Impact of *Oportunidades* on Skilled Attendance at Delivery in Rural Areas

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Introduction

Although low in absolute numbers, Mexico's maternal mortality ratio of 63.3 per 100,000 live births per year in 2005 (Secretaría de Salud 2006) appears high when compared with that of other countries of similar or lower economic development such as Argentina, Bulgaria, Costa Rica, and Moldova (World Health Organization 2004). The reduction of maternal mortality is a priority for the Mexican health sector, and it is one of the commitments of the Mexican government for achieving the Millennium Goals (Torres and Mújica 2004). As a means to reduce maternal mortality, Mexican health institutions have been working to increase skilled attendance at delivery (Secretaría de Salud 1995, 2002), defined as the attendance at a delivery by skilled personnel under conditions that allow the provision of quality delivery services (Graham, Bell, and Bullough 2001).

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Among several strategies adopted by the Mexican health sector to increase skilled attendance, the Human Development Program *Oportunidades* (previously called PROGRESA) plays an important role. *Oportunidades* includes efforts to improve the health of mothers and children through the provision of skilled attendance at delivery. While the impact of *Oportunidades* on several other reproductive health outcomes has been documented, there is little information about the impact of this program on the incidence of deliveries with skilled attendance among the beneficiary population. The objective of this paper is to assess the impact of *Oportunidades* on skilled attendance at delivery in rural areas through the application of a variety of evaluation techniques, taking advantage of the experimental design implemented for the evaluation of this program in rural areas.

The main results of the study indicate that *Oportunidades* had, at best, only a small effect on skilled attendance at delivery in treatment communities. The program appears to have had a larger effect on the relatively high-fertility women who had one birth just prior to the experimental treatment and another subsequent to the experimental treatment.

Background

Oportunidades is a conditional cash transfer program that started in rural areas in 1997. Its aim is to improve the education, health, nutrition, and living conditions of population groups in extreme poverty and to break the intergenerational cycle of poverty. By 2005, the program had enrolled 5 million families in urban and rural areas, containing more than 25 million people across the country. The program works through three major components (Secretaría de Desarrollo Social 1997):

a. Cash transfers to families for children attending school: These payments vary according to the number of children in the family and their age and gender. Payments are larger for girls in higher education grades. These cash payments are made to the female head of the household, and they are conditioned on compliance with the attendance of children at school and other responsibilities.

b. In the area of health, the program offers an essential health care package that includes pregnancy and delivery care for women enrolled in the program. Health care is provided by either the Ministry of Health (MOH) or the Mexican Institute of Social Security (IMSS-*Oportunidades*). The program also includes a series of health promotion talks that are presented monthly by MOH or IMSS-*Oportunidades* personnel. These talks include information on family planning, prenatal care, alarm signs during pregnancy, and newborn care. In the case of delivery attendance, health institutions are responsible for providing, as part of the essential health package of *Oportunidades*, delivery attendance in their facilities. Attendance at the health promotion talks and medical checkups are requirements for continuing enrollment in the program and receiving cash payments.

c. The program also distributes nutritional supplements with minerals and vitamins for all children under 2 years of age, pregnant or lactating women, and undernourished children 2–4 years old.[1]

Oportunidades focuses on families living in extreme poverty. To identify eligible families in rural areas, program administrators selected areas with high concentrations of poor households that also had schools and health care facilities available for the implementation of the program. They then conducted a census of households in those areas, applied a survey questionnaire, and conducted a visual inspection of household characteristics to construct a poverty score for each household. Families with a score below a program-determined threshold were considered eligible to participate in the program.[2] As an additional step, the eligibility status results were validated in a community assembly. In the vast majority of cases, community assemblies confirmed the household's eligibility status (Skoufias et al. 2000). Families deemed eligible received further information about the benefits and requirements of the program and were invited to enroll. Nearly all eligible households in participant localities (97%) agreed to comply with the eligibility rules and coresponsibilities of the program (Orozco, Parker, and Hernández 2000).

Oportunidades may increase the proportion of deliveries with skilled attendance through several mechanisms. First, women in enrolled households receive free delivery attendance as part of the essential health package provided by the program, as long as they comply with the coresponsibilities of the program and make at least five prenatal care visits. Women who are not in *Oportunidades* may have to pay for delivery attendance at public or private hospitals. Second, *Oportunidades* may increase skilled delivery attendance through an income effect, because the cash transfers may motivate a higher demand for skilled attendance, in either private or public facilities. Third, the health promotion talks may increase the contact of women with the health services providers. This likely

[1] In recent years, *Oportunidades* added a component of support to senior adults and a savings fund for youth.

