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Saving Lives: Evidence from a Conditional Food Supplementation Program*

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Abstract

Many governments in developing countries implement programs that aim to address nutrional failures in early childhood, yet evidence on the effectiveness of these interventions is scant. This paper evaluates the impact of a conditional food supplementation program on child mortality 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 food, *Mi Papilla,* for children aged 6 to 24 months in exchange for routine health check-ups for the children. Our regression discontinuity design exploits the fact that at its inception, the *PANN* 2000 was running for about 8 months only in the poorest communities (*parroquias*) of certain provinces. Our main result is that the presence of the program reduced child mortality in cohorts with 8 months of differential exposure from a level of about 2.5 percent by 1 to 1.5 percentage points.

Keywords: early childhood nutrition, child mortality, food supplementation, regression discontinuity, Ecuador

JEL: I15, I18

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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).[2] For example, community-level efficacy trials have consistently shown that vitamin A supplementation reduces child mortality across many different settings (Beaton et al. 1993; Mayo-Wilson et al. 2011).[3] Efficacy trials also provide evidence that complementary feeding interventions can improve growth and micronutrient status and may also reduce morbidity (Dewey and Adu-Afarwuah 2008).[4] However, much less is known about the effectiveness of these interventions when conditions for both service delivery and recipient compliance are "routine", as in typical scaled-up programs, rather than "ideal".[5] Moreover, we know little about the impact of complementary feeding interventions on child mortality, probably because existing studies were not powered to detect small—yet practically important—reductions in mortality.[6]

This paper provides evidence on the effectiveness of a conditional food supplementation program in reducing child mortality 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 food, *Mi Papilla,* for children aged 6 to 24 months in exchange for routine health check-ups for the children.[7] We address concerns about endogenous program

[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]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).

[3]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).

[4]Positive and substantial education impacts of a protein- and energy-fortification intervention in Guatemala have been documented even three decades after it ended (Maluccio, Hoddinott, Behrman, Martorell, Quisumbing and Stein 2009).

[5]Recognizing the scant evidence on program effectiveness, many studies call for more research in this area. Dewey and Adu-Afarwuah (2008: 33) conclude that "The key challenge is how to implement high-quality programmes that are sustainable when delivered on a large scale." Regarding key evidence gaps, Bhutta et al. (2008: 434) 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) for example.

[6]For example, to have a 90% chance of detecting a mortality reduction from 3% to 2% at 5% significance would require a sample size of over 10'000. The vast majority of studies reviewed in Dewey and Adu-Afarwuah (2008) have sample sizes below 1'000.

[7]*Mi Papilla* is designed to provide about 50% of required caloric intake; 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

placement by exploiting that at its inception in August 2000, the *PANN* 2000 was running for about 8 months only in the poorest communities (*parroquias*)[8] of certain provinces, as measured 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. We exploit this variation to estimate the effect of the program on child mortality at the community level as measured in the 2001 census using (sharp) regression discontinuity (RD) analysis. We interpret our estimates as intent-to-treat because we do not observe *Mi Papilla* uptake at the individual level in the census data.

The internal validity of our results hinges on the identifying assumption that communities had (at most) only imprecise control over the value of their poverty index. As long as this control was imprecise at best, treatment assignment was randomized around the cutoff (Lee and Lemieux 2010). This identifying assumption is highly plausible in our context as the poverty index was created several years before the *PANN* 2000 was announced. We also test an empirical implication of this assumption, 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 statistical evidence of discontinuities in important determinants of child mortality (Charmarbagwala et al. 2004), 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 community level.[9] To our knowledge, there were no other programs at the time that used the same cutoff for targeting as the *PANN* 2000.

Our main result is that the presence of the program reduced child mortality in cohorts with 8 months of differential exposure from a level of about 2.5 percent by 1 to 1.5 percentage points. 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 re-

addition to other nutrients (Lutter et al. 2008).

[8]The median population of the *parroquias* in our estimation sample is 4'873. 80% of *parroquias* have between 1'679 and 9'683 inhabitants.

[9]Another key determinant of child mortality is income, which in our analysis is proxied by the poverty index and is thus held constant by design at the cutoff. Two remaining important determinants of child mortality are unobserved in our study: the presence of younger siblings in the household and whether the child is vaccinated against DPT3, measles or polio for example. The presence of younger siblings cannot bias our estimates because by necessity we focus on mortality of last-born children, as further explained below. Vaccination rates were above 90% in 2001 according to both WHO-UNICEF and official country estimates (WHO 2013).

ports[10]—and based on results from efficacy studies, showing that complementary feeding interventions can improve micronutrient status and may also reduce morbidity. Undernutrition is thought to account for more than 50% of deaths of children aged 6 to 59 months worldwide (Pelletier et al. 1995):[11] if the program improved the nutritional status in the target population sufficiently, one would expect child mortality in the age range 6 to 24 months to fall approximately by half.[12] Assuming an impact of -1 death per 100 children exposed to the *PANN* 2000, a coverage rate of 80%, an annual cost per beneficiary of US$ 30, and a potential program exposure of 8 months, the implied cost of a life saved amounts to about US$ $30 \times 80 \times 8/12 =$ US$ 1'600.[13]

We also use administrative data on the number of check-ups at local health posts to investigate potential mechanisms for the mortality reduction. While we find small and insignificant estimates on average, we do find evidence of an increase in health check-ups in treatment communities of the *Sierra* region. The magnitude of the discontinuity implies that mothers added about 0.66 visits per month in program communities. This finding implies that the mortality reduction we document might be due to improved detection and treatment of common childhood illnesses, such as diarrhoea and acute respiratory infection, in addition to a reduced case fatality rate due to improved nutrition. While the program design does not allow us to separate whether the food supplement or the health check-ups drive the mortality reduction, the design does enable us to rule out the influence of other common additional program components, such as nutrition information and education, which were phased in only at a later stage. Similarly, cash transfers were never part of the *PANN* 2000 even though the in-kind transfer might have freed-up some income to spend on other relevant inputs into child health such as medicines or fuel to boil water.[14]

[10]Because take-up or coverage was below 100%, the cohorts we consider had a *potential* differential exposure to the program of 8 months. We usually omit this qualification for expositional simplicity.

[11]The estimate for Ecuador in Pelletier et al. (1995) is 32%. However, in our sample of *parroquias* that are all relatively poor and mostly from the *Sierra* region, the percentage of deaths attributable to the potentiating effects of malnutrition are likely higher. 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.

[12]For example, suppose that mortality in the age range 0-5 months is 2%, that the program reduced mortality in the age range 6-24 months from 2% to 1%, and that check-ups further reduced mortality by 0.5%. Further, suppose that only 60% of true mortality is reported in the census. In the absence of the program, we would then observe a mortality rate of $4\% \times 60\% = 2.4\%$, and with the program of $2.5\% \times 60\% = 1.5\%$.

[13]This figure is consistent with estimated costs per death averted from vitamin A supplementation trials that range from 90 to 4'127 US$ (2005 prices), with a median of 409 US$ (Fiedler, Sanghvi, and Saunders 2008).

[14]For other common program components see the evaluations of community-based nutrition programs in Haiti (Ruel et al. 2008) or Senegal (Linnemayr and Alderman 2011), or evaluations of the conditional cash transfer program *PROGRESA,* such as Gertler (2004) or Rivera et al. (2004). 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.

