Going Big in Health: Effect of a Large-Scale Preventive Health Policy*

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Despite the benefits of preventive healthcare, uptake is typically low. This paper studies how embedding healthcare in a conditional cash transfer program affects utilization by exploiting the roll-out of PROGRESA in Mexico. We estimate a sizable 12% increase in outpatient visits at public clinics, driven by children and women aged 20-49. This translates into improvements in reproductive healthcare and screenings for chronic diseases. However, these effects are also accompanied by increased congestion, measured with waiting times, and reductions in proxies for healthcare quality. Overall, this suggests that the benefits of this policy lever may carry unintended displacement effects.

JEL Codes: I18, I15

Key words: preventive health, conditional cash transfers, health utilization, congestion.

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1 Introduction

Across a variety of contexts, the existing literature has estimated high returns to preventive health. Despite this, uptake of preventive health interventions is typically quite low and highly sensitive to prices, even in situations where it is considerably subsidized.[1] The literature has identified present bias, information barriers, and accessibility as possible drivers of this under-investment. Kremer and Glennerster (2011) review some behavioral interventions that have sought to, at least partially, overcome this problem. Nonetheless, Bai et al. (2021) warn that, under certain conditions, such devices could even be welfare diminishing in practice.

An alternative mechanism for encouraging the adoption of preventive healthcare is to embed it as a condition for other desirable (and potentially more salient) benefits, for which the take-up elasticity is lower. Interventions such as school vaccine mandates (Lawler, 2017; Abrevaya and Mulligan, 2011) and conditional cash transfer programs (Levy and Ohls, 2010) are examples of these policy levers.[2]

In this paper, we take advantage of the national expansion of the PROGRESA conditional cash transfer (CCT) program in Mexico from 2000 to 2003 to estimate its impact on health behaviors and outcomes.[3] The program required all family members of a recipient household to attend preventive healthcare visits regularly.[4] During its initial roll-out, the program was expanded in disadvantaged rural locations, reaching over 4.2 million households in 70,436 localities by 2003, which amounts to approximately 30% of the population living in the areas served by the program (Hernández Licona et al., 2019). At its peak, the program reached 6.5 million households (equivalent to 18% of total households in Mexico), meaning that the increase in potential demand for health services created by the program is sizeable.

The questions we seek to answer in this paper are threefold. First, using administrative information, we verify that the conditionality indeed led to a significant increase in the demand for health services at public clinics and explore the demographic drivers of said increase. Second, we study

[1] See Figure 2 in Dupas and Miguel (2017) for a summary of the existing evidence.

[2] Workplace or employer-based wellness programs are another example, although recent large-scale studies have found mostly negligible effects (Cawley and Price, 2013; Jones et al., 2019).

[3] PROGRESA is a well-known and widely researched CCT program that was implemented in Mexico starting in 1997 and expanded nationally thereafter.

[4] This requirement varied in frequency depending on the age, gender and condition of each member. For example, pregnant and breastfeeding women were required to attend more frequently. See online appendix Table A6 for details.

which health behaviors changed due to the program. Finally, we analyze if the sudden increase in demand — which was unaccompanied by an increase in the supply of public clinics — led to congestion of health services which in turn could be crowding-out other types of healthcare use.[5]

To identify the causal impact of the program, we employ a regression discontinuity analysis. We mostly follow the strategy in Alix-Garcia et al. (2013), which consists of exploiting an administrative rule that defined how the program would be rolled out at the locality level. However, instead of relying on a single cutoff rule, as previously done, we use detailed administrative records and a data-driven procedure to identify region-specific cutoffs during our sample period. To be included in the program, localities were required to have access to public clinics and schools (to be able to enforce the conditionality) and had to be poor, as defined by a pre-specified government-measured index. This index and the corresponding regional cutoff levels form the basis for the discontinuity during the first years of PROGRESA's expansion.

Our first set of results shows that program eligibility indeed increased healthcare utilization at public facilities, with an average increase of around 12% more outpatient care visits relative to comparable non-eligible localities. We also show that this increase in formal, professional healthcare is accompanied by a decline in seeking alternative health services, such as non-Western medicine. This change is driven by both children and women aged 20 to 49, which is consistent with the program requirements.

We then focus on observable health behaviors related to reproductive health and chronic conditions, showing positive impacts on both access to and utilization of preventive care, such as contraceptives, prenatal doctor visits, and screenings for chronic diseases and cancer.

Our last set of outcomes explores whether this increased utilization led to congestion at these public clinics, given that infrastructure expansions did not accompany the program. We find evidence of an increase in waiting times and a decline in self-reported patient satisfaction. These findings are consistent with a decline in some dimensions of quality of care, although the data does not allow us to identify any potential adverse health impacts stemming from congestion.

[5]The implementation of the program acknowledged the need for strengthening the supply of health services to meet the additional demand (Secretaría de Desarrollo Social, 2000). However, the government only devoted resources to improving staffing and medical supply needs at the existing clinics, without increasing the actual supply of clinic services (Skoufias, 2001).

Our paper contributes to different strands of the literature. First, we add to work that has analyzed how changes in administrative rules that impose or suggest a health requirement might impact health outcomes. Studies for the US have shown that vaccine mandates for school enrollment were essential drivers for eradicating transmissible diseases among children (Lawler, 2017) and that the recent push for exemptions has led to adverse health effects (Hair et al., 2021). Other studies have focused on guidelines issued by governments and non-profit organizations. Einav et al. (2020) employ a change in the age screening requirement for mammograms and show that using RCTs to measure compliers' effects on mammogram recommendation might significantly underestimate the benefits of screening.

Second, we advance the literature on the health impacts that result from CCT program designs. The latter has been studied from different angles. Some papers have analyzed the impact of implementing a CCT versus not. They have confirmed increased demand resulting from these designs and analyzed specific outcomes influenced by the program. Analyzing a policy in Tanzania, Evans et al. (2019) find an initial surge in demand which dissipated after 2.5 years, and an increase in the likelihood of seeking treatment when ill. For a CCT in Nicaragua, Barham and Maluccio (2009) show that the program managed to increase vaccination rates and successfully reached levels required to eradicate certain diseases, an achievement that could not be met using other interventions. For the case of PROGRESA, Barham and Rowberry (2013) use the program's phase-in to find a reduction in elderly mortality, mainly driven by impacts on diabetes and infectious diseases. In the same context, Barham (2011) also finds a decline in rural infant mortality associated with the program.

Other work has focused on the contribution of the health conditionality itself. Akresh et al. (2013) do so by comparing conditional and unconditional cash transfer programs. They find that the conditionality matters since only then did health take-up increase. Attanasio et al. (2015) compare children born before and after their mother registered to the Colombian CCT program. Those excluded from the conditionality display lower utilization of clinic services and poorer health.

Finally, there has been work in the CCT literature looking at spillovers. Guerrero et al. (2020) analyze how the Peruvian CCT motivated the substitution from informal to formal health services both for targeted and non-targeted recipients. As for PROGRESA, Avitabile (2021) finds an increase in papanicolau screening among ineligible women, but no externalities in non-gender-specific tests, perhaps driven by a weakening of husbands' opposition to papanicolau screening. Gertler (2000) finds that PROGRESA significantly increased utilization of public health clinics for preventive care, including prenatal care, child nutrition monitoring, and adult check-ups. The program also lowered the number of inpatient hospitalizations, which is consistent with the hypothesis that PROGRESA lowered the incidence of severe illness. Moreover, there was no reduction in the utilization of private providers, suggesting that the increase in utilization at public clinics was not substituting public for private care. Lastly, Hernández et al. (1999) find an increase in visits to PROGRESA clinics compared to the rest. This increase ranges from 11.5% to 19.5%, and is greater for age groups 25-44 and 5-14. The authors associate this with the co-responsibility scheme that involves all household members. We extend this literature by focusing on various outcomes of preventive healthcare and by quantifying any potential downsides (in terms of waiting times and self-reported quality of care) from this surge in demand that was unaccompanied by an increase in supply.

The remainder of the paper is organized as follows. The next section provides details on the program. Section 3 describes the data. Section 4 lays out the empirical strategy. Section 5 presents the results. Lastly, Section 6 concludes.

2 Background on PROGRESA

PROGRESA is one of the most well-known CCT programs, backed by its solid institutional foundation and rigorous evaluation design.[6] Introduced in 1997, the main objective of PROGRESA was to improve the health and development of children, including education and nutrition (Secretaría de Desarrollo Social, 2000). A secondary objective was to improve adult health (Barham and Rowberry, 2013). PROGRESA sought to promote health care for all family members via a predominantly preventive approach (Secretaría de Desarrollo Social, 2000). Before its expansion in 2004, PROGRESA targeted poor rural localities only.[7] By 2001, the program had already been extended

[6]The design of PROGRESA included a randomized trial implemented between 1997 and 2000, with follow-up surveys in 2003 and 2007 to assess its short and medium-term benefits (Skoufias, 2001; Behrman et al., 2005). Eligible households in treatment localities began receiving benefits in 1998, and in 2000, eligible households in control localities joined the program (Skoufias et al., 1999; Coady, 2000; Gertler and Fernald, 2004). More recent studies have also evaluated the long-run effects of the program (Aguilar et al., 2019; Parker and Vogl, 2021).

[7]Localities are the smallest administrative unit in Mexico, which are in turn grouped into municipalities. The 2000 census recorded 199,391 localities in 2,445 municipalities. Rural localities are defined as those below 2,500 inhabitants. However, this restriction was not strictly implemented in practice, although the program was indeed focused on smaller and less developed localities.

to $67,539$ localities, which amounts to a third of all localities in Mexico. By 2004, the program changed its expansion design, opening to urban areas and reaching a total of $82,973$ localities.

Before 2004, eligibility for PROGRESA was described in the program's documentation as a multi-stage process. First, localities lacking access to schooling and healthcare infrastructure were not made eligible due to the impossibility of verifying the school and health attendance requirements established by the program (Hernández Licona et al., 2019). Second, the remaining localities were ranked based on a poverty index constructed from indicators collected during the 1990 census and the 1995 short census. Government officials established regional cutoff values for program eligibility, which is the source of variation that we exploit and further describe below. Third, once a locality was eligible, detailed information (including demographic characteristics and durable asset ownership) was gathered from every household in the locality to determine whether each particular household would become a program recipient.[8] Precise eligibility factors were not known by beneficiaries nor local authorities in order to avoid strategic manipulation (Aguilar and Vicarelli, 2022).

Household cash transfers were conditioned on children's school enrollment, preventive healthcare visits for all household members, and participation in health education training sessions to be attended by at least one member (Barham and Rowberry, 2013). Online appendix Table A6 shows the mandatory number of health check-ups per year by different age groups. In addition, pregnant women were required to have five check-ups and two additional ones while breastfeeding (Secretaría de Desarrollo Social, 2000). Cash transfers were delivered to the female head of the household every two months. Compliance was fostered by providing an appointment book to beneficiaries (Barham, 2011).

Under PROGRESA, public health clinics were required to provide a package of services (see Table A7). This included: family planning, education on basic sanitation and accident prevention, prenatal, delivery, and postpartum care, child growth monitoring, vaccination, anti-parasitic treatments, and prevention and treatment of diarrhoea, respiratory infections, tuberculosis, high blood pressure, and diabetes (Barham and Rowberry, 2013). PROGRESA had the support of 10,141 clinics, in which almost 40,000 institutional service providers collaborated, including 12,787 physicians (1.3 per clinic on average) and more than 14 thousand nurses (1.4 per clinic). Additionally, it

[8] Additionally, a fourth step consisted of a verification done by a local council, which would vouch if eligible households were in actual need of the cash transfer. Qualitative evidence suggests that this step rarely influenced the final list of recipients.

had 23,830 health assistants (2.3 per clinic) in charge of health promotion and prevention activities (Hernández et al., 1999). PROGRESA focused exclusively on primary health care services (Secretaría de Desarrollo Social, 2000).

3 Data

We combine information on program roll-out with healthcare utilization outcomes from various administrative records and a national health survey. This methodology allows us to construct a locality-level data set for our primary empirical analysis. This section describes each data source in detail.

PROGRESA Administrative Records. We obtained access to data detailing the number of cash transfers paid out at each locality per year. With this information we know how the program roll-out occurred by looking at the first payment at each locality. From PROGRESA we also obtain the geographical organization of the program by regions. We add the poverty index (or marginality index) calculated by the Federal government agency CONAPO (National Population Council) in 1995. Note that a *higher* index value corresponds to a *lower* socioeconomic status. This continuous index is the result of a principal components analysis that uses as inputs variables from the 1990 census and 1995 short census (Skoufias et al., 1999). Altogether, these data allow us to infer the regional poverty index cutoffs for inclusion into the program, which we will employ in a regression discontinuity design.

National Health Survey (ENSA). The 2000 wave of the ENSA is a nationally representative survey of the population's health status and healthcare utilization. It is an important public policy tool that disseminates the health status and nutritional conditions of various population groups in Mexico (Gutierrez et al., 2012). ENSA collects information on households at the individual level and has age-specific questionnaires for adults aged 20 or older, teenagers aged 10 to 19, and children under ten. In addition, it contains specific questions for all household members that utilized health services during the year prior to the survey. We construct various measures from this publicly available survey.

First, we generate indicators for medical care if at least one person in the household was sick during the two weeks prior to the survey. Medical care information includes: whether the person was attended by a physician in a clinic, whether the doctor prescribed medications, and whether the person self-medicated. To complement this information, an indicator for whether anyone in the household used healthcare services during the last 12 months is available. For young children aged four and under, we observe if they received medical attention conditional on being sick within the last two weeks. Lastly, we construct a self-reported indicator for whether a person's health improved after receiving medical care. We also break down some of these utilization metrics by age groups.