[2] The cutoff point for eligibility to the program was defined by the administrators of the program, and it varied by region of the country (Skoufias, Davis, and Behrman 2000).

provides more information about care during pregnancy, and it may increase the likelihood of having a birth in a medical unit.

Although maternal mortality has shown a reduction in Mexico, falling 12.7% between 1995 and 2000 and 12.8% between 2000 and 2005 (Secretaría de Salud 2006), many of those deaths could be prevented with good-quality health care during pregnancy and delivery. Even more, because a substantial proportion of maternal deaths occur around the delivery, and sometimes because of complications difficult to identify even with adequate prenatal care, skilled attendance at delivery has been identified as an effective intervention to reduce maternal mortality and to raise the chances of neonatal survival (Donnay 2000; Campbell and Graham 2006; Filippi et al. 2006).

Skilled attendance at delivery in Mexico was 81.5% of all births in the period 1994–97, but it had a level of only 58.9% in rural areas (CONAPO 2000). Since the mid-1990s, official efforts have been made to provide skilled attendance at all births and to reduce disparities between urban and rural areas. It is one of the actions included in *Oportunidades*, focusing on the poor population, where maternal deaths occur more frequently. Therefore, the evaluation of the effect that *Oportunidades* has in skilled attendance is crucial to define whether this approach is working or whether different strategies should be used to increase skilled attendance at delivery and in turn reduce maternal mortality in Mexico.

Several studies have documented positive impacts of *Oportunidades* in rural and urban areas in the area of education (Parker 2003, 2005; Behrman, Parker, and Todd 2005; Parker, Behrman, and Todd 2005; Todd et al. 2005), consumption (Angelucci, Attanasio, and Shaw 2005; Attanasio and Di Maro 2005), and nutritional status (Rivera et al. 2004). In the area of health, the evaluations indicate that *Oportunidades* has led to a reduction in the number of episodes of disease and an increase in the use of health services among the beneficiary population (Bautista et al. 2003; Gutiérrez et al. 2005). In the area of reproductive health, several studies have documented a positive impact of *Oportunidades* in the knowledge and use of family-planning methods and antenatal care (Huerta and Hernández 2000; Hernández, Urquieta, et al. 2005).

No study has yet analyzed in detail the impact of *Oportunidades* on skilled attendance. The only analysis of the impact of the program on this outcome was descriptive and found no differences in the overall proportion of deliveries attended by physicians or in medical units in rural or urban areas (Hernández, Urquieta, et al. 2005). This paper examines in detail the impact of *Oportunidades* on skilled attendance through the application of more rigorous program effect estimation techniques.

Evaluation Design and Data

At the inception of *Oportunidades* in rural areas, an experimental design was implemented to assess the impact of the program. Some rural localities were initially randomized to have families eligible to enroll in the program in early 1998 and other localities were randomized to be control areas. A total of 320 localities were defined as intervention localities and 186 as controls in seven of the states in which the program started operations in 1997 (Guerrero, Hidalgo, Michoacán, Puebla, Querétaro, San Luis Potosí, and Veracruz).

A baseline survey, called the Encuesta de Características Socioeconómicas de los Hogares 1997 (ENCASEH 1997), collected basic household information to determine their classification as eligible to the program or not. This information was collected in both intervention and control localities between October and November 1997, before the random assignment of communities to treatment and control areas. Panel surveys called Encuesta de Características de los Hogares (ENCEL) collected information in two rounds per year in 1998, 1999, and 2000. The first round in 1998 (ENCEL 1998) constitutes, along with the ENCASEH 1997, the baseline of the evaluation.

Enrollment of households into the program started in intervention localities in 1998. Starting in August 1999 households in control areas became eligible to enter the program. By 2000 all localities in the control group had been incorporated into the program. Because of this, one can use the experiment to measure impacts only between January 1998 and July 1999.

The eligibility status of a household was determined by program administrators through the estimation of a poverty score and the validation of results in a community assembly. Consequently, eligibility status does not exactly match the criterion of having a poverty score under the eligibility cutoff point.

Fertility histories, including information on birth order, prenatal care, and type of attendant at delivery, were collected in the ENCEL 1998 and in the first follow-up round of the ENCEL 2000. The information was provided typically by the woman who was in charge of the care of children in the household, and in some cases by other women of reproductive age (15–49 years old). Table 1 shows the number of households included in the analysis. Out of 24,077 households in the baseline survey, 20,493 were observed in the 2000 follow-up survey (ENCEL 2000), which represents a follow-up rate of 85.1%. The follow-up rate did not vary by treatment area or eligibility status.

