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

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ABSTRACT *Low population density in rural developing countries coupled with deficient infrastructure, weak state capacity and limited budgets makes increasing health care coverage difficult. Contracting-out mobile medical teams may be a helpful solution in this context. This article examines the impact of a large-scale programme of this type in Guatemala. We document large impacts on immunisation rates for children and prenatal care provider choices. The programme increased substantially the role of physician and nurses at the expense of traditional midwives. The results indicate that mobile medical teams substantially increased coverage of health care services in Guatemala, and could be effective in other developing countries.*

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, & 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 diseases such as diarrhea, malaria and pneumonia that can be prevented or treated successfully by inexpensive and simple measures (Jones, Steketee, Black, Bhutta, & Morris, 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 medical 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 mobile medical teams that regularly visit communities and provide basic health and preventive services. This intervention

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can be implemented directly under the public health system or it can be contracted to NGOs that have flexibility to adapt to local contexts to ensure better implementation. Knowing whether mobile medical teams can provide significant improvements in coverage could help policy-makers in designing effective strategies to improve services in hard to reach areas. 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 question, this article evaluates an extensive health-coverage expansion programme that targeted rural areas in Guatemala through contracted out mobile medical teams. The *Programa de Extension de Cobertura* (Coverage Extension Programme, 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 portion 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 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 programme expanded rapidly, covering about three million individuals by 2000. Enrolment was stable until 2003, when the programme entered a second expansion, wave adding about 0.9 million people in newly covered communities by 2005 (Figure 1).

We exploit this second expansion and combine information on programme coverage with the 2000 and 2006 Guatemalan Living Standards Measurement Surveys (LSMS) data to estimate effects on intermediate health utilisation indicators such as prenatal care, childhood immunisations and family planning methods. Because the programme was targeted at rural, indigenous population, we restrict the sample to this population. We also restrict to communities not covered by 2003. This sample restrictions increase homogeneity before the intervention. 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).

The main contribution of the article 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 mobile medical teams administered by contracted-out NGOs. The contracting out programme evaluated here involved providing services to about 4.2 million people in Guatemala by 2006, about a third of the country's population. The findings, presented in more detailed in the next sections, suggest that large expansions of preventive health services are possible in a short period through the contracted-out mobile medical teams.

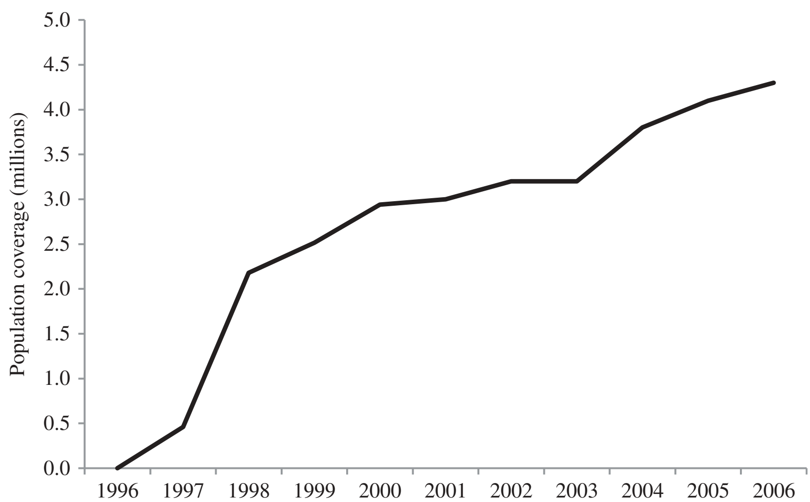

Figure 1. Population covered by PEC.

2. Background

A. The Health Care System of Guatemala

Guatemala is considered a lower-middle-income country with a current 2005 PPP-adjusted GDP per capita of $5,772 (World Bank, 2014). Although Guatemala's per capita GDP is similar to its neighbours, the country fares worse against the rest of the Central American countries in health indicators. For example, infant and child mortality rate (25/1,000 and 41/1,000 respectively) in Guatemala are higher compared with the rest of Central America (21/1,000 and 31/1,000 respectively).