Second, we focus on health behaviors related to reproductive health and chronic diseases. For the former, we measure whether individuals aged 12 and over received family planning information, whether pregnant women received prenatal information, whether adults use contraceptive methods, the number of prenatal care visits, whether expectant mothers received prenatal care starting in their first trimester, and whether women have ever been pregnant since 1994 and during the year 2000. For chronic conditions, we consider indicators for having a diabetes and/or high blood pressure (HBP) diagnosis, whether patients are on a prevention program for diabetes and/or HBP, and indicators for whether adult women received a pap smear test or mammogram.

Lastly, we compile information on proxies for self-reported quality of care. We consider waiting times, a dummy indicating if the waiting time was short,[9] and a dummy indicating if patients consider that the time spent with the doctor was enough.

Public clinics' administrative data. We complement the survey with utilization data from administrative records of public clinics. These data are collected by the Ministry of Health. The information available is at the clinic level from the years 2000 through 2003. The data allow us to observe consultations, excluding emergency room visits, which we aggregate up to the yearly level. Although all public clinics are required by law to report this information, we consider only facilities belonging to the Ministry of Health since PROGRESA recipients were more likely to utilize services

[9] The benchmark for a standard wait time is 50 minutes for rural public clinics in Mexico (Ruelas et al., 2002).

at clinics from this institution.[10] We use geographic identifiers to locate the clinic and associate its information with localities.

National System of Health Quality (INDICAS). This system is maintained by the Ministry of Health and is designed to track different measures of quality of care at public clinics. We use information for 2003 since data for previous years is unavailable. The 2003 records include information from 3,794 sampled clinics. To the best of our knowledge, these clinics are randomly sampled by the Ministry of Health to be representative of the quality of care throughout the public system. From INDICAS, we obtain average waiting time reported by the clinic's personnel, the share of users satisfied with waiting times, whether any complaints were filed, and the share of patients that received an adequate explanation of their health from the doctor.[11]

Table 1 presents descriptive statistics. Each panel corresponds to a different set of variables. Columns indicate the sample (based on available responses), the average, the standard deviation, and the data set from which we obtain each variable.

[10] The Mexican Institute for Social Security (IMSS) provides healthcare for formal workers and their families. The Civil Service Social Security and Services Institute (ISSSTE) provides care for government employees and their families. The target population of PROGRESA is unlikely to be eligible for care at facilities run by either of these public institutions. Results hold but are noisier if we include all public clinics in our analysis.

[11] In order to collect this information, the Ministry of Health trained personnel to capture the survey information from the sampled clinics. The reporting frequency is every two months. Surveyors report on waiting times obtained from clinic staff and sample a subset of patients for questions about satisfaction with waiting times and whether the doctor explained their health status to them. Surveyors are also asked to log complaints.

Table 1: **Descriptive Statistics**

Variable	Description	N	Mean	SD	Data set
	Panel A: First Stage				
1995 marginality index	Locality poverty threshold to receive treatment	934	-0.939	0.936	ENSA
Treatment 2000 (0,1)	Locality dummy if entered program after 1999	934	0.480	0.500	ENSA
1995 marginality index	Locality poverty threshold to receive treatment	10608	-0.608	0.920	Admin. data
Treatment 2000 (0,1)	Locality dummy if entered program after 1999	10608	0.625	0.484	Admin. data
	Panel B: Use of health services				
Total visits	Total medical visits in 2000 (national)	14373	837.1	1512	Admin. data
Received doctor servcies (0,1)	User dummy if seen by doctor	174346	0.0836	0.277	ENSA (households)
Child received treatment (0,1)	Child dummy if received doctor treatment	10460	0.248	0.432	ENSA (minors)
Health services user (0,1)	HH dummy if received attention last year	174566	0.287	0.453	ENSA (households)
Medicated by doctor (0,1)	User dummy if medicated by doctor	174092	0.0817	0.274	ENSA (households)
Self-medicated (0,1)	User dummy if self-medicated	174092	0.0210	0.143	ENSA (households)
Health outcome (0,1)	User D if health improved after consultation	80873	0.648	0.478	ENSA (users)
	Panel C: Use of health services by ages				
Users aged 0-4 yo (0,1)	HH dummy if utilized services and aged 0-4 yo	19700	0.395	0.489	ENSA (households)
Users aged 5-14 yo (0,1)	HH dummy if utilized services and aged 5-14 yo	41646	0.243	0.429	ENSA (households)
Users aged 15-19 yo (0,1)	HH dummy if utilized services and aged 15-19 yo	16970	0.196	0.397	ENSA (households)
Users aged 20-49 yo (0,1)	HH dummy if utilized services and aged 20-49 yo	68965	0.262	0.440	ENSA (households)
Users aged 50-64 yo (0,1)	HH dummy if utilized services and aged 50-64 yo	16935	0.373	0.484	ENSA (households)
Users aged 65+ yo (0,1)	HH dummy if utilized services and aged 65+ yo	10208	0.445	0.497	ENSA (households)
	Panel D: Reproductive health				
Family planning talks (0,1)	D if adult/teen received family planning information	125571	0.00542	0.0734	ENSA (households)
Contraceptives (0,1)	Dummy if adult uses effective contraceptives	22914	0.481	0.500	ENSA (adults)
Pregnancy care talks (0,1)	D if woman/teen received pregnancy care information	67488	0.0168	0.129	ENSA (households)
Prenatal check-ups	Number of pregnancy check-ups	10846	6.249	3.771	ENSA (adults & teens)
Early checkup (0,1)	D if woman/teen had check-up in first trimester	10716	0.659	0.474	ENSA (adults & teens)
Has been pregnant (0,1)	D if woman/teen has been pregnant since 1994	20781	0.527	0.499	ENSA (adults & teens)
Recent pregnancy (0,1)	Dummy if woman/teen was pregnant in 2000	24104	0.0664	0.249	ENSA (adults & teens)
	Panel E: Chronic disease and prevention				
Diabetes (0,1)	D if adult has been diagnosed with diabetes	40517	0.0666	0.249	ENSA (adults)
High blood pressure (0,1)	D if adult diagnosed with HBP	34647	0.173	0.378	ENSA (adults)
Diabetes control program (0,1)	Dummy for preventive diabetes control	96378	0.0284	0.166	ENSA (households)
HBP control program (0,1)	Dummy for preventive HBP control	96378	0.0328	0.178	ENSA (households)
Pap smear test (0,1)	Dummy for preventive pap smear testing	52537	0.105	0.307	ENSA (households)
Breast cancer (0,1)	Dummy for preventive breast cancer testing	27431	0.106	0.308	ENSA (adults)
	Panel F: Service characteristics				
Waiting time	Survey user-reported waiting time (minutes)	79350	43.59	63.61	ENSA (users)
Waited 50- minutes (0,1)	D if user-reported waiting was below 50 mins	79350	0.718	0.450	ENSA (users)
Short wait (0,1)	Dummy if user perceived short waiting time	7878	0.618	0.486	ENSA (users)
Sufficient duration (0,1)	D if user perceived sufficient consultation time	2145	0.625	0.484	ENSA (users)
Average waiting time	Clinic-reported average waiting time	2375	25.64	29.33	INDICAS
Wait satisfaction	Share of users satisfied with waiting time	2375	87.30	16.81	INDICAS
Complaint (0,1)	Dummy if there was a user complaint	2375	0.086	0.281	INDICAS
Diagnosis explained	% of users to which doctor explained health status	2375	93.64	13.99	INDICAS

Note: (0,1) denotes a dummy variable. Statistics are reported for 2000, expect INDICAS which reports information for 2003.
HH: Household. D: Dummy. yo: years old.

4 Empirical Strategy

We focus our analysis on the pre-2004 rural expansion of the program. Following the expansion strategy and rules described in Section 2, we employ a regression discontinuity design, since localities were added to the program based on their poverty index and specific regional cutoffs.

4.1 Eligibility for PROGRESA

For purposes of this program, the country was partitioned into 41 regions.[12] As outlined before, the federal government established different thresholds of the locality poverty index for program eligibility that varied by region and time. Whenever PROGRESA was expanded (which occurred almost every year before 2004), localities with an index value above the cutoff were determined as eligible. Although we were unable to retrieve official documents containing precise details of the regional cutoffs, we know from official documentation that the key input to determine eligibility was the 1995 poverty index provided by CONAPO (Secretaría de Desarrollo Social, 2000).

Using the fact that administrative records allow us to identify the exact year of enrollment to PROGRESA at the locality level, we follow a data-driven approach to identify the regional thresholds for inclusion into the program. First, we use actual payments to determine the entry year of each locality. That way, at the locality level, we know for each year if the locality is already enrolled in the program. Then, using enrollment as a dependent variable and CONAPO's poverty index as the running variable, we follow the standard approach for an RDD estimation by implementing a local linear regression discontinuity (see online appendix Appendix 2 for details). We implement this recursively for different values of the cutoff per year-region. We keep the cutoff value with the maximum discontinuous jump in localities' enrollment. Overall, this allows us to identify eligibility thresholds for each region.[13]

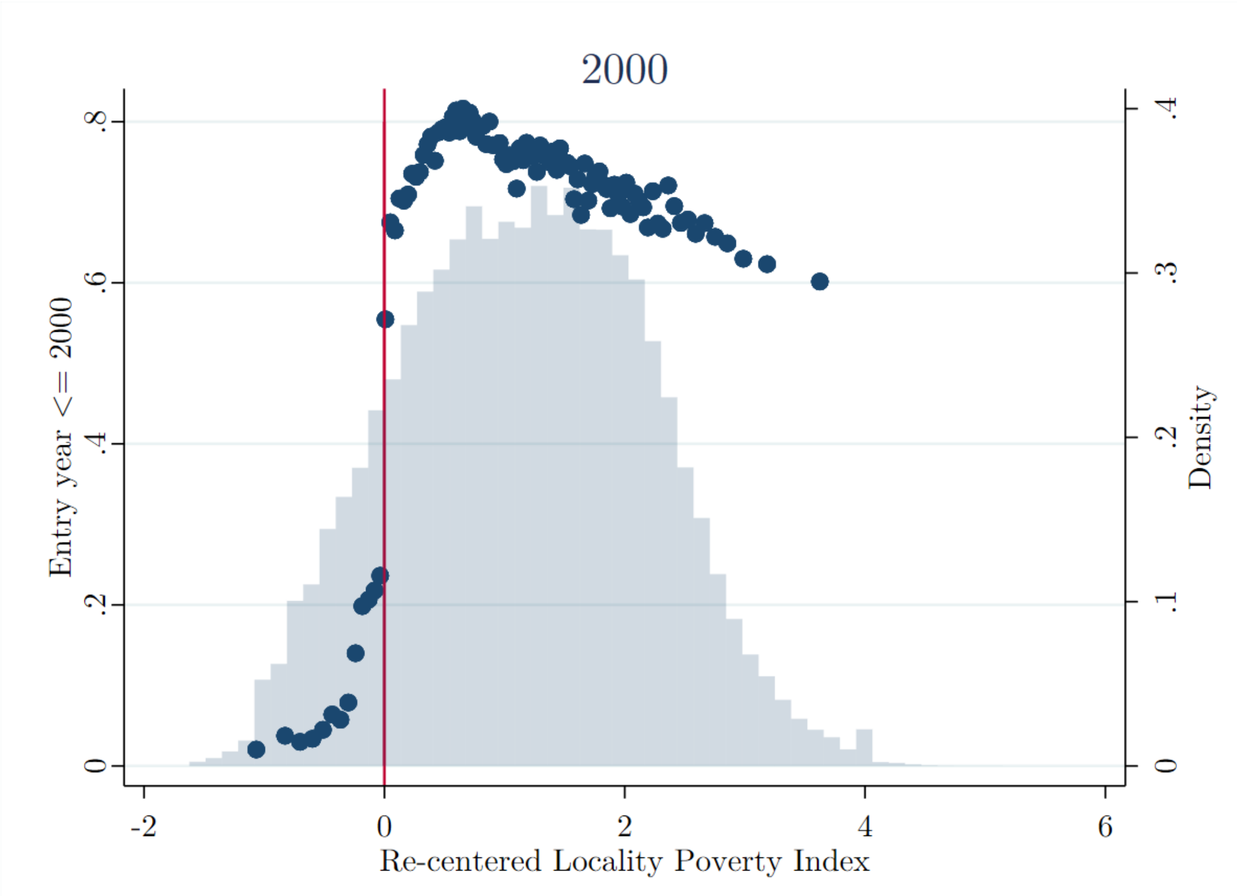
Figure 1 shows the result of this discontinuity identification by pooling together all the regions and re-centering all the cutoffs at zero. We present a binned scatterplot of the share of localities that began receiving PROGRESA in the year 2000 for different bins of the distance to the region-specific

[12] See online appendix Table A5 for details. These regions do not correspond to the 32 Mexican administrative states. Some regions are located within a single state, but others expand beyond state boundaries. Later iterations of the program condensed these 41 regions into 19.

[13] Previous work analyzing program effects in rural areas has used a single cutoff value of -1.22 (Alix-Garcia et al., 2013). However, our analysis corresponds to areas and program roll-out not included in that sample of rural localities from 1998 to 2000.

threshold. As can be seen, we obtain a strong first stage: the discontinuous change at zero in the percent of localities enrolled is considerable, with an estimated increase of 35%. Additionally, we overlay in gray the density of localities by values of the re-centered running variable, showing that it is continuous around the threshold. This suggests that there was no evident strategic modification of the poverty index in order to become eligible for the program.