This analysis includes only households with at least one woman reporting a birth in either the ENCEL 1998 or the ENCEL 2000 evaluation survey. The unit of observation of the study is the birth, and the outcome of interest, skilled attendance at delivery, is defined as whether or not the delivery was

TABLE 1
NUMBER OF HOUSEHOLDS IN THE SAMPLE

	All Evaluation Sample		Analysis Sample	
	Baseline (ENCEL 1998) (1)	Follow-up (ENCEL 2000) (2)	Baseline[a] (ENCEL 1998) (3)	Follow-up[b] (ENCEL 2000) (4)
Intervention localities	14,856	12,509	2,011	1,659
Eligible households	7,837	6,685	1,531	1,155
Noneligible households	7,019	5,824	480	504
Enrolled in the program	…	7,767	…	1,222
Control localities	9,221	7,984	1,269	1,056
Eligible households	4,682	4,099	925	701
Noneligible households	4,539	3,885	344	355
Total sample of households	24,077	20,493	3,280	2,715

[a] Analysis sample includes all households with women with a birth between January 1996 and July 1997.
[b] Analysis sample includes all households with women with a birth between January 1998 and July 1999.

attended by either physicians or nurses or in a health care facility. The two ENCEL surveys contain information on skilled attendance for births to women 15–49 years old. The baseline information used in this analysis includes births that occurred between January 1996 and July 1997, and the follow-up information refers to births that occurred between January 1998 and July 1999. The analysis sample includes 3,280 households with women who had births in the baseline measure and 2,715 households with women who had births in the follow-up measure, as can be seen in columns 3 and 4 of table 1. In this analysis we use as covariates information on eligibility status, poverty score, age, state of residence, schooling of women, and women's ability to speak an indigenous language.[3]

We conducted a comparison of the baseline characteristics of women and households in the intervention and control groups using preintervention data from the ENCASEH 1997. Table 2 presents baseline characteristics of households ($n = 2{,}715$) and women ($n = 2{,}732$) included in the follow-up from the analysis sample. We compared characteristics of households and women in intervention versus control areas for eligible and noneligible households using regression analysis including as covariate a dummy variable for state and adjusted by clustering at the locality level. We find that eligible and noneligible households in treatment and control localities have baseline characteristics similar to those found in other studies (Schultz 2004).

Table 3 shows the proportion of births with skilled attendance at baseline

[3] Urquieta et al. (2008) examined whether there was differential sample attrition of fertility by treatment status. They found no evidence that these two outcomes were related to the assignment of treatment and control communities.

TABLE 2
BASELINE CHARACTERISTICS OF FOLLOW-UP SAMPLE (THOSE WITH BIRTHS FROM JANUARY 1998 TO JULY 1999)

	Intervention		Control	
	Eligible (1)	Noneligible (2)	Eligible (3)	Noneligible (4)
Household characteristics:				
Observations	1,155	504	701	355
Eligibility (poverty) score	633.91	839.31	632.54	846.59
	(84.78)	(91.19)	(80.89)	(97.56)
% enrolled in *Oportunidades*	95.67	23.21	…	…
Household size (number of persons in the household)	6.19	5.30	6.16	5.47
	(2.33)	(2.59)	(2.42)	(2.74)
Women living in the household/household size	.50	.51	.51	.58
	(.16)	(.15)	(.16)	(.16)
Number of women 15 years old or older	1.43	1.75	1.43	1.76
	(.77)	(1.02)	(.78)	(1.03)
Number of children of age 12 or younger	3.14	1.73	3.10	1.81
	(1.62)	(1.43)	(1.66)	(1.53)
Number of persons of age 18 or younger	3.81	2.43	3.78	2.62
	(2.06)	(1.87)	(2.02)	(2.02)
Number of persons of age 65 or older	.13	.17	.11	.18
	(.39)	(.47)	(.36)	(.46)
Number of indigenous members/household size	.34	.21	.38	.21
	(.36)	(.35)	(.38)	(.37)
Number of literate members/number of household members 5 years of age or older	.49	.66	.48	.67
	(.22)	(.22)	(.23)	(.22)
% households with migrant last 5 years	.7	3.1	1.8*	2.5
% households with a person with disability	3.9	2.3	2.7	3.3
% households that had social security	6.1	7.1	4.5	10.4
% households that own farm animal	31.2	40.5	28.5	37.5
Women's characteristics:				
Observations	1,158	507	708	359
Age	27.82	25.04	27.11	25.61
	(7.34)	(7.47)	(7.83)	(8.71)
Education:				
% without instruction	28.7	9.9	30.4	11.7
% elementary	65.8	68.4	63.1	66.3
% secondary or higher	5.5	21.7	6.5	22.0
% indigenous language	47.2	25.0	50.3	26.7

Note. Standard deviations are in parentheses.
* Significant at 10% for the comparison between eligible intervention vs. eligible control groups, by regression analysis adjusted by clustering at the locality level.

