary providers of prenatal care and large increases in coverage of vaccinations rates for children. In contrast, results indicate a lack of impacts in knowledge and use of family planning methods.

A limitation of this study is that we cannot assess effects in health outcomes, such as infant mortality, because of data limitations. However, existing evidence suggests that the changes in health-care use documented above should produce improvements in health outcomes. There is a large literature that has established the significant health benefits, and even productivity gains, produced by increases in vaccination rates (Ehreth, 2003; Bloom, Canning, and Weson, 2005; Lee, 2012). Moreover, the increased role of physicians and nurses as prenatal care providers, at the expense of traditional midwives, should lead to health improvements. This could be expected given the body of evidence documenting the beneficial effects of specific medical interventions to be performed during checkups and survey results that suggest that traditional midwives, even trained ones, may provide substandard care (Jones et al., 2003; Goldman and Glei, 2003). The effectiveness of this program in producing large improvements in key health-care utilization in a short timespan provide some hope to policymakers around the world frustrated for the failure of many previous efforts.

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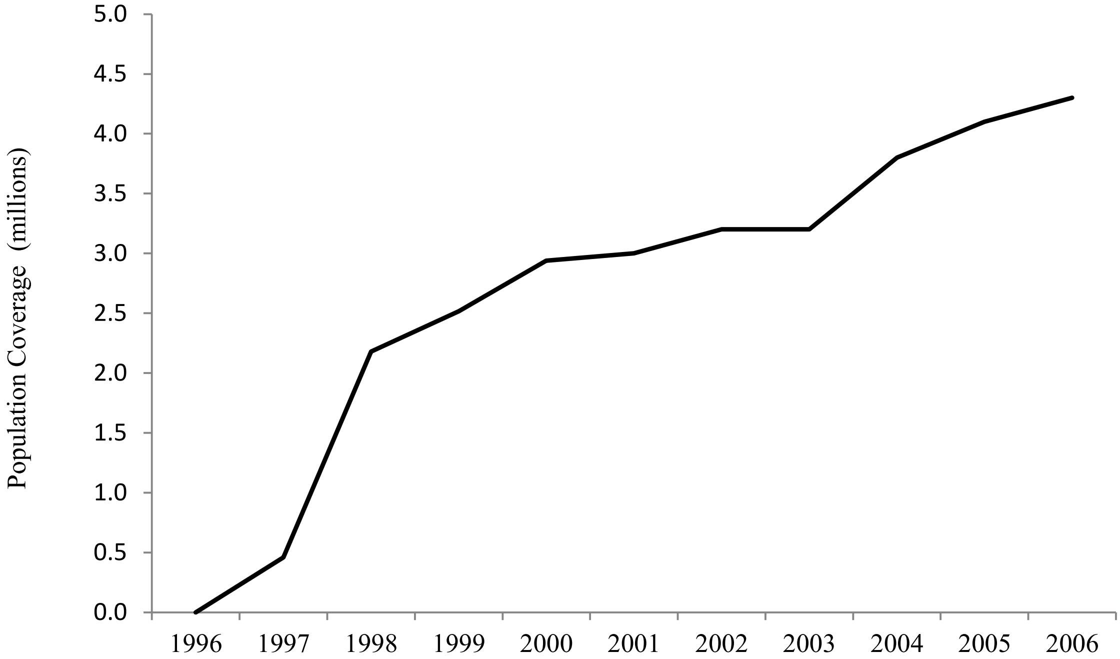

