Saving Lives: Evidence from a Nutrition Program in Ecuador*

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Abstract

Many governments in developing countries implement programs that aim to address nutrional failures in early childhood, yet credible evidence on the effectiveness of these interventions is scant. This paper evaluates the impact of a large-scale, government-run, food fortification program—*the Programa de Alimentación y Nutrición Nacional (PANN)* 2000—on child mortality in Ecuador. The program was implemented by regular administrative staff at local public health posts and consisted of offering a free micronutrient-fortified pap, *Mi Papilla,* for children aged 6 to 24 months in exchange for health check-ups for the children. Our regression discontinuity design exploits the fact that at its inception, the *PANN* 2000 was running for about eight months only in the poorest communities (*parroquias*) of the country. Our main result is that the presence of the program reduced child mortality in the exposed cohort from a level of about 2.5 percent by 1 to 1.5 percentage points.

Keywords: early childhood nutrition, mortality, nutritional interventions, regression discontinuity, Ecuador
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1 Introduction

Many governments in developing countries implement programs that aim to address nutritional failures in early childhood, raise survival rates, and improve human capital formation.[1] Recent meta-studies conclude that there is convincing evidence, usually based on randomized trials, that specific interventions improve the nutritional status of children and save lives under clinical or ideal field conditions (Bhutta et al., 2008; Ainsworth et al., 2010). For example, community-level efficacy trials have consistently shown that vitamin A supplementation reduces child mortality across many different settings (Beaton et al., 1993; West and Darnton-Hill, 2008; Mayo-Wilson et al., 2011).[2] Efficacy trials also provide evidence that multiple micronutrient interventions can improve child health and growth (Ramakrishnan, Goldenberg, and Allen, 2011). However, much less is known about the effectiveness of nutrition interventions when conditions for both service delivery and recipient compliance are "routine", rather than "ideal", as in typical scaled-up programs.[3] Recognizing the scant evidence on the effectiveness of nutrition programs—particularly food fortification programs[4]—, many studies call for more research in this area.[5]

This paper provides the first regression discontinuity evidence on the effectiveness of a nutrition program in reducing child mortality, focusing on a large-scale, government-run, food fortification program in Ecuador. The *Programa de Alimentación y Nutrición Nacional* (*PANN*) 2000 was implemented by regular staff at local public health posts and consisted of offering a free micronutrient-fortified pap, *Mi Papilla,* for children aged 6 to 24 months in exchange for monthly health check-ups for the children.[6] We address concerns about endogenous program placement by exploiting the fact that at its inception in August 2000, the *PANN* 2000 was running for about 8 months only in the poorest communities (*parroquias*) of the country, as measured

[1]The rationale for these programs is that child undernutrition is associated with increased mortality risk (Black et al., 2008), as well as impaired physical and cognitive development of those children who survive (Victora et al., 2008).

[2]However, by far the largest vitamin A supplementation trial on record, implemented through the Indian public health system, finds a small and statistically insignificant impact on child mortality (Awasthi, Peto, Read, et al., 2007).

[3]Under "ideal" conditions, researchers control both service delivery and compliance with the treatment by recipients. Under "routine" conditions, there is no researcher involvement whatsoever. "Best practice" conditions are an intermediary case. These distinctions are further discussed in Victora, Habicht and Bryce (2004).

[4]Bhutta et al. (2008) note on page 434 that "Evidence about food fortification programmes and their effect on maternal and childhood micronutrient status and health is especially scarce."

[5]Bhutta et al. (2008) conclude that "Given the paucity of effectiveness data, strengthening of monitoring and rigorous assessment of large-scale nutrition programmes are imperative." Similar calls for research on program effectiveness are made by Ainsworth et al. (2010), and by Habicht and Pelto (2012), for example.

[6]*Mi Papilla* is designed to provide about 50% of daily energy requirements; 100% of the requirements of protein, iron, folic acid, and zinc; 60% of vitamin C, the B vitamins, and magnesium; and 30% of vitamin A, calcium and phosphorus, in addition to other nutrients (Lutter et al., 2008).

by a community-level consumption poverty index. Under a relatively weak—and to some extent testable—assumption, this assignment rule generated variation in program availability that was "as good as" random locally around the targeting cutoff of 89.05% poverty.[7] We exploit this variation to estimate the effect of making the program available in the community (an intent-to-treat effect) on child mortality using (sharp) regression discontinuity (RD) analysis.[8]

The internal validity of our results hinges on the identifying assumption that *parroquias* had (at most) only imprecise control over the value of their community-level consumption poverty index. As long as this control was imprecise at best, treatment assignment was randomized around the cutoff (Lee and Lemieux, 2010). We think that this identifying assumption is highly plausible in our context, not least because the poverty index was created several years before the *PANN* 2000 was announced. Moreover, we test an empirical implication of this assumption, namely that pre-treatment observables should exhibit no discontinuity at the cutoff, which is equivalent to testing for balance at baseline in randomized designs. We find no evidence of economically or statistically significant discontinuities in important determinants of child mortality, such as household sanitation infrastructure (access to piped water, availability of toilets), mothers' education, or pre-treatment child mortality and health check-ups at the *parroquia* level.

Our main result is that the presence of the program reduced child mortality in the exposed cohort from a level of about 2.5 percent by 1 to 1.5 percentage points.[9] The magnitude of this impact is plausible given what we know about program coverage—*Mi Papilla* was distributed to more than 80 percent of the target population according to program progress reports—and based on results from efficacy studies, showing that fortification of food with iron and other micronutrients improves the nutritional status of infants and children. Undernutrition is thought to account for more than 50% of deaths of children aged 6 to 59 months worldwide (Pelletier et al., 1995):[10] if the program improved the nutritional status in the target population sufficiently, one would therefore

[7]A potential concern with any RD design is that other government policies were also related to the cutoff. If so, we would identify only the combined causal effect of the PANN 2000 together with these other policies. To our knowledge, however, there are no other programs that used the same cutoff during our study period.

[8]We interpret our estimates as intent-to-treat because we do not observe *Mi Papilla* uptake at the individual level in the census data. From the administrative data we only know whether the program was available in the community and that the coverage rate was more than 80%.

[9]Assuming an impact of -1 death per 100 children exposed to the program, a coverage rate of 80%, an annual cost per beneficiary of US$ 30, and a program exposure of 8 months, the cost of a life saved amounts to about (US$ $30 \times 80) \times 8/12 =$ US$ 1600).

[10]Black et al. (2008) attribute 35% of deaths from ages 0 to 59 months to undernutrition. Given the target population of *Mi Papilla* (6 to 24 months), the estimate by Pelletier and colleagues is more appropriate as a reference for our study.

expect child mortality to fall approximately by half.[11]

We corroborate our estimates in a number of ways. First, we provide evidence based on administrative data that the number of check-ups at health posts increased in treatment *parroquias* of the *Sierra* region, which is as one would expect since check-ups in this region were lower to start with. The magnitude of the discontinuity in health check-ups implies that mothers added about 0.66 visits per month in *Sierra parroquias*. Second, we present results on mortality for cohorts that were already too old to be included when the program started. As expected for a program targeted based on age, impact estimates for these cohorts are essentially zero and nowhere near statistical significance. Third, we estimate the impact on mortality for a broader set of cohorts that were exposed to the program for a shorter period—rather than a full 8 months as in our main specification—and find effects that are smaller in magnitude but still consistently negative, which further adds to the plausibility of our results.

Our paper provides the first regression discontinuity evidence on the effectiveness of a nutrition program in reducing child mortality.[12] We are aware of four other evaluations of nutrition programs—broadly defined to include cash transfer programs with a nutritional component—that report child (or infant) mortality reductions.[13] Two of these studies consider government-run vitamin A supplementation programs, one from Yemen (Banajeh 2003), the other from Nepal (Thapa, Choe, and Retherford 2005). The third study is a vitamin A fortification trial using commercially marketed monosodium glutamate in Indonesia (compared to non-fortified monosodium glutamate) (Muhilal et al., 1988). The fourth study examines the conditional cash transfer program *PROGRESA* (Barham, 2011). The research designs used in these studies rely on relatively strong identifying assumptions.[14] A key advantage of our study—inherent in any RD or randomized

[11] Vitamin A supplementation alone has been found to have impacts of similar magnitude as our study (relative mortality reduction of between 40 and 60%) under ideal field conditions (Mayo-Wilson et al., 2011).

[12] In contrast to existing efficacy trials of specific nutrition interventions, our study examines the effectiveness of a nutrition program under truly "routine" conditions. As noted in Victora, Habicht and Bryce (2004), the mere presence of an evaluation team—as in any prospective evaluation—is likely to affect provider behavior and encourage service delivery under "best practice" rather than "routine" conditions.

[13] While program impacts on child mortality are rarely documented, a large literature, recently reviewed in Ainsworth et al. (2010) examines impacts of large-scale cash transfer, community nutrition, and early child development programs on anthropometric outcomes. On conditional cash transfers: Attanasio et al. (2005); on community nutrition programs: White and Masset (2007); Hossain et al. (2005); Galasso and Umapathi (2009); Galasso, Umapathi, and Yau (2011); Alderman et al. (2009); on early child development programs: Armecin et al. (2006); Alderman (2007); Alderman et al. (2006); Behrman, Cheng, and Todd (2004).

[14] Banajeh (2003) compares mortality rates before and after a nationwide vitamin A supplementation campaign. Thapa, Choe, and Retherford (2005) use multiple (logistic) regression to control for observables. Barham (2011) uses a fixed-effects approach. Muhilal et al. (1988) compare mortality rates across treatment and nearby (non-randomized) control villages.

design—is that institutional knowledge about program assignment alleviates concerns about endogeneity of treatment (Lee and Lemieux, 2010; Lee and DiNardo, 2010).