The health care system can be characterised as fragmented and with low levels of coordination. The Ministry of Health and the Guatemalan Social Security Institute (*Instituto Guatemalteco de Seguridad Social*, IGSS) provides health services along with 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 that were staffed by an auxiliary nurse and cover about 2,000 individuals. The second level was composed of 335 health centres that had differing capability levels but were all staffed by at least a certified nurse. In general, there was a health centre in each county located in the capital (16% of counties did not have a health centre, 75% had exactly one centre and 9% have more than one).[3] The third level included 42 hospitals. In each state capital there was one hospital and nine states had more than one. The health infrastructure network remained virtually unchanged in the 1996 to 2006 period. The major health reform during this period was the introduction and expansion of PEC (Becerrill-Montekio & Lopez-Davila, 2011).

B. The Coverage Extension Programme

In 1997, the government of Guatemala launched the Coverage Extension Programme (*Programa de Extension de Cobertura,* PEC) to rapidly scale-up the provision of primary health services in rural, underserved areas. Under this programme, NGOs were contracted to provide a basic package of health services in a group of assigned communities through the creation of mobile medical teams.[4] These teams were composed of a physician or a nurse and a health assistant, and were expected to visit the communities at least monthly. During these visits immunisations, check-ups and other mostly preventive services focused on pregnant mothers and young children were delivered and occasionally education sessions were held.[5] The provision of services involved building strong relationships with key actors in the community.[6] Based on the existing documentation, NGOs did not run health posts or health centres. In areas not covered by the PEC, the rural population had to seek primary health services from the network of health posts (Danel & La Forgia, 2005). Because of the weak transportation system and rugged geography, access to health services was difficult for many rural communities not served by PEC.

NGOs were selected based on a two-step process. In the first step, NGOs have to present information on legal status, location, staff, infrastructure, equipment and experience in providing medical care to become licensed. In the second step, because the price paid was fixed, licensed NGOs competed on the technical merits of their proposal and they were assessed using a pre-determined criteria (La Forgia, Mintz, & Cerezo, 2005). Each contract awarded to an NGO typically covered 10,000 individuals. 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 contract defined the services covered, the corresponding targets, the communities to be served, and the amount, terms and conditions for payments to the NGO. The contract became more explicit and the renewal process also became stricter over time (La Forgia et al., 2005). In fact, 23 agreements were renewed conditionally on improvements in 2006 and 5 of them were eliminated in 2007 (Lao Pena, 2013).

The first phase of the programme (1997–1999), can be characterised as one of rapid expansion under a weak management environment for planning, supervision and monitoring. The programme experienced only a slight increase in enrolment and suffered some deep budget cuts between 2000 and 2004. The new president elected in 2004 envisioned PEC as one of its key programmes. As a result, population coverage started to increase again, reaching 4.1 million in 2005. Payments per capita increased substantially and returned to the typical 1997–1999 levels (around $8 per capita annually).[7] Supervision was strengthened and some performance targets were increased. Our evaluation exploits the geographical expansion of the programme that took place in 2004–2005. Therefore, the estimated effects correspond to the strengthened version of the programme 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 to be covered. Public officials report that there was no strict procedure followed to determine communities selected; instead, decisions were taken in an ad hoc manner.[8]

During the second expansion of the programme, the focus of this study, there were 21 health service targets that were monitored for contract renewal (Table A1 in the Online Appendix). Four indicators focused on prenatal and postnatal care. Twelve indicators were related to children aged under five years and included vaccines, provision of Vitamin A and iron, regular check-ups, and appropriate treatment for pneumonia and diarrhea. Finally, there were five indicators of services for women aged between 15 and 49, and these focused on the provision of family planning services, the use of Diphtheria, Tetanus and Pertussis vaccines, and pap smears.[9]

3. Data

The article 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 include 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 aged 0–5 and for women aged 15–49 that had given birth in the previous five years; information on knowledge and the use of family planning services is elicited. Because a sampling frame that involved unequal probabilities of selection was used, all results in the paper are generated using survey weights for representativeness.

To determine coverage of the programme over time, we obtained administrative records containing the list of covered communities in 2003 and 2005.[10] These lists were matched to the census of communities in 2002, which in turn was matched to the LSMS 2000 and 2006.[11] Programme administrators advised us that between 2000 and 2003 there were minor changes in geographic coverage.