We corroborate our results on child mortality reduction in a number of ways. First, we present results on mortality for cohorts that were already too old to be eligible for the program when it started in August 2000. As expected for a program targeted based on age, impact estimates for these cohorts are essentially zero and insignificant, even though the confidence intervals often do not rule out meaningful effects. Second, we estimate the impact on mortality for a broader set of cohorts that were differentially exposed to the program for a shorter period—at least one month 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. Third, we test whether there is a jump in mortality rates across communities at the program cutoff in provinces where the *PANN* 2000 had not been implemented yet and again find no statistical evidence of an effect. Fourth, we test for jumps in mortality rates at the program cutoff in 1990 (before the *PANN* 2000 implementation) and in 2010 (after the program had been scaled up to the entire country) and again find no statistical evidence of an effect.

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. At this later stage, the *PANN* 2000 also included nutrition information and education components, in addition to the food supplement and health check-ups. The results of the Lutter et al. study show that children in program communities consumed significantly more energy, protein, fat, iron, zinc, vitamin A, and calcium than children in neighboring comparison areas due to the fortified food supplement. There were also impacts on height and weight for relatively older children (12 to 14 months when the program began) and the prevalence of underweight was reduced by 65%. The study also finds that the odds of being anemic were 58% lower for *PANN* 2000 children.

While our own study is silent on nutritional mechanisms due to data limitations, the Lutter et al. (2008) study provides evidence on the entire causal pathway linking availability of the *PANN* 2000 in the community to *Mi Papilla* intake at home and to micronutrient status and child weight improvements. Since nutrition information and education components had been phased-in by the time of the Lutter et al. study, it is in principle possible that these components were driving their results. However, it seems unlikely that counseling alone was responsible for the nutritional improvements in Lutter et al. because their study finds no evidence that mothers' knowledge about appropriate age-specific feeding practices improved due to the program. Our finding on child mortality reduction during a period when nutrition information and education were not part of the *PANN* 2000 confirms the importance of food supplementation. Our study also reinforces the interval validity of the Lutter et al. study since the mortality reduction we find is consistent with their results on nutritional improvements. Taken together, the Lutter et al. study and ours indicate that food supplementation—rather than nutrition counseling—provided by the *PANN* 2000 led to nutritional improvements, which in turn contributed to reduce child mortality.

Evaluations of food or micronutrient supplementation programs rarely document impacts on child mortality.[15] We are aware of four other evaluations—broadly defined to include cash transfer programs with a nutrition component—that report child (or infant) mortality reductions. Two of these studies consider government-run vitamin A supplementation programs, one from Yemen (Banajeh 2003) and the other, which also included health worker visits, 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). Our own study confirms that conditional food supplementation programs can save lives using a comparatively weak identifying assumption with many testable implications (Lee and Lemieux 2010; Lee and DiNardo 2010).[16]

The 1 to 1.5 percentage point or 40 to 60 percent mortality reduction for 6 to 24 month-olds we document in our study is quantitatively in line with existing evidence.[17] The vitamin A supplementation coupled with community health worker visits analyzed in Thapa, Choe, and Retherford

[15] A large literature, recently reviewed in Ainsworth et al. (2010), examines impacts of 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).

[16] 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 and perform one falsification test based on children that were too young to be eligible for the vitamin A supplements. Muhilal et al. (1988) compare mortality rates across treatment and nearby (non-randomized) control villages. Barham (2011) exploits the phasing-in of *PROGRESA* across localities over time and provides a careful assessment of the common trends assumption for groups of municipalities that were phased into the program at different points in time.

[17] Technically, our mortality measure is for children up to 31 months. Although we do not know the age at death in the census data, we consider it unlikely that the program reduced mortality at age 0 to 5 months or much after the age of 24 months.

(2005) reduced the odds of dying at age 12 to 59 months by slightly more than half.[18] And the Muhilal et al. (1988) study found a relative mortality reduction for 12 to 60 month-olds of about 45% coming from vitamin A fortification alone (without food supplementation or health check-ups). For infants, below 12 months of age, the Muhilal et al. (1988) study found smaller and insignificant impacts. Barham's (2011) study also looks at infant mortality and finds impacts that are again smaller, about one-fourth to one-sixth the size of our impact estimates.[19]

Infant or child mortality reductions have also been achieved through health service delivery and infrastructure interventions and it is instructive to compare these to the food or micronutrient supplementation programs discussed above. One example is the integrated management of childhood illness (IMCI) strategy, consisting mainly of health-worker training, health-systems improvements, and family and community activities. Evaluations of IMCI have found a 13% reduction in child mortality, statistically significant in Tanzania (Schellenberg et al. 2004) but not in Bangladesh (Arifeen et al. 2009), and a significant 15% reduction in infant mortality in India (Bhandari et al. 2012). Other IMCI evaluations also found impacts on neonatal mortality. For health infrastructure interventions there is evidence from Bolivia that health facility upgrading and provision of supplies and equipment reduced child mortality by about 44 percent (Newmann et al. 2002). The same study also evaluates health impacts of water supply investments in Bolivia and estimates a child mortality reduction of about 42 percent.

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 communities. 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 of our findings.

[18] Banajeh (2003) looks at the case fatality rate among children suffering from severe dehydrating diarrhoea—rather than mortality in a given age range as in the other studies—which complicates comparability.

[19] Our intent-to-treat estimates correspond to a take-up rate of approximately 80%. The point estimate in Table 3, Panel A, column 1 in Barham (2011) is -3.1 deaths per thousand life births or -0.31% for an increase in the proportion of beneficiary households from 0 to 100%. For a municipality with 80% treated households we would expect an effect of $-0.31 \times 80\% = 0.248\%$.

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.[20] 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 month-olds and thus address undernutrition in early childhood.[21] The objectives of the program were to reduce child growth retardation, undernutrition, and micronutrient deficiencies (*MSP* 2000).[22] The four main components of the program as initially planned were: 1) to inform mothers about the importance of good nutrition during early childhood using flip chart presentations and other materials; 2) to train health workers so they could advise mothers on appropriate age-specific child feeding practices; 3) the distribution of the free micronutrient-fortified food, *Mi Papilla; 4)* in exchange for health check-ups for the children. In its initial stage from August 2000 through the end of March 2001, the information and education components 1) and 2) were still under preparation, leaving provision of the fortified food combined with health check-ups as the only two active components during this period (Chauvin 2001).

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

[21] 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.

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

The food supplement *Mi Papilla* is designed to provide about 50% of required caloric intake; 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), corresponding to about 0.4% of average household income.[23] 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 community.

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 *parroquias* have one health subcenter or post; those without any are included in the catchment area of the health subcenter/post in a neighboring community.[24] 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

[23] According to the World Development Indicators, GDP per capita for Ecuador in 2001 was US$ 1’693, while according to the 2001 census, the average household size was about 4.2.

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

promoted the initiative through the provincial directorates among health workers from the *parroquias*, who typically attended information and training sessions at the canton or province headquarters. The *parroquias* 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 communities 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. While it is possible that some children from comparison communities got *Mi Papilla* from health posts in neighboring treatment communities, such spill-overs would bias our estimates towards zero.