Figure 1: **First Stage**



Note: This figure shows a scatter plot of the share of localities that have entered the program (left axis) by values of the re-centered poverty index at the locality level, and the density of this re-centered running variable (right axis). The vertical line denotes the minimum value for program eligibility. See Appendix 2 for more details.

4.2 Regression Discontinuity Design

Given the design of the program, a simple comparison between localities that were enrolled in PROGRESA and those that were not could be confounded by unobserved factors correlated with eligibility. To address this, we exploit the discontinuity described before to compare localities marginally included in the program with those just excluded. Although many socioeconomic factors determine eligibility via the poverty index, these variables do not change discontinuously around the threshold. In contrast, the program roll-out does change abruptly, as shown in Figure 1, allowing us to estimate a causal effect of PROGRESA.

Figure 2 shows a map of the proportion of treated localities by municipality using the localities in the ENSA and administrative records samples. There is considerable heterogeneity across space and no clear spatial or regional correlations.

Assuming (and testing for) continuity of other socioeconomic characteristics around the threshold[14], the RDD estimator obtains the intent to treat (ITT) effect of the PROGRESA CCT on health outcomes. Our specification of interest is the following:

$$y_{ijr} = \beta \ \mathbb{1}(index_{jr} > c_r) + f(index_{jr} - c_r) + \varepsilon_{ijr} \tag{1}$$

where y_{ijr} is an outcome for surveyed individual i in locality j and region r (or, alternatively for clinic i in locality j and region r), $index_{jr}$ is CONAPO's locality-level poverty index, and c_r is the region r specific cutoff identified above, $f(\cdot)$ is a smooth non-parametric function, and ε_{ijr} is the idiosyncratic error term. We follow Calonico et al. (2014b) for calculating local polynomial RDD estimators.[15] We estimate this equation using a triangular kernel, as well as using a local-polynomial of degree one, akin to a local linear regression. We specify a fixed bandwidth of 0.5 for all specifications (see Appendix 5 for sensitivity analyses).[16]

The parameter of interest is β, which represents the ITT effect of PROGRESA. It is important to mention that this parameter captures the overall effect of the program. Even though we might think that the effects found are mainly driven by the conditionality established by the program –of regularly attending the clinic for preventive checkups–, we cannot rule out that other components of the program might be partially driving the effects. For instance, the cash transfer component might induce a greater demand for preventive checkups if they are considered a normal good in this context.

[14] Appendix Appendix 7 shows continuity of four variables relevant to health that were selected indicators from the 1990 Census. Several indicators were tested and showed no sign of discontinuity. They are available upon request.

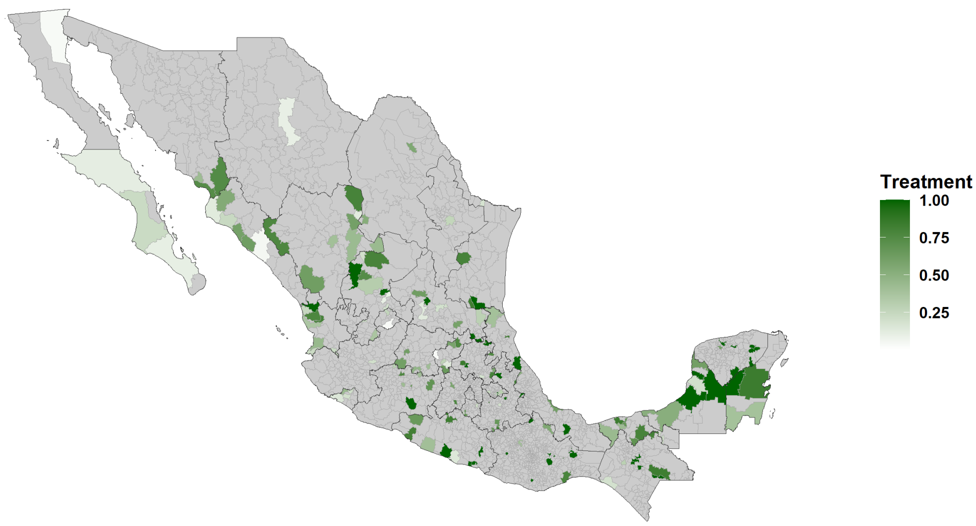
[15] We rely on the Stata command developed in Calonico et al. (2017).

[16] This bandwidth is similar to the one obtained using the mean-squared error-optimal bandwidths for several of our outcomes across different data sets (Calonico et al., 2017; Imbens and Kalyanaraman, 2012).

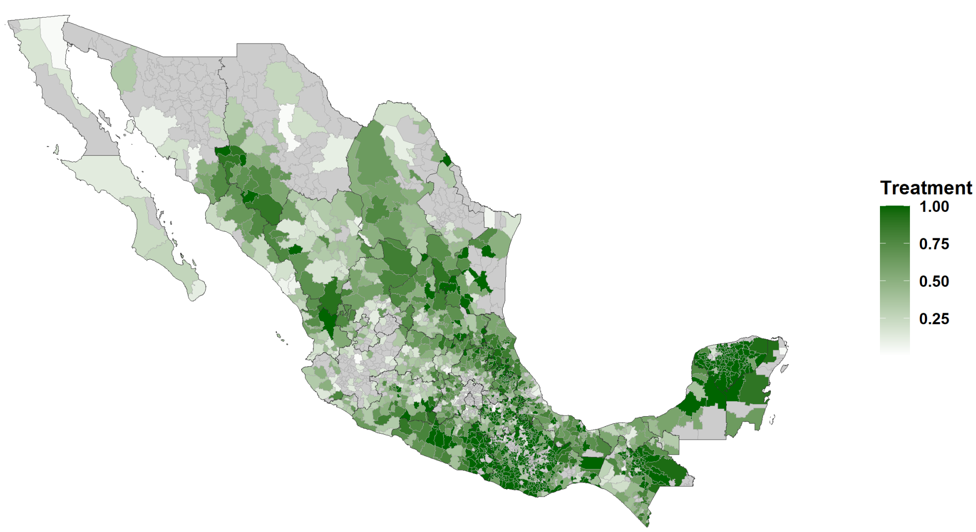
To help validate the RD approach, we show first-stage evidence that the probability of receiving PROGRESA cash transfers increased discontinuously around the eligibility cutoff as discussed above in Figure 1. We also note that we do not find evidence that localities manipulated their score in order to become eligible for the program. A formal test of this (McCrary, 2008) is shown in Appendix 3.

Figure 2: **Treatment per Municipality**

(a) Health services household survey (ENSA 2000)



(b) Administrative data



Note: This figure shows the share of localities that are treated by municipality. The top map considers the sample included in the ENSA survey and the bottom map the sample from administrative records.

5 Results

We present our results in this section by groups of outcomes, drawing on different variables from three data sets. All specifications follow the empirical strategy outlined above and use a bandwidth of 0.5 across all outcomes for consistency.[17] We show robustness to alternative bandwidth choices in online Appendix 5. For clarity, we present RDD plots showing binned means of the outcome variable around the program eligibility cutoff and a local polynomial of degree one. Shaded areas represent 95% confidence intervals. We also complement this with tables showing the RDD estimates with standard errors robust to heteroskedasticity. Online appendix Table A8 also considers standard errors clustered at the locality level.

5.1 Utilization of health services

We begin with utilization measures from administrative records corresponding to public clinics ascribed to the Ministry of Health. We take the log of total yearly visits as our outcome variable and exploit records from multiple years (2000 through 2003). We show plots of each measure against the distance to the threshold for PROGRESA eligibility in panels (a)-(d) of Figure 3. There is a clear increase in utilization right at the threshold across samples. Panel A of Table 2 shows the corresponding point estimates, all of which are positive, large and mostly statistically significant.[18] We cannot reject that effect sizes are the same across years. On average, we find about a 12% increase in total visits, ranging from a 7% increase in 2000 to a 16% increase in 2003.

We then turn our attention to similar measures in the survey data in panels (e)-(j) of Figure 3 and show the corresponding point estimates in panel B of Table 2. Overall, we obtain positive and significant effects showing that the CCT program led to increased health services utilization. Under PROGRESA, the probability of seeing a doctor when sick increases by 1.3 percentage points (pp) out of a baseline probability of 7.9%. Likewise, we see an increase in children seeing a doctor when sick, households using health services in general, and individuals obtaining medications from a doctor. We also see a decline in the probability of self-medicating. Online appendix Figure A2

[17] We estimated the optimal bandwidth for each specification and found that 0.5 was a good approximation for standardizing across outcomes.

[18] Only the effect for the year 2000 is not significant at conventional levels. However, given the size of the point estimate, we do not consider this to be a precisely estimated zero, but instead a noisily estimated positive impact. Furthermore, we obtain significant effects for this sample under alternative specifications (see Appendix 1).

and Table A4 show complementary results indicating that medical care and prescriptions from formal doctors increased while utilization of alternative health services (such as non-professional medical staff or non-Western medicine) was either unchanged or decreased.

Table 2: **Use of health services**

Panel A: Use of health services (Administrative census data)

	ln(Total visits)			
	(1)	(2)	(3)	(4)
Year	2000	2001	2002	2003
$\mathbb{1}(index_j > 0)$	0.0747	0.1293**	0.1142*	0.1578**
	(0.0612)	(0.0613)	(0.0631)	(0.0625)
Observations	8926	8977	8594	9444
P-value	0.2220	0.0351	0.0706	0.0115
Mean dependent variable	6.7640	6.7290	6.7620	6.7400
Mean dep. var. left of cutoff	6.7070	6.6850	6.6610	6.6700

Panel B: Use of health services (ENSA 2000 sample)

	(1) Received doctor services	(2) Child received treatment	(3) Used health services	(4) Medicated by doctor	(5) Self-medicated	(6) Health improved
$\mathbb{1}(index_j > 0)$	0.0132**	0.0818**	0.0397***	0.0122**	-0.0085**	0.0664***
	(0.0062)	(0.0385)	(0.0099)	(0.0061)	(0.0035)	(0.0158)
Observations	174346	10460	174566	174092	174092	80873
P-value	0.0320	0.0338	0.0000	0.0452	0.0136	0.0000
Mean dependent variable	0.0836	0.2480	0.2870	0.0817	0.0210	0.6480
Mean dep. var. left of cutoff	0.0792	0.2210	0.2640	0.0768	0.0288	0.6240

Note: This table shows RD estimates of the impact of Progresa eligibility on different outcomes from estimating Equation 1. All outcome variables are defined in Table 1. Panel A uses administrative data, while Panel B uses survey data. We report conventional standard errors (SE) robust to heteroskedasticity in parenthesis. Additionally, we include the overall mean of the dependent variable and its mean for localities just to the left of the cutoff value for eligibility (-0.1, 0). Stars denote significance from conventional, heteroskedasticity-robust p-values: *** p<0.01; ** p<0.05; * p<0.1

The PROGRESA transfer was conditioned on different requirements of healthcare visits for different age groups, with a particular target on children, pregnant women, and new mothers. We partition survey respondents into six age brackets to disentangle our effects by age group. We show RDD plots for these age-specific outcomes in Figure 4, and the corresponding estimates in Table 3. We find large and significant increases in healthcare utilization for children (ages 0 to 4 and ages 5 to 14) and adults (ages 20 to 49). The latter is concentrated among women rather than men, which is consistent with the program requirements for pregnant and breastfeeding women (see online appendix Table A2). We find small and insignificant effects for individuals aged 15 to 19 and elderly people aged 65 and over. Lastly, although noisily estimated, we find a large positive point estimate for older adults (ages 50 to 64).

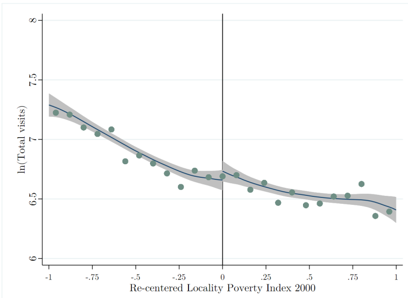
Table 3: **Use of health services by ages**

	(1) 0-4 yo user	(2) 5-14 yo user	(3) 15-19 yo user	(4) 20-49 yo user	(5) 50-64 yo user	(6) 65+ yo user
$\mathbb{1}(index_j > 0)$	0.0557*	0.0433**	0.0084	0.0368**	0.0531	-0.0081
	(0.0316)	(0.0180)	(0.0265)	(0.0159)	(0.0345)	(0.0437)
Observations	19700	41646	16970	68965	16935	10208
P-value	0.0776	0.0163	0.7520	0.0206	0.1240	0.8530
Mean dependent variable	0.3950	0.2430	0.1960	0.2620	0.3730	0.4450
Mean dep. var. left of cutoff	0.3820	0.2020	0.1720	0.2350	0.3520	0.4960

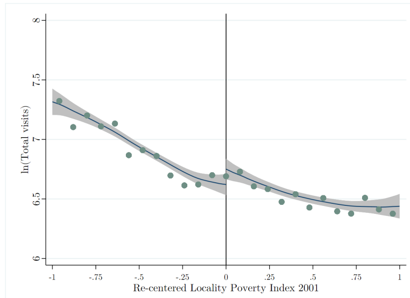
Note: This table shows RD estimates of the impact of Progresa eligibility on different outcomes from estimating Equation 1. All outcome variables are defined in Table 1. We report conventional standard errors (SE) robust to heteroskedasticity in parenthesis. Additionally, we include the overall mean of the dependent variable and its mean for localities just to the left of the cutoff value for eligibility (-0.1, 0). Stars denote significance from conventional, heteroskedasticity-robust p-values: *** p<0.01; ** p<0.05; * p<0.1