4. Methodology

A. Sample Construction

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. Because the programme was targeted to rural, indigenous populations, 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 analyse 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.

To analyse impacts on prenatal care, we restrict the sample to women aged 15 to 49 who gave birth in the 12 months before the surveys.[12] Table 1 presents summary statistics pooling data from 2000 and 2006 for the treatment and comparison groups. Column 1 reports statistics for the sample used to estimate effects on prenatal care. About 60 per cent of these women did not have any formal education and access to housing services is low (for example, only 6% had flush toilets). Regarding prenatal care, 73 per cent of them receive some type of prenatal care service, though a physician or nurse

Table 1. Descriptive statistics 2000 and 2006 LSMS

	Means by subsample		
	Women aged 15–49 who gave birth in the past 12 months	Children aged 2–24 months	Women aged 15–49 who gave birth in the past 5 years
	(1)	(2)	(3)
Demographic characteristics			
Female	1.000	0.477	1.000
Age	26.918	0.915	28.756
Married	0.935		0.908
Currently employed	0.286		0.373
No education	0.589		0.625
1–3 years of education	0.246		0.237
House had running water?	0.522	0.537	0.554
House had flush toilet?	0.061	0.053	0.057
House had electricity?	0.467	0.513	0.517
House had concrete floor?	0.227	0.238	0.241
Prenatal Care (PNC)			
Had prenatal care visit?	0.731		
Number of PNC	3.439		
PNC by physician or nurse	0.268		
≥3 PNC by physician or nurse	0.226		
Vaccination (children 2–12 months)			
Had vaccination card?		0.836	
Showed vaccination card?		0.486	
BCG		0.813	
DPT		0.644	
Polio		0.676	
Vaccination (children 12–24months)			
Measles		0.666	
DPT booster		0.426	
Polio booster		0.470	
Family planning variables			
Heard about birth control?			0.475
Used birth control?			0.281
Observations	504	898	1,564

Notes: The sample includes indigenous individuals living in rural communities not covered by PEC in 2003. There are 477 children aged 2 to 12 months and 460 children aged 12 to 24 months.

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 per cent of them have three or more prenatal care visits with a physician or nurse.

To examine the programme impacts on vaccination rates, we exploit the available information in the surveys and focus on two groups of children. The LSMS surveys report first dose of Bacille de Calmette et Guérin (BCG), Diphtheria, Pertussis, Tetanus (DPT) and polio. The only dose of BCG should be administered right after birth and the first dose of DPT and polio when children are two months old.[13] Hence, we check effects on the first dose of BCG, DPT and polio on children aged 2–12 months old. We also check for this sample whether respondents have vaccination cards for children and are able to show them to enumerators. Additionally, the LSMS surveys report measles vaccinations (to be administered at 12 months) and first boosters for DPT and polio. For this second set of indicators, we check effects on children 12–24 months old. Column 2 of Table 1 presents summary statistics for the vaccination sample. About 84 per cent of survey respondents report that these children have vaccination cards, though about only half can show the cards to the enumerators.

Vaccination rates in the described samples hover between 65 and 80 per cent for DPT, polio and BCG, although coverage for measles and boosters for DPT and polio present significantly lower rates. Note that if the respondent adult did not have a vaccination card for a child, information was collected through self-report.

Finally, we analyse impacts on family planning by focusing on women aged between 15 and 49. Unfortunately, questions about knowledge and use of contraception methods were only made to women that had given birth in the previous five years in the 2000 and 2006 LSMS (40% of women in this age group). This sample selection suggests caution in interpreting results on this outcome. Column 3 of Table 1 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 per cent using them.

B. Empirical Strategy

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 lend themselves 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 $\bar{y}^{tb}$ and $\bar{y}^{ta}$ 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:

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

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 Equation (1) can also be captured in a regression model of the form:

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

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.