Figure 1: Population Covered by PEC

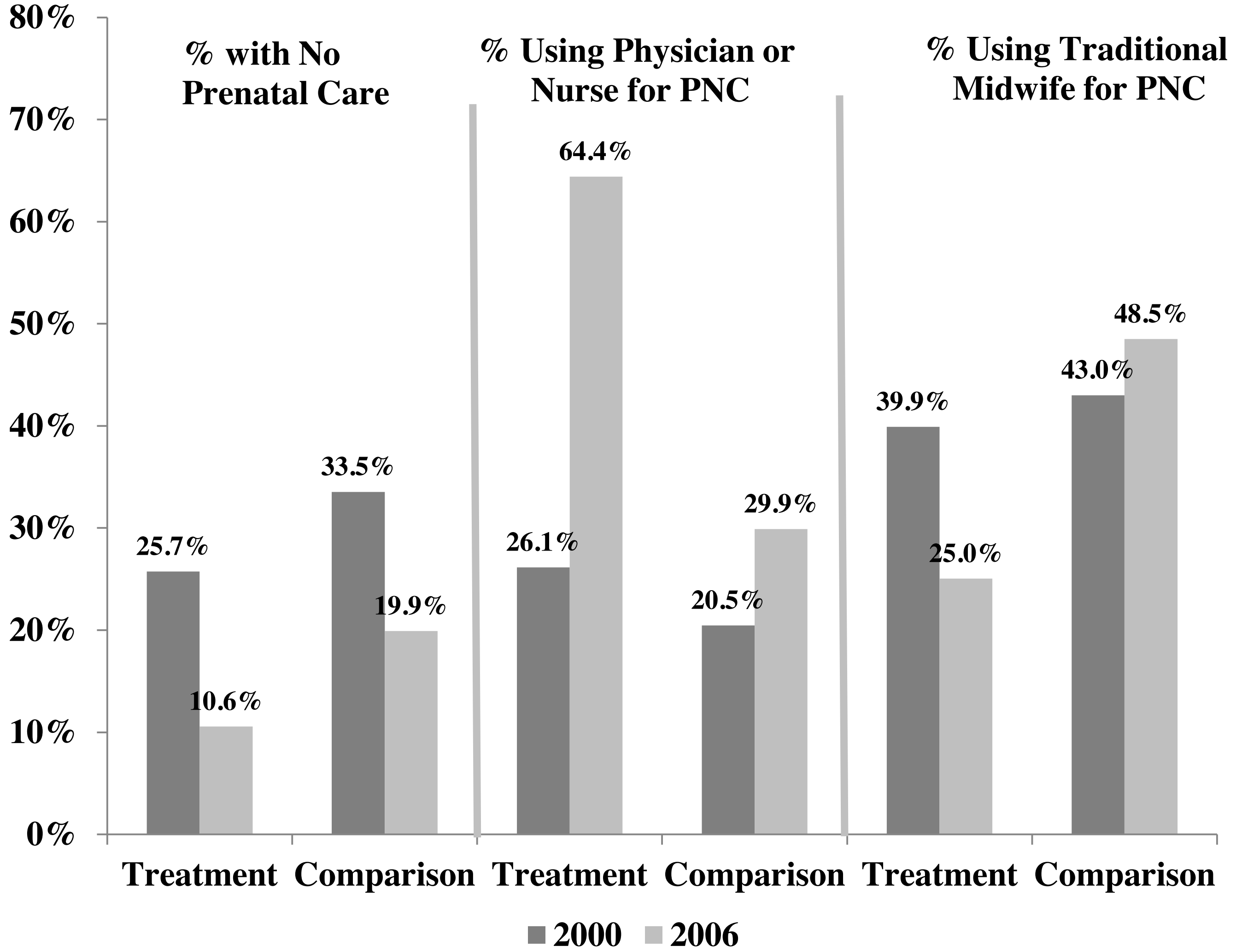

Figure 2: Distribution of Women by Provider of Prenatal Care

Notes: The sample includes indigenous women living in rural communities not covered by PEC in 2003 who gave birth in the last 12 months before the survey. Sample weights are used for calculations.