Figure 3: **Use of health services**

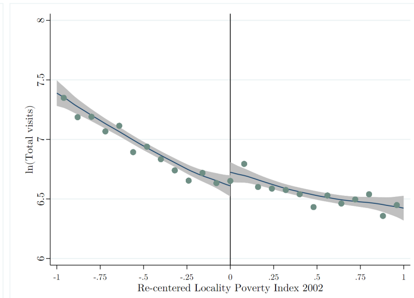
(a) Total medical visits (2000)



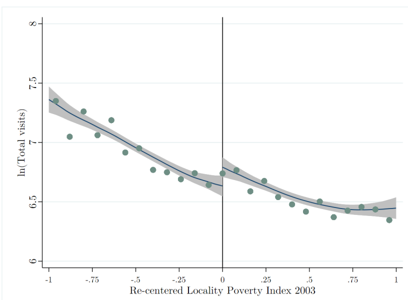
(b) Total medical visits (2001)



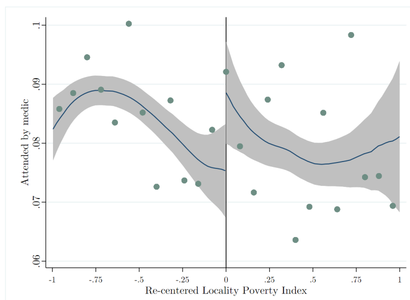
(c) Total medical visits (2002)



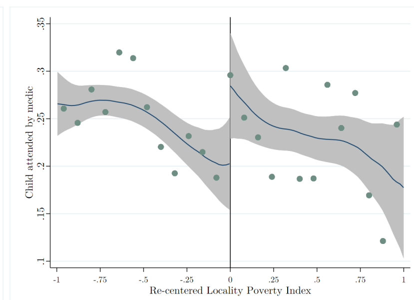
(d) Total medical visits (2003)



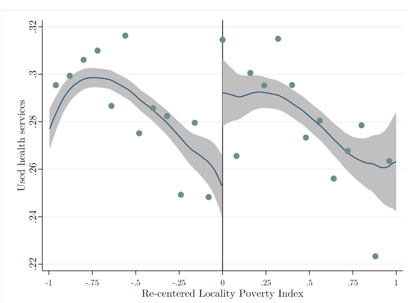
(e) Received doctor services



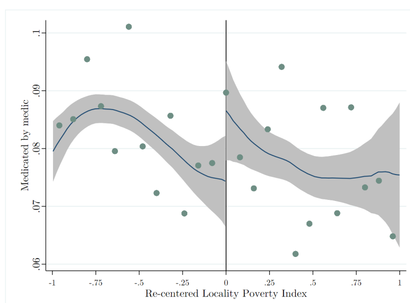
(f) Child received treatment



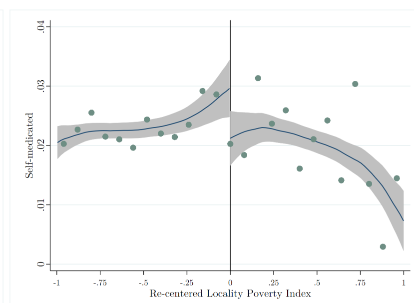
(g) Received medical attention



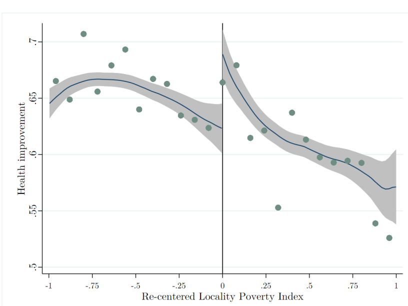
(h) Medicated by doctor



(i) Self-medicated



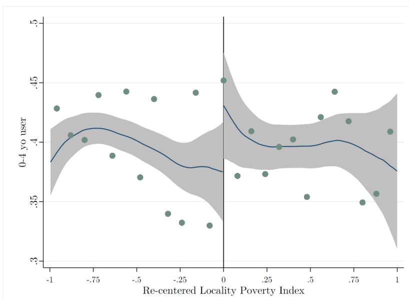
(j) Health improved after visit



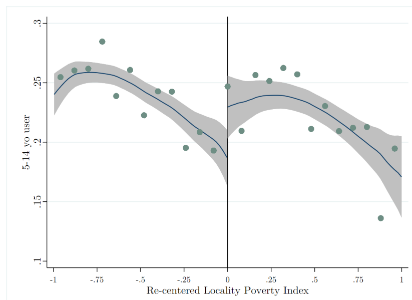
Note: These figures show RDD plots for our outcomes of interest. We present binned means of the outcome variables around the program eligibility cutoff and a local polynomial of degree one. Panels (a)-(d) refer to administrative census data. Panels (e)-(j) consider ENSA 2000 sample data.

Figure 4: **Use of health services by ages**

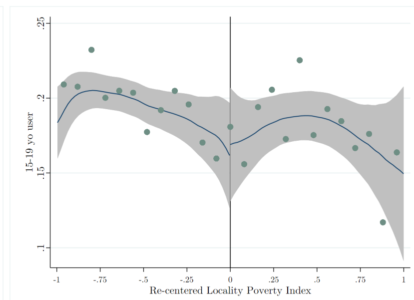
(a) 0-4 yo users



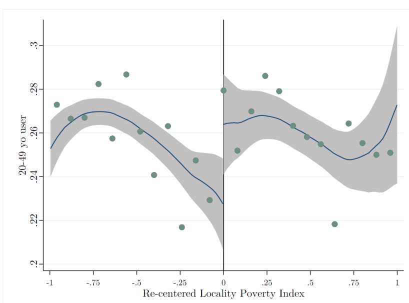
(b) 5-14 yo users



(c) 15-19 yo users

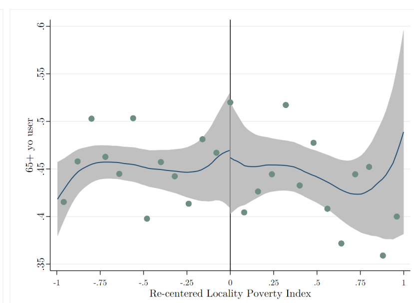


(d) 20-49 yo users



(e) 50-64 yo users

(f) 65+ yo users



Note: These figures show RDD plots for our outcomes of interest. We present binned means of the outcome variables around the program eligibility cutoff and a local polynomial of degree one.

5.2 Reproductive health and chronic diseases

Figure 5 reports impacts on a variety of outcomes related to reproductive health, where we mainly find increases in utilization of contraceptives and prenatal doctor visits. We report point estimates in Table 4. Although we find small and insignificant effects on receiving family planning informative talks, there is a large and significant increase in the actual use of contraceptives (7.5 pp increase on a baseline utilization of 43%). As for services during pregnancy, we do not find any effects on pregnancy care talks. However, we find an average increase of 0.55 pregnancy checkups (or equivalently, 10%) in areas just above the eligibility cutoff for PROGRESA. We also find a large but noisily estimated increase in the probability of having had a pregnancy checkup during the first trimester. Lastly, we show that fertility rates are not a potential driver of these results as neither the likelihood of being pregnant since 1994 nor the likelihood of being pregnant in the year 2000 changes across the eligibility threshold.

Table 4: **Reproductive health**

	(1) Family planning	(2) Contra- ceptives	(3) Prenatal talks	(4) Prenatal checkups	(5) Revision in 1st trim.	(6) Ever pregnant since 1994	(7) Pregnant in 2000
$\mathbb{1}(index_j > 0)$	0.0014	0.0755**	-0.0045	0.5530*	0.0591	0.0078	0.0044
	(0.0021)	(0.0312)	(0.0039)	(0.2971)	(0.0421)	(0.0319)	(0.0147)
Observations	125571	22914	67488	10846	10716	20781	24104
P-value	0.4980	0.0155	0.2450	0.0627	0.1600	0.8060	0.7630
MDV	0.0054	0.4810	0.0168	6.2490	0.6590	0.5270	0.0664
MDV left of cutoff	0.0051	0.4320	0.0138	5.6510	0.5920	0.5680	0.0658

Note: This table shows RD estimates of the impact of Progresa eligibility on different outcomes from estimating Equation 1. All outcome variables are defined in Table 1. We report conventional standard errors (SE) robust to heteroskedasticity in parenthesis. Additionally, we include the overall mean of the dependent variable and its mean for localities just to the left of the cutoff value for eligibility (-0.1, 0). MDV = mean dependent variable. Stars denote significance from conventional, heteroskedasticity-robust p-values: *** p<0.01; ** p<0.05; * p<0.1

Figure 6 and Table 5 focus on outcomes related to chronic diseases and preventive care. We find that PROGRESA eligibility increases the probability of having been diagnosed with HBP but has no effect on diabetes diagnoses. The latter may relate to the fact that HBP is more prevalent and more widely under-diagnosed than diabetes in Mexico (Campos-Nonato et al., 2018; Basto-Abreu et al., 2020). This increase in diagnoses is therefore consistent with more access to healthcare. We show positive effects for diabetes, HBP, pap smears, and breast cancer examinations in terms of actual testing. However, most of these results are noisily estimated, although the plots in Figure 6 are suggestive of positive impacts.

Taken together, the reproductive health and chronic disease estimates suggest that PROGRESA eligibility had a positive impact on access to and utilization of preventive care. Although we cannot observe direct health outcomes related to these variables, prenatal care visits and timely diagnosis of chronic conditions have been linked in the literature to better health outcomes (Almond and Currie, 2011; Zhao et al., 2013; Oster, 2018).

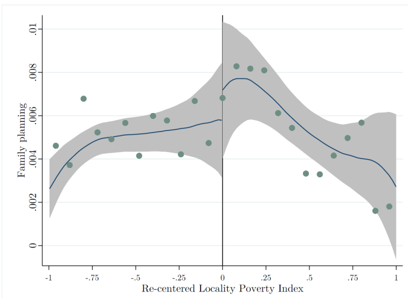
Table 5: **Chronic disease and prevention**

	(1) Diabetes diagnostic	(2) HBP diagnostic	(3) Diabetes test	(4) HBP test	(5) Pap smear test	(6) Breast cancer test
$\mathbb{1}(index_j > 0)$	-0.0009	0.0318*	0.0047	0.0092*	0.0076	0.0199
	(0.0115)	(0.0193)	(0.0049)	(0.0055)	(0.0129)	(0.0150)
Observations	40517	34647	96378	96378	52537	27431
P-value	0.9410	0.0989	0.3350	0.0972	0.5570	0.1860
Mean dependent variable	0.0666	0.1730	0.0284	0.0328	0.1050	0.1060
Mean dep. var. left of cutoff	0.0687	0.1730	0.0243	0.0312	0.1070	0.0692

Note: This table shows RD estimates of the impact of Progresa eligibility on different outcomes from estimating Equation 1. All outcome variables are defined in Table 1. We report conventional standard errors (SE) robust to heteroskedasticity in parenthesis. Additionally, we include the overall mean of the dependent variable and its mean for localities just to the left of the cutoff value for eligibility (-0.1, 0). Stars denote significance from conventional, heteroskedasticity-robust p-values: *** p<0.01; ** p<0.05; * p<0.1

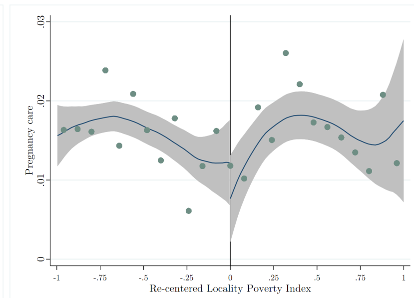
Figure 5: **Reproductive health**

(a) Family planning talks at prevention services

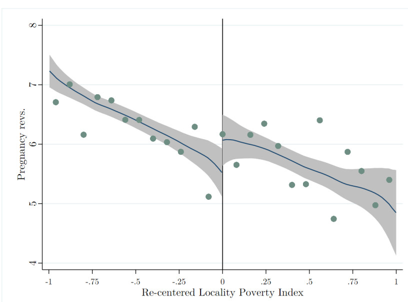


(b) Effective contraceptive methods

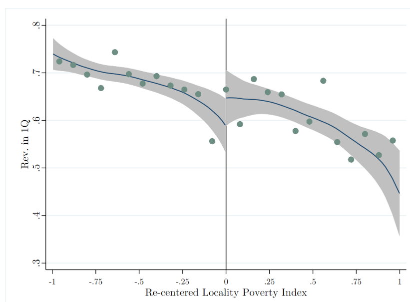
(c) Pregnancy care talks at prevention services



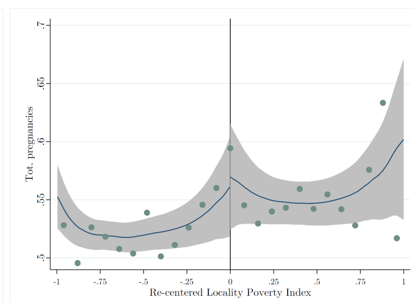
(d) Prenatal check-ups



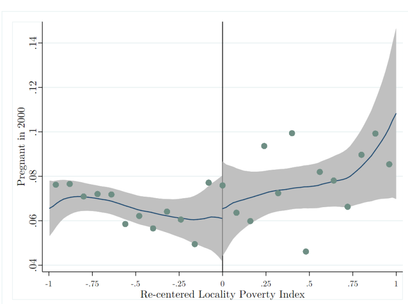
(e) Had pregnancy check-up in first trimester



(f) Woman has been pregnant since 1994



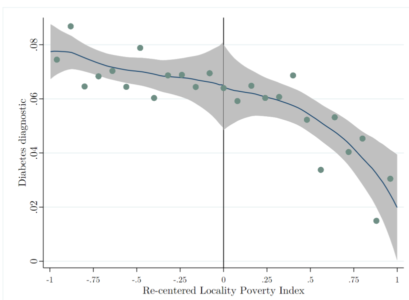
(g) Woman pregnant in 2000



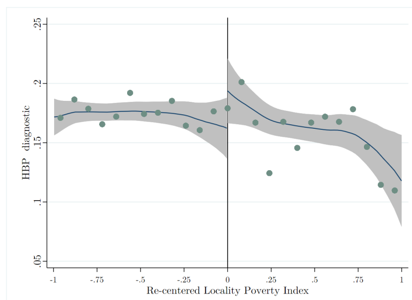
Note: These figures show RDD plots for our outcomes of interest. We present binned means of the outcome variables around the program eligibility cutoff and a local polynomial of degree one.