Using this specification, we will obtain unbiased estimate of β 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 issue by presenting estimated differences between the treatment and comparison groups for demographic and outcome variables in the pre-treatment period. These estimates are obtained running regressions of demographic and outcome variables on treatment indicators separately for the different samples. In the sample of women aged 15–49 that gave birth in the previous year, we list 15 variables that will be either outcomes or covariates in our regressions. For children, we check balance of demographic characteristics for those aged 2–24 months old. We check baseline differences on having a vaccination card, showing it and first dose of BCG, DPT and polio for children aged 2–12 months. We assess baseline balance on measles and DPT and polio boosters in the sample of children 12–24 months old. For family planning, we explore baseline balance for women aged 15–49 that had given birth in the previous five years. Results indicate that the variables are well balanced across the treatment and comparison groups for the analysed samples.

Table 2. Differences between treatment and comparison groups, pretreatment period 2000 LSMS

	Women aged 15–49 who gave birth in the past 12 months		Children aged 2–24 months		Women aged 15–49 who gave birth in the past 5 years	
	(Coefficient)	(SE)	(Coefficient)	(SE)	(Coefficient)	(SE)
	(1)	(2)	(3)	(4)	(5)	(6)
Demographic characteristics						
Female			0.028	0.089		
Age	0.521	0.461	–0.117	0.050**	0.237	0.681
Married	–0.009	0.046			–0.003	0.033
Currently employed	0.039	0.106			0.063	0.090
No education	–0.041	0.079			–0.054	0.061
1–3 years of education	0.080	0.056			0.079	0.044
House had running water?	0.019	0.083	0.111	0.094	0.055	0.082
House had flush toilet?	–0.089	0.057	–0.048	0.028	–0.053	0.030
House had electricity?	–0.033	0.094	–0.025	0.109	–0.072	0.088
House had concrete floor?	–0.017	0.062	–0.002	0.073	0.025	0.076
Prenatal Care (PNC)						
Had prenatal care visit?	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 (2–12 months old)						
Had vaccination card?			–0.193	0.141		
Showed vaccination card?			–0.059	0.120		
BCG			–0.176	0.108		
DPT			–0.226	0.140		
Polio			–0.218	0.136		
Vaccination (12–24 months old)						
Measles			0.007	0.118		
DPT booster			0.059	0.194		
Polio booster			–0.072	0.152		
Family Planning						
Heard about birth control?					0.016	0.058
Used birth control?					–0.009	0.016
Observations	274		412		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 for each variable in the table are run. Coefficients (and standard errors) are presented in odd (even) columns. Negative coefficient indicates a lower rate for the treatment group. There are 241 children aged 2–12 months and 196 children aged 12–24 months. Standard errors are clustered at the state level. *10 per cent, **5 per cent, ***1 per cent.

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:

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

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, currently employed), but we include indicators for categories of mother's education because this variable has been linked to children's outcomes.

The PEC programme was implemented in many small communities throughout rural Guatemala. Because there are potentially 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:

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

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

As a further effort of controlling for local time-variant 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 analysed 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:

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

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 of 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 behaviour 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. Empirical Results

In this section, we describe the main results of the study. First, we provide evidence on the programmes impact on prenatal care. Figure 2 provides a graphical illustration of the results where we present the distribution of women with respect to the prenatal care provider in 2000 and 2006 for the treatment and comparison samples respectively. The fraction of women without prenatal care falls by 1.5 percentage points ((10.6–25.7)–(19.9–33.5)). In contrast, the fraction using a physician or nurse for their prenatal care increases dramatically by 28.8 percentage points ((64.6–26.1)–(29.9–20.5)). It is