2.4 Targeting of the *PANN* 2000

The community-level consumption poverty index used for program assignment was estimated by ODEPLAN (1999) and is publicly available in the *Sistema Integrado de Indicadores Sociales de Ecuador* (*SIISE*, 2010). The index measures for each community 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.). The relationship between household per capita consumption and household characteristics is estimated based on the 1995 Living Conditions Survey, *Encuesta de Condiciones de Vida* (*ECV*), which contains a detailed questionnaire on household expenditures but is not representative at the *parroquia* level.[25] Based on common household characteristics in the *ECV* and the National Census, *Censo Naciónal de Población y de Vivienda,* household per capita consumption is projected on the households registered in the 1990 *Censo*. The assignment variable was thus created several years before the *PANN* 2000 was announced.[26]

To determine participation in the initial program stage, *parroquias* in the seven poorest provinces (Bolívar, Chimborazo, Cotopaxi, Imbabura, Loja, Manabí, and Orellana) were ranked by the consumption poverty index and pre-selected if it exceeded 90%, yielding 145 *parroquias*. Eventually,

[25] The *ECV* investigates expenditures on about 100 food items and 108 non-food items. Recall periods vary from 1 week to 1 year depending on the size of the expenditure.

[26] Although there is certainly measurement error in the poverty index, this should be without consequence for our study as long as the mismeasurement does not change discontinuously at the cutoff. Random measurement error will bias estimation of the relationship between the poverty index and mortality towards zero.

urban *parroquias* would be removed from this list to be targeted differently, with their places substituted by the next poorest rural units.[27] This left the final cutoff for rural *parroquias* at 89.05%.

The *PANN* 2000 progress reports in Chauvin (2001) show that in the provinces of Bolívar, Chimborazo, Imbabura, and Manabí, the initially selected rural *parroquias* coincided sharply with those which report numbers of actually attended children. 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.

For most of our analysis 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. Our reason for doing so is that we need to normalize our health check-ups outcome by the number of kids of target age, and the officially estimated number of 6-24 month-olds in 2000 is only available for the poorest 40% of *parroquias*—corresponding to those with a poverty index above 82.27%. For our child mortality outcome we show results also for broader samples, including *parroquias* with a poverty index within up to ten percentage points distance from the cutoff.

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.[28] This allows us to identify the impact of the program 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, communities to the left of the treatment-determining consumption poverty cutoff

[27] When the *PANN* 2000 started, rural Ecuador was divided into 22 provinces, 176 cantons, and 776 *parroquia*s.
[28] By 2003 the program was available in nearly all *parroquias* of the country (Lutter et al., 2007).

should provide valid counterfactual outcomes for communities on the right-hand side of the cutoff (where the micronutrient-fortified food, *Mi Papilla,* was being offered in exchange for health check-ups for the children). 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:[29]

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

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 community 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.[30] Moreover, in Section 6 below, we test an empirical implication

[29]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.

[30]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.

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 for cohorts that were already too old to be included when the program started, as well as for our main cohorts but before implementation of the *PANN* 2000 or after its scaling-up to the entire country or in provinces where the program had not been implemented yet as of August 2000. As expected for a program targeted based on age, province, and *parroquia*-level poverty, impact estimates from these falsification tests are essentially zero and insignificant even though the confidence intervals often do not rule out meaningful effects.

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 therefore aggregated at the *parroquia*-level based on the late-1999 *parroquia* borders. We first add the November 1990 and November 2001 National Censuses, as well as the year 1999, 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 *parroquias* in the country and serve as sources for outcomes and control variables used in our analysis. We verified in the program documentation that the *PANN* 2000 did not contain any performance awards or resource assignment mechanisms that would have created incentives for overstating the number of check-ups in 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 Child mortality

Given the target age of 6 to (under) 24 months, and an initial program stage that lasted from August 2000 until March 2001 before the *PANN* 2000 was scaled up, potential differential exposure to the program varied by birth month as illustrated in Figure 3.[31] Children born in August 1998 or earlier were never eligible for the program since they were already 24 months or older when the *PANN* 2000 began in August 2000. Children born between September 1998 and April 1999 had 1 to 8 potential months of exposure in early treatment *parroquias*. Although the May 1999 cohort had 9 potential months of exposure in treatment *parroquias*, this cohort had a *differential* exposure relative to comparison *parroquias* of only 8 months because the program was scaled up in April 2001 when the comparison group cohort was 23 months old and thus eligible for the program for one month. Starting with the March 2000 birth cohort, differential exposure in early treatment *parroquias* was successively reduced to zero because these cohorts were too young to be included in the initial stage of the program.

Given this potential exposure pattern, we define our maximum differential exposure cohorts—corresponding to 8 potential months of differential exposure to the program—as those born from April 1999 (children who only fell out of target age when the *PANN* 2000 was extended to comparison *parroquias*) to February 2000 (those who were just old enough to receive program benefits when the program started).

We use census data—rather than vital statistics—to measure child mortality. We prefer the census data over administrative records because the undercount is likely less severe particularly for the rural *parroquias* considered in our study.[32] 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 a given cohort who had died until census day (November 25 2001). For the maximum differential exposure cohorts the mortality rate is measured as follows:

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

[31] We refer to potential exposure because coverage or take-up of the program was below 100 percent.

[32] For example, in 2006, only 56% of infant deaths were registered in Ecuador according to the World Bank (2012). Comparing the death counts from the census to those from the vital statistics in our sample, we estimate that about 67% of deaths went unreported in the official statistics.

This child mortality rate corresponds to 21 to 31 month-olds and captures about two thirds of all children in the program cohorts.[33] 75% of *parroquias* in our sample had 0 to 2 deaths in the April 1999 to February 2000 birth cohorts by November 2001 and the maximum number of deaths was 9. Because our measure of mortality is driven by the target age of the *PANN* 2000 and the length of the initial stage of the program before it was scaled up it is admittedly unconventional. Unfortunately we were unable to find any studies comparing the validity and reliability of the 0 to 21-31 month mortality rate that we use here to more conventional measures.

As noted above, children born in August 1998 or earlier, were already 24 months or older at program inception in August 2000, and likely never received *Mi Papilla*. For one of our falsification tests we therefore construct the mortality rate of slightly older cohorts born between October 1997 and August 1998 (containing the same number of birth months as our maximum differential exposure cohorts):

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

As an additional falsification exercise, we also construct the mortality rate for non-exposed cohorts born between April 1997 and February 1998, thus holding seasonal factors—relative to our maximum differential exposure cohorts—constant. 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.

4.2 Fertility

One concern with our focus on last-born children is that it might generate a purely mechanical mortality reduction if the *PANN* 2000 made it more likely that a death in our maximum differential exposure cohorts was "replaced" with another birth. To address this issue we investigate whether the program indeed had an impact on fertility. Since the program started in August 2000 we look at the number of live-births in the period April 2001 to November 2001 to women aged 15 to 44

[33] The total child population in our 11-month cohort cannot be calculated from the 2001 census due to missing birth dates for children other than the last-born. However, Flores (2001) provides official estimates for the population of 6 to 24 month-olds (an 18-month cohort) developed specifically 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.

years on census day (November 25 2001). Specifically, fertility is measured as follows:

$$\frac{\text{Number of live-births between April and November 2001}}{\text{Number of women aged 15-44 years on November 25 2001}}$$

Because the program might have been anticipated we also look at live-births from September 2000 to November 2001.

4.3 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:

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

The official number of 6-24 month-olds in 2000 is only an estimate because direct measures are not available. Some additional measurement error may arise from the fact that not all children visit the health facility in their own *parroquia*. While our normalized health check-ups are thus certainly measured with error, this should not bias our impact estimates as long as the mismeasurement does not change discontinuously at the cutoff. Although random measurement error in the dependent variable will inflate standard errors, there is nothing we can do about it.