The most closely related study to ours by Lutter et al. (2008) also evaluates the *PANN* 2000—without examining its impact on child mortality—, using health centers in one district that initiated the program in May 2002 as the treatment group and neighboring communities, where the program was scheduled to be initiated one year later, as the comparison group. The results of this study show that children in program communities consumed significantly more energy, protein, fat, iron, zinc, vitamin A, and calcium than children in neighboring (non-randomly chosen) comparison areas. The study also finds that the odds of being anemic were 58% lower for *PANN* 2000 children, as well as impacts on height and weight for relatively older children (12 to 14 months when the program began). Taken together, our studies thus provide convincing evidence on the entire causal pathway linking availability of the *PANN* 2000 in the community to *Mi Papilla* intake at home, nutritional status, morbidity and child mortality.

Another advantage of our study is that at its inception, the nutrition component of the *PANN* 2000 was not bundled with any other services—such as counseling mothers on infant and child feeding practices[15]—, except for check-ups for the children.[16] In fact, beyond the vitamin A supplementation and fortification evaluations that look at child mortality mentioned above, we are aware of only one other effectiveness study that is able to isolate the impact of a specific nutritional component, namely iron fortification of milk (compared to non-fortified milk) on anemia in Mexico (Rivera, Shamah, Villalpando, and Monterrubio, 2010). Existing large-scale studies—whether randomized or not—typically cannot disentangle the impact of nutritional program components from the effect of counseling or other services simply because these interventions tend to come as a package in real-world government or NGO programs. For example, such is the case with the randomized evaluations of community-based nutrition programs in Haiti (Ruel et al., 2008) or Senegal (Linnemayr and Alderman, 2011), or with evaluations of the conditional cash transfer

[15]Educational components were phased in only at a later stage of the program according to progress reports from August 2000 through March 2001. Even in the later period analyzed in Lutter et al. (2008)—when nutrition counseling was a component of the *PANN* 2000—there is no evidence that better feeding practices were a channel through which the program operated to affect the nutritional status of children.

[16]As noted above, the program had an effect on the number of health check-ups in the *Sierra* region. This means that the mortality rate reduction we find might be due to improved detection and treatment of common childhood diseases, in addition to a reduced case fatality rate due to improved nutrition.

program *PROGRESA,* such as Gertler (2004) or Rivera et al. (2004).[17]

The paper is organized as follows. Section 2 provides background on nutrition and public health service delivery in Ecuador and describes the *PANN* 2000 intervention in more detail. Section 2 also describes the construction of the consumption poverty index that was used to target the program to the poorest *parroquias*. In Section 3, we discuss the identifying assumption for a causal interpretation of our estimates. We present the data in Section 4. Section 5 discusses the estimation approach and Section 6 evaluates the internal validity of the study. Section 7 presents estimation results. We conclude with a discussion of limitations and extensions.

2 Background and program description

2.1 Child nutrition prior to the *PANN* 2000

In 1999, one year before the *PANN* 2000 was initiated, an estimated 31.1% of children under 5 in Ecuador were stunted, or chronically undernourished, according to their height-for-age index. These undernutrition levels rose to 42.8% in rural areas, and even 54.3% in the central mountain regions, the *Sierra* (*SIISE* 2010). As in other countries, most undernutrition in Ecuador occurred between the age of 6 to 24 months. Micronutrient deficiencies were similarly widespread—earlier studies had found deficiencies of iron (anemia) in over 60% and of vitamin A in about 17% of children from high risk groups aged 6 to 36 months.[18] To reduce these nutritional failures, Ecuador had initiated a series of feeding interventions in the 1990s, such as a school meal initiative for children of 5 years and older, and some smaller interventions with special components for children aged 2 to 5 years.

2.2 Program components, costs and financing

The *PANN* 2000 was officially launched in August 2000 and was the first large-scale, government-run intervention in Ecuador to specifically target the age group of 6 to 24 months-olds and thus address undernutrition in very early childhood.[19] The objectives of the program were to reduce

[17] Another example is Ludwig and Miller's (2007) regression discontinuity analysis of the U.S. "Head Start" program, which has been providing pre-school, health and other social services to poor children and their families since 1965.

[18] For a summary, see Lutter et al. (2008), or Carranza (2011).

[19] According to the 1999 Ecuador Living Conditions Survey, *Encuesta de Condiciones de Vida* (*ECV*), only 3.6% of children 6 to 24 months old in rural areas were receiving free food by the government before the *PANN* 2000 started.

child growth retardation, undernutrition, and micronutrient deficiencies (*MSP* 2000).[20] The two main components of the program as initially planned were 1) to counsel mothers on appropriate age-specific child feeding practices and 2) the distribution of the free micronutrient-fortified pap, *Mi Papilla,* in exchange for health check-ups for the children. In its initial stage from August 2000 through the end of March 2001, the training component was still under preparation, leaving provision of the fortified food combined with health check-ups as the only active component during this period (*PANN* 2000 progress reports).

Mi Papilla is designed to provide about 50% of daily energy requirements; 100% of the requirements of protein, iron, folic acid, and zinc; 60% of vitamin C, the B vitamins, and magnesium; and 30% of vitamin A, calcium and phosphorus, in addition to other nutrients (Lutter et al., 2008). After the check-up for the children, mothers would receive a voucher for a 2 kg bag of *Mi Papilla* powder—a monthly ration—, which they could redeem in local pharmacies and food stores. The porridge would be prepared at home by adding boiled or otherwise purified water to the powder.

The initial program stage was financed with $1,340,000 from international donor agencies and $500,000 from the national government (Carranza 2011). By 2003, the external aid share was replaced with government financing, and the yearly cost per beneficiary at that time was estimated at $29.7 (Gordillo 2005). The *PANN* 2000 budget was largely administered through the World Food Program, which purchased the powdered food rations through annual public auctions from private companies on behalf of the government. Private companies also shipped the food rations to pharmacies and food stores in each program *parroquia*.

2.3 Administrative background

Primary health services in Ecuador are provided by the central government. The administration runs from the ministry of health down to provincial directorates, health areas, and individual health facilities of different types. In rural areas, most regular control visits related to reproductive and child health, such as those required under the *PANN* 2000, took place in so-called health subcenters or health posts (*subcentros/puestos de salud*). These are generally staffed with 2-3 registered or auxiliary nurses, and possibly a general doctor. Most rural *parroquia*s have one health subcenter

[20] In a later period, pregnant and lactating mothers also started receiving fortified drinks and nutrition counseling (Carranza 2011).

or post; those without any are included in the catchment area of the health subcenter/post in a neighboring *parroquia*.[21] Mothers and their children under 5 have universal, free access to primary health care since 1999.

Within this public health system, the *PANN* 2000 was implemented as follows. The ministry promoted the initiative through the provincial directorates among health workers from the *parroquia*s, who typically attended information and training sessions at the canton or province headquarters. The *parroquia*s then formed committees that helped to spread information about the program and select beneficiaries within their area (through health centers, house visits, food stores, community associations, churches, etc.). Targeting final beneficiaries within *parroquia*s based on poverty or basic needs soon turned out to be too difficult, and the product was essentially offered to every mother who attended the health center with a child in the eligible age range of 6 to 24 months.[22]

2.4 Targeting of the *PANN* 2000

The community-level consumption poverty index used for treatment assignment was estimated by ODEPLAN (1999) and is publicly available in the *Sistema Integrado de Indicadores Sociales del Ecuador* (*SIISE*, 2010). The index measures for each *parroquia* the proportion of individuals whose estimated consumption falls below the poverty line. The poverty line is the monetary cost of a "basic needs" basket of goods and services, defined by the *SIISE* Poverty Commission (food, housing, education etc.). Consumption is projected from the 1995 Living Conditions Survey, *Encuesta de Condiciones de Vida* (*ECV*), on the households registered in the 1990 National Census, *Censo Naciónal de Población y de Vivienda*. The assignment variable was thus created several years before the *PANN* 2000 was announced.

To determine participation in the initial program stage, *parroquia*s in the seven poorest provinces (Bolívar, Chimborazo, Cotopaxi, Imbabura, Loja, Manabí, and Orellana) were ranked by the index and pre-selected if it exceeded 90%, yielding 145 *parroquia*s. Eventually, urban *parroquia*s would be removed from this list to be targeted differently, with their places substituted by the next poorest rural units. This left the final cutoff for rural *parroquia*s at 89.05%.[23]

[21] In our largest estimation sample one treatment *parroquia* and three comparison *parroquias* did not have their own health post.

[22] Is is possible that some kids from comparison *parroquias* got *Mi Papilla* from health posts in neighboring treatment *parroquias*. Such spillovers would would bias our estimates towards zero.

[23] When the *PANN* 2000 started, rural Ecuador was divided into 22 provinces, 176 cantons, and 776 *parroquia*s.

The *PANN* 2000 progress reports in Chauvin (2001) show that the initially selected rural *parroquias* coincided sharply with those which report numbers of actually attended children—at least in the provinces of Bolívar, Chimborazo, Imbabura, and Manabí. In contrast, Cotopaxi and Loja had not initiated the program by March 2001, and the *PANN* 2000 in Orellana had been extended to the entire province by that time. Based on this program information, we restrict our estimation sample to *parroquias* from provinces Bolívar, Chimborazo, Imbabura, and Manabí. We further restrict the sample of parroquias to those with a poverty index within six percentage points on either side of the 89.05% cutoff. Results using broader neighborhoods around the cutoff are quantitatively similar in terms of both point estimates and standard errors and are available on request. Figure 1 shows the geographical locations of treatment and comparison *parroquias* in the broadest estimation sample from the four program provinces. The (sharp) first stage and histogram of the consumption poverty index for our estimation sample are depicted in Figure 2.

In winter 2000/01, the government drafted an emergency plan to increase the coverage of social programs in response to the prevailing economic crisis in Ecuador. This led to the *PANN* 2000 being rapidly scaled up throughout the rest of the country from April 2001 onward, thereby ending the initial stage of the program and the quasi-experiment. This allows us to identify the impact of the infant nutrition component of the *PANN* 2000 over the 8-month-period from August 2000 to March 2001.

3 Identification

The basic intuition behind the regression discontinuity design is that, in the absence of program manipulation, *parroquia*s to the left of the treatment-determining consumption poverty cutoff should provide valid counterfactual outcomes for *parroquia*s on the right-hand side of the cutoff (where the micronutrient-fortified powder, *Mi Papilla,* was being offered). More formally, let Y denote an outcome variable at the *parroquia* level (child mortality, number of health check-ups per child), τ the (constant) treatment effect, D the indicator function for treatment (availability of the program), X the *parroquia* consumption poverty index, c the cutoff 89.05%, $f(X)$ a polynomial function of the poverty index, and U unobserved additional factors that affect outcomes. The model is as follows:

$$\begin{aligned} Y &= \tau D + f(X) + U \\ D &= 1[X \geq c] \end{aligned}$$

If the potential regression functions $E[Y|D=1,X]$ and $E[Y|D=0,X]$ are both continuous in the poverty index, or equivalently, if $E[U|X]$ is continuous, then the difference in conditional expectations identifies the treatment effect at the threshold:[24]

$$\lim_{X \downarrow c} E[Y|X] - \lim_{X \uparrow c} E[Y|X] = \tau \tag{1}$$

The key assumption for this study concerns the continuity of the potential regression functions, or equivalently, the continuity of $E[U|X]$, which gives the estimand in equation (1) above a causal interpretation. Intuitively, the continuity assumption requires that unobservables vary smoothly as a function of the poverty index and, in particular, do not jump at the cutoff. As shown in Lee and Lemieux (2010), sufficient for the continuity of the regression functions (or the continuity of $E[U|X]$) is the assumption that individual densities of the treatment-determining variable are smooth. In our case, this assumption explicitly allows for individuals in the *parroquia* to have some control over their particular value of the poverty index. As long as this control is imprecise, treatment assignment is randomized around the cutoff. Since the assignment variable was created several years before the *PANN* 2000 was announced, we think that the continuity assumption is highly plausible in our context.[25] Moreover, in Section 6 below, we test an empirical implication of this assumption, namely that pre-treatment observables should exhibit no discontinuity at the cutoff, which is equivalent to testing for balance at baseline in randomized designs.

A final potential concern is that other government policies were also related to the 89.05% cutoff. If so, τ would reflect the combined causal effect of the *PANN* 2000 and other policies. To our knowledge, however, there are no other programs that used the same cutoff during our study period. This conjecture is corroborated in Section 7 below, where we present results on mortality

[24] With heterogeneous treatment effects, the RD gap identifies the average treatment effect at the cutoff. See Lee (2008) for an alternative interpretation of the treatment effect identified in this case as a weighted average of individual treatment effects, where the weights reflect the ex ante probability that a parroquia's score is realized close to the cutoff.

[25] In our case, the continuity of individual poverty index density functions also directly ensures that treatment status is randomized close to the cutoff. An additional concern would be imperfect compliance with the treatment rule, but in our study period all eligible parroquias received the program, and none of the ineligible ones did.

for cohorts that were already too old to be included when the program started. As expected for a program targeted based on *both* age and community-level poverty, impact estimates for these cohorts are essentially zero and nowhere near statistical significance.

4 Data

The poverty index is based on the political-administrative division in late 1999, which we adopt as an anchor to link *parroquia*-level data over time. All observations from individual units, such as children and health centers, are aggregated at the *parroquia*-level and linked by taking into account changes in the *parroquia* composition over time. We first add the November 1990 and November 2001 National Censuses, as well as the year 2000 and 2001 rounds of the Census on Health Center Resources and Activities, *Recursos y Actividades de Salud* (*RAS*). The National Census and the *RAS* provide universal coverage of all *parroquia*s in the country and serve as sources for outcomes and control variables used in our analysis. We verified that the *PANN* 2000 did not contain any performance awards or resource assignment mechanisms that would have created incentives for manipulating the *RAS* data. More generally, there is no reason to believe that measurement error in any of our outcome variables would systematically jump at the poverty index cutoff and thus generate a bias.

4.1 Health check-ups

Yearly visits of children for two age groups (0 to under 1 year, 1 to under 5 years) are reported by all health centers. We extract the absolute numbers of visits for both age groups in 2000, add them up, and normalize the total with the imputed number of children based on target population estimates in Flores (2001). The number of health-check ups per 0-4 year-old is thus given by:

$$\frac{\text{Number of visits among 0-59 month-olds during 2000}}{\text{Officially estimated number of 6-24 month-olds from Flores (2001)} \times (60/18)}$$

Some additional measurement error may arise from the fact that not all children visit the health facility in their own *parroquia*.

4.2 Child mortality

The child mortality rate is computed from the 2001 Census. One particularity with the census is that it did not collect the mortality history of other children than the last born in a given household. In our analysis, the child mortality rate is thus the percentage of last-born children in the relevant cohort who died until census day (25th of November 2001). With a target age of 6 to 24 months, the fully exposed cohorts include the birth months April 1999 (children who only fell out of target age when the *PANN* 2000 was extended to comparison *parroquia*s) to February 2000 (those who were just old enough to receive program benefits when the program started). Our mortality outcome is measured as follows:

$$\frac{\text{Number of last-born children born April 1999 to February 2000 who died by census day in 2001}}{\text{Total number of last-born children born April 1999 to February 2000}}$$

This measure captures about two thirds of all children in the program cohorts.[26] We prefer the census data over mortality records kept by the health administration, which largely contain urban health units and residents, but show fairly low coverage in rural parroquias.

Note that children born in August 1998 or earlier, who were already 24 months or older at program inception in August 2000, did not receive *Mi Papilla* at any point in time. We use this fact to construct the mortality rate of slightly older cohorts born between October 1997 and August 1998 (containing the same number of birth months as the program cohorts):

$$\frac{\text{Number of last-born children born October 1997 to August 1998 who died by census day in 2001}}{\text{Total number of last-born children born October 1997 to August 1998}}$$

Since the target population was children 6 to 24 months, one would not expect the *PANN* 2000 to have an effect on child mortality for these older cohorts. Note that the last-born children in these cohorts could not have received *Mi Papilla* through younger siblings since, by definition, they did not have any.

[26]The total child population in our 11-month cohort cannot be calculated from the 2001 census due to missing birth dates for children of higher birth orders. However, Flores (2001) provides official estimates for the population of 6 to 24 month-olds (an 18-month cohort) developed for the *PANN* 2000. For the parroquias in our estimation sample, the census reports mortality status for 6,163 children in the cohort. The total estimated population in Flores (2001) in the same areas is 14,974. Our measure of child mortality hence captures approximately (6,163/14,974)× (18/11) = 67.3 percent of all children.

4.3 Nutrition status and anthropometric outcomes

Unfortunately, our data on nutritional status and anthropometric outcomes are very limited. Neither the ministry nor the provinces have kept records for the relevant cohort of children that would be sufficiently disaggregated for our analysis. The originally intended registration system for the *PANN* 2000 (*SIPPAN*) was never fully implemented, and the data collected in the initial years were entirely lost. It took until 2006 to again systematically compile comprehensive data on program beneficiaries and benefits. We have verified at the Ministry of Public Health that the nutrition data bases from 2006-10 do not include any data on children exposed to the *PANN* 2000 during the initial stages of the program.

Other administrative records that we attempted to access also failed to include the program cohort, or were not available at the level of *parroquia*s or health centers, such as the national growth monitoring system (*SISVAN*). Under this system, health centers were supposed to register the height-for-age of children at every visit, but data at the *parroquia* level or below are only available, if at all, at the headquarters of health areas. Besides, actual SISVAN coverage was very incomplete, and the *PANN* 2000—through its effect on health check-ups—probably influenced who got registered.[27]

Given the lack of administrative health records, we also tried to obtain anthropometric data for young children from a series of socioeconomic and health surveys. The pre-program measures we have come from two sources. First, stunting of children under 5 was observed in the 1988 Food, Nutrition, and Health Diagnosis, *Diagnóstico de la Situación Alimentaria, Nutricional y de Salud* (*DANS*). The *SIISE* (2010) provides stunting rates projected from the *DANS* on the 1990 census and thus full coverage for all *parroquia*s. Second, we pooled cohort-specific weight- and height-for-age for the children in 1998 and 1999 rounds of the *Encuesta de Condiciones de Vida* (*ECV*). Unfortunately, coverage of *parroquias and* sample sizes turned out too small to be useful for our purposes.[28]

For post-program anthropometric outcomes, we pooled children from the Reproductive Health Survey 2004, *Encuesta Demográfica de Salud Materna e Infantil* (*ENDEMAIN*), and the March

[27] In the population of 1-4 year-olds, only 21 to 28 percent of the children got their height registered through the SISVAN in the years 1999 to 2001 (World Bank, 2007).

[28] The *ECV* covered 18 out of the 75 *parroquias* in our largest estimation sample. For covered *parroquias,* the average sample size was 2.06 children in the relevant age range.

2004 Labor Force Survey, *Encuesta de Empleo, Desempleo y Subempleo* (*ENEMDU*). However, survey coverage of rural parroquias (about a third) and the number of children within parroquias (less than 2 percent of the target age group) turned out too small for statistical analysis.[29] Descriptive statistics of pre-treatment variables (Panel A) and outcomes (Panel B) are given in Table 1.