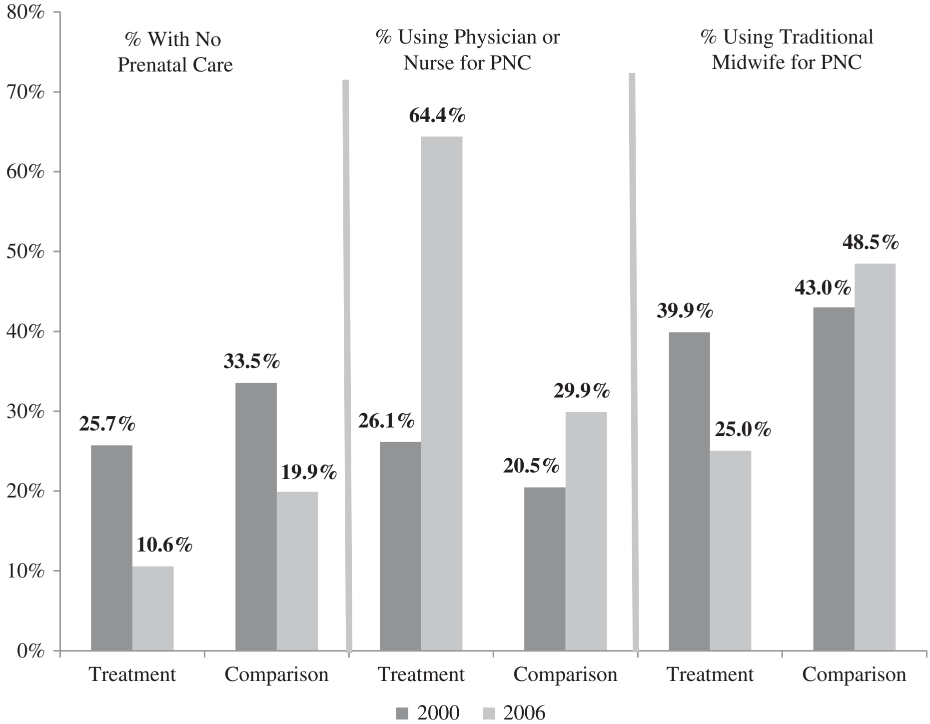

Figure 2. Distribution of women by provider of prenatal care.
Notes: The sample includes indigenous women living in rural communities not covered by PEC in 2003 who gave birth in the last 12 months before the survey.

no surprise there is a corresponding drop in the fraction that use a traditional midwife for prenatal care by 9.4 percentage points.

These important changes are also present when considering the distribution of women with respect to the number of prenatal care check-ups 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 three or more prenatal care visits.

In Table 3 we report the treatment effect coefficient for the various difference-in-difference models for prenatal care outcomes. The first column in Table 3 presents the results for the simple difference-in-difference specification formalised in Equation (2) above, where errors are clustered at the state level. These results are, as expected, identical to the numeric calculations suggested by Figures 2 and 3. Results indicate that the programme had modest and statistically insignificant impacts on the fraction of women receiving prenatal care services and the number of average visits. However, results indicate that the fraction of women receiving prenatal care by a physician or nurse increased by 29 percentage points (p-value < 0.10). Even larger effects, closer to 38 percentage points, are found when analysing the fraction of women receiving three or more prenatal care visits by a physician or nurse.[14]

To explore the robustness of the findings, in columns 2 to 4 we implement the empirical specifications described 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