TABLE 3
PROPORTION OF BIRTHS WITH SKILLED ATTENDANCE AT DELIVERY

		Intervention Areas				Control Areas			
		Eligible		Noneligible		Eligible		Noneligible	
Time	Birth During	%	*N*	%	*N*	%	*N*	%	*N*
		A. Women with Births in Any Study Period							
Preintervention[a]	January 1996–July 1997	46.31	1,531	68.96	480	48.54	925	72.38	344
Postintervention[b]	January 1998–July 1999	43.28	1,183	71.04	518	38.09	722	71.66	367
Δ pre-post		−3.03		2.08		−10.45		−.72	
		B. Only Women with Births in Both Study Periods							
Preintervention[a]	January 1996–July 1997	28.87	284	60.71	56	32.19	146	60	55
Postintervention[b]	January 1998–July 1999	35.92	284	69.64	56	28.08	146	70.91	55
Δ pre-post		7.05		8.93		−4.11		10.91	

Note. *N* denotes all births in the respective group.
[a] Source of data: ENCEL 1998.
[b] Source of data: ENCEL 2000.

and after the intervention started among the eligible and noneligible individuals, for intervention and control groups. Panel A presents results for all births from women who had a birth in any of the periods under study, and panel B pertains to women who had births in both periods. Interestingly, there was a decrease in the proportion of deliveries with skilled attendance between the pre- and postintervention measures among eligible women in intervention areas (−3.03 percentage points), but the decline was larger in control areas (−10.45 percentage points). When we consider only the sample of women who had a birth in both waves of the study, the proportion of births with skilled attendance among eligible women after the start of operation of the program shows an increase of 7.05 percentage points in intervention areas and a decrease of 4.11 percentage points in control areas.

Methodology

We take advantage of the experimental design implemented in rural areas to estimate different program effects on women's use of skilled attendance at delivery. We start by estimating intention to treat (ITT) in the overall population and on those eligible for the program. We examine potential endogeneity eligibility status using difference-in-difference (DID) estimation strategies. The features of the program eligibility process made it possible to use a regression discontinuity analysis (RDA) approach to estimate program effects (Angrist and Krueger 1999; Hahn, Todd, and Van der Klaauw 2001). The DID constitute our preferred set of impact estimates and are the ones presented in more detail; we use the other methods to check for robustness of results. The analytic approaches are described in detail below.

In the experimental design, a simple comparison between women in treatment and control areas would be enough to estimate an ITT effect of the program. The main assumption of this approach is that treatment assignment took place independently of the potential outcomes with and without the treatment. Even if this were not the case, it may be possible to obtain more accurate estimates of the treatment effect by controlling for preassignment variables. By randomization, these pretreatment variables are unrelated to the actual treatment status. The postintervention information is sufficient to identify the impact of the program. Using the information on all women in the evaluation sample who had births in the postintervention period (January 1998–July 1999), we estimate the following empirical specification using ordinary least squares (OLS):

$$y_{ij} = X_{ij} \cdot \beta + \delta \cdot T_j + \mu_{ij}. \quad (1)$$

The dependent variable y_{ij} takes the value one if the birth of woman i of locality j had skilled attendance, and zero if not; T_j is a dummy variable indicating whether locality j is a treatment area; X_{ij} is a vector of individual and household exogenous characteristics measured at the baseline;[4] β and δ are parameters to be estimated; and μ_{ij} is the error term. We include the X_{ij} in this equation to control for some particular forms of differences between treatment and control areas that were not perfectly eliminated by the randomization. The coefficient δ in equation (1) is our ITT estimator. It measures the average difference in the proportion of women with skilled attendance between treatment and control areas after controlling for the linear effects of the variables in X_{ij}. It makes no distinction, however, between the impact of the program on the eligibles and the noneligibles within the treatment area.