Figure 6: **Chronic disease and prevention**

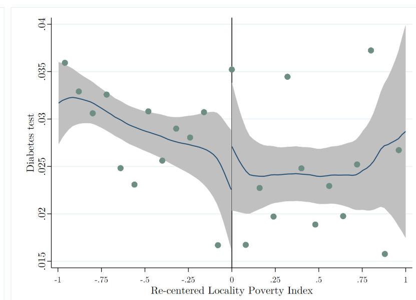
(a) Adult diagnosed with diabetes



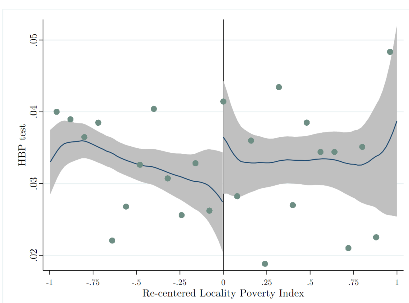
(b) Adult diagnosed with HBP



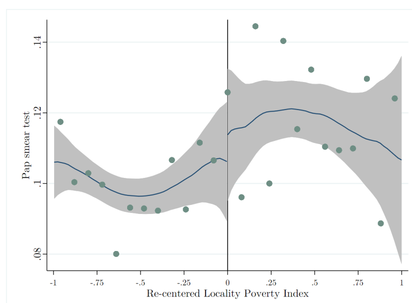
(c) Diabetes test



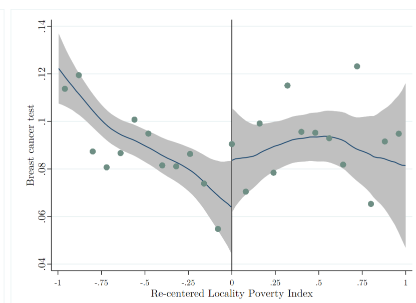
(d) HBP test



(e) Pap smear



(f) Breast cancer test



Note: These figures show RDD plots for our outcomes of interest. We present binned means of the outcome variables around the program eligibility cutoff and a local polynomial of degree one. HBP: high blood pressure.

5.3 Quality of care

Our final set of results considers measures or proxies for quality of care and congestion at public health clinics. As shown above, PROGRESA led to an increase in the utilization of health services. However, during this study period, the program did not consider infrastructure expansions, such as building new clinics or increasing medical staffing. Therefore, a natural question is whether the sudden increase in utilization driven by the PROGRESA conditionality led to changes in the characteristics of health services provided.

Figure 7 and Table 6 show results using both the health survey data and records from the government's quality audit system INDICAS. Focusing on the former, we find a positive effect on waiting times equivalent to a 6% increase, although standard errors are quite large. Given the government's benchmark of a 50-minute wait, we also analyze the probability of waiting below this benchmark. We find a significant decline of 3 pp in the probability of waiting less than 50 minutes, from a baseline share of 72%. The final two columns in panel A of Table 6 show declines in whether patients perceived their waiting period to be short and whether they were satisfied with the duration of time spent with the doctor.

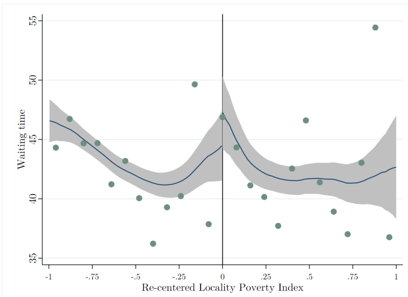
Focusing on the government's audit system, we find evidence of an increase in average waiting times, a decline in the share of clinic patients satisfied with the duration of the wait, an increase in registered complaints, and a decline in the share of patients reporting that they received an explanation of their health status from the doctor. Although these effects are not significant, point estimates are quite large: for example, the effect on waiting times implies a 15% increase from the baseline mean. Moreover, the plots in panels (e)-(h) of Figure 7 suggest that the effects were not zero.

Overall, our estimates are consistent with the PROGRESA incentive having pushed patients toward higher utilization of health services. However, the lack of additional resources for infrastructure then led to a deterioration in the quality of care as measured by waiting times and satisfaction. An important caveat of this result is that we cannot link it to worse health outcomes. Given the potential gains from increasing healthcare utilization (beyond what we can measure here), it does not seem plausible that this additional congestion was, on average, detrimental to patient health.

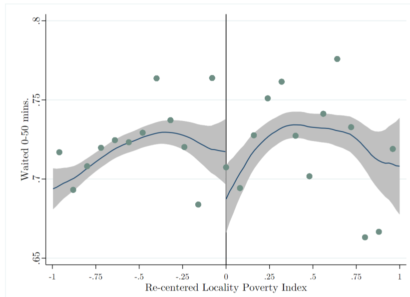
However, our findings suggest that conditioning government programs without expanding resources may lead to congestion, which may have far-reaching impacts depending on the context.

Figure 7: **Service characteristics**

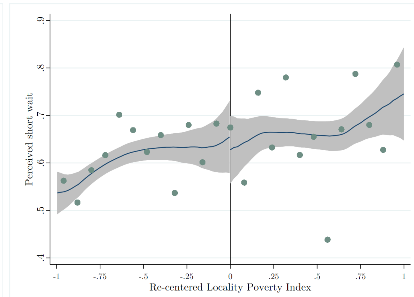
(a) Waiting time for consultation (ENSA)



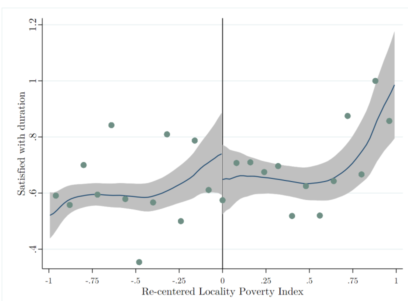
(b) Waiting time below 50 mins (ENSA)



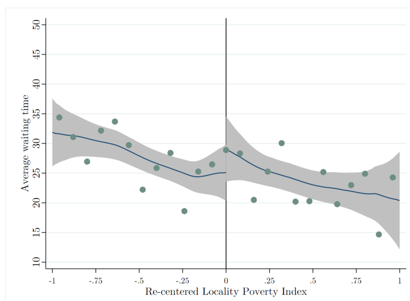
(c) User perceived short waiting time (ENSA)



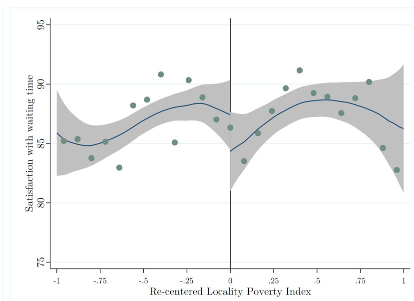
(d) Sufficient consultation time (ENSA)



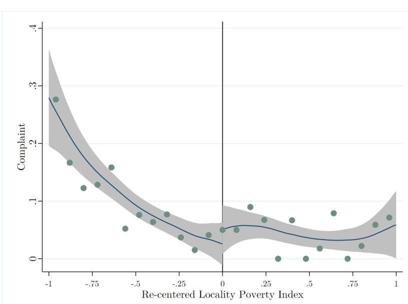
(e) Average waiting minutes (INDICAS)



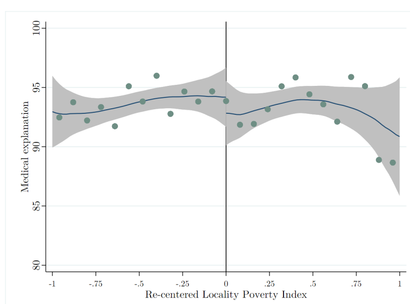
(f) Satisfied with waiting time (INDICAS)



(g) Complaint was logged (INDICAS)



(h) Doctor explained health status (INDICAS)



Note: These figures show RDD plots for our outcomes of interest. We present binned means of the outcome variables around the program eligibility cutoff and a local polynomial of degree one.

Table 6: **Service characteristics**

Panel A: Health services users surveyed at households (ENSA 2000)

	(1) Waiting time	(2) Waited 0-50 mins.	(3) Perceived short wait	(4) Satisfied with duration
$\mathbb{1}(index_j > 0)$	2.7547	-0.0299**	-0.0278	-0.0915
	(2.1745)	(0.0151)	(0.0535)	(0.1015)
Observations	79350	79350	7878	2145
P-value	0.2050	0.0478	0.6040	0.3670
Mean dependent variable	43.5900	0.7180	0.6180	0.6250
Mean dep. var. left of cutoff	42.8600	0.7240	0.6590	0.5670

Panel B: Health services users and providers surveyed at clinics (INDICAS)

	(1) Average waiting time	(2) Satisfaction with waiting time	(3) Complaint was logged	(4) Medical explanation
$\mathbb{1}(index_j > 0)$	3.9556	-3.0930	0.0252	-1.3520
	(3.6876)	(2.1854)	(0.0279)	(1.7860)
Observations	2375	2375	2375	2375
P-value	0.2830	0.1570	0.3670	0.4490
Mean dependent variable	25.6400	87.3000	0.0863	93.6400
Mean dep. var. left of cutoff	26.9200	87.7500	0.0455	95.2200

Note: This table shows RD estimates of the impact of Progresa eligibility on different outcomes from estimating Equation 1. All outcome variables are defined in Table 1. Panel A uses uses survey data for 2000 and Panel B uses different survey data for 2003. We report conventional standard errors (SE) robust to heteroskedasticity in parenthesis. Additionally, we include the overall mean of the dependent variable and its mean for localities just to the left of the cutoff value for eligibility (-0.1, 0). Stars denote significance from conventional, heteroskedasticity-robust p-values: *** p<0.01; ** p<0.05; * p<0.1

6 Conclusion

This paper analyzes how a CCT policy that focused on children's schooling and health also led to improvements in adult preventive health due to this additional conditionality of the program. Results show that PROGRESA had positive health effects on the targeted population, following a significant increase in the utilization of health services. We estimate important improvements in reproductive health and chronic disease screening for adults. However, we also find evidence that this program increased waiting times and led to declines in proxies for the quality of health services at public clinics. The latter may suggest critical distributional consequences, with welfare losses for the population segment that was regularly using health services prior to the program, such as those with more inelastic demand for healthcare. However, it is unlikely that this welfare loss would offset the large gains associated with the program, not just in healthcare but in education and other dimensions. Nevertheless, our results suggest that conditioning transfers to public services without the appropriate infrastructure expansion—such as building new schools and clinics—may lead to over-congestion and worse outcomes on some dimensions for at least a subset of the population.

To gauge the benefits of such a policy lever, we present a simple back-of-the-envelope calculation focusing on the increase in HBP diagnoses. As in many other contexts, HBP is underdiagnosed in Mexico, with an average of 1.3 undiagnosed cases per diagnosed case (Villarreal-Ríos et al., 2002). Furthermore, managing HBP cases costs the public health system an estimated 354 million USD per year (Arredondo and Zuniga, 2006). These costs include the present value of current and future diagnoses, consultations, drugs, hospitalizations, and treatment of complications (nephropathy, nonfatal myocardial infarction, and nonfatal stroke). The costs of HBP management may be offset by savings from avoiding complications from undiagnosed patients.

Our estimates imply that the program induced 1.7 million new HBP diagnoses among adults due to the program's conditionality. This suggests an increase of USD 17.6 million in costs associated with chronic disease management (checkups and medication) and a decrease of USD 36.4 million in costs associated with complications and hospitalizations of undiagnosed cases. This calculation assumes that an HBP diagnosis reduces the chances of complications by 98% (Rosas-Peralta et al., 2016). Therefore, even without considering the private value of health or quality of life, these additional diagnoses potentially saved the public health system around USD 18.8 million annually. Based on our estimated increase in HBP diagnoses, a prevention effectiveness of 60% would balance out the increased cost of disease management with the savings induced by decreased hospitalizations and treatment of complications, implying that these new diagnoses almost surely were cost-effective.

Since our identification relies on reduced-form local average effects around the cutoffs and given the data availability, we cannot quantify the distribution of welfare consequences in this population. Future work may try to shed light on this issue and identify mediating factors that may have improved the effects on preventive health behaviors or tempered the negative impacts of congestion. Furthermore, we rely on rich but broad administrative and survey data that does not allow us to observe more nuanced outcomes from health visits, such as doctors' treatment choices, nor can we follow the same patients over time. A more detailed dataset may allow for additional insights into how providers adjusted to this inflow of patients and how that may have further impacted health outcomes.