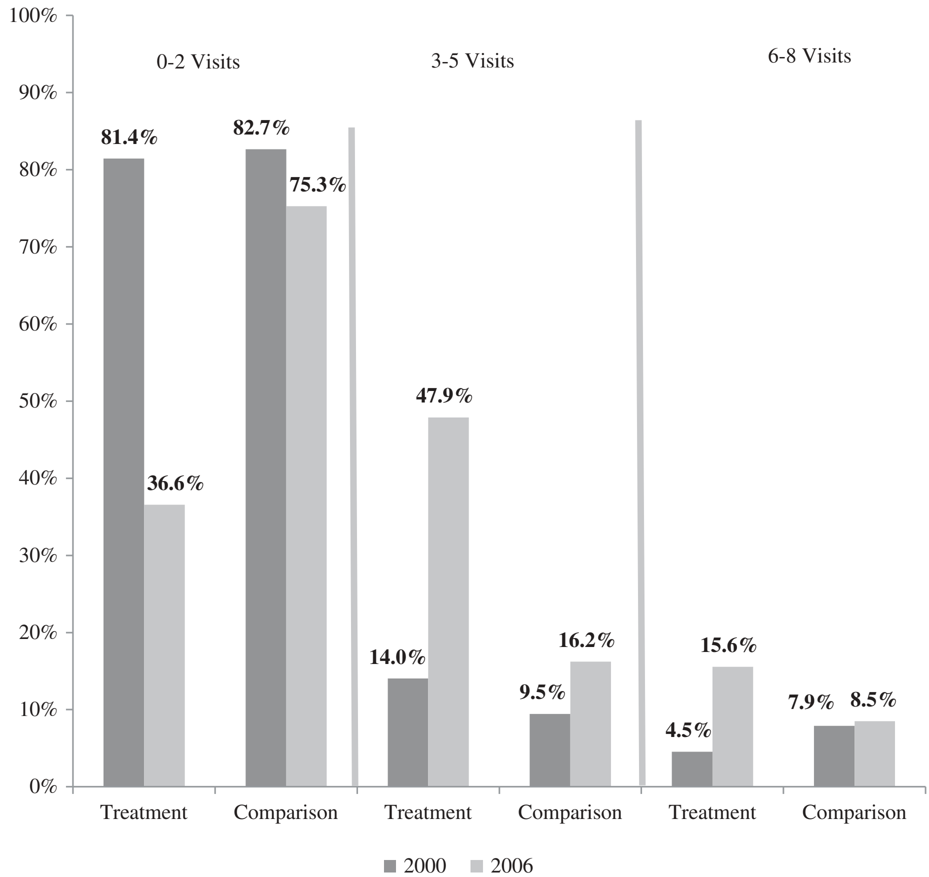

Figure 3. Distribution of women by number of prenatal care visits to a physician or nurse.
Notes: The sample includes indigenous women living in rural communities not covered by PEC in 2003 who gave birth in the last 12 months before the survey.

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 a physician or nurse, loses statistical significance.[15]

Table 4 presents estimated impacts on children immunisation. Panel A presents results on the first dose of BCG, DPT and polio for children aged 2–12 months while panel B reports effects on measles and boosters for DPT and Polio for children aged 12–24 months. As before, we present in column 1 results from the simple difference-in-difference specification. Results suggest that the programme was effective in improving vaccination outcomes across the board. Estimates point to an increase in about 38 percentage points in the fraction of vaccination card holders but no statistically significant impact in the fraction showing it. For initial dose vaccines, there are positive statistically significant impacts in the range of 32 percentage points (BCG) to 45 percentage points (polio).

The lower panel reports estimated effects for measles and polio booster are of 36 and 41 percentage points respectively although effects for polio booster are not statistically significant. Results are robust to adding controls and state dummies (columns 2 and 3) though when adding county dummies the estimates become imprecise and lose statistical significance (column 4).[16]

Turning to family planning outcomes, Table 5 documents that the impact on knowledge and use of contraception for women ages 15–49 are small in magnitude and not statistically significant at the 10 per cent level. However, because of data limitations, these effects are estimated for women that had given

Table 3. Difference-in-difference estimates, prenatal care sample 2000 and 2006 LSMS

	All observations			Observations in counties surveyed both years
Outcomes	(1)	(2)	(3)	(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)
Controls	N	Y	Y	Y
State fixed effects	N	N	Y	N
County fixed effects	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 prior to the survey. Each cell corresponds to one OLS regression of the variable presented in the row on treatment status. Controls include 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. *10 per cent, **5 per cent, ***1 per cent.

birth in the previous five years to the time of the survey. Because only 40 per cent of women aged 15–49 had given birth in the last five5 years, the results on these outcomes should be taken with caution.

Turning to the robustness of the main findings, in the Online Appendix we report results showing that trends in demographics variables have evolved similarly across the treatment and comparison groups, providing some evidence that outcome variables should have evolved in the same fashion across groups in the absence of treatment. Additionally, we explore the robustness of the results to applying procedures for adequate inference to take into account the small clusters present in specifications that require clustering standard errors at the state level.

6. Conclusions and Discussion

Improving the health of rural populations in developing countries will require increasing significantly the coverage of proven effective medical services. This is a challenging endeavour 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 programmes aiming to improve health coverage in rural areas implemented at a large scale. This article aims to contribute to this literature by evaluating a large-scale programme that contracted out mobile medical units to reach previously underserved population in rural Guatemala. The article exploits the 2004–2005 expansion of this programme with 2000 and 2006 LSMS data, and a difference-in-difference framework to analyse the programme's impacts on prenatal care, child vaccination and family planning measures. 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.