4.4 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 exposed cohorts, or were not available at the level of *parroquias* or health centers, such as those from 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.[34]

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 *parroquias*. 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.[35]

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 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.[36] Descriptive statistics of pre-treatment variables (Panel A) and outcomes (Panel B) are given in Table 1.

[34]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).

[35]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.

[36]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 anthropometric data for 1.57/120 $<$ 2% of the children in the program cohort.

5 Estimation approach

Following Hahn, Todd, and Van der Klaauw (2001), Imbens and Lemieux (2008) and Lee 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 *parroquias* 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, X the *parroquia* consumption poverty index, c the cutoff 89.05%, $\mathbf{W}$ 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:

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

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. For mortality we also show estimates within 7, 8, 9, and 10 percentage points of the cutoff using quadratic specifications.[37]

[37] We have also experimented with procedures for choosing the "optimal" bandwidth for child mortality, our main outcome variable. The cross-validation criterion (Ludwig and Miller 2007, Imbens and Lemieux 2008, Lee and Lemieux 2010) is essentially flat over the range of 2 to 10 percentage points distance from the cutoff, thus providing little guidance regarding bandwidth choice. If anything, the cross-validation function is lower for smaller bandwidths up to about 5 percentage points compared to larger bandwidths (results available on request). The Imbens-Kalyanaraman (2012) "optimal" bandwidth is 8.07 percentage points.

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).[38]

In Table 2, we estimate equation (2) for a host of pre-treatment outcomes and other covariates. The results show that there is no statistical evidence of discontinuities in any of these variables. At the bottom of Table 2 we also show F-test results for successively larger neighborhoods—corresponding to our main estimation samples—which fail to reject the joint null hypotheses of no discontinuity in any pre-treatment covariates at conventional levels of significance, except for one bandwidth choice.[39]

Nonetheless, some of the standardized discontinuity estimates in Table 2 are quite large. This is the case for the proportion of dwellings with access to piped water or the number of last-born children born April 1999 to February 2000, for example. In Section 7 below we show that our impact estimates are robust to the inclusion of these covariates, including the pre-treatment mortality and check-up outcomes shown in Table 2, thus providing additional evidence regarding the internal validity of the design.

7 Estimation results

7.1 Impact on child mortality, maximum differential exposure cohorts

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 Panel A are negative, with 6 out of 8 estimates falling in the range from -1 to -1.5 percentage points. The estimates are essentially unchanged when pre-treatment controls are included and are statistically significant (usually at 10% and sometimes at 5%) even in small neighborhoods of 4 to 5 percentage

[38]The discontinuity estimate and (standard error) in our estimation sample are 0.34 (0.80).

[39]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).

points around the cutoff. The estimates that are not significant in Table 3 Panel A are in the widest, 6 percentage point bandwidth, which likely reflects a specification error: with the quadratic specification in Table 5 Panel A, these estimates are highly significant.

In the *Sierra* sample (Table 3, Panel B), where child mortality tends to be higher in the absence of the program (3% vs. 2.5%), 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.[40]

Table 4 gives Poisson regression estimates of the discontinuity as well as the implied incidence rate ratios. The magnitude of the estimated mortality reduction ranges mostly between 40 to 60% for the full sample, very similar to the relative mortality risk reduction we estimate in Table 3. With both OLS and Poisson regression, estimates are larger (in absolute value) in the Sierra region. Statistical precision is considerably enhanced with the Poisson estimates, especially for the Sierra sample.[41]

Tables 5 and 6 show that the above results are robust to a quadratic specification of the poverty index for $h = 3, 4, 5, 6$ percentage points and $h = 7, 8, 9, 10$ percentage points, respectively. In Table 5, impact estimates are consistently negative but they tend to to be more variable compared to the local linear estimates in Table 3, while in Table 6 impact estimates are of similar magnitude, with 7 out of 8 falling in the -1 to -1.5 percentage point range. Standard errors are also similar across bandwidths, despite the larger sample sizes in Table 6, presumably because the fit is systematically worse with the larger bandwidths. Looking at panels A in Tables 3 and 6 together, there is a total of 16 estimates, 10 of which are significant at 10% and 3 at 5% while random chance would predict at most 2 significant at 10% and 1 at 5%.

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 see whether linearity of the polynomial in the poverty index can be rejected. As shown in Tables 5 and 6, except for the specification in the widest bandwidths, there is virtually no statistical evidence against the null hypothesis of a linear model, which corroborates

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

[41]Quadratic specifications yield similar results and are available on request.

our focus on the linear estimates (and standard errors) in Table 3. We conclude from these results that the presence of the *PANN* 2000 program reduced child mortality in cohorts with 8 months of differential exposure from a level of about 2.5 percent by 1 to 1.5 percentage points.

Figure 4 presents graphical evidence of the discontinuity in child mortality for $h = 6$ and Figure 1 in the online Appendix for $h = 10$. Each dot represents the average residual from a regression of child mortality at the *parroquia* level on the list of control variables from Tables 3 and 6. 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 the vertical black line represents the sample mean of partialled-out child mortality for all *parroquias* within one percentage point to the left of the 89.05% cutoff. Each graph also shows the fitted lines from our preferred linear specification with $h = 5$ and a quadratic specification using the largest neighborhood, $h = 6$ in Figure 4 and $h = 10$ in Figure 1 in the online Appendix, respectively.

Figure 4 shows a reduction of about 1 percentage point in child mortality at the cutoff and it additionally shows that the drop 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. Figure 1 in the online Appendix shows that there is a nonlinearity or discontinuity in child mortality to the left of the cutoff starting at around 83.05%. Since 82.27% marks the cutoff for the 40% poorest *parroquias,* it is possible that this mortality pattern reflects the effect of other programs that were targeted based on the poverty index. In any case, there is a jump at 89.05%, exactly where we would expect to see one based on the research design. Overall, we conclude that there is strongly suggestive—even if not fully conclusive—graphical evidence of a drop in child mortality at the cutoff.

7.2 Impact on fertility

Tables 1 and 2 in the online Appendix show impacts on fertility over the period April to November 2001 and September 2000 to November 2001, respectively. Estimates are almost evenly split into positive and negative and very far from statistical significance throughout. The 95% confidence interval based on the largest standard error (0.015) in Panel A, Table 1 is about [-0.03, 0.03]. So fertility increased by at most 3 children per 100 women. Taking into account that some of these children would have been first-borns and therefore would not have led to an undercount of deaths of older siblings and that it is unlikely that the "replacement" was one to one, it seems unlikely that fertility had a substantial impact on measured mortality in our maximum differential exposure cohorts.

7.3 Impact on health check-ups

Table 7 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 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 discussed above, there is considerable regional heterogeneity in the impact estimates on check-ups. Panel B of Table 7 restricts the sample to the three program provinces from the *Sierra* region, where check-ups were lower to start with (about 0.63 check-ups per child compared to 0.83 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*.[42]

Table 7.1 in the online Appendix 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 5 presents the discontinuity in health check-ups graphically. Figure 5 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

[42] Jump in visits per 0-4 year-old during 2000 from Table 6 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.

additionally shows that the discontinuity is visually robust irrespective of the width of the neighborhood examined.