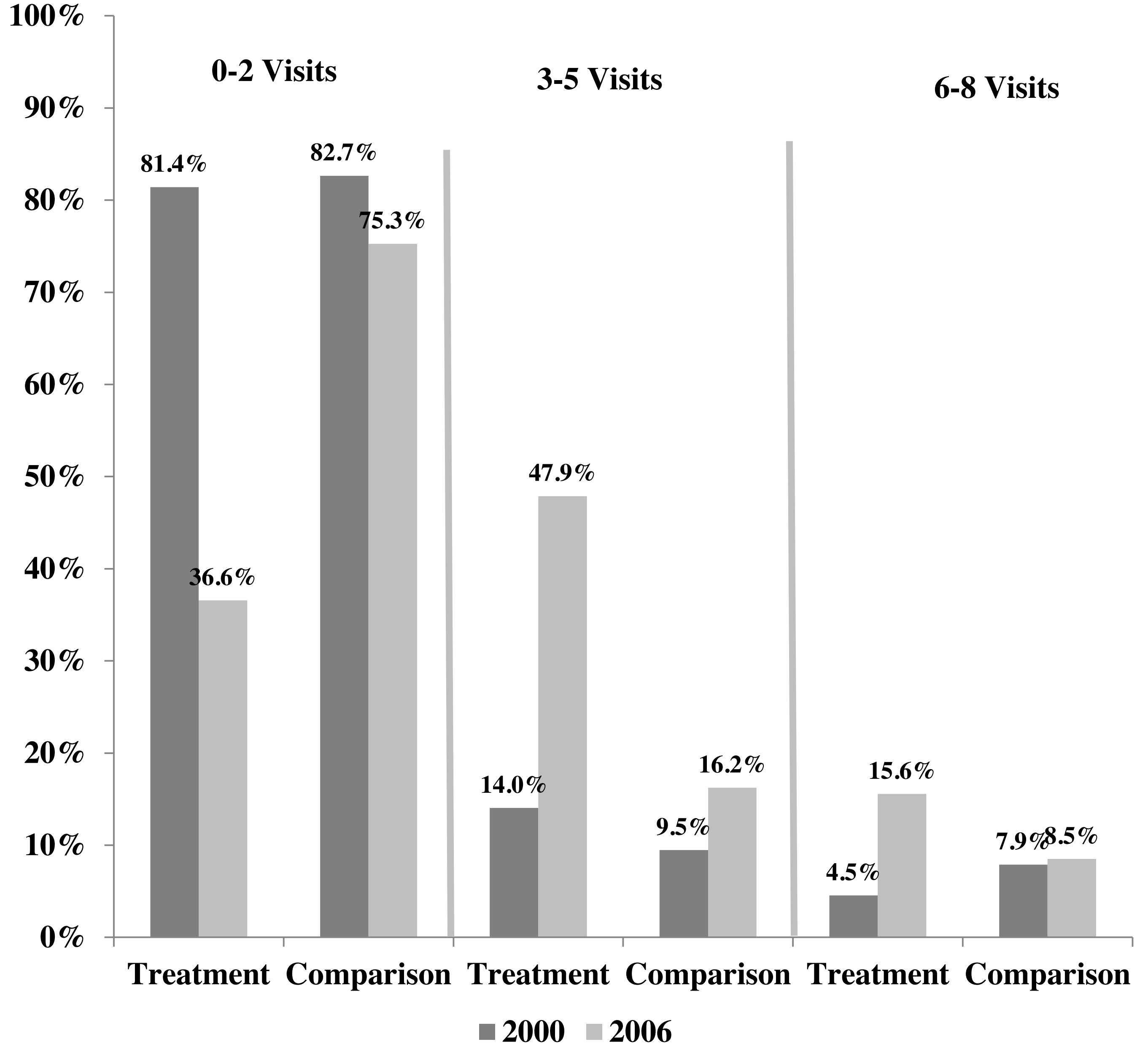

Figure 3: Distribution of Women by Number of Prenatal Care Visits to a Physician or Nurse

Notes: See notes for Figure 2

Table 1: Descriptive Statistics, 2000 and 2006 LSMS

	Samples and Sample Means		
	Women aged 15-49 who gave birth in the past 12 months (1)	Children younger than 18 months (2)	Women aged 15-49 who gave birth in the past 5 years (3)
Demographic characteristics			
Female	1.000	0.474	1.000
Age	26.918	0.915	28.756
Married	0.935		0.908
Currently employed	0.286		0.373
Years of education			
0 years	0.589		0.625
1-3 years	0.246		0.237
4+ years	0.165		0.138
House had running water?	0.522	0.529	0.554
House had flush toilet?	0.061	0.062	0.057
House had electricity?	0.467	0.504	0.517
House had concrete floor?	0.227	0.229	0.241
Outcomes			
Prenatal care			
Had prenatal care visit (PNC)?	0.731		
Number of PNC	3.439		
PNC by physician or nurse	0.268		
≥3 PNC by physician or nurse	0.226		
Vaccination			
Had vaccination card?		0.820	
Showed vaccination card?		0.488	
BCG		0.812	
DPT		0.651	
Polio		0.712	
Measles		0.406	
DPT booster		0.245	
Polio booster		0.272	
Family Planning			
Heard about birth control?			0.475
Used birth control?			0.281
Observations	504	846	1564

Notes: The sample includes indigenous individuals living in rural communities not covered by PEC in 2003. Weights are used in all calculations.