We are also interested in examining the effect of *Oportunidades* on the program's target population, that is, on those deemed eligible to receive the intervention. We estimate another type of ITT effect that we call the focused or targeted ITT effect. In order to examine the differential effect of the program on the eligible group, we can estimate the following equation for all women who had births in the postintervention period (January 1998–July 1999) using OLS:

[4] The vector X_{ij} includes age (using dummy variables for the following categories: 15–19 and 35 or more years old), schooling (using dummies for primary and secondary or more), speaking an indigenous language, and a polynomial for the poverty score including the natural log of the poverty score and its square.

$$y_{ij} = X_{ij} \cdot \beta_1 + \beta_2 \cdot T_j + \beta_3 \cdot E_{ij} + \delta \cdot (T_j \cdot E_{ij}) + \mu_{ij}, \tag{2}$$

where E_{ij} is a dummy variable indicating that the woman is in an eligible household. In equation (2), our impact estimate δ measures the average difference in the proportion of women with skilled attendance between treatment and control areas for eligible women compared to noneligible women, after controlling for the linear effects of the variables in X_{ij}. In other words, here δ indicates the effect of the program on the eligibles relative to the effect on the noneligibles. An advantage of this approach is that it provides a measure of the ITT effect on the subset of women who were the target of the program. It also permits an examination of potential spillover program effects on the noneligible group of women under the assumption of perfect randomization (denoted in the model by the term β_2). One can also estimate equation (1) only on the sample of eligible women in the treatment and control areas to measure this focused ITT effect.

The panel structure of the data enables us to deal with the potential endogeneity problem using DID. This approach would enable us to remove time-invariant unobserved community characteristics that could be potential sources of endogeneity. To do this, we take advantage of the skilled attendance at delivery information in pretreatment time reported at the baseline round. In order to consider the same window of time, for pretreatment time we include births from January 1996 to July of 1997 and for posttreatment time, births from January 1998 to July 1999. The sample of analysis was restricted to eligible women. We estimated the following model:

$$y_{ijt} = X_{ijt}\beta_1 + \beta_2 T_j + \beta_3 t_t + \delta(T_j \cdot t_t) + \mu_{ijt} \tag{3}$$

if the woman was eligible ($E_{ij} = 1$), where the subscript t denotes the time period. The dummy variable t_t takes a value of one for the follow-up time period and a value of zero to indicate the baseline time period; μ_{ijt} represents the stochastic error term. The vector X_{ijt} includes only baseline characteristics. In equation (3), β_3 measures the change in the skilled attendance between the baseline and follow-up in nontreatment areas, and δ measures the differential change in skilled attendance at delivery for women in treatment localities experienced between the baseline and follow-up relative to the change experienced by women in nontreatment localities. That is, δ is the DID estimator of the targeted ITT effect after controlling for pretreatment differences between the eligible groups in the treatment and control areas.

To conduct the DID analysis we considered only one birth per woman in the postintervention period. For women with two births in the postintervention period, we chose the last birth that occurred during January 1998–July 1999. We fit equation (3) with OLS for eligible women using an unbalanced panel (women with births reported in any of the pre- or postintervention periods) and also with a balanced panel (women with births in both pre-and postintervention periods). For the latter group, we also fit the model in equation (3) controlling for fixed effects at the individual level.

As indicated above, eligibility for the program was defined using a poverty score (PS), and final eligibility status was determined in community assemblies in which results from the PS-based classification were validated. As a robustness check, we took advantage of this mechanism to estimate program effects using an RDA approach. We compared women who were just below the cutoff point to women who were just above the cutoff point (Hahn et al. 2001; Van der Klaauw 2002).

The RDA approach is intuitively appealing: given that treatment localities were selected because of their overall condition of poverty, we would not expect large differences in observed and unobserved characteristics between resident women located immediately above and those immediately below the cutoff point, except for the fact that those below were deemed eligible for the program. For this RDA approach to work, program eligibility must be discontinuous at the threshold and the cutoff point must be exogenous for the groups of women that will be compared. Figure 1 shows the distribution of the proportion of households classified as eligible for the program by different PS levels.[5]

For the RDA we use observations with PS that are within "windows" of varying width centered at a score of 752. The sample of analysis is then defined by

$$W = 1(|PS_i - 752| \leq \eta_k) \quad \text{for } \eta_k \in \{20, 30, 50, 75, 100, 120\}.$$

The use of different-size windows in the RDA poses a trade-off between bias and precision. With smaller windows, one may have less bias because the women are more similar in unobserved characteristics. At the same time, however, using a smaller window means that one would also have fewer observations and consequently less precision. Similarly, with larger windows one would have more precision, but one runs the risk of comparing less similar

[5] Because the actual value of the eligibility cutoff point used by the program administrators was not available, we estimated it using a discriminant analysis in which the group variable was being eligible, and the discriminatory variable was the PS (details of this estimation can be obtained from the authors on request).