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

Table 8 presents impact estimates for child mortality of cohorts that were too old to be exposed to the program, given the target age of 6 to 24 months. As expected, estimates for these cohorts are essentially zero and nowhere near statistical significance.[43] Still, the confidence intervals are quite large and often consistent with an effect size that is similar to that found for exposed cohorts above. Figure 6 confirms the zero effect visually, showing no evidence of a discontinuity in mortality rates at the cutoff. As an additional robustness check, we also performed the same falsification exercise with non-exposed cohorts born between April 1997 and February 1998, thus holding seasonal factors constant, again with essentially the same result (Table 8.1 and Figure 2 in the online Appendix).

We also estimate the impact on mortality for a broader set of cohorts that were differentially exposed to the program for a shorter period—at least one month rather than a full 8 months as in our main specification—and find effects that are smaller in magnitude but still consistently negative (Table 8.2 and Figure 3 in the online Appendix).

7.5 Impact on child mortality, non-program provinces

Another natural falsification exercise is to test whether there is a jump in mortality rates at the program cutoff for our maximum differential exposure cohorts but in provinces where the *PANN* 2000 was implemented only from April 2001 onwards. We restrict the sample to *parroquias* from *Sierra* or coastal provinces—excluding provinces in the Amazon region of the country—because the four initial *PANN* 2000 provinces were in the *Sierra* or on the coast.[44] Table 9 presents impact estimates using linear specifications of the poverty index.[45] As expected, estimates for these

[43] For these non-exposed cohorts, mortality in comparison communities is lower than for exposed cohorts despite observing them at up to 49 months of age versus up to 31 months. A potential explanation for this pattern comes from selection effects: if having a younger sibling worsens survival prospects of the older sibling we would expect lower mortality in older cohorts once we only look at last-borns. The meta-analysis in Charmarbagwala et al. (2004) shows that short subsequent birth spacing is associated with higher mortality of the older child. De Haan, Plug, and Rosero (2013) provide evidence of birth order effects on human capital development in Ecuador that is also consistent with this explanation.

[44] Coastal provinces are El Oro, Esmeraldas, Guayas, and Los Ríos. Sierra provinces are Azuay, Carchi, Cañar, Pichincha, and Tungurahua.

[45] Quadratic specifications yield similar results and are available on request.

cohorts are essentially zero and nowhere near statistical significance. Again, however, the confidence intervals are often consistent with an effect size that is similar to that found for exposed cohorts above. Figure 7 confirms the zero effect visually, showing no evidence of a discontinuity in mortality rates at the cutoff.

7.6 Impact on child mortality, before the *PANN* 2000 and after scaling it up

As a final falsification test, we estimate jumps in mortality rates for our maximum differential exposure cohorts at the program cutoff in 1990 (before the *PANN* 2000 implementation) and in 2010 (after the program had been scaled up to the entire country). Tables 3 and 4 in the online Appendix present impact estimates using linear specifications of the poverty index for 1990 and 2010, respectively.[46] Estimates for these cohorts are typically half or one-third the size of the estimates in 2001 and nowhere near statistical significance. Still, the confidence intervals are again often consistent with our point estimates for 2001. Figures 4 and 5 in the online Appendix confirm these results visually, showing no evidence of a discontinuity in mortality rates at the cutoff, except perhaps for the Sierra region in 1990.

8 Conclusion

This paper provides evidence on the effectiveness of a conditional food supplementation program in reducing child mortality in Ecuador. Our main result is that the presence of the *PANN* 2000 reduced mortality in cohorts with 8 months of differential exposure from a level of about 2.5 percent by 1 to 1.5 percentage points. This relative mortality reduction of 40 to 60 percent for 6 to 24 month-olds is quantitatively in line with existing evidence from vitamin A supplementation programs. 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. While we cannot disentangle whether the food supplement or the health check-ups drive the mortality reduction, the program design enables us to rule out the influence of other common additional program components, such as nutrition information and education (which were phased in only at a later stage) or cash transfers (which were never part of the *PANN* 2000).

[46]Quadratic specifications yield similar results and are available on request.

Although 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 relevant birth 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 literature—relative to quasi-experimental work—and given the complementarities between these research designs, we feel that our approach is still underexploited.

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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 non-missing values for all variables in Panel A. 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	-0.077	-0.132	-0.110
	(0.288)	(0.229)	(0.222)	(0.200)
	[-0.060]	[-0.132]	[-0.226]	[-0.188]
Proportion of last-born children born April 1988 to February 1989 who died by November 1990 (1990 census)	-0.008	-0.005	-0.004	-0.003
	(0.011)	(0.010)	(0.009)	(0.008)
	[-0.364]	[-0.227]	[-0.182]	[-0.136]
Proportion of stunted children under 5 years old in 1990 (1990 census and 1988 nutrition survey)	0.003	-0.028	-0.057	-0.041
	(0.070)	(0.056)	(0.052)	(0.050)
	[0.028]	[-0.259]	[-0.528]	[-0.379]
Presence of a health center in the parroquia in 2000 (yes = 1) (2000 health census)	-0.158	-0.057	-0.004	0.028
	(0.104)	(0.098)	(0.048)	(0.038)
	[-0.699]	[-0.252]	[-0.018]	[0.124]
Proportion of women aged 18 or older who completed primary schooling (2001 census)	-0.024	0.014	-0.016	-0.022
	(0.065)	(0.057)	(0.048)	(0.045)
	[-0.224]	[0.131]	[-0.150]	[-0.206]
Proportion of dwellings with access to piped water (2001 census)	-0.024	-0.140	-0.249*	-0.185
	(0.176)	(0.137)	(0.130)	(0.125)
	[-0.084]	[-0.420]	[-0.870]	[-0.647]
Proportion of households with toilet (2001 census)	-0.061	0.022	0.022	0.008
	(0.069)	(0.063)	(0.058)	(0.054)
	[-0.442]	[0.159]	[0.159]	[0.058]
Average number of persons per room (2001 census)	-0.050	-0.114	0.064	-0.002
	(0.206)	(0.174)	(0.163)	(0.144)
	[-0.151]	[-0.344]	[0.193]	[-0.006]
Number of last-born children born April 1999 to February 2000 living in the parroquia (2001 census)	-41.018	-39.401	-17.682	-18.448
	(28.568)	(27.133)	(22.330)	(20.683)
	[-0.745]	[-0.716]	[-0.322]	[-0.334]
Number of last-born children born April 1988 to February 1989 living in the parroquia (1990 census)	-13.336	-11.801	6.748	8.315
	(30.962)	(25.923)	(21.954)	(20.254)
	[-0.252]	[-0.229]	[0.130]	[0.160]
Estimated population of 6-24 month-olds in 2000 (Flores, 2001)	-87.466	-71.257	-16.808	-7.264
	(65.656)	(57.731)	(47.385)	(45.250)
	[-0.702]	[-0.573]	[-0.135]	[-0.059]
F-statistic	2.31	1.05	1.11	0.92
[p-value]	[0.02]	[0.41]	[0.36]	[0.52]

Notes: OLS discontinuity 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. Standardized discontinuity estimates (estimate divided by sample standard deviation from Table 1) in brackets.