Table 2: Differences between Treatment and Control Group, Pretreatment Period
2000 LSMS

	Women aged 15-49 who gave birth in the past 12 months		Children younger than 18 months		Women aged 15-49 who gave birth in the past 5 years	
	(Coefficient)	(S. E.)	(Coefficient)	(S. E.)	(Coefficient)	(S. E.)
	(1)	(2)	(3)	(4)	(5)	(6)
Demographic characteristics						
Female	0.000	0.000	0.058	0.057	0.000	0.000
Age	0.521	0.461	-0.134*	0.071	0.237	0.681
Married	-0.009	0.046			-0.003	0.033
Currently employed	0.039	0.106			0.063	0.090
Years of education						
0 years	-0.041	0.079			-0.054	0.061
1-3 years	0.080	0.056			0.079	0.044
4+ years	-0.039	0.060			-0.025	0.030
House had running water?	0.019	0.083	0.063	0.082	0.055	0.082
House had flush toilet?	-0.089	0.057	-0.064	0.037	-0.053	0.030
House had electricity?	-0.033	0.094	0.003	0.098	-0.072	0.088
House had concrete floor?	-0.017	0.062	-0.018	0.058	0.025	0.076
Outcomes						
Prenatal care						
Had prenatal care visit (PNC)?	0.078	0.080				
Number of PNC	0.569	0.419				
PNC by physician or nurse	0.057	0.083				
≥3 PNC by physician or nurse	0.012	0.084				
Vaccination						
Had vaccination card?			-0.080	0.107		
Showed vaccination card?			-0.067	0.083		
BCG			-0.071	0.095		
DPT			-0.105	0.127		
Polio			-0.109	0.109		
Measles			-0.115*	0.065		
DPT booster			-0.054	0.135		
Polio booster			-0.111	0.109		
Family Planning						
Heard about birth control?					0.016	0.058
Used birth control?					-0.009	0.016

Observations	274	427	650

Notes: The sample includes indigenous individuals living in rural communities not covered by PEC in 2003. Data, from the 2000 LSMS, correspond to the pre-treatment period. Results are obtained from running OLS regressions of demographic characteristics and outcome variables on treatment status in 2006. Separate regressions are run for each variable and sub-sample. Coefficients (and standard errors) are presented in odd (even) columns. Standard errors are clustered at the state level. Sample weights are used in all calculations. * 10%, ** 5%, *** 1%

Table 3: Difference-in-Difference Estimates, Prenatal Care Sample
2000 and 2006 LSMS

Outcomes	All observations (1)	(2)	(3)	Observations in municipalities surveyed both years (4)
Had prenatal care visit (PNC)	0.015	0.030	0.001	-0.046
	(0.105)	(0.128)	(0.162)	(0.247)
Number of PNC	0.174	0.424	0.134	-0.741
	(0.755)	(0.764)	(0.829)	(1.790)
PNC by physician or nurse	0.288	0.284	0.205	0.171
	(0.157)*	(0.150)*	(0.140)	(0.202)
≥3 PNC by physician or nurse	0.375	0.382	0.306	0.274
	(0.154)**	(0.152)**	(0.135)**	(0.222)
With state fixed effect?	N	N	Y	N
With municipality fixed effect?	N	N	N	Y
Observations	504	504	504	280
Number of communities	112	112	112	33

Notes: The sample includes indigenous women living in rural communities not covered by PEC in 2003 that gave birth in the 12 months previous to the survey. Each cell corresponds to one OLS regression of the variable presented in the row on treatment status. Controls are age, married, employed, education (1-3 years, ≥4 years, no education as reference), running water, flush toilet, electricity, concrete floor. Standard errors (in parentheses) are clustered at the state level in regressions presented in columns 1-3 and at the county level in those presented in column 4. Sample weights are used in all calculations. * 10%, ** 5%, *** 1%

Table 4: Difference-in-Difference Estimates, Childhood Vaccination Sample
2000 and 2006 LSMS

	All observations			Observations in municipalities surveyed both years
Outcomes	(1)	(2)	(3)	(4)
Had vaccination card?	0.234**	0.182**	0.211***	0.315***
	(0.099)	(0.072)	(0.058)	(0.099)
Showed vaccination card?	0.183	0.187	0.103	0.108
	(0.158)	(0.160)	(0.134)	(0.175)
BCG	0.176	0.120	0.167**	0.267**
	(0.102)	(0.073)	(0.060)	(0.107)
DPT	0.241*	0.166	0.212*	0.248
	(0.131)	(0.111)	(0.113)	(0.189)
Polio	0.298**	0.222**	0.277**	0.283
	(0.108)	(0.092)	(0.104)	(0.168)
Measles	0.313**	0.152**	0.230***	0.219**
	(0.115)	(0.068)	(0.057)	(0.103)
DPT booster	0.245	0.166	0.221	0.197*
	(0.156)	(0.140)	(0.142)	(0.113)
Polio booster	0.371***	0.279***	0.298***	0.182
	(0.120)	(0.096)	(0.098)	(0.131)
With state fixed effect?	N	N	Y	N
With municipality fixed effect?	N	N	N	Y
Observations	846	846	846	511
Number of communities	124	124	124	45