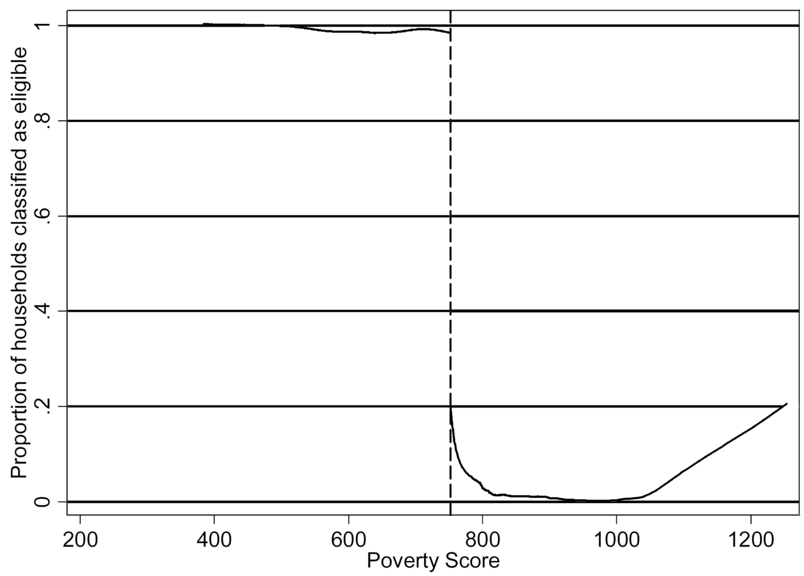

Figure 1. Lowess curve for the proportion of households classified by the program as eligible by poverty score. Vertical line marks a poverty score of 752, the estimate of the cutoff point for eligibility status.

women who might differ significantly and systematically in unobserved characteristics. We present results for a variety of windows.

For each of the windows, we estimate the following model using OLS:

$$y_{ij} = X_{ij} \cdot \beta_1 + \delta \cdot E_{ij} + \varepsilon_{ij} \tag{4}$$

if treatment area equals one and $W = 1(|\mathrm{PS}_i - 752| \leq \eta_k)$, where δ is our impact estimate for the targeted ITT. It measures the average difference in the proportion of women with skilled attendance between eligible and non-eligible women in treatment areas after controlling for the linear effects of the variables in X_{ij} for women in each of the windows of size W.

The estimation of equation (4) assumes that the window was small enough so that one could consider women on either side of the threshold to be quite similar in all other characteristics. As an alternative, for each of the windows, we use a DID estimator that exploits the panel data we have available. We also fit equation (4) controlling for fixed effects at the individual level, where our impact estimates refer to the targeted ITT effect purged of time-invariant women-specific unobservables.

Results

Table 4 presents the results of models estimated using only cross-sectional follow-up (postintervention) data. The two columns of estimates present the

TABLE 4
CROSS-SECTIONAL EFFECTS ON SKILLED ATTENDANCE AT DELIVERY, USING FOLLOW-UP SAMPLE, ITT AND TARGETED ITT

	OLS	
y = {1: Qualified, 0: Nonqualified}	ITT (1)	Targeted ITT (2)
Area × eligible status		.028
		[.038]
Area {1: intervention, 0: control}	.028	.009
	[.027]	[.035]
Eligible status {1: eligible, 0: noneligible}		−.088***
		[.033]
Age 1 {1: 15–19 years old, 0: 20–34 and 35 or more years old}[a]	.048	.041
	[.040]	[.040]
Age 2 {1: 35 years or more, 0: 15–19 and 20–34 years old}[a]	.018	.017
	[.021]	[.021]
Schooling 1 {1: primary, 0: none and secondary or more}[b]	.092***	.091***
	[.024]	[.024]
Schooling 2 {1: secondary or more, 0: none and primary school}[b]	.216***	.215***
	[.038]	[.038]
Indigenous language	−.153***	−.152***
	[.030]	[.030]
Ln poverty score	−3.959*	−2.039
	[2.121]	[2.161]
Ln poverty score^2	.343**	.186
	[.163]	[.167]
Constant	11.61*	5.839
	[6.906]	[6.999]
Observations	2,790	2,790

Note. Robust standard (by locality) errors are in brackets. Includes dummies by state.
[a] The coding of dummy variables for age groups leaves the 20–34-year-old women as the reference category.
[b] The coding of dummy variables for schooling leaves women with no schooling as the reference category.
* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