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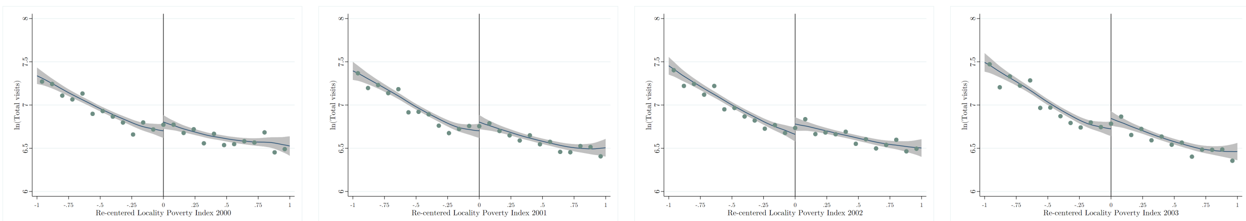
Appendix

Appendix 1 Additional Results

Figure A1: **Use of health services Admin. Data (consistent non-zero consultations for more than 26 weeks)**

(a) Total medical visits (2000) | (b) Total medical visits (2001) | (c) Total medical visits (2002) | (d) Total medical visits (2003)



Note: Authors' calculations with data from Progresa, Conapo, and the Ministry of Public Health. This figure is analogous to Panels (a)-(d) of Figure 3 in the main text. However, the sample is restricted here to Ministry of Health clinics for which we consistently observe more than 26 weeks of non-zero consultations.

Table A1: **Admin. data (consistent non-zero consultations for more than 26 weeks)**

VARIABLES	ln(Total visits)			
	(1)	(2)	(3)	(4)
Year	2000	2001	2002	2003
$\mathbb{1}(index_j > 0)$	0.1023*	0.1005*	0.1191**	0.1196**
	(0.0573)	(0.0596)	(0.0590)	(0.0608)
Observations	8255	8009	7963	7964
P-value	0.0744	0.0917	0.0436	0.0492
Mean dependent variable	6.8570	6.8230	6.8550	6.8430
Mean dep. var. left of cutoff	6.7470	6.7630	6.7120	6.7700

Note: This table is analogous to Panel A of Table 2 in the main text. However, the sample is restricted here to Ministry of Health clinics for which we consistently observe more than 26 weeks of non-zero consultations. This table shows RD estimates of the impact of Progresa eligibility on different outcomes from estimating equation 1. We report conventional standard errors (SE) robust to heteroskedasticity in parenthesis. Additionally, we include the overall mean of the dependent variable and its mean for localities just to the left of the cutoff value for eligibility (-0.1, 0). Stars denote significance from conventional, heteroskedasticity-robust p-values: *** p<0.01; ** p<0.05; * p<0.1

Table A2: **Use of health services by gender (ages 20-49)**

VARIABLES	Use of health services for 20-49 yo		
	(1)	(2)	(3)
Gender	All	Female	Male
$\mathbb{1}(index_j > 0)$	0.0368**	0.0517**	0.0181
	(0.0159)	(0.0224)	(0.0216)
Observations	68965	37807	31158
P-value	0.0206	0.0213	0.4030
Mean dependent variable	0.2620	0.3080	0.2060
Mean dep. var. left of cutoff	0.2350	0.2780	0.1820

Note: This table is analogous to Table 3 in the main text. Column (1) replicates the result of Table 3 column (4). Subsequent columns in this table decompose the effect by gender. This table shows RD estimates of the impact of Progresa eligibility on different outcomes from estimating equation 1. We report conventional standard errors (SE) robust to heteroskedasticity in parenthesis. Additionally, we include the overall mean of the dependent variable and its mean for localities just to the left of the cutoff value for eligibility (-0.1, 0). Stars denote significance from conventional, heteroskedasticity-robust p-values: *** p<0.01; ** p<0.05; * p<0.1

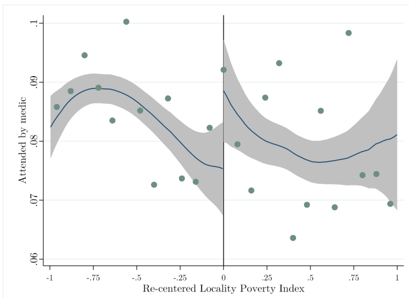
Table A3: **Additional Descriptive Statistics Regarding Usage of Health Services**

Variable	Description	N	Mean	SD	Data set
Panel A: Medical attention when sick					
Attended by medic (0,1)	User dummy if attended by medic	174346	0.0836	0.277	ENSA (households)
Professional non-medic (0,1)	Dentist or nurse	174346	0.00147	0.0383	ENSA (households)
Alternative medic (0,1)	Homeopathy, healer, pharmacist	174346	0.00254	0.0503	ENSA (households)
Non-professional (0,1)	Family, friend, community head, other	174346	0.00369	0.0607	ENSA (households)
Panel B: Medication when sick and attended					
Medicated by medic (0,1)	User dummy if medicated by medic	174092	0.0817	0.274	ENSA (households)
Professional non-medic (0,1)	Dentist or nurse	174092	0.00142	0.0376	ENSA (households)
Alternative medic (0,1)	Homeopathy, healer, pharmacist	174092	0.00453	0.0672	ENSA (households)
Non-professional (0,1)	Family, friend, community head, other	174092	0.0296	0.170	ENSA (households)
Panel C: Usage of private clinics for all survey respondents					
Private clinics	Preference for private clinics when sick	168797	0.257	0.437	ENSA (households)

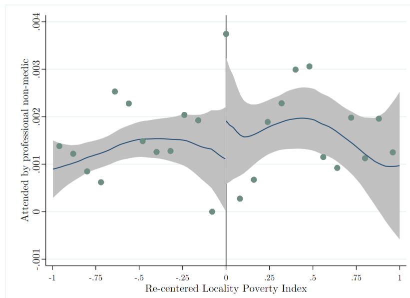
Note: (0,1) Denotes a dummy variable. Statistics are reported for 2000.

Figure A2: **Medical attention when sick, medication when attended, and private clinics preference**

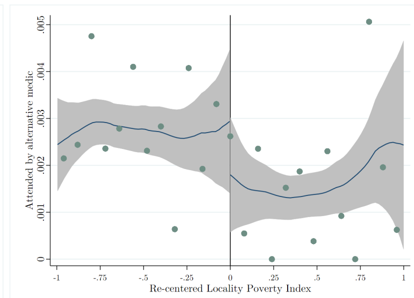
(a) Attended by medic



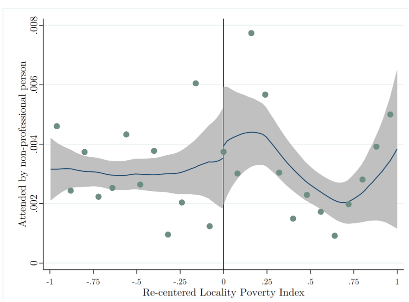
(b) Attended by professional non-medic



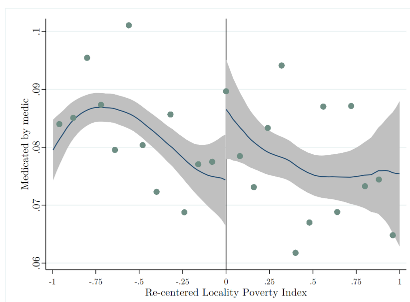
(c) Attended by alternative medic



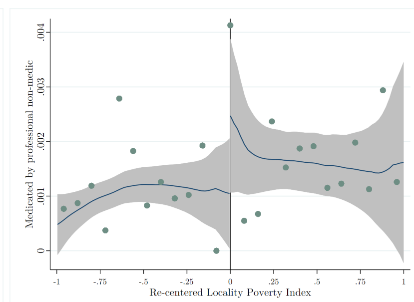
(d) Attended by non-professional person



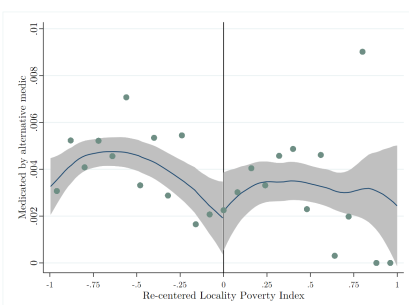
(e) Medicated by medic



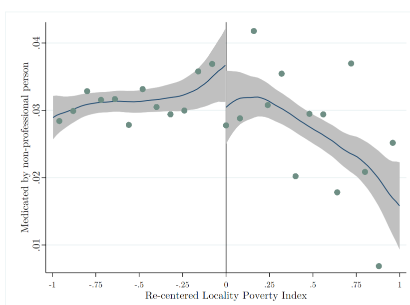
(f) Medicated by professional non-medic



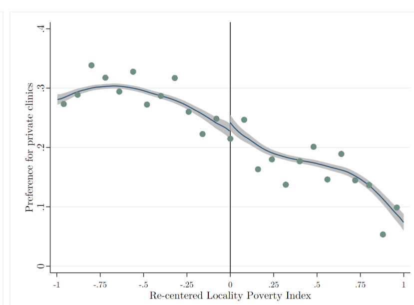
(g) Medicated by alternative medic



(h) Medicated by non-professional person



(i) Preference for private clinics (all survey respondents)



Note: Authors' calculations with data from Progresa, Conapo, and ENSA 2000. This Figure expands on Figure 3 in the main text; attended by medic and medicated by medic refer to panels (e) and (h), respectively. The rest of the variables in this Figure deepen the analysis presented in the main text.

Table A4: **Medical attention when sick, medication when attended, and private clinics preference**

Panel A: Medical Attention

VARIABLES	(1) Attended by medic	(2) Professional non-medic	(3) Alternative medic	(4) Non- professional	(5) Private preference
$\mathbb{1}(index_j > 0)$	0.0132**	-0.0005	-0.0003	-0.0003	0.0158
	(0.0062)	(0.0010)	(0.0017)	(0.0020)	(0.0131)
Observations	174346	91545	91545	91545	88966
P-value	0.0320	0.6120	0.8700	0.8580	0.2290
Mean dependent variable	0.0836	0.0017	0.0028	0.0038	0.2570
Mean dep. var. left of cutoff	0.0792	0.0013	0.0018	0.0044	0.2230

Panel B: Medication

VARIABLES	(1) Medicated by medic	(2) Professional non-medic	(3) Alternative medic	(4) Non- professional
$\mathbb{1}(index_j > 0)$	0.0122**	0.0001	0.0017	-0.0099*
	(0.0061)	(0.0012)	(0.0017)	(0.0057)
Observations	174092	91407	91407	91407
P-value	0.0452	0.9120	0.2960	0.0788
Mean dependent variable	0.0817	0.0016	0.0048	0.0316
Mean dep. var. left of cutoff	0.0768	0.0013	0.0013	0.0419

Note: This table shows RD estimates of the impact of Progresa eligibility on different outcomes from estimating equation 1. All outcome variables are defined in Table A3. Panel A in this table deepens on column (1) of Panel B in Table 2. Panel B in this table deepens on column (4) of Panel B in Table 2. We report conventional standard errors (SE) robust to heteroskedasticity in parenthesis. Additionally, we include the overall mean of the dependent variable and its mean for localities just to the left of the cutoff value for eligibility (-0.1, 0). Stars denote significance from conventional, heteroskedasticity-robust p-values: *** p<0.01; ** p<0.05; * p<0.1

Appendix 2 Treatment Threshold

PROGRESA classifies localities into 41 regions (see Table A5). Government agency CONAPO provides the 1995 poverty index by locality. We identify the regional poverty threshold eligibility for the program directly from the data. Specifically, for a given region r, we estimate the following equation:

$$Enrolled_{jr} = \beta_0^s + \beta_1^s(index_{jr} - c_r) + \epsilon_{jr} \tag{A1}$$

where $Enrolled_{jr}$ is a dummy indicating if locality j in region r is enrolled in the program, $index_{jr}$ is the CONAPO 1995 poverty index for locality j in region r, c_r denotes a fixed value of the poverty index, and ϵ_{jr} is an idiosyncratic error term.

In this specification, $s = \{l, r\}$, where l and r stand for left and right with respect to the discontinuity c_r. Essentially, we estimate (A1) twice: one with localities with $index_{jr} \in [c_r - h, c_r)$ and a second for localities with $index_{jr} \in [c_r, c_r + h]$. A triangular kernel is used to weight observations, giving a larger importance to localities with $index = c_r$. With this two estimations, the discontinuous jump of proportion of localities enrolled in the program is estimated as $\theta = \beta_0^r - \beta_0^l$.

We recursively estimate θ by changing the value of c_r by region-year. We then compare the estimates of θ and identify the cutoff as the value c_r for which θ is largest, conditional on being significant at the 5% significance level.

Figure A3 shows three examples of our estimated cutoffs via this algorithm. The first plot denotes a fuzzy threshold, the second plot shows a situation where the cutoff was more challenging to identify, and the last plot shows a very clean and sharp threshold.