The evaluation documented substantial increases in immunisation coverage. The vast majority of mothers in rural Guatemala consider vaccines as effective in preventing disease (Humpage, 2013). However, achieving complete immunisation for children in areas with poor access to health clinics

Table 4. Difference-in-difference estimates, childhood vaccination sample 2000 and 2006 LSMS

	All observations			Observations in counties surveyed both years
Outcomes	(1)	(2)	(3)	(4)
Panel A: children aged 2–12 months				
Had vaccination card?	0.381**	0.361**	0.350***	0.332**
	(0.145)	(0.139)	(0.118)	(0.139)
Showed vaccination card?	0.070	0.088	–0.029	0.079
	(0.150)	(0.160)	(0.127)	(0.215)
BCG	0.320**	0.310**	0.311***	0.387**
	(0.126)	(0.114)	(0.091)	(0.146)
DPT	0.413**	0.360**	0.337*	0.406*
	(0.161)	(0.166)	(0.176)	(0.239)
Polio	0.446**	0.394**	0.386**	0.473*
	(0.152)	(0.171)	(0.173)	(0.247)
Observations	477	477	477	275
Number of communities	103	103	103	32
Panel B: children aged 12–24 months				
Measles	0.356**	0.262*	0.367**	0.465
	(0.154)	(0.147)	(0.136)	(0.285)
DPT booster	0.149	0.099	0.151	–0.014
	(0.233)	(0.223)	(0.254)	(0.229)
Polio booster	0.410**	0.361**	0.324	–0.076
	(0.172)	(0.156)	(0.199)	(0.241)
Controls	N	Y	Y	Y
State fixed effects	N	N	Y	N
County fixed effects	N	N	N	Y
Observations	460	460	460	234
Number of communities	102	102	102	31

Notes: The samples include indigenous children 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 include age, gender, mother's education (1–3 years, ≥4 years, no education as reference) and household services including, running water, flush toilet, electricity and cement 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. *10 per cent, **5 per cent, ***1 per cent.

requires significant transportation costs for mothers who may not be willing or able to cover them. The programme decreased substantially these costs and this may explain the documented large effects on immunisation coverage.

In terms of prenatal care, the programme produced a 30 percentage point increase in prenatal care by a physician or nurse, but did not change the fraction of women with any prenatal care or the number of visits. Note that NGOs had to achieve targets of 75 per cent of pregnant women receiving prenatal care from a physician or nurse and 40 per cent receiving three check-ups. These targets were quite ambitious, taking into account that, prior to the programme, only 18 per cent of women in treatment areas receive three prenatal care visits from a physician or nurse. To achieve these targets, NGOs relied on forging strong relationships with the communities. This strategy seems to have been successful, considering the large increase in the fraction of physician and nurses that provided prenatal care services.

Regarding family planning, we have documented no effects on women ages 15–49 that had given birth in the last five years. Whether these results hold for all women ages 15–49 is an open question. Notwithstanding these data limitations, qualitative evidence also suggests limited effects on family planning outcomes. Former programme officials report that there are strong cultural barriers that need

Table 5. Difference-in-difference estimates, family planning sample 2000 and 2006 LSMS

	All observations			Observations in counties surveyed both years
Outcomes	(1)	(2)	(3)	(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)
Controls	N	Y	Y	Y
State fixed effects	N	N	Y	N
County fixed effects	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 last 5 years. Each cell corresponds to one regression of the variable presented in the row on treatment status. Controls include 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. *10 per cent, **5 per cent, ***1 per cent.

to be surmounted for increased adoption of family planning methods. In their opinion, these strong social norms explain why NGOs typically faced significant challenges in achieving the targets related to the use of contraceptive methods.

The documented programme effects on immunisation rates and prenatal care are important, given that evidence has linked changes in coverage of these services and health outcomes.[17] A large literature has established the significant health benefits, and even productivity gains, produced by increases in vaccination rates (Bloom, Canning, & Weson, 2005; Ehreth, 2003; Lee, 2012). Moreover, the increased role of physicians and nurses as prenatal care providers, at the expense of traditional midwives, should also 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 check-ups and survey results that suggest that traditional midwives, even trained ones, may provide substandard care (Goldman & Glei, 2003; Jones et al., 2003). The effectiveness of this programme in producing large improvements in key health care utilisation in a short timespan may provide some hope to policy-makers around the world frustrated by the failure of many previous efforts.