ITT and the targeted ITT results that correspond to equations (1) and (2), respectively, described in the previous section. After controlling for exogenous differences between treatment and control areas, we find that, on average, the program increased the probability of using skilled attendance at delivery by 2.8 percentage points in the treatment areas and among eligible women. These program effects, however, are not statistically different from zero at the 10% significance level. In column 2 of table 4, the nonsignificant estimate of 0.9 percentage points for the area of residence suggests that the program had no spillover effects on the noneligible group. In all models presented in table 4, higher schooling was positively associated with skilled attendance and speaking an indigenous language was negatively associated with it. In order to explore whether the program had an impact on the poorest women, we replicated the

TABLE 5
PROGRAM EFFECTS ON SKILLED ATTENDANCE AT DELIVERY, DIFFERENCE-IN-DIFFERENCE RESULTS

	Targeted ITT[a]	
	Unbalanced Sample[b] (1)	Balanced Sample[c] (2)
OLS	.048	.114**
	[.031]	[.048]
Fixed effects	...	.118**
		[.047]
Observations	4,315	860

Source. ENCEL 1998 and ENCEL 2000.
Note. Robust standard (by locality) errors are in brackets. To conduct the DID analysis we considered only one birth per woman. For women with two births in the postintervention period, we considered the last birth that occurred between January 1998 and July 1999. Therefore, the number of births in this table differs from the ones presented in table 3.
[a] Using T_j. All regressions include controls for age, schooling, indigenous language, poverty score, poverty score squared, and dummies by state. A full set of estimates is available from the authors on request.
[b] Includes all women for each wave with any birth reported.
[c] Includes only women with births in both pre- and postintervention periods.
** Significant at 5%.

ITT by OLS estimation using a sample of women with a poverty score below or equal to 577.[6] In this sample, we found an ITT effect estimate of 6.1 percentage points (SE = .0491); however, we found no statistically significant impact estimates at the 5% or 10% level. It is important to note that the sample size reduced to only 446 women. We also estimate DID models using two samples of births. Column 1 of table 5 shows results from pooling all last births observed in the baseline (January 1996–July 1997) with all births in the follow-up (January 1998–July 1999) periods. We call this the unbalanced sample. Column 2 presents results from the sample of women who had births in both the baseline and the follow-up periods, which is called the balanced sample.

Table 5 presents the estimated program effects from targeted ITT models as specified in equation (3). The first row presents results from the simple specification of the DID model and the second row presents results from further adding to the model fixed-effects controls at the individual level. Using the

[6] All women with a poverty score of 577 or less were considered eligible by both the program administrators' criteria and by the communities' assemblies.

unbalanced sample, we estimated that the ITT program effect on eligible women was 4.8 percentage points; however, it was not significantly different from zero at the 10% level. Using the balanced sample, we estimated a larger ITT program effect of 11.4 percentage points, which is significantly different from zero at the 5% level. It is considerably larger than the point estimate we obtained for the unbalanced sample, suggesting that these women differ considerably from the sample of all mothers. Adding individual-level fixed effects, however, changed the magnitude of the program effect only slightly. The fact that the adjustment for fixed effects does not change the estimates to any appreciable extent suggests that better controls for individual-level effects might have only a limited impact on the estimated program effects for the unbalanced panel.[7]

Since the program was established in early 1998 and in our analysis the postintervention period is defined from January 1998 to July 1999, it is possible that some women in the intervention group were not fully exposed to the program during the whole duration of their pregnancy. The consequence of this differential exposure to the intervention is that it could affect the likelihood of having skilled attendance at delivery. To explore this problem, we replicated the analysis presented in tables 3, 4, and 5 excluding births occurring in the first quarter of 1998. We obtained results similar to those of the whole analysis sample. The proportions of births with skilled attendance in intervention and nonintervention areas, for either eligible or noneligible women, remained similar after we removed those births, and the effect estimates obtained for the ITT and targeted ITT in the cross-sectional analysis or using DID methods were also similar whether those births were included or excluded (results are available from the authors on request).

Results from the RDA approach to estimate program effects on the eligible women are presented in table 6. Simple OLS estimates show a small targeted ITT effect in the range of −1.2 to 3.5 percentage points, but all of them are not significantly different from zero at the 10% level. Controlling for potential endogeneity using DID models increases the magnitude of the program effect

[7] It can be argued that skilled attendance may be more likely to be sought for first births. To test whether there was a difference in the impact of the program conditional on birth order, we replicated the analysis presented in table 5 for women in their first birth (primiparous women, $n = 329$ births) in the baseline or follow-up and for women in subsequent births (multiparous women, $n = 3{,}987$ births) in both measures (results available from the authors on request). For multiparous women, we found impact estimates slightly larger than the ones found for the unbalanced panel using OLS (5.5 percentage points, significantly different from zero at the 10% confidence level). For primiparous women, we found a negative impact estimate of 3.3 percentage points, not significant at the 10% or 5% confidence level. However, the sample size for the analyses of primiparous women is small, and therefore the results should be considered with caution.