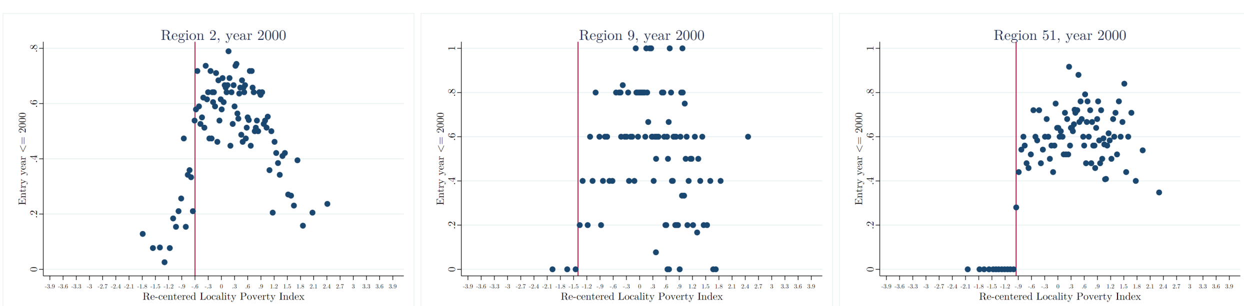
Table A5: **Progresa Regions**

Region	States
1	Chiapas
2	Chiapas
3	Oaxaca, Pueba, Veracruz
4	Hidalgo, Pueba, Tlaxcala
5	Guanajuato, Hidalgo, Querétaro, San Luis Potosí
6	Guerrero
7	Chihuahua, Durango
8	Coahuila, Durango, Nuevo León, Zacatecas
9	Campeche
10	Guerrero, Oaxaca
11	Campeche, Quintana Roo
12	San Luis Potosí
13	Oaxaca
14	Guanajuato
15	Chiapas
16	Oaxaca
27	Guanajuato, Guerrero, Edomex, Michoacán
28	San Luis Potosí
31	Sinaloa, Sonora
32	Tamaulipas
33	Jalisco, Nayarit
34	Jalisco
35	Jalisco, Michoacán
36	Colima, Jalisco , Michoacán
37	Yucatán
38	Morelos
39	Tabasco
40	Tabasco
41	Oaxaca, Veracruz
42	Veracruz
43	Jalisco
44	Puebla
45	Baja California
46	Baja California Sur
47	Coahuila
48	Tamaulipas
49	Sonora
50	Oaxaca
51	Veracruz
52	Aguascalientes
53	Edomex

Note: Regions 17 to 26, 29 and 30 denote an old classification no longer in place during the study period.

Figure A3: **Examples of the Construction of Regional Cutoffs**

(a) Fuzzy cutoff (b) Almost non-existent cutoff (c) Sharp cutoff

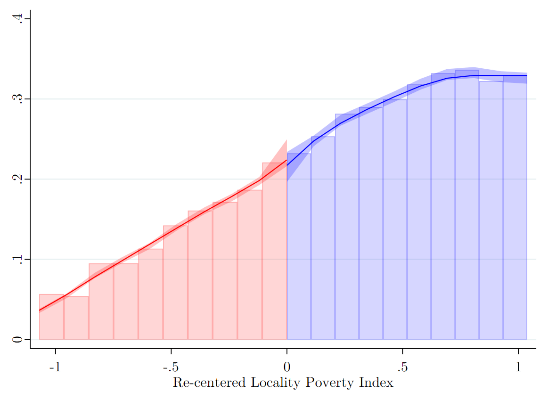


Note: Authors' calculations with data from Progresa and Conapo.

Appendix 3 McCrary Test

To shed some light on the validity of our identification strategy, we implement a McCrary test (McCrary, 2008) using the local polynomial density estimators proposed in Cattaneo et al. (2020) and implementing confidence bands using the results in Cattaneo et al. (2021). We find no evidence of manipulation at the cutoff, as shown in Figure A4. We obtain a McCrary test statistic of -1.37 with a p-value of 0.17.

Figure A4: **McCrary Manipulation Test**



Note: Authors' calculations with data from Progresa and Conapo.

Appendix 4 Progresa check-ups

Table A6: **Progresa required health check-ups and actions**

Panel A: Children

Age group	**Check-ups frequency**	**Actions**
less than 4 months	3 check-ups: At 7 and 28 days	Vaccines and growth control weight and height control
4 to 24 months	8 check-ups: At 4, 6, 9, 12, 15, 18 21, and 24 months. Additionally, 1 monthly weight and height check-up.	Vaccines and development control Weight and height control Nutrition monitoring Early disease detection
2 to 4 years	3 yearly check-ups: Once every 4 months.	Vaccines and growth control Weight and height control Deworming Early disease detection
5 to 16 years	2 yearly check-ups: Once every 6 months.	Vaccines and growth monitoring Early disease detection

Panel B: Women

Group	**Check-ups frequency**	**Actions**
Pregnant	5 prenatal check-ups	Nutrition orientation Pregnancy development monitoring Iron and tetanus toxoid provision
Puerperium and lactation	2 check-ups: 1 immediately after childbirth 1 during lactation period	Family planning talks Nutrition orientation Newborn care talks Breast feeding promotion talks

Panel C: Teens and adults

Age group	**Check-ups frequency**	**Actions**
17 to 60 years	1 yearly check-up	Reproductive health talks Family planning talks Early disease detection
More than 60 years	1 yearly check-up	Early detection of chronic disease and neoplasm

Source: Secretaría de Desarrollo Social (2000).

Table A7: **Progresa basic package of health service**

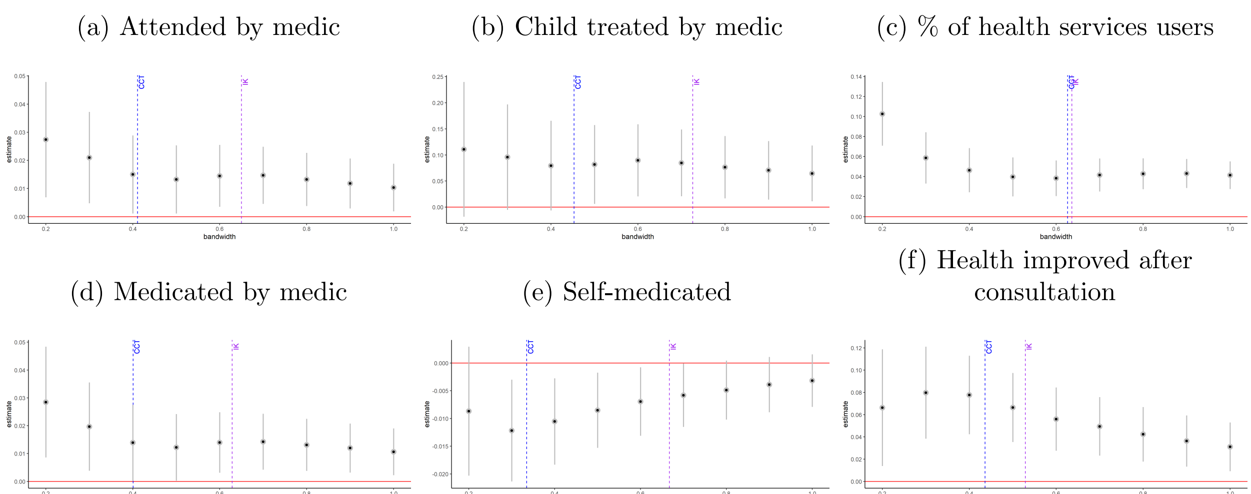
Basic sanitation at family level
Family planning
Prenatal, childbirth, puerperium, and newborn care
Surveillance of child nutrition and growth
Immunizations
Management of diarrhea cases at home
Antiparasitic treatment for families
Management of acute respiratory infections
Prevention and control of pulmonary tuberculosis
Prevention and control of arterial hypertension and diabetes mellitus
Prevention of accidents and initial management of injuries
Community training for health self-care
Detection and control of cervical cancer

Source: Secretaría de Desarrollo Social (2000).

Appendix 5 Bandwidth Sensitivity

We perform an analysis of bandwidth sensitivity and compare the results to the optimal bandwidths of Calonico et al. (2014b), denoted as CCT below, and Imbens and Kalyanaraman (2012), denoted as IK. The plots include 95% confidence intervals. We use a bandwidth of 0.5 in our main results.

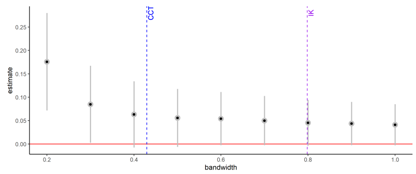
Figure A5: **Use of health services**



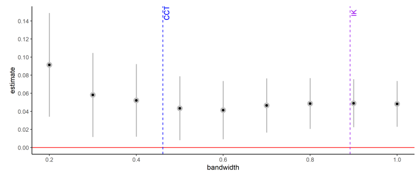
Note: Authors' calculations with data from Progresa, Conapo, ENSA 2000, and the Ministry of Public Health. Plots include 95% heteroskedasticity-robust confidence intervals. CCT: Calonico et al. (2014a) optimal bandwidth. IK: Imbens and Kalyanaraman (2012) optimal bandwidth.

Figure A6: **Use of health services by ages**

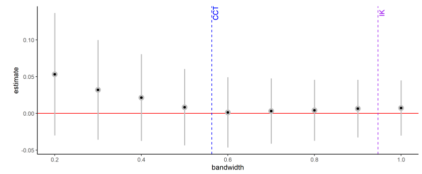
(a) 0-4 yo users



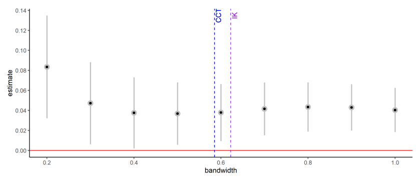
(b) 5-14 yo users



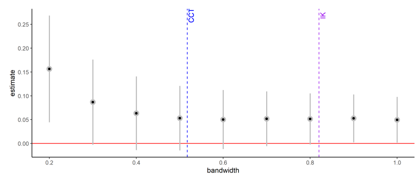
(c) 15-19 yo users



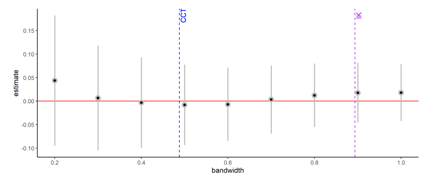
(d) 20-49 yo users



(e) 50-64 yo users



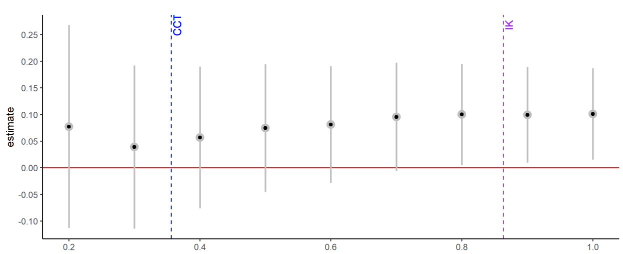
(f) 65+ yo users



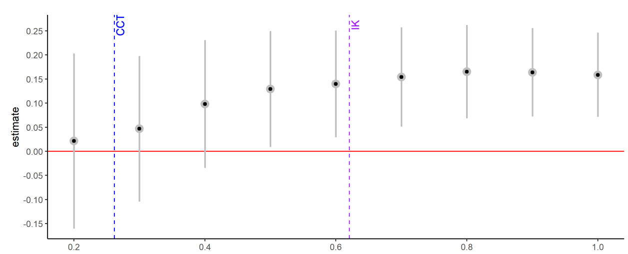
Note: Authors' calculations with data from Progresa, Conapo, ENSA 2000, and the Ministry of Public Health. Plots include 95% heteroskedasticity-robust confidence intervals. CCT: Calonico et al. (2014a) optimal bandwidth. IK: Imbens and Kalyanaraman (2012) optimal bandwidth.

Figure A7: **Use of health services with Administrative Data**

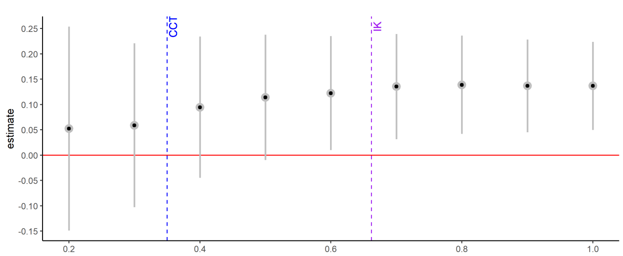
(a) Total medical visits (2000)



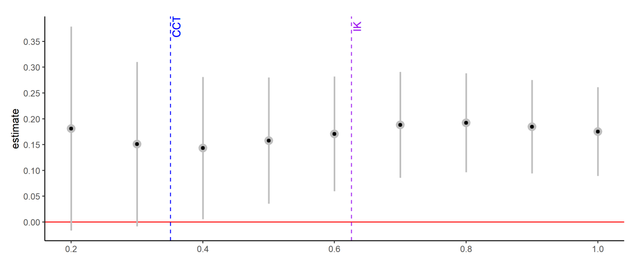
(b) Total medical visits (2001)



(c) Total medical visits (2002)



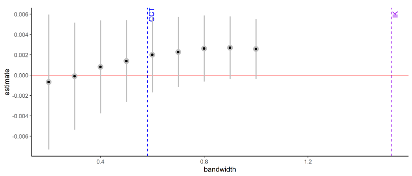
(d) Total medical visits (2003)



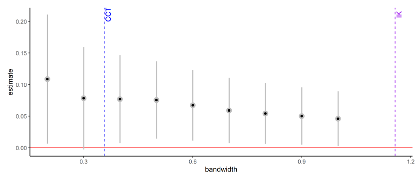
Note: Authors' calculations with data from Progresa, Conapo, ENSA 2000, and the Ministry of Public Health. Plots include 95% heteroskedasticity-robust confidence intervals. CCT: Calonico et al. (2014a) optimal bandwidth. IK: Imbens and Kalyanaraman (2012) optimal bandwidth.