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Notes

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, Ruel, & Ndiaye, 2005). For example, 44 per cent of children in rural areas have the recommended immunisation compared to 62 per cent in urban areas. Van de Poel, O'Donnell, and Van Doorslaer (2007) analyse DHS data for 47 countries and find that stunting and under-five child mortality are significantly higher in rural areas versus urban areas.
2. Macinko, Guanais, and Marinho (2006) use administrative panel data at the state level in Brazil to evaluate a large programme that implemented mobile medical 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 et al., 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. The nature of the NGOs seems to differ considerably. There were religious, indigenous-focused, community-based and other NGOs that seem to have originated from economic groups. Examples of NGOs in these four groups were: Fundacion Menonita Kekchi de Guatemala; Centro Maya para el Desarrollo Comunal; Asociacion de Padres de Familia del Proyecto Nuevo Amanecer; and Fundacion de la Caficultura para el Desarrollo Rural (Ministry of Health of Guatemala, 2007). The large majority of NGOs were local (Lao Pena, 2013).
5. Medical teams were encouraged to also provide curative services, though in practice these actions were not prioritised, given the absence of targets on these dimensions and that the mobile medical teams were present only once a month in each village.
6. The provision of services also involved a facilitator in each community that provided local support during the visits of the medical team. The facilitator should promote attendance to the convergence centres and also participate with the medical team in the attention of beneficiaries. There were also 'health volunteers' who had 30 families assigned each. They were expected to promote attendance to the convergence centres and monitor growth and adequate related practices for children younger than three years old (Ministry of Health of Guatemala, 2007).
7. Public health expenditure per capita was $56 and total health expenditure per capita was $171 in 2006 (World Bank, 2014).
8. Still, we empirically checked that indeed selected communities were mostly rural, indigenous and did not have a public health post or health centre.
9. More information about this survey can be obtained at: http://www.ine.gob.gt/index.php/encuestas-de-hogares-y-personas/condiciones-de-vida.
10. Unfortunately, we could only access administrative records for these years.
11. To match the lists of covered communities by the programme 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 per cent of localities that comprises about 80 per cent of population in both the 2003 and 2005 registries. Correlations between population totals reported in the 2002 census and in the programme covered lists were around 0.65.
12. Communities were added to the programme in January 2004 and January 2005 and the second survey was implemented approximately in September 2006. Therefore, this sample restriction ensures that all women in treatment areas had their full pregnancy covered by the programme.
13. BCG is the vaccine against tuberculosis. DPT is the vaccine against Diphteria, Pertussis (whooping cough) and Tetanus. Three doses of DPT and polio should be administered when children are two, four and six months old. The vaccination schedule for Guatemala can be found at http://munisalud.muniguate.com/2012/01feb/estilos_saludables01.php.
14. Throughout the article, effects on binary dependent variables are estimated using linear probability models. Marginal effects estimated from probit models following Ai and Norton (2003) are qualitatively similar. In particular, estimated marginal effects from the (linear probability model) and the [probit model] for our statistically significant results from Tables 3–5 are: three or more prenatal care visits by physician or nurse (0.382) and [0.398]; had vaccination card (0.361) and [0.340]; BCG vaccinations (0.310) and [0.291]; DPT vaccinations (0.360) and [0.369]; polio vaccinations (0.394) and [0.401]; measles vaccinations (0.262) and [0.320]; and polio booster (0.361) and [0.386]. Standard errors are also qualitatively similar. Complete results estimated using probit models are available upon request.
15. We also intended to examine effects on prevalence of delivery in a health facility or with a skilled birth attendant for women that had given birth in the last year. However, these variables in the 2000 LSMS are missing for all women that had given birth in 2000. This problem reduces the sample to only 72 observations (26% of the original sample), making the estimation of effects unreliable.
16. Estimated impacts are qualitatively similar when focusing on the sample of respondents that reported having or that actually showed their vaccination cards.
17. Because of data limitations, we cannot assess effects on 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 in historically underserved areas.

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