TABLE 6
REGRESSION DISCONTINUITY ANALYSIS, PROGRAM EFFECTS ON SKILLED ATTENDANCE AT DELIVERY BASED ON ELIGIBILITY TO THE PROGRAM (COEFFICIENTS OF E_{ij})

	Cross-Sectional Targeted ITT OLS (1)	Targeted ITT Panel Data DID (2)
Window 20 (*N* = 228; panel *N* = 38)	.016	.192
	[.113]	[.198]
Window 30 (*N* = 327; panel *N* = 58)	.002	.261*
	[.098]	[.150]
Window 50 (*N* = 501; panel *N* = 83)	.006	.165
	[.076]	[.124]
Window 75 (*N* = 693; panel *N* = 106)	.033	.125
	[.059]	[.109]
Window 100 (*N* = 888; panel *N* = 151)	.035	.095
	[.053]	[.095]
Window 120 (*N* = 998; panel *N* = 177)	−.012	.056
	[.049]	[.094]

Source. ENCEL 1998, ENCEL 2000.
Note. Robust standard errors are in brackets. All regressions control for age, schooling, indigenous language, log poverty score, log poverty score squared, and dummies at the state level. The panel analyses included fixed effects at the individual level. Analysis is conducted with the balanced panel.
* Significant at 10%.

to a range of 5.6–26.1 percentage points, with almost all point estimates not significantly different from zero at the 10% level.

Discussion

A previous study has identified a reduction of maternal mortality rates in areas served by the program (Hernández, Ramírez, et al. 2005). However, we know little about the mechanisms by which the program may reduce this kind of death. A possible explanation is that the program induces an increase in the use of modern delivery care. This paper evaluates the impact of *Oportunidades* on skilled attendance at delivery in rural areas of Mexico during the first years of the program. We take advantage of the experimental design implemented for the purpose of evaluating the program. However, even though there was a random allocation of localities to the intervention and control groups, there may be self-selection of households to participate in the program, as was pointed out by Behrman and Hoddinott (2005). Therefore, we used analytical strategies, mainly DID and RDA, to control for this potential endogeneity. Women enrolled in the program have levels of skilled attendance lower than the national average of 74.2% in 2004 (Dirección General de Información en Salud, Secretaría de Salud 2005). This suggests that there may be an important window of opportunity to increase skilled attendance at delivery among the poor population targeted by the program. However, the results based on the analysis of eligibility to the program indicate that the program had, at best, a small effect on women in intervention areas.

We also find that the program had a larger effect on women who had a birth in both the pre- and postintervention study periods. These women have had the experience of a birth but are also a group with an overall lower proportion of births with skilled attendance. However, the program seems to have a null effect on women who are going through their first birth experience, although we have limited power to properly test these hypotheses because of the small sample size. These results identify groups of women on whom the program should focus future efforts to increase skilled attendance at delivery.

It could be argued that if delivery attendance is more likely to be sought after a complicated pregnancy, then improvements in prenatal care in treatment areas may be expected to result in lower delivery attendance. As we have mentioned before, *Oportunidades* has shown a positive impact on providing prenatal care to pregnancies of enrolled women. Other studies have also shown a high coverage of prenatal care in this population (Hernández, Urquieta, et al. 2005). However, prenatal care could be considered an endogenous variable, and hence it was not included in our analysis.

This study has some limitations that should be considered in the interpretation of results. The time period of analysis is short, using data collected from 1997 to 2000, when the program was in its first years of operation. It is possible that trends and impacts of *Oportunidades* on skilled attendance may have varied in the subsequent years as the program matured in rural areas.

Other limitations of the study are related to the sources of information. No information was available on complications during pregnancy, a variable that may confound our analysis. However, given the similarity of intervention and control groups, we assume that the incidence of complications during pregnancy may not be different in both groups and therefore should not bias our results substantially. Additionally, we did not have information on the characteristics of the providers available to women in the study localities. Therefore, it was not possible to examine whether the program effect varied by the characteristics of the service providers in the localities.

These results suggest a revision of the strategies used by *Oportunidades* to increase skilled attendance at delivery. Qualitative studies in Mexican rural populations have shown that a woman's decision on the place in which her delivery should be attended is heavily influenced by significant others, especially her mother-in-law, her mother, or her husband (Freyermuth 1999). *Oportunidades*, however, directs its incentives to pregnant women. It might be important for the program to explore whether it is necessary to redesign its strategies to include those household members who heavily influence the decision about place of delivery.

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