Figure A8: **Reproductive health**

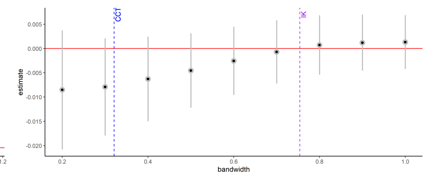
(a) Family planning talks at prevention services



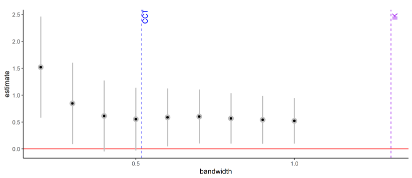
(b) Effective contraceptive methods



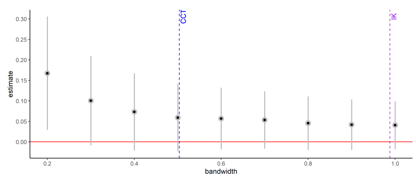
(c) Pregnancy care talks at prevention services



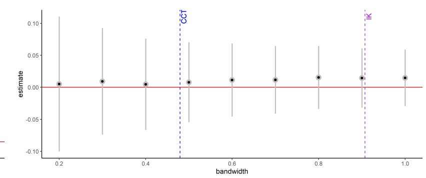
(d) Prenatal check-ups



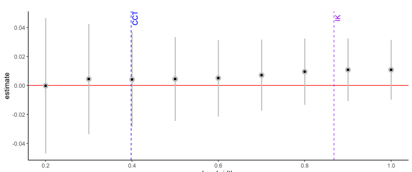
(e) Had pregnancy revision in first quarter



(f) Woman has been pregnant



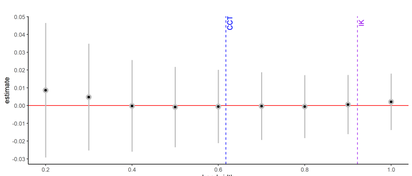
(g) Woman has been pregnant in 2000



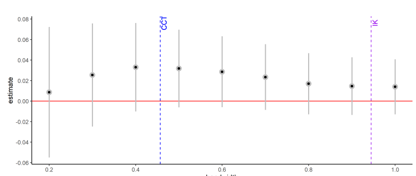
Note: Authors' calculations with data from Progresa, Conapo, ENSA 2000, and the Ministry of Public Health. Plots include 95% heteroskedasticity-robust confidence intervals. CCT: Calonico et al. (2014a) optimal bandwidth. IK: Imbens and Kalyanaraman (2012) optimal bandwidth.

Figure A9: **Chronic disease and prevention**

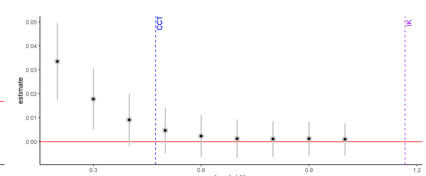
(a) Adult diagnosed with diabetes



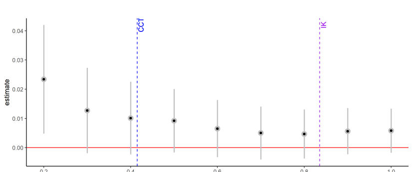
(b) Adult diagnosed with high blood pressure



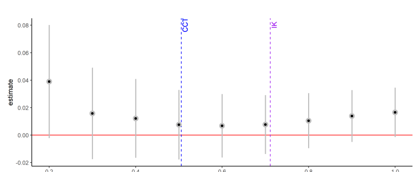
(c) Diabetes check at prevention services



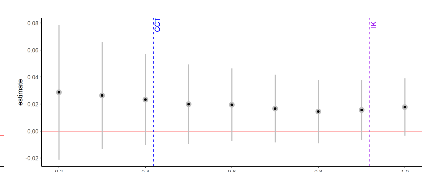
(d) HBP check at prevention services



(e) Pap smear check at prevention services



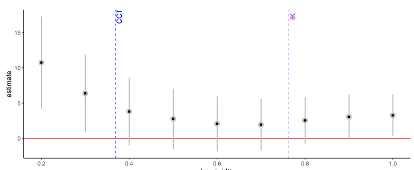
(f) Preventive breast cancer revision



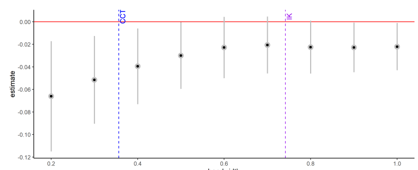
Note: Authors' calculations with data from Progresa, Conapo, ENSA 2000, and the Ministry of Public Health. Plots include 95% heteroskedasticity-robust confidence intervals. CCT: Calonico et al. (2014a) optimal bandwidth. IK: Imbens and Kalyanaraman (2012) optimal bandwidth.

Figure A10: **Waiting time**

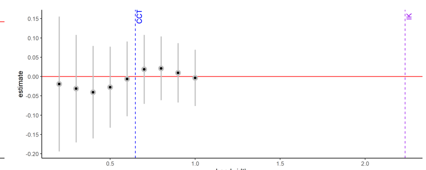
(a) Waiting time for consultation (ENSA)



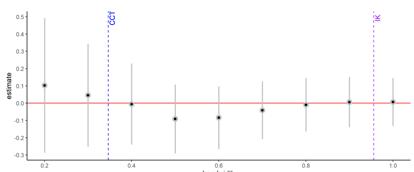
(b) Waiting time below 50 mins (ENSA)



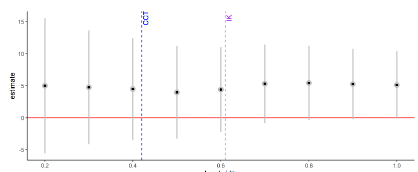
(c) User perceived short waiting time (ENSA)



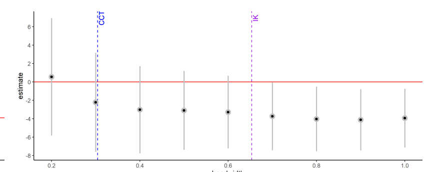
(d) Sufficient consultation time (ENSA)



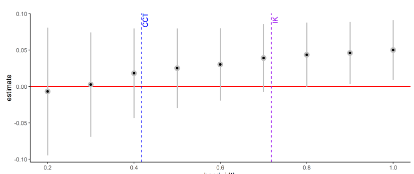
(e) Average waiting minutes (INDICAS)



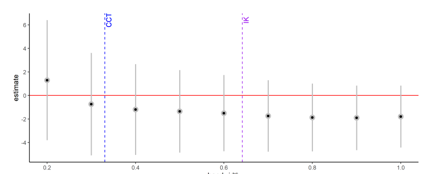
(f) Satisfied with waiting time (INDICAS)



(g) There was a complaint (INDICAS)



(h) Dr. explained health status (INDICAS)



Note: Authors' calculations with data from Progresa, Conapo, ENSA 2000, and the Ministry of Public Health. Plots include 95% heteroskedasticity-robust confidence intervals. CCT: Calonico et al. (2014a) optimal bandwidth. IK: Imbens and Kalyanaraman (2012) optimal bandwidth.

Appendix 6 Cluster errors by locality

In this section, we replicate the results in the main text. We find consistent results but larger standard errors, given that most of the employed data come from survey data.[19]

Table A8: **Replicating Results in Tables 2 - 6 with standard errors clustered by locality**

	Variable	$\mathbb{1}(index_j > 0)$	Heteroskedasticity robust p-value	Locality clustered p-value	Data
(1)	ln(Total visits 2000)	0.0747	0.2223	0.2379	A
(2)	ln(Total visits 2001)	0.1293	0.0351	0.0389	A
(3)	ln(Total visits 2002)	0.1142	0.0706	0.0724	A
(4)	ln(Total visits 2003)	0.1578	0.0115	0.0116	A
(5)	Attended by medic	0.0132	0.0320	0.1790	S
(6)	Child attended by medic	0.0818	0.0338	0.0710	S
(7)	Used health services	0.0397	0.0001	0.0835	S
(8)	Medicated by medic	0.0122	0.0452	0.2053	S
(9)	Self-medicated	-0.0085	0.0136	0.1860	S
(10)	Health improvement	0.0664	0.0000	0.0352	S
(11)	0-4 yo user	0.0557	0.0776	0.2503	S
(12)	5-14 yo user	0.0433	0.0163	0.1236	S
(13)	15-19 yo user	0.0084	0.7516	0.7447	S
(14)	20-49 yo user	0.0368	0.0206	0.1379	S
(15)	50-64 yo user	0.0531	0.1242	0.2389	S
(16)	65+ yo user	-0.0081	0.8527	0.8789	S
(17)	Family planning	0.0014	0.4983	0.4948	S
(18)	Contraceptives	0.0755	0.0155	0.0815	S
(19)	Prenatal talks	-0.0045	0.2448	0.2289	S
(20)	Prenatal checkups	0.5530	0.0627	0.1671	S
(21)	Rev. in 1Q	0.0591	0.1604	0.2359	S
(22)	Has been pregnant	0.0078	0.8062	0.8150	S
(23)	Pregnant in 2000	0.0044	0.7630	0.7560	S
(24)	Diabetes diagnostic	-0.0009	0.9405	0.9445	S
(25)	HBP diagnostic	0.0318	0.0989	0.0863	S
(26)	Diabetes test	0.0047	0.3354	0.5227	S
(27)	HBP test	0.0092	0.0972	0.2645	S
(28)	Pap smear test	0.0076	0.5566	0.6336	S
(29)	Breast cancer test	0.0199	0.1864	0.2721	S
(30)	Waiting time	2.7547	0.2052	0.5923	S
(31)	Waited 0-50 mins.	-0.0299	0.0478	0.3461	S
(32)	Perceived short wait	-0.0278	0.6037	0.7899	S
(33)	Satisfied with duration	-0.0915	0.3673	0.5705	S
(34)	Average waiting time	3.9556	0.2834	0.2816	S
(35)	Satisfaction with waiting time	-3.0930	0.1570	0.1552	S
(36)	Complaint	0.0252	0.3671	0.3646	S
(37)	Medical explanation	-1.3520	0.4491	0.4471	S

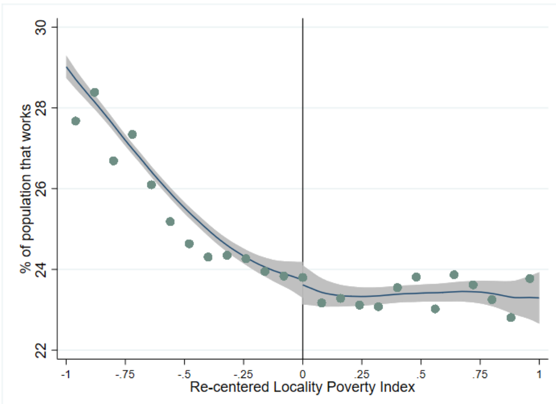
Note: This table replicates Tables 2, 3, 4, 5, and 6 in the main text. Additional to reporting heteroskedasticity-robust p-values, we report p-values that correspond to locality-clustered standard errors. In the last column, A stands for administrative data, and S stands for survey data. Statistics are reported for 2000, except rows (2)-(4) which also report information for 2001-2003, and rows (34)-(37) which report information for 2003.

[19] We also estimated bias-corrected robust standard errors and p-values (Calonico et al., 2014b), both for the heteroskedastic and cluster cases, with similar results.

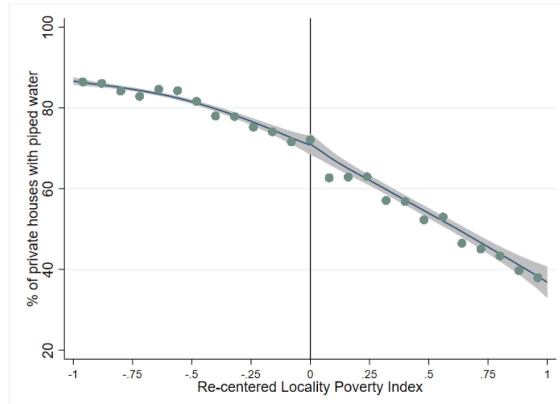
Appendix 7 Continuity test

Figure A11: **Continuity test with 1990 data**

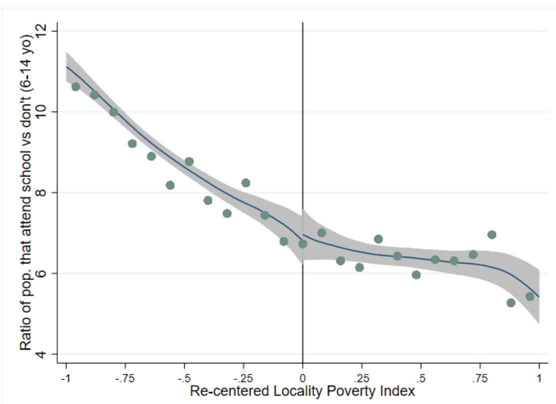
(a) % of population that works



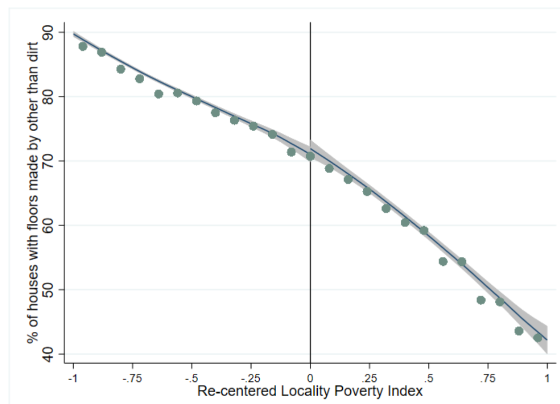
(b) % of private houses with piped water



(c) Ratio of pop. that attend school vs don't (6-14 yo)



(d) % of houses with floors made by other than dirt



Note: Authors' calculations with Progresa and INEGI's 1990 Population Census data. These figures show RDD plots for health-associated outcomes in 1990, before Progresa was implemented. We present binned means of the outcome variables around the program eligibility cutoff and a local polynomial of degree one. More indicators were tested using the 1990 census data, showing a continuous behavior around the discontinuity.