Notes: The sample includes indigenous children younger than 18 months living in rural communities not covered by PEC in 2003. Each cell corresponds to one OLS regression of the variable presented in the row on treatment status. Controls are age, gender and household services including, running water, flush toilet, electricity and concrete floor. Standard errors (in parentheses) are clustered at the state level in regressions presented in columns 1-3 and at the county level in those presented in column 4. Sample weights are used in all calculations. * 10%, ** 5%, *** 1%

Table 5: Difference-in-Difference Estimates, Family Planning Sample
2000 and 2006 LSMS

Outcomes	All observations (1)	(2)	(3)	Observations in municipalities surveyed both years (4)
Heard about birth control?	-0.050	-0.033	-0.027	-0.024
	(0.090)	(0.069)	(0.069)	(0.126)
Used birth control?	-0.033	-0.031	-0.035	0.019
	(0.050)	(0.043)	(0.026)	(0.069)
With state fixed effect?	N	N	Y	N
With municipality fixed effect?	N	N	N	Y
Observations	1,564	1,564	1,564	914
Number of communities	142	142	142	48

Notes: The sample includes indigenous women ages 15-49 living in rural communities not covered by PEC in 2003 that gave birth in the five years previous to the survey. Each cell corresponds to one regression of the variable presented in the row on treatment status. Controls are age, married, employed, education (1-3 years, ≥4 years, no education as reference), running water, flush toilet, electricity, concrete floor. Standard errors (in parentheses) are clustered at the state level in regressions presented in columns 1-3 and at the county level in those presented in column 4. Sample weights are used in all calculations. * 10%, ** 5%, *** 1%

Table 6: Robustness Check, Difference-in-Difference Estimates for Demographic Variables
2000 and 2006 LSMS

	All observations		Observations from municipalities observed in both years	
	(1)	(2)	(3)	(4)
Women aged 15-49 who gave birth in the past 12 months				
Age	-0.304	(0.847)	0.676	(2.377)
Married	-0.037	(0.108)	-0.049	(0.160)
Currently employed	-0.115	(0.172)	-0.295	(0.217)
Education: 0 years	-0.045	(0.126)	0.004	(0.229)
Education: 1-3 years	-0.164*	(0.079)	-0.103	(0.143)
Education: 4+ years	0.209	(0.147)	0.100	(0.182)
House had running water?	0.108	(0.152)	-0.039	(0.241)
House had flush toilet?	0.032	(0.038)	-0.005	(0.027)
House had electricity?	-0.045	(0.143)	0.091	(0.188)
House had concrete floor?	0.031	(0.133)	-0.217	(0.159)
Observations	504		280	
Children younger than 18 months				
Age	0.231*	(0.123)	0.091	(0.166)
Female	0.177*	(0.093)	0.138	(0.157)
House had running water?	0.225	(0.169)	0.052	(0.166)
House had flush toilet?	0.019	(0.054)	0.001	(0.047)
House had electricity?	0.215	(0.138)	0.162	(0.162)
House had concrete floor?	0.074	(0.116)	-0.224*	(0.126)
Observations	846		511	
Women aged 15-49 who gave birth in the past 5 years				
Age	-0.023	(1.281)	1.810	(1.462)
Married	0.014	(0.042)	-0.019	(0.083)
Currently employed	-0.199	(0.123)	-0.282**	(0.132)
Education: 0 years	-0.031	(0.084)	0.046	(0.159)
Education: 1-3 years	-0.106*	(0.053)	-0.083	(0.100)
Education: 4+ years	0.137	(0.087)	0.037	(0.142)
House had running water?	0.155	(0.172)	0.146	(0.170)
House had flush toilet?	0.058	(0.048)	0.037	(0.036)
House had electricity?	0.207	(0.121)	0.215	(0.160)
House had concrete floor?	0.041	(0.105)	-0.207*	(0.103)
Observations	1,564		914	