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

Table 3: Impact on child mortality, maximum differential exposure cohorts, 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* (0.009)	-0.015* (0.008)	-0.015* (0.008)	-0.018** (0.008)	-0.012* (0.007)	-0.015** (0.007)	-0.007 (0.007)	-0.011 (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)	0.029	-0.021 (0.013)	-0.023 (0.017)	-0.020* (0.011)	-0.020 (0.014)	-0.019* (0.011)	-0.017 (0.013)	-0.012 (0.010)	-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* = 3 without controls. Robust standard errors in parentheses.

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

Table 4: Impact on child mortality, maximum differential exposure cohorts, Poisson regression

Dependent variable: Number 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
Panel A: Bolívar, Chimborazo, Imbabura, Manabí								
Treatment parroquia (yes = 1)	-1.009	-0.774**	-1.125**	-1.048***	-0.594	-0.732**	-0.495	-0.639**
	(0.680)	(0.379)	(0.512)	(0.385)	(0.433)	(0.325)	(0.397)	(0.301)
Incidence rate ratio	0.364	0.461	0.325	0.351	0.552	0.481	0.610	0.528
Observations	48	48	58	58	70	70	75	75
Pseudo R^2	0.027	0.277	0.066	0.345	0.022	0.299	0.023	0.289
Panel B: Bolívar, Chimborazo, Imbabura								
Treatment parroquia (yes = 1)	-2.115**	-1.778**	-2.069***	-1.364**	-1.385**	-1.046**	-1.155**	-0.807
	(0.880)	(0.838)	(0.648)	(0.664)	(0.599)	(0.527)	(0.549)	(0.495)
Incidence rate ratio	0.121	0.169	0.126	0.256	0.250	0.351	0.315	0.446
Observations	30	30	39	39	46	46	49	49
Pseudo R^2	0.076	0.366	0.103	0.441	0.042	0.397	0.033	0.378

Notes: Poisson regression discontinuity and incidence rate ratio estimates using a linear specification of the poverty index. 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). Robust standard errors in parentheses.

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

Table 5: Impact on child mortality, maximum differential exposure cohorts, quadratic specification, h<6

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 6: Impact on child mortality, maximum differential exposure cohorts, quadratic specification, h<10

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

Neighborhood h (percentage points)		7		8		9		10	
Pre-program controls		N	Y	N	Y	N	Y	N	Y
	Comparison mean	Panel A: Bolívar, Chimborazo, Imbabura, Manabí							
Treatment parroquia	0.027	-0.014*	-0.017**	-0.012	-0.014*	-0.011	-0.014*	-0.010	-0.012
(yes = 1)		(0.008)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)
F-test (quadratic terms)		1.093	1.979	1.541	1.684	2.132	2.685	4.282	4.120
[p-value]		[0.340]	[0.146]	[0.220]	[0.192]	[0.124]	[0.074]	[0.016]	[0.019]
Observations		86	86	98	98	107	107	110	110
R^2		0.100	0.239	0.059	0.200	0.071	0.193	0.070	0.189
	Comparison mean	Panel B: Bolívar, Chimborazo, Imbabura							
Treatment parroquia	0.032	-0.024*	-0.024*	-0.018	-0.010	-0.015	-0.011	-0.015	-0.011
(yes = 1)		(0.014)	(0.014)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
F-test (quadratic terms)		1.001	1.981	0.944	1.183	1.087	1.995	1.087	1.995
[p-value]		[0.375]	[0.151]	[0.395]	[0.315]	[0.343]	[0.145]	[0.343]	[0.145]
Observations		55	55	65	65	73	73	73	73
R^2		0.151	0.481	0.176	0.504	0.057	0.306	0.057	0.306

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 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 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)	0.829	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)	0.625	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 $h = 3$ without controls. Robust standard errors in parentheses.

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

Table 8: 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	0.006	-0.002	0.005	0.002	0.000	-0.004	-0.010
		(0.012)	(0.012)	(0.010)	(0.011)	(0.010)	(0.010)	(0.010)	(0.011)
Observations		48	48	58	58	70	70	75	75
R^2		0.042	0.269	0.004	0.305	0.016	0.243	0.004	0.212
	Comparison mean	Panel B: Bolívar, Chimborazo, Imbabura							
Treatment parroquia (yes = 1)	0.025	0.014	0.030	-0.010	0.016	-0.003	0.003	-0.005	-0.001
		(0.025)	(0.033)	(0.017)	(0.021)	(0.015)	(0.016)	(0.014)	(0.015)
Observations		30	30	39	39	46	46	49	49
R^2		0.074	0.524	0.011	0.472	0.026	0.450	0.035	0.455

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.

Table 9: Impact on child mortality, maximum differential exposure cohorts, non-program provinces

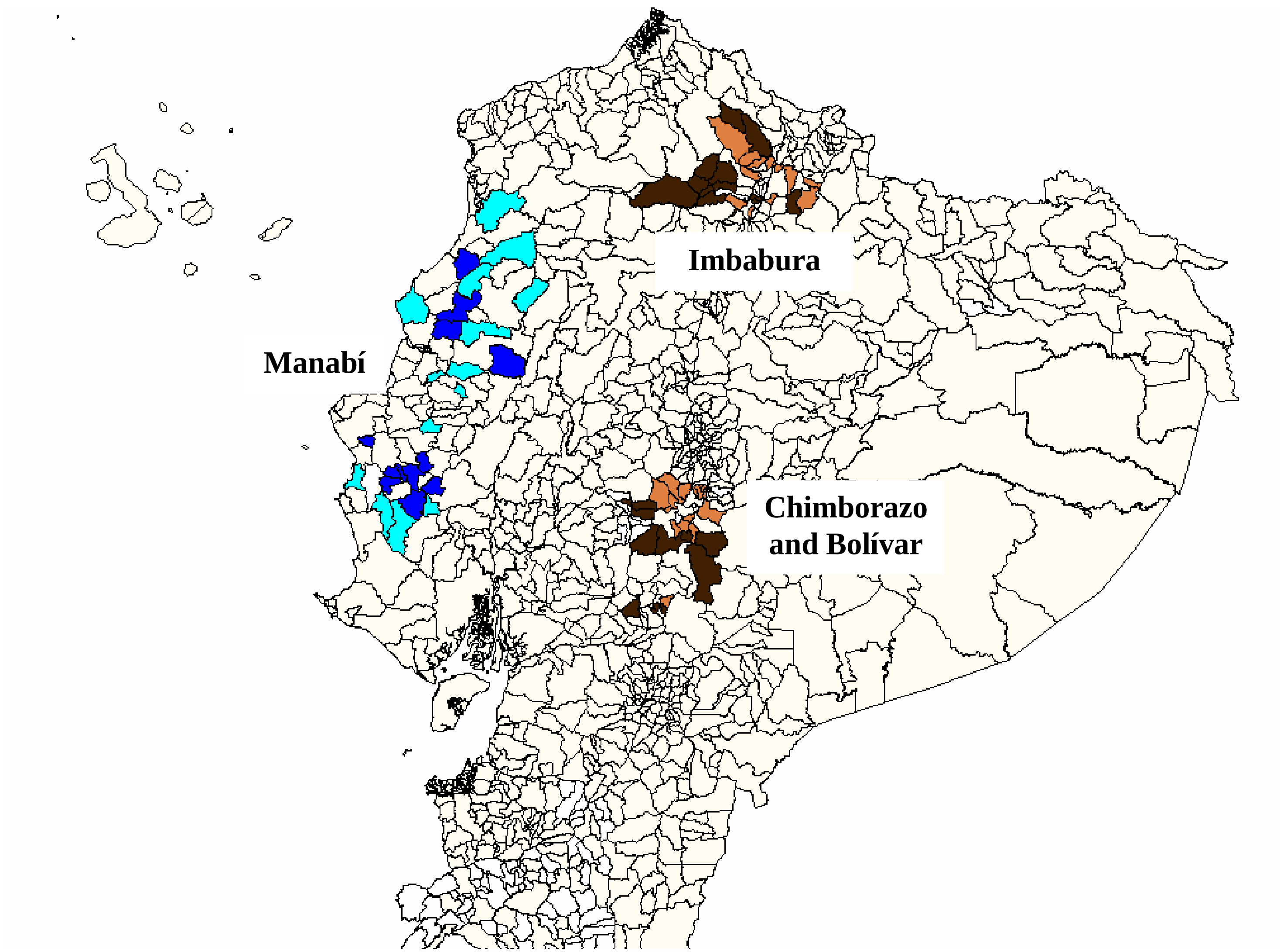
Dependent variable: Proportion of last-born children born between April 1999 and February 2000 who had died by November 2001

Neighborhood *h* (percentage points)		3		4		5		6	
Pre-program controls		N	Y	N	Y	N	Y	N	Y
	Comparison mean	Panel A: Non-program provinces, Coast and Sierra							
Treatment parroquia (yes = 1)	0.014	-0.008 (0.009)	-0.008 (0.011)	-0.003 (0.008)	-0.004 (0.010)	0.002 (0.008)	0.002 (0.008)	0.010 (0.007)	0.009 (0.007)
Observations		64	64	79	79	96	96	122	122
R^2		0.130	0.269	0.128	0.244	0.115	0.211	0.068	0.140
	Comparison mean	Panel B: Non-program provinces, Sierra							
Treatment parroquia (yes = 1)	0.013	-0.011 (0.011)	-0.016 (0.019)	-0.006 (0.011)	-0.008 (0.017)	-0.001 (0.009)	0.003 (0.012)	0.005 (0.008)	0.006 (0.010)
Observations		41	41	49	49	63	63	77	77
R^2		0.135	0.282	0.114	0.241	0.098	0.176	0.045	0.134

Notes: OLS estimates. The unit of observation is the rural *parroquia*. *h* is the percentage point distance from the eligibility cutoff 0.8905. Coastal provinces are El Oro, Esmeraldas, Guayas, and Los Ríos. Sierra provinces are Azuay, Carchi, Cañar, Pichincha, and Tungurahua 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* = 3 without controls. Robust standard errors in parentheses.

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

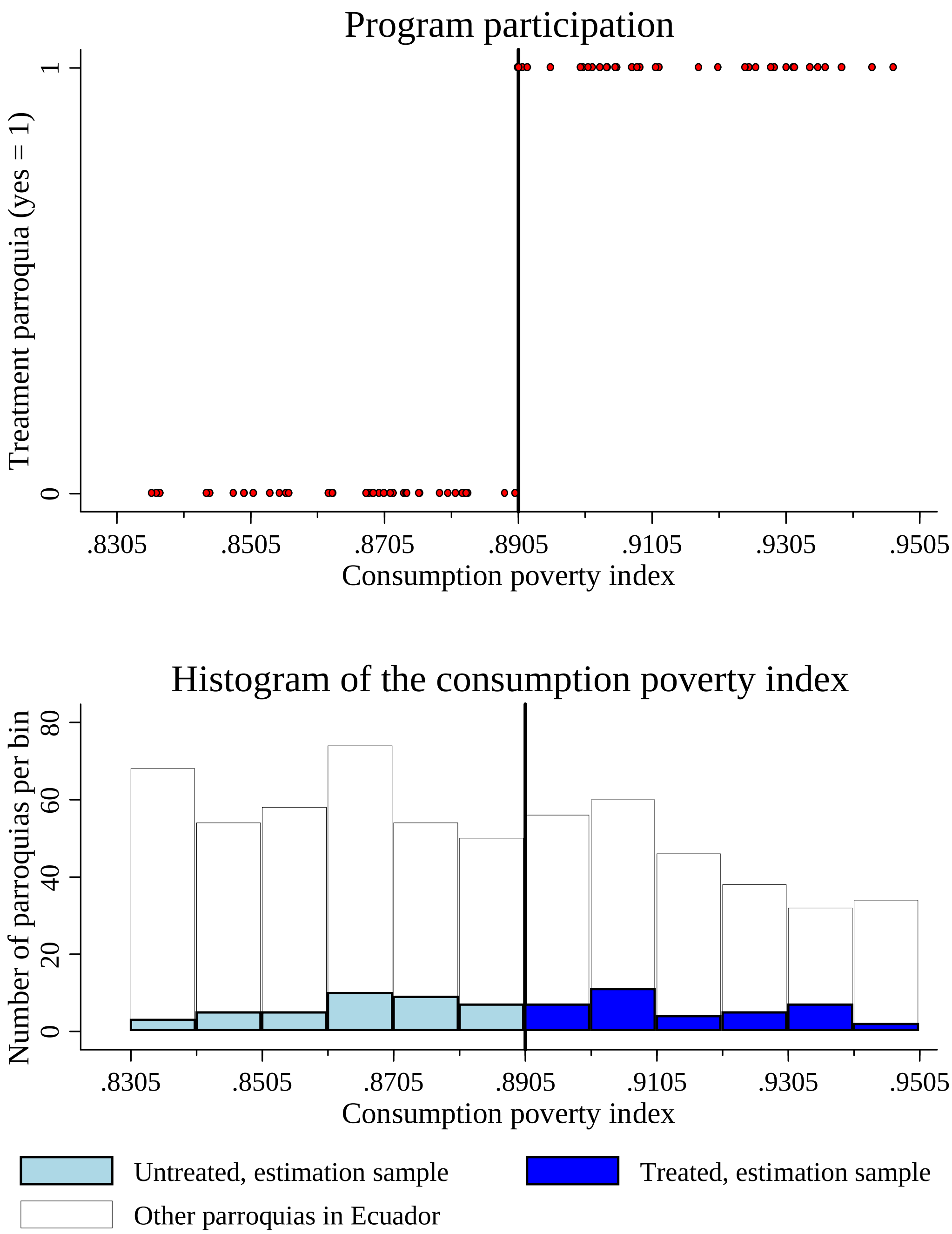
Figure 1: Treatment and comparison *parroquias* in the estimation sample



Source: Chauvin (2001).

Notes: The estimation sample includes rural *parroquias* in program provinces within 6 percentage points distance to the eligibility cutoff for the consumption poverty index (89.05 percent) and without missing values for all variables in Table 1. Dark-colored areas are treatment *parroquias* above the cutoff. 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 sample is restricted to *parroquias* within 6 percentage point distance to the cutoff. The estimation sample contains *parroquias* in program provinces (Bolívar, Chimborazo, Imbabura, Manabí) without missing values for all variables in Table 1.

Figure 3: Months of potential differential program exposure by birth month

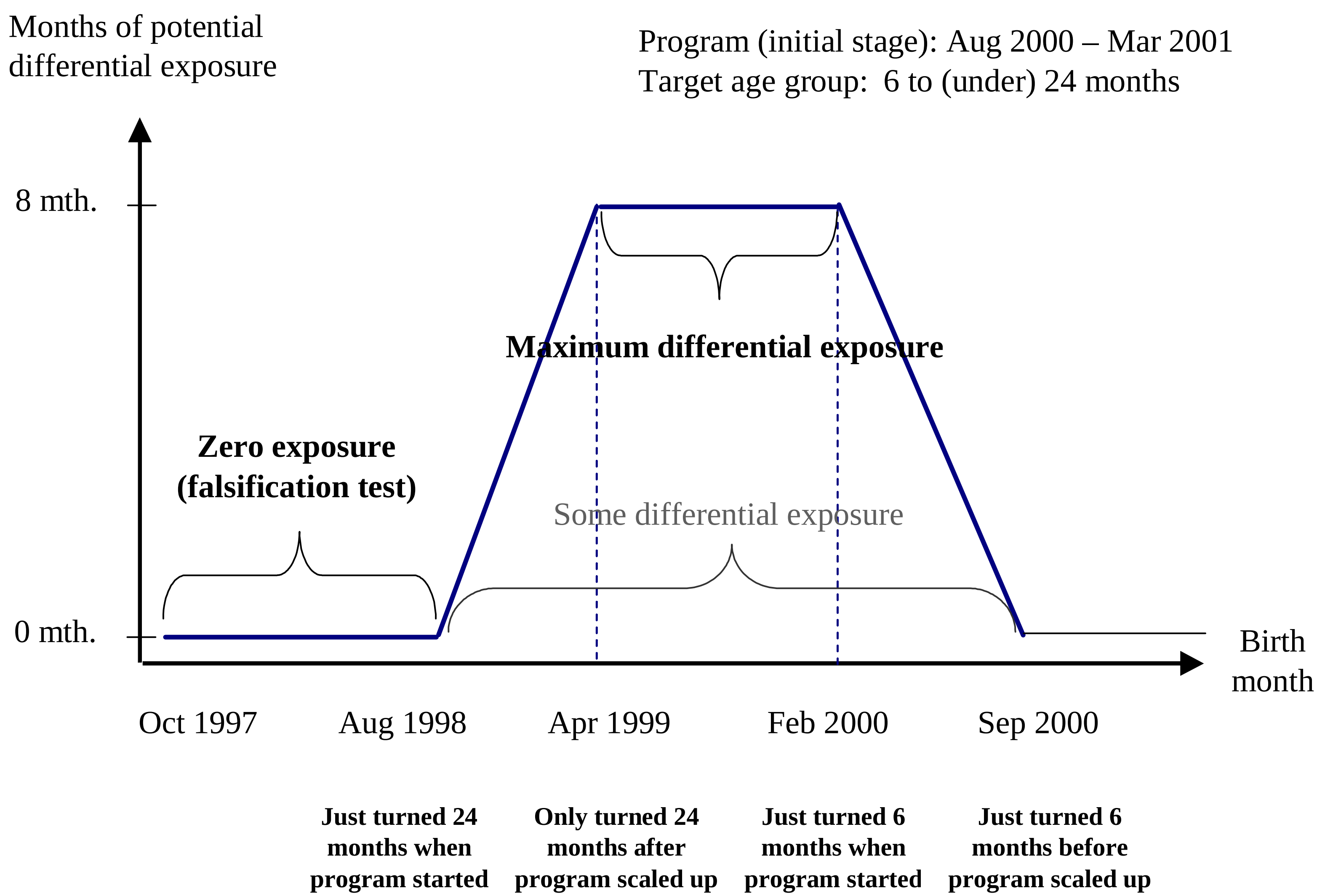
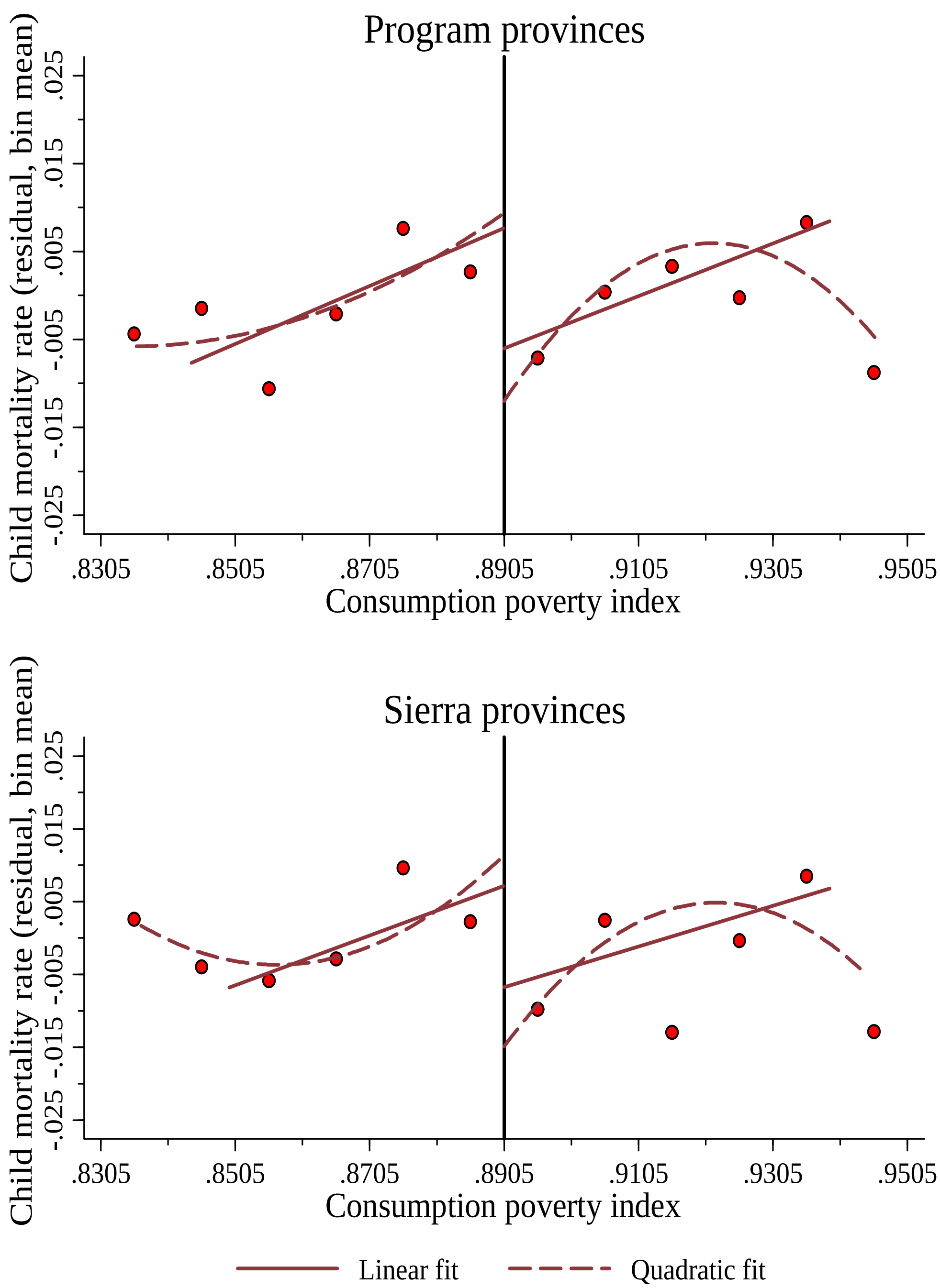