5 Estimation approach

Following Hahn, Todd, and Van der Klaauw (2001) and Imbens and Lemieux (2010), our main estimation approach is to use local linear regression in samples around the discontinuity, which amounts to running simple linear regressions allowing for different slopes of the regression function in the neighborhood of the cutoff. Allowing for slope is particularly important in the present application because child mortality is increasing as the poverty index approaches the cutoff from below, and again increasing after the threshold. A simple comparison of means for *parroquia*s above and below the cutoff would therefore bias estimates of the treatment effect towards zero. We follow the suggestions by Imbens and Lemieux (2008) and use a rectangular kernel (i.e. equal weight for all observations in the estimation sample). To ensure that our findings are not driven by functional form assumptions, we focus on estimation results from linear specifications in the discontinuity samples, adding nonlinear specifications as a robustness check.

Let Y denote an outcome; $\mathbf{W}$ a set of pre-treatment covariates and U an error term for each *parroquia*. Covariates are not needed for identification. We include them to guard against chance correlations with treatment status and to increase the precision of the estimates. For observations within a given percentage point distance h from the cutoff we estimate the following specification:

$$Y = \tau 1[X \geq c] + \alpha_0 + \alpha_1 X \times 1[X \geq c] + \alpha_2 X \times 1[X < c] + \gamma \mathbf{W} + U \qquad (2)$$

We use observations within successively larger neighborhoods ($h = 3, 4, 5, 6$ percentage points) around the cutoff in order to assess the robustness of the results.

[29] The *ENDEMAIN* and *ENEMDU* surveys together covered 28 out of the 75 *parroquias* in our largest estimation sample. For covered *parroquias,* average sample size was 1.57 for children in the relevant age range. The *parroquia* mean of last-born children in our program cohort is 80.733 (see Table 1). We estimate that these last-born children represent 67.3 of all children (see footnote 26). Our program cohort therefore has a mean of 80.733/0.673 =120 children per parroquia. Hence, the pooled *ENDEMAIN* and *ENEMDU* surveys only provide anthrometric data for 1.57/120 $<$ 2% of the children in the program cohort.

6 Internal validity checks

Since extensive manipulation of the poverty index would cast serious doubts on the internal validity of the research design, we check for any evidence of sorting, notably discontinuities in the distribution of the poverty index. The lower panel of Figure 2 plots the histogram of the consumption poverty index. Visual inspection reveals no discontinuities and the null hypothesis of a smooth density cannot be rejected according to the density test suggested by McCrary (2008).[30]

In Table 2, we estimate equation (2) for a host of pre-treatment outcomes and other covariates. The results show that there is no evidence of economically or statistically significant discontinuities in any of these variables. In Section 7 below we show that the estimated effects are robust to the inclusion of these covariates, including the pre-treatment mortality outcome shown in Table 2, which provides additional evidence regarding the internal validity of the design. At the bottom of Table 2 we also show F-test results for successively larger neighborhoods—corresponding to our estimation samples—which fail to reject the joint null hypotheses of no discontinuity in any pre-treatment covariate at conventional levels of significance, except for one bandwidth choice.[31]

7 Estimation results

7.1 Impact on child mortality

Table 3 presents impact estimates for child mortality of last-borns, born between April 1999 and February 2000, using linear specifications of the poverty index. All estimates in Table 3 are negative, ranging mostly from -1 to -1.5 percentage points. The estimates are essentially unchanged when pre-treatment controls are included and are statistically significant (at 5%) even in small neighborhoods of 3 to 5 percentage points around the cutoff. The estimates that are not significant in Table 3 Panel A are in the widest, 6 percentage point bandwidth, which might reflect a specification error: with the quadratic specification in Table 4 Panel A, these estimates are highly significant and linearity is rejected at the 10% level for one of the estimates. We conclude from these results that the presence of the *PANN* 2000 program reduced child mortality in the exposed

[30]The estimate (and standard error) are 0.34 (0.80).

[31]The test of the joint null hypotheses of no jumps in pre-treatment covariates is done by stacking these variables and running a joint estimation of individual discontinuities (Lee and Lemieux, 2010).

cohort from a level of about 2.5 percent by 1 to 1.5 percentage points.

In the *Sierra* sample (Table 4, Panel B), where child mortality tends to be higher in the absence of the program (2.5% vs. 3%), impact estimates are somewhat larger (in absolute value), about -2 percentage points. Most of the estimates from the *Sierra* provinces alone are not significantly different from zero, because of higher standard errors (rather than lower estimates) since in these provinces we only have two thirds of the total sample.[32]

Table 4 shows that the above results are robust to a quadratic specification of the poverty index. If anything, the impact estimates tend to become larger in absolute value compared to the local linear estimates in Table 3. We use an F-test of the joint hypotheses that the coefficients on the quadratic terms on either side of the cutoff are zero to test whether linearity of the polynomial in the poverty index can be rejected. As shown in Table 4, except for the specification in the widest 6 percentage point bandwidth with covariates in Panel A, there is virtually no statistical evidence against the null hypothesis of a linear model, which corroborates our focus on the linear estimates (and standard errors).

Figure 3 presents graphical evidence of the discontinuity in child mortality. Each dot represents the average residual from a regression of child mortality at the *parroquia* level on the list of control variables from Table 2. These are included to absorb some of the variation in the dependent variable and make the jump at the cutoff more easily visible. For example, the first dot to the left of zero represents the sample mean of partialled-out child mortality for all *parroquia*s within one percentage point to the left of the 89.05% cutoff. The figure shows that the reduction of about 1 percentage point in child mortality is visually robust irrespective of the width of the neighborhood or province sample examined. The fact that the regression lines slope upward without exception is further evidence favoring the validity of the design, since one would expect child mortality to increase with poverty.

7.2 Impact on Health Check-ups

Table 5 gives estimates of the jump in the number of check-ups at health centers per child for 0 to 4 year-olds during the year 2000 using linear specifications of the poverty index. The estimates in

[32]Estimates for the coastal province Manabí (available on request) are almost all negative as well, but they are more variable.

Panel A suggest that check-ups per child increased by about 0.2 although none of the estimates are statistically significant.

In contrast to the child mortality results above, there is considerable regional heterogeneity in the impact estimates on check-ups. Panel B of Table 5 restricts the sample to the three program provinces from the *Sierra* region, where check-ups were lower to start with (about 0.68 check-ups per child compared to 0.75 check-ups when the coastal province Manabí is included). The estimates in Panel B suggest that check-ups per child increased by about 0.8 and nearly all of the estimates are statistically significant at 5%, even in the smallest neighborhoods of 3 percentage points around the cutoff. The magnitude of the discontinuity implies that mothers added about 0.66 visits to health centers per month in *Sierra parroquias*.[33]

Table 6 shows that the results on check-ups are robust to a quadratic specification of the poverty index. As with child mortality above, there is virtually no statistical evidence against the null hypothesis of a linear model based on F-statistics in almost all specifications (the only exception being the 6 percentage point bandwidth with covariates in Panel B), which again corroborates our focus on the linear estimates (and standard errors).

Figure 4 presents the discontinuity in health check-ups graphically. Figure 4 shows clear evidence of a discontinuity in the number of check-ups per child at the cutoff in the *Sierra* provinces but not for all program provinces together, including Manabí. The figure for the *Sierra* provinces additionally shows that the discontinuity is visually robust irrespective of the width of the neighborhood examined.

7.3 Impact on child mortality, non-exposed cohorts and less-exposed cohorts

Table 7 presents impact estimates for child mortality of cohorts that were just too old to be exposed to the program. As expected, estimates for these cohorts are essentially zero and nowhere near statistical significance. Figure 5 confirms this result visually, showing no evidence of a discontinuity in mortality rates at the cutoff.

Finally, we estimate the impact on mortality for a broader set of cohorts that were exposed to the

[33] Jump in visits per 0-4 year-old during 2000 from Table 3 Panel B: 0.8; Age group in months: 60 (0 to 59 months); Target age group in months: 18 (6 to 23 months); Program period in 2000: 5 months (August through December); Coverage rate: 0.8. Impact on monthly check-ups per *PANN* 2000 child = [0.8 visits$\times$(60 months /18 months) / 0.8] / 5 months = 0.66 visits/month.

program for a shorter period—rather than a full 8 months as in our main specification—and find effects that are smaller in magnitude but still consistently negative (results available on request).

8 Conclusion

This paper provides the first regression discontinuity evidence on the effectiveness of a nutrition program in reducing child mortality. Our main result is that the presence of the program reduced child mortality in the exposed cohort from a level of about 2.5 percent by 1 to 1.5 percentage points. We also find evidence that the number of check-ups at health posts increased, even if only in the *Sierra* region, where check-ups were lower to start with. Unlike most existing effectiveness studies of nutrition programs broadly defined, we are able to attribute our estimates entirely to the availability of a fortified food (coupled with health check-ups) rather than to nutrition counseling or cash transfers (also typically coupled with health check-ups) for example.

While our exact knowledge about program assignment is a distinct advantage over existing observational studies, our approach also has the drawback that we cannot say anything about program impacts on children's physical and cognitive development because of data limitations. In future work, we might attempt to measure long-term effects of the *PANN* 2000 by tracking individuals from the exposed cohorts. Another limitation—inherent in any RD design—is that our estimates recover an impact that is local to the targeting cutoff. However, given the preponderance of observational program evaluations and experimental efficacy studies in the area of nutrition policy (relative to quasi-experimental work), and given the complementarities between these research designs, we feel that our approach is still underexploited.

9 References

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Table 1: Descriptive statistics

Sample	Ecuador: All Provinces	Ecuador: h = 6 All Provinces	Estimation Sample: h = 6 Program Provinces	Estimation Sample: h = 6, Sierra Provinces
Observations	776	312	75	49
Panel A: Pre-treatment variables				
Consumption poverty index for year 2000 (1990 census and 1995 living standards survey)	0.810 [0.130]	0.884 [0.033]	0.889 [0.029]	0.891 [0.030]
Number of check-ups at health post per 0 to 4 year-old during year 1999 (1999 health census)	0.720 [0.928]	0.745 [0.934]	0.541 [0.584]	0.563 [0.650]
Proportion of last-born children born April 1988 to February 1989 who died by November 1990 (1990 census)	0.024 [0.029]	0.024 [0.028]	0.025 [0.022]	0.023 [0.023]
Proportion of stunted children under 5 years old in 1990 (1990 census and 1988 nutrition survey)	0.545 [0.111]	0.571 [0.109]	0.587 [0.108]	0.663 [0.025]
Presence of a health center in the parroquia in 2000 (yes = 1) (2000 health census)	0.790 [0.408]	0.840 [0.367]	0.947 [0.226]	0.918 [0.277]
Proportion of women aged 18 or older who completed primary schooling (2001 census)	0.504 [0.148]	0.445 [0.123]	0.405 [0.107]	0.400 [0.102]
Proportion of dwellings with access to piped water (2001 census)	0.643 [0.281]	0.581 [0.306]	0.652 [0.286]	0.843 [0.088]
Proportion of households with toilet (2001 census)	0.563 [0.212]	0.539 [0.206]	0.638 [0.138]	0.584 [0.127]
Average number of persons per room (2001 census)	2.253 [0.612]	2.397 [0.665]	2.188 [0.331]	2.195 [0.336]
Number of last-born children born April 1999 to February 2000 living in the parroquia (2001 census)	86.735 [194.787]	71.939 [70.029]	80.733 [55.129]	70.327 [57.252]
Number of last-born children born April 1988 to February 1989 living in the parroquia (1990 census)	58.067 [66.192]	59.212 [57.023]	79.053 [51.616]	68.653 [53.133]
Estimated population of 6-24 month-olds in 2000 (Flores, 2001)	n/a [a]	158.147 [140.037]	199.653 [124.010]	163.163 [122.240]
Panel B: Outcomes				
Number of check-ups at health post per 0 to 4 year-old during year 2000 (2000 health census)	0.811 [0.936]	0.866 [0.998]	0.750 [0.692]	0.693 [0.645]
Proportion of last-born children born April 1999 to February 2000 who died by November 2001 (2001 census)	0.015 [0.030]	0.013 [0.020]	0.016 [0.017]	0.017 [0.019]

Notes: The unit of observation is the rural *parroquia*. h is the percentage point distance from the eligibility cutoff 0.8905. Table entries are sample means and sample standard deviations (in square brackets). The estimation sample contains *parroquias* in the program provinces (Bolívar, Chimborazo, Imbabura, Manabí) with complete observations for all variables. Sierra provinces are Bolívar, Chimborazo, and Imbabura.

[a] Numbers only available for the 460 poorest parroquias in 2000 (poverty index of 82.27 percent and higher).

Table 2: Test of discontinuities in pre-treatment variables

Neighborhood *h* (percentage points)	3	4	5	6
Observations	48	58	70	75
Number of check-ups at health post per 0 to 4 year-old during year 1999 (1999 health census)	-0.035 (0.288)	-0.077 (0.229)	-0.132 (0.222)	-0.110 (0.200)
Proportion of last-born children born April 1988 to February 1989 who died by November 1990 (1990 census)	-0.008 (0.011)	-0.005 (0.010)	-0.004 (0.009)	-0.003 (0.008)
Proportion of stunted children under 5 years old in 1990 (1990 census and 1988 nutrition survey)	0.003 (0.070)	-0.028 (0.056)	-0.057 (0.052)	-0.041 (0.050)
Presence of a health center in the parroquia in 2000 (yes = 1) (2000 health census)	-0.158 (0.104)	-0.057 (0.098)	-0.004 (0.048)	0.028 (0.038)
Proportion of women aged 18 or older who completed primary schooling (2001 census)	-0.024 (0.065)	0.014 (0.057)	-0.016 (0.048)	-0.022 (0.045)
Proportion of dwellings with access to piped water (2001 census)	-0.024 (0.176)	-0.140 (0.137)	-0.249* (0.130)	-0.185 (0.125)
Proportion of households with toilet (2001 census)	-0.061 (0.069)	0.022 (0.063)	0.022 (0.058)	0.008 (0.054)
Average number of persons per room (2001 census)	-0.050 (0.206)	-0.114 (0.174)	0.064 (0.163)	-0.002 (0.144)
Number of last-born children born April 1999 to February 2000 living in the parroquia (2001 census)	-41.018 (28.568)	-39.401 (27.133)	-17.682 (22.330)	-18.448 (20.683)
Number of last-born children born April 1988 to February 1989 living in the parroquia (1990 census)	-13.336 (30.962)	-11.801 (25.923)	6.748 (21.954)	8.315 (20.254)
Estimated population of 6-24 month-olds in 2000 (Flores, 2001)	-87.466 (65.656)	-71.257 (57.731)	-16.808 (47.385)	-7.264 (45.250)
F-statistic [p-value]	2.31 [0.02]	1.05 [0.41]	1.11 [0.36]	0.92 [0.52]

Notes: OLS estimates. The unit of observation is the rural *parroquia*. *h* is the percentage point distance from the eligibility cutoff 0.8905. Sample *parroquias* are from the program provinces Bolívar, Chimborazo, Imbabura, Manabí. Robust standard errors in parentheses.

*, **, and *** indicate significance at 10, 5, and 1 levels, respectively.

Table 3: Impact on child mortality, linear specification

Dependent variable: Proportion of last-born children born between April 1999 and February 2000 who had died by November 2001

Neighborhood *h* (percentage points)		3		4		5		6	
Pre-program controls		N	Y	N	Y	N	Y	N	Y
	Comparison mean	Panel A: Bolívar, Chimborazo, Imbabura, Manabí							
Treatment parroquia (yes = 1)	0.025	-0.015*	-0.015*	-0.015*	-0.018**	-0.012	-0.015**	-0.007	-0.011
		(0.009)	(0.008)	(0.008)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)
Observations		48	48	58	58	70	70	75	75
R^2		0.064	0.401	0.119	0.420	0.094	0.299	0.057	0.249
	Comparison mean	Panel B: Bolívar, Chimborazo, Imbabura							
Treatment parroquia (yes = 1)	0.030	-0.021	-0.023	-0.020*	-0.020	-0.019*	-0.017	-0.012	-0.010
		(0.013)	(0.017)	(0.011)	(0.014)	(0.011)	(0.013)	(0.010)	(0.012)
Observations		30	30	39	39	46	46	49	49
R^2		0.065	0.475	0.131	0.487	0.099	0.418	0.040	0.345

Notes: OLS estimates. The unit of observation is the rural *parroquia*. *h* is the percentage point distance from the eligibility cutoff 0.8905. Child mortality is the proportion of last-born children born between April 1999 and February 2000 who had died by November 2001 (2001 census). Pre-program controls include number of check-ups at health posts per 0 to 4 year-old during year 1999 (1999 health census), proportion of last-born children born between April 1988 and February 1989 who had died by November 1990 (1990 census), proportion of stunted children under 5 years old in 1990 (1990 census and 1988 nutrition survey), an indicator for the presence of a health center in the parroquia in 2000 (2000 health census), proportion of women aged 18 or older who completed primary schooling (2001 census), proportion of dwellings with access to piped water (2001 census), proportion of households with toilet (2001 census), average number of persons per room (2001 census), number of last-born children born between April 1999 and February 2000 living in the *parroquia* in 2001 (2001 census), number of last-born children born between April 1988 and February 1989 living in the *parroquia* in 1990 (1990 census). Comparison means are the estimated constant terms in *h* = 6 without controls. Robust standard errors in parentheses.

*, **, and *** indicate significance at 10%, 5%, and 1% levels, respectively.

Table 4: Impact on child mortality, quadratic specification

Dependent variable: Proportion of last-born children born between April 1999 and February 2000 who had died by November 2001

Neighborhood *h* (percentage points)		3		4		5		6	
Pre-program controls		N	Y	N	Y	N	Y	N	Y
	Comparison mean	Panel A: Bolívar, Chimborazo, Imbabura, Manabí							
Treatment parroquia (yes = 1)	0.027	0.002 (0.008)	-0.008 (0.011)	-0.006 (0.008)	-0.016** (0.008)	-0.014 (0.009)	-0.021** (0.009)	-0.018** (0.009)	-0.023*** (0.009)
F-test (quadratic terms) [p-value]		2.369 [0.106]	0.222 [0.802]	1.893 [0.161]	1.715 [0.192]	0.161 [0.852]	0.525 [0.594]	1.679 [0.194]	2.583* [0.084]
Observations		48	48	58	58	70	70	75	75
R^2		0.138	0.407	0.152	0.447	0.098	0.312	0.085	0.286
	Comparison mean	Panel B: Bolívar, Chimborazo, Imbabura							
Treatment parroquia (yes = 1)	0.032	-0.015 (0.009)	-0.032 (0.021)	-0.017 (0.011)	-0.028 (0.019)	-0.020 (0.014)	-0.026 (0.018)	-0.033** (0.015)	-0.035** (0.017)
F-test (quadratic terms) [p-value]		1.927 [0.167]	0.160 [0.853]	1.644 [0.209]	0.514 [0.605]	0.438 [0.648]	0.132 [0.877]	2.097 [0.135]	1.832 [0.176]
Observations		30	30	39	39	46	46	49	49
R^2		0.151	0.481	0.176	0.504	0.111	0.423	0.084	0.382

Notes: OLS estimates. The unit of observation is the rural *parroquia*. *h* is the percentage point distance from the eligibility cutoff 0.8905. Child mortality is the proportion of last-born children born between April 1999 and February 2000 who had died by November 2001 (2001 census). Pre-program controls include number of check-ups at health posts per 0 to 4 year-old during year 1999 (1999 health census), proportion of last-born children born between April 1988 and February 1989 who had died by November 1990 (1990 census), proportion of stunted children under 5 years old in 1990 (1990 census and 1988 nutrition survey), an indicator for the presence of a health center in the parroquia in 2000 (2000 health census), proportion of women aged 18 or older who completed primary schooling (2001 census), proportion of dwellings with access to piped water (2001 census), proportion of households with toilet (2001 census), average number of persons per room (2001 census), number of last-born children born between April 1999 and February 2000 living in the *parroquia* in 2001 (2001 census), number of last-born children born between April 1988 and February 1989 living in the *parroquia* in 1990 (1990 census). Comparison means are the estimated constant terms in *h* = 6 without controls. Robust standard errors in parentheses.

*, **, and *** indicate significance at 10%, 5%, and 1% levels, respectively.

Table 5: Impact on health check-ups, linear specification

Dependent variable: Number of check-ups at health posts per 0 to 4 year-old during year 2000

Neighborhood *h* (percentage points)		3		4		5		6	
Pre-program controls		N	Y	N	Y	N	Y	N	Y
	Comparison mean	Panel A: Bolívar, Chimborazo, Imbabura, Manabí							
Treatment parroquia (yes = 1)	0.750	0.172 (0.320)	0.238 (0.319)	0.073 (0.288)	0.218 (0.254)	0.164 (0.303)	0.236 (0.264)	0.138 (0.267)	0.199 (0.242)
Observations		48	48	58	58	70	70	75	75
R^2		0.015	0.579	0.078	0.607	0.019	0.579	0.015	0.569
	Comparison mean	Panel B: Bolívar, Chimborazo, Imbabura							
Treatment parroquia (yes = 1)	0.688	1.078** (0.513)	1.528** (0.620)	0.774** (0.366)	0.802** (0.376)	0.566 (0.351)	0.739** (0.318)	0.620* (0.339)	0.846** (0.317)
Observations		30	30	39	39	46	46	49	49
R^2		0.139	0.753	0.207	0.713	0.163	0.732	0.125	0.694

Notes: OLS estimates. The unit of observation is the rural *parroquia*. *h* is the percentage point distance from the eligibility cutoff 0.8905. The total number of check-ups of 0 to 4 year-olds is normalized by the estimated population of 0 to 4 year-olds in 2000 based on Flores (2001). Pre-program controls include number of check-ups at health posts per 0 to 4 year-old during year 1999 (1999 health census), proportion of last-born children born between April 1988 and February 1989 who had died by November 1990 (1990 census), proportion of stunted children under 5 years old in 1990 (1990 census and 1988 nutrition survey), an indicator for the presence of a health center in the *parroquia* in 2000 (2000 health census), proportion of women aged 18 or older who completed primary schooling (2001 census), proportion of dwellings with access to piped water (2001 census), proportion of households with toilet (2001 census), average number of persons per room (2001 census), number of last-born children born between April 1999 and February 2000 living in the *parroquia* in 2001 (2001 census), number of last-born children born between April 1988 and February 1989 living in the *parroquia* in 1990 (1990 census). Comparison means are the estimated constant terms in h = 6 without controls. Robust standard errors in parentheses.

*, **, and *** indicate significance at 10%, 5%, and 1% levels, respectively.

Table 6: Impact on health check-ups, quadratic specification

Dependent variable: Number of check-ups at health posts per 0 to 4 year-old during year 2000

Neighborhood *h* (percentage points)		3		4		5		6	
Pre-program controls		N	Y	N	Y	N	Y	N	Y
	Comparison mean	Panel A: Bolívar, Chimborazo, Imbabura, Manabí							
Treatment parroquia (yes = 1)	0.869	0.001 (0.436)	0.377 (0.411)	0.119 (0.373)	0.312 (0.313)	0.072 (0.356)	0.172 (0.256)	0.144 (0.333)	0.235 (0.268)
F-test (quadratic terms) [p-value]		1.159 [0.324]	0.116 [0.891]	1.461 [0.241]	0.311 [0.734]	0.211 [0.810]	0.065 [0.937]	0.666 [0.517]	0.861 [0.428]
Observations		48	48	58	58	70	70	75	75
R^2		0.045	0.581	0.110	0.612	0.024	0.580	0.022	0.574
	Comparison mean	Panel B: Bolívar, Chimborazo, Imbabura							
Treatment parroquia (yes = 1)	0.783	0.460 (0.339)	1.122 (0.640)	0.920** (0.439)	1.504** (0.663)	1.165** (0.495)	1.340** (0.557)	0.986* (0.499)	1.180** (0.467)
F-test (quadratic terms) [p-value]		1.949 [0.164]	0.519 [0.606]	0.569 [0.572]	1.252 [0.305]	1.069 [0.353]	0.995 [0.382]	1.284 [0.287]	3.275 [0.050]
Observations		30	30	39	39	46	46	49	49
R^2		0.196	0.770	0.220	0.741	0.188	0.747	0.159	0.728

Notes: OLS estimates. The unit of observation is the rural *parroquia*. *h* is the percentage point distance from the eligibility cutoff 0.8905. The total number of check-ups of 0 to 4 year-olds is normalized by the estimated population of 0 to 4 year-olds in 2000 based on Flores (2001). Pre-program controls include number of check-ups at health posts per 0 to 4 year-old during year 1999 (1999 health census), proportion of last-born children born between April 1988 and February 1989 who had died by November 1990 (1990 census), proportion of stunted children under 5 years old in 1990 (1990 census and 1988 nutrition survey), an indicator for the presence of a health center in the *parroquia* in 2000 (2000 health census), proportion of women aged 18 or older who completed primary schooling (2001 census), proportion of dwellings with access to piped water (2001 census), proportion of households with toilet (2001 census), average number of persons per room (2001 census), number of last-born children born between April 1999 and February 2000 living in the *parroquia* in 2001 (2001 census), number of last-born children born between April 1988 and February 1989 living in the *parroquia* in 1990 (1990 census). Comparison means are the estimated constant terms in *h* = 6 without controls. Robust standard errors in parentheses.

*, **, and *** indicate significance at 10%, 5%, and 1% levels, respectively.

Table 7: Impact on child mortality, non-exposed cohorts

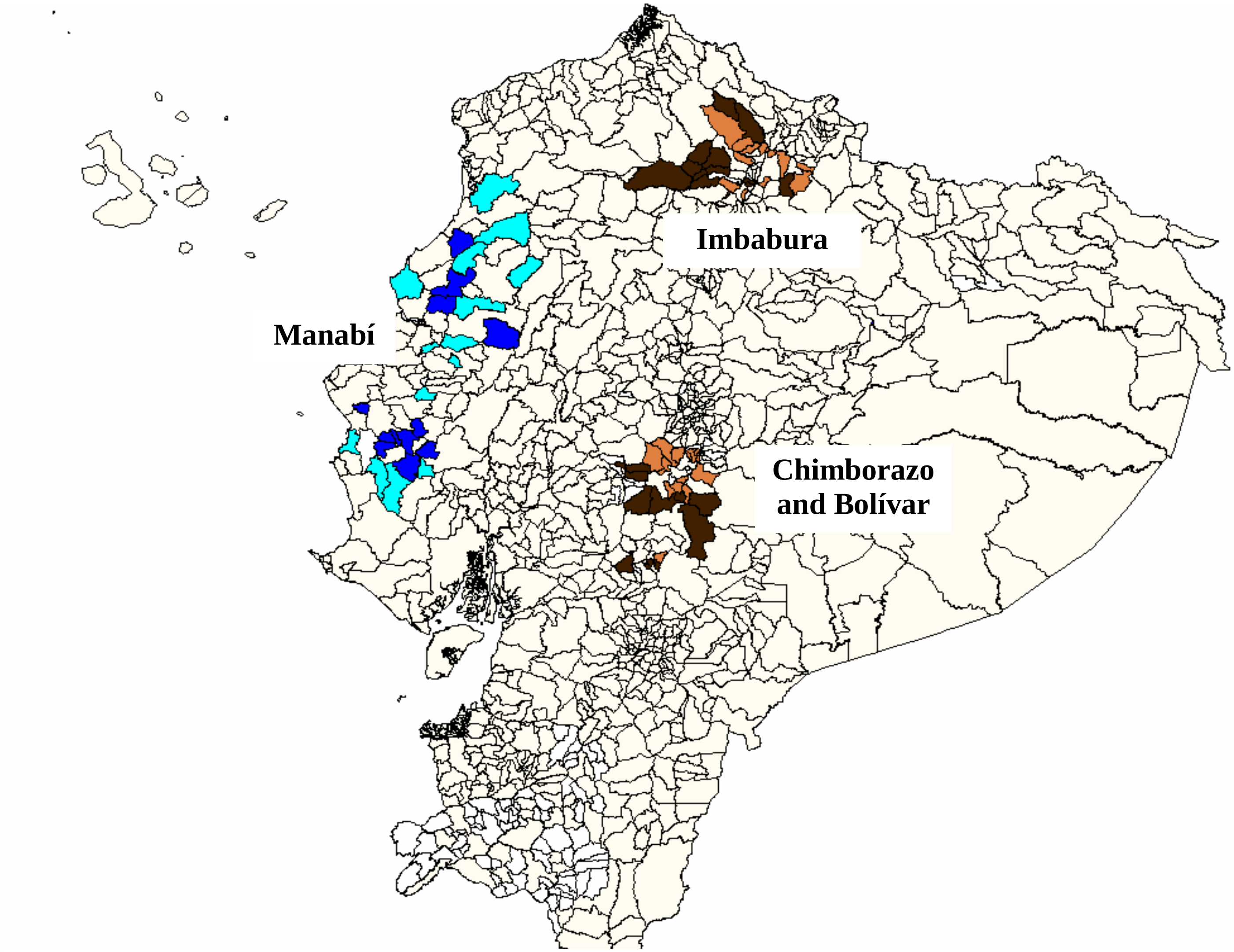
Dependent variable: Proportion of last-born children born between October 1997 and August 1998 who had died by November 2001

Neighborhood *h* (percentage points)		3		4		5		6	
Pre-program controls		N	Y	N	Y	N	Y	N	Y
	Comparison mean	Panel A: Bolívar, Chimborazo, Imbabura, Manabí							
Treatment parroquia (yes = 1)	0.021	0.011 (0.012)	0.007 (0.012)	-0.002 (0.010)	0.006 (0.011)	0.002 (0.010)	0.002 (0.010)	-0.004 (0.010)	-0.008 (0.012)
Observations		48	48	58	58	70	70	75	75
R^2		0.042	0.261	0.004	0.305	0.016	0.231	0.004	0.200
	Comparison mean	Panel B: Bolívar, Chimborazo, Imbabura							
Treatment parroquia (yes = 1)	0.025	0.014 (0.025)	0.036 (0.038)	-0.010 (0.017)	0.019 (0.022)	-0.003 (0.015)	0.006 (0.018)	-0.005 (0.014)	0.002 (0.016)
Observations		30	30	39	39	46	46	49	49
R^2		0.074	0.516	0.011	0.483	0.026	0.458	0.035	0.464

Notes: OLS estimates. The unit of observation is the rural *parroquia*. Child mortality is the proportion of last-born children born between October 1997 and August 1998 who had died by November 2001 (2001 census). Pre-program controls include number of check-ups at health posts per 0 to 4 year-old during year 1999 (1999 health census), proportion of last-born children born between October 1986 and August 1987 who had died by November 1990 (1990 census), proportion of stunted children under 5 years old in 1990 (1990 census and 1988 nutrition survey), an indicator for the presence of a health center in the *parroquia* in 2000 (2000 health census), proportion of women aged 18 or older who completed primary schooling (2001 census), proportion of dwellings with access to piped water (2001 census), proportion of households with toilet (2001 census), average number of persons per room (2001 census), number of last-born children born between October 1997 and August 1998 living in the *parroquia* in 2001 (2001 census), number of last-born children born between October 1986 and August 1987 living in the *parroquia* in 1990 (1990 census). Comparison means are the estimated constant terms in $h = 6$ without controls. Robust standard errors in parentheses.

*, **, and *** indicate significance at 10%, 5%, and 1% levels, respectively.

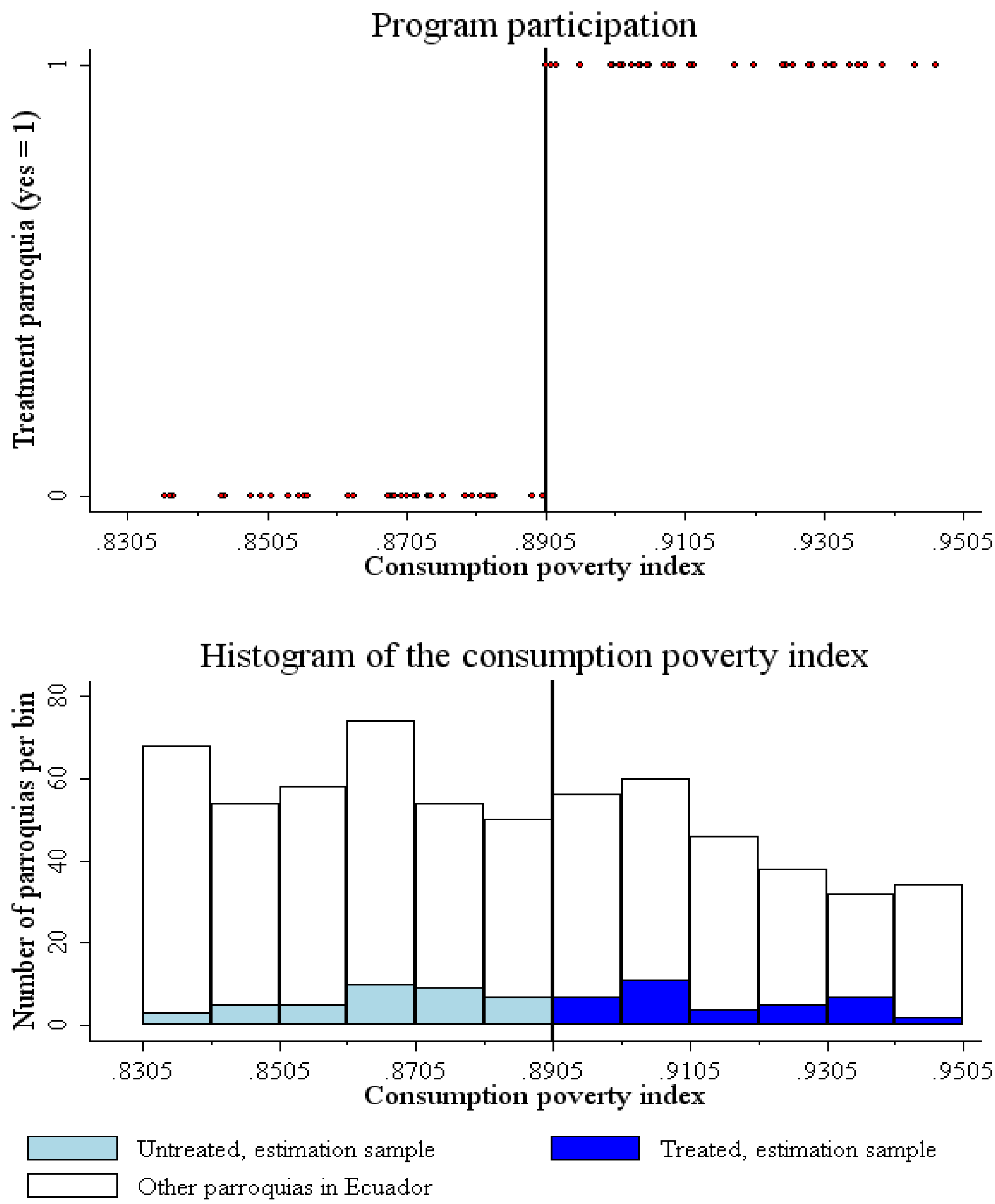
Figure 1: Treatment and comparison *parroquias* in the estimation sample



Source: Chauvin (2001).

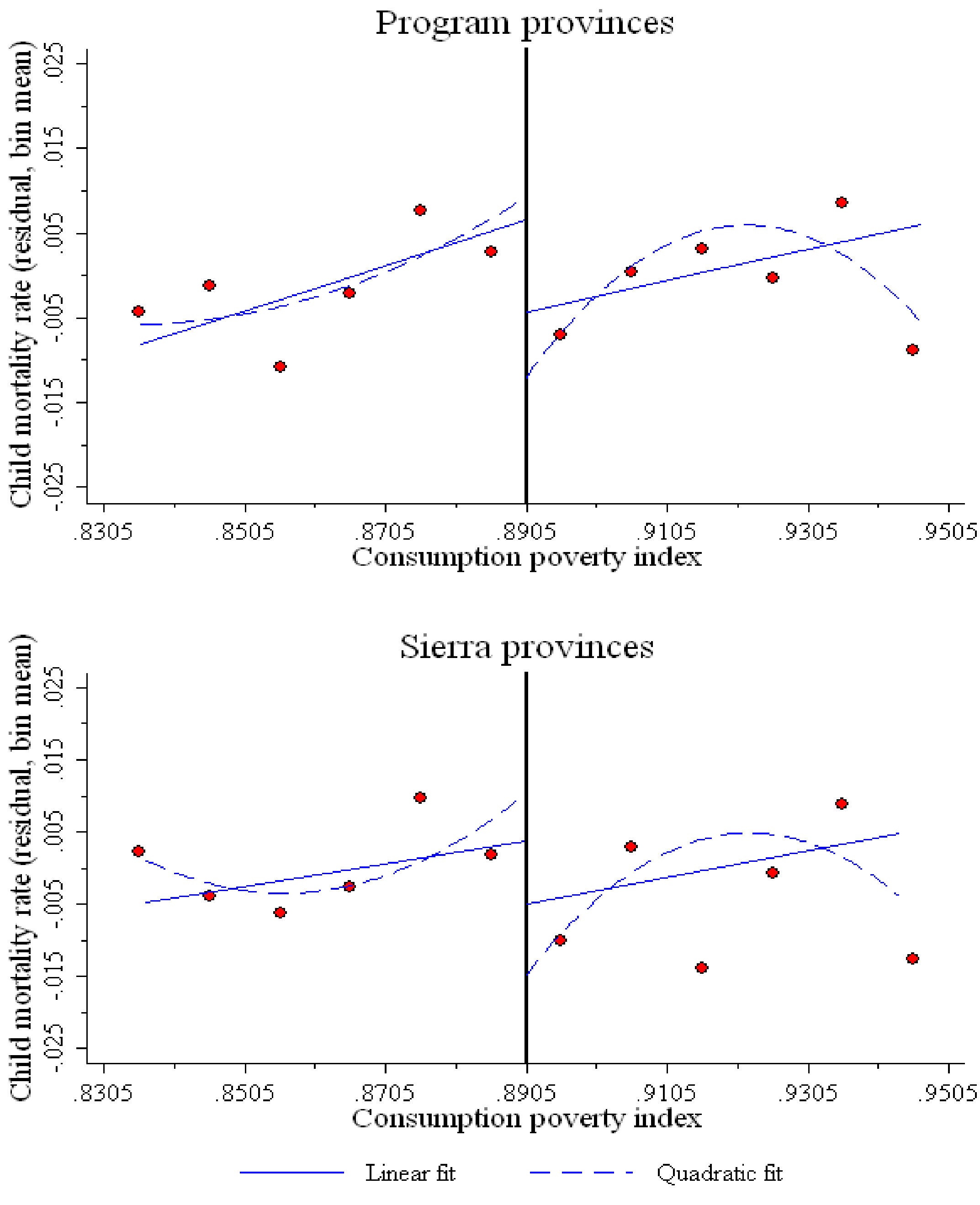
Notes: The estimation sample includes rural parroquias in the program provinces with complete observations and with a consumption poverty index within 6 percentage points distance to the eligibility cutoff of 89.05 percent. Dark-colored areas are parroquias above the cutoff, which actually received the PANN 2000. Light-colored areas are comparison parroquias below the cutoff. The Sierra provinces are Bolívar, Chimborazo, and Imbabura.

Figure 2: First stage and histogram of the consumption poverty index



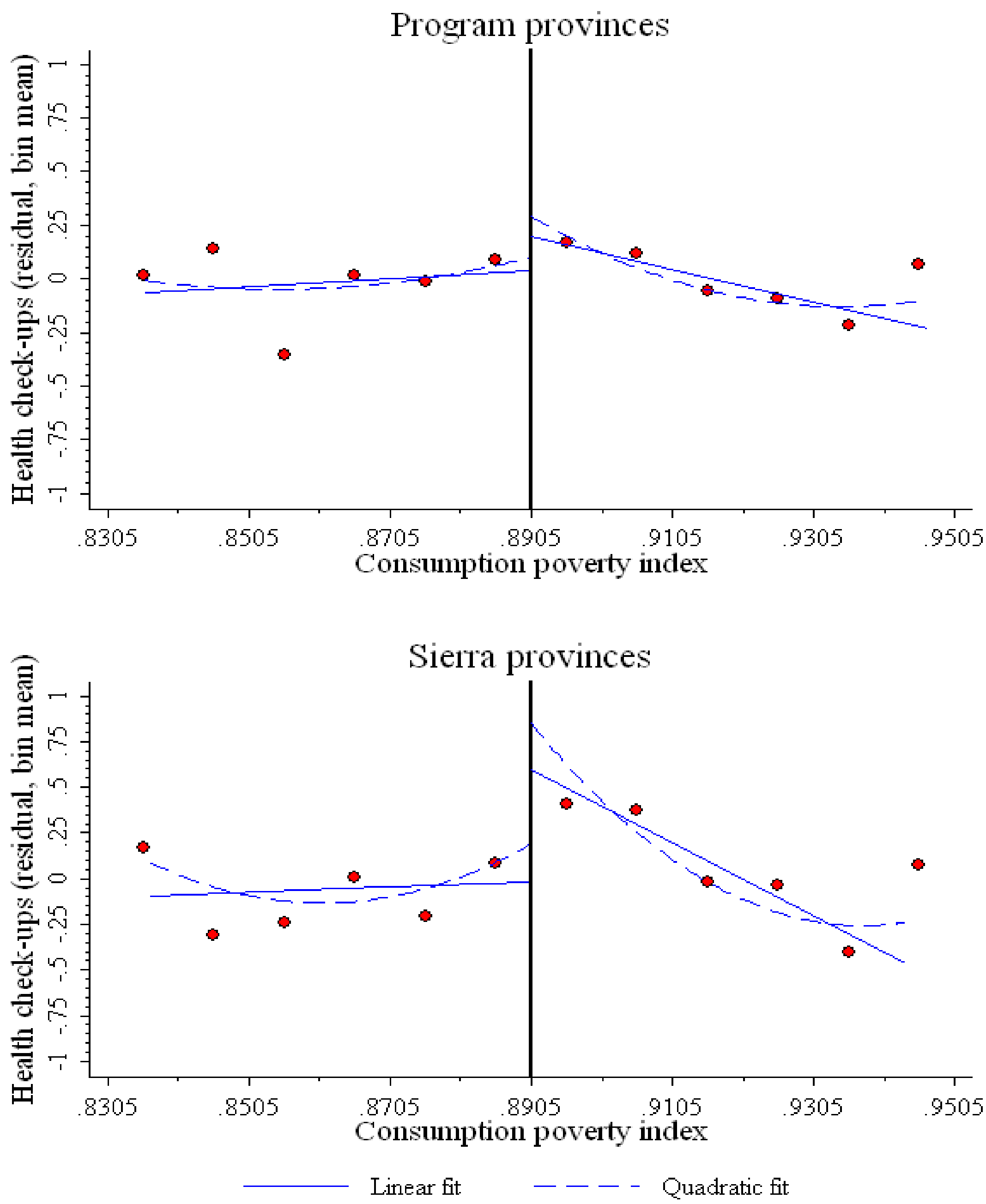
Notes: The unit of observation is the rural parroquia. The samples are restricted to parroquias within 6 percentage point distance to the cutoff. The estimation sample contains parroquias with complete observations in the provinces of Bolívar, Chimborazo, Imbabura, and Manabí.

Figure 3: Impact on child mortality



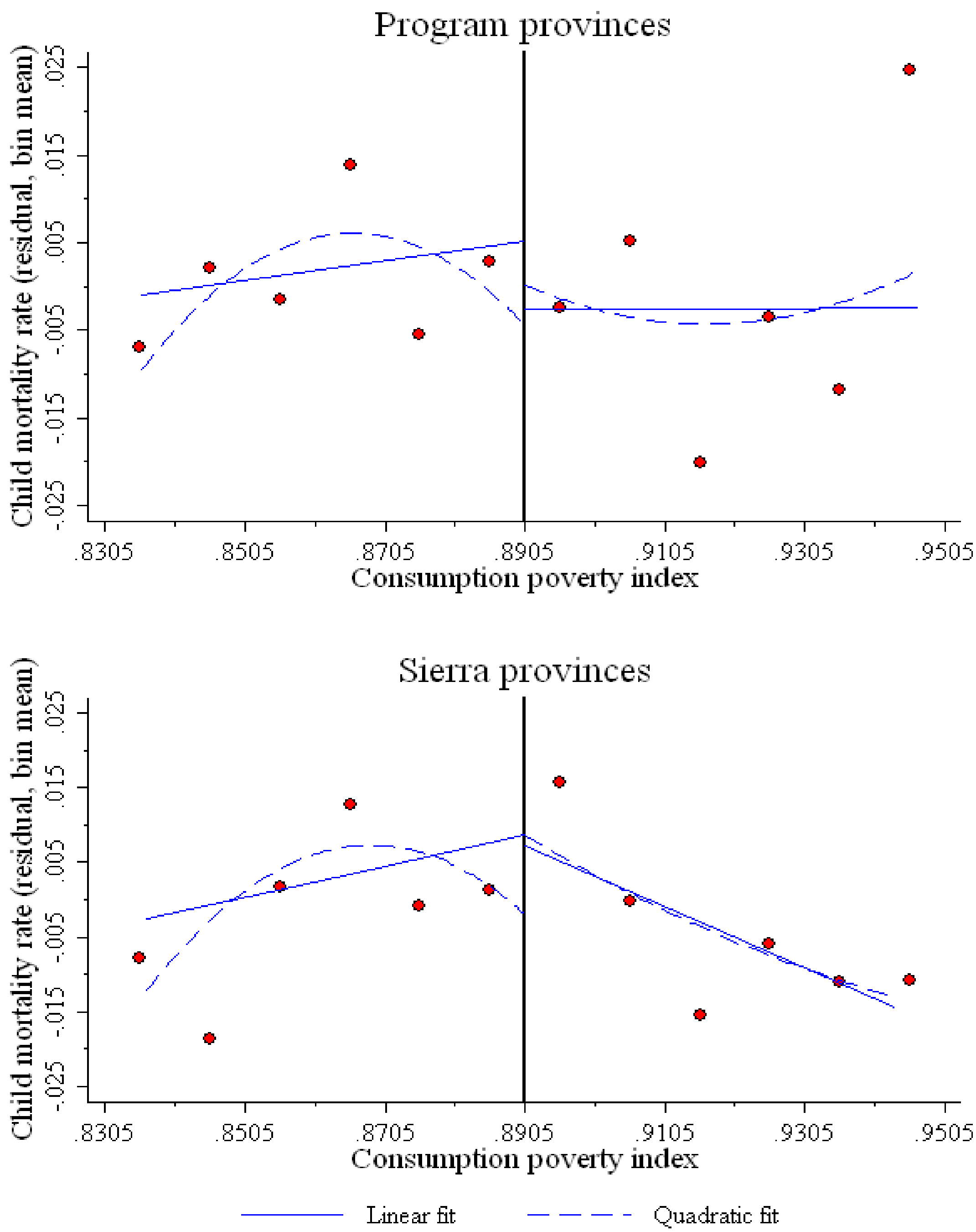
Notes: The unit of observation is the rural *parroquia*. The sample is restricted to *parroquias* within 6 percentage point distance to the cutoff. Residuals are computed from regressions with the control variables listed in Table 3. Program provinces are Bolívar, Chimborazo, Imbabura, and Manabí. Sierra provinces are Bolívar, Chimborazo, and Imbabura.

Figure 4: Impact on health check-ups



Notes: The unit of observation is the rural *parroquia*. The sample is restricted to *parroquias* within 6 percentage point distance to the cutoff. Residuals are computed from regressions with the control variables listed in Table 5. Program provinces are Bolívar, Chimborazo, Imbabura, and Manabí. Sierra provinces are Bolívar, Chimborazo, and Imbabura.

Figure 5: Impact on child mortality among non-exposed cohorts



Notes: The unit of observation is the rural *parroquia*. The sample is restricted to *parroquias* within 6 percentage point distance to the cutoff. Residuals are computed from regressions with the control variables listed in Table 7. Program provinces are Bolívar, Chimborazo, Imbabura, and Manabí. Sierra provinces are Bolívar, Chimborazo, and Imbabura.