Notes: The sample includes indigenous individuals living in rural communities not covered by PEC in 2003. Results are obtained from running differences-in-differences regressions of demographic characteristics on treatment status in 2006. Separate regressions are run for each variable and sub-sample. Regressions in columns 1-2 (3-4) contain state (county) fixed effects. Coefficients (and standard errors) are presented in odd (even) columns. Standard errors are clustered at the state level for regressions presented in columns 1-2 and at the county level for those presented in columns 3-4. * 10%, ** 5%, *** 1%

Table A.I: Indicators and Targets for NGOs

Group	Indicator	Target
Panel A: Pregnant Women and Mothers		
Prenatal Care	At least one prenatal care check-up	75
	Prenatal care check-up in the first trimester	21
	Three prenatal care check-ups	40
Postnatal Care	Postpartum check-up during first 40 days (mothers)	44
Panel B: Children		
Vaccines	Complete vaccine schedule by age 1	95
Vitamines, iron	Vitamin A after turning 6 months	85
	Two vitamin A doses between ages 1 and 5	55
	Iron by age 1	60
	Iron every 3 months between ages 1 and 5	60
Check-ups	Postpartum check-up during first 28 days	44
	Two medical check-ups before turning 1	90
	One yearly medical check-up between ages 1 and 5	90
	Growth assessments in 2 consecutive months per trimester by age 2	55
	One growth assessment per trimester between ages 2 and 3	70
Curative care	Pneumonia cases receiving standard treatment	100
	Dhiarrea cases receiving starndard treatment	100
Panel C: Women 15-49		
Family Planning	Using a family planning method	17
	Starting to use a family planning method	24
	Continuing the use of a family planning method	60
Other	Third doses of TDA	90
	PAP smear	10

Notes: Constructed based on documents from the Guatemalan Ministry of Health.

[1] In a pooled sample of Demographic and Health Surveys (DHS) from 36 developing countries, it is found that about two thirds of children are born in rural areas and coverage of health services are significantly lower in these areas compared to urban populations (computed from Smith et al., 2005). For example, 44 percent of children in rural areas have the recommended immunization compared to 62 percent in urban areas. Van de Poel et al. (2009) analyze DHS data for 47 countries and find that stunting and under-5 child mortality are significantly higher in rural areas versus urban areas.

[2] Macinko, Guanais, & Marinho (2006) use administrative panel data at the state level in Brazil to evaluate a large program that implemented medical mobile teams and find evidence of reductions on infant mortality. Several experimental small-scale field trials have shown evidence that reductions in neonatal mortality can be produced through interventions that train and direct community health workers to provide home-based neonatal services (Bang, Bang, Baitule, Reddy, & Deshmukh, 1999; Manandhar, Osrin, Shrestha, Mesko, & Morrison, 2004; Kumar et al., 2008).

[3] This level also includes the NGOs contracted under PEC to provide mostly preventive services focused on mother and child health.

[4] There are 330 counties in the country (called municipalities in Guatemala).

[5] Medical teams were encouraged to also provide curative services though in practice these actions were not prioritized given the absence of targets on these dimensions and that the medical mobile teams were present only once a month in each village.

[6] Still, we empirically checked that indeed selected communities were mostly rural, indigenous and did not have a public-health post or health center.

[7] Unfortunately, we could only access administrative records for these years.

[8] To match the lists of covered communities by the program with the census of communities we searched for communities in the same municipality and with exactly the same name and for unmatched locations, we then manually searched for communities with slightly different spelling. We were able to match close to 75% of localities that comprises about 80% of population in both the 2003 and 2005 registries. Correlations between population totals reported in the 2002 Census and in the program covered lists were around 0.65.

[9] Communities were added to the program in January 2004 and January 2005 and the second survey was implemented in September 2006. Therefore, this sample restriction assures that women in treatment areas had their full pregnancy covered by the program.

[10] The recommended vaccination schedule can be found at:

http://www.cdc.gov/vaccines/recs/schedules/downloads/child/0-6yrs-schedule-pr.pdf

[11] BCG is administered right after birth, DPT and POLIO when children turn 2 months and Measles at 12 months.

[12] Estimated impacts are qualitatively similar when focusing on the sample of respondents that reported having or that actually showed their vaccination cards.

[13] In the case of the last specification (Column 4) in Tables 3 to 5, we cluster standard errors at the county level. Because in this case the number of clusters is significantly larger (68 to 83, depending on the sample) the estimation of standard errors is safely executed using standard statistical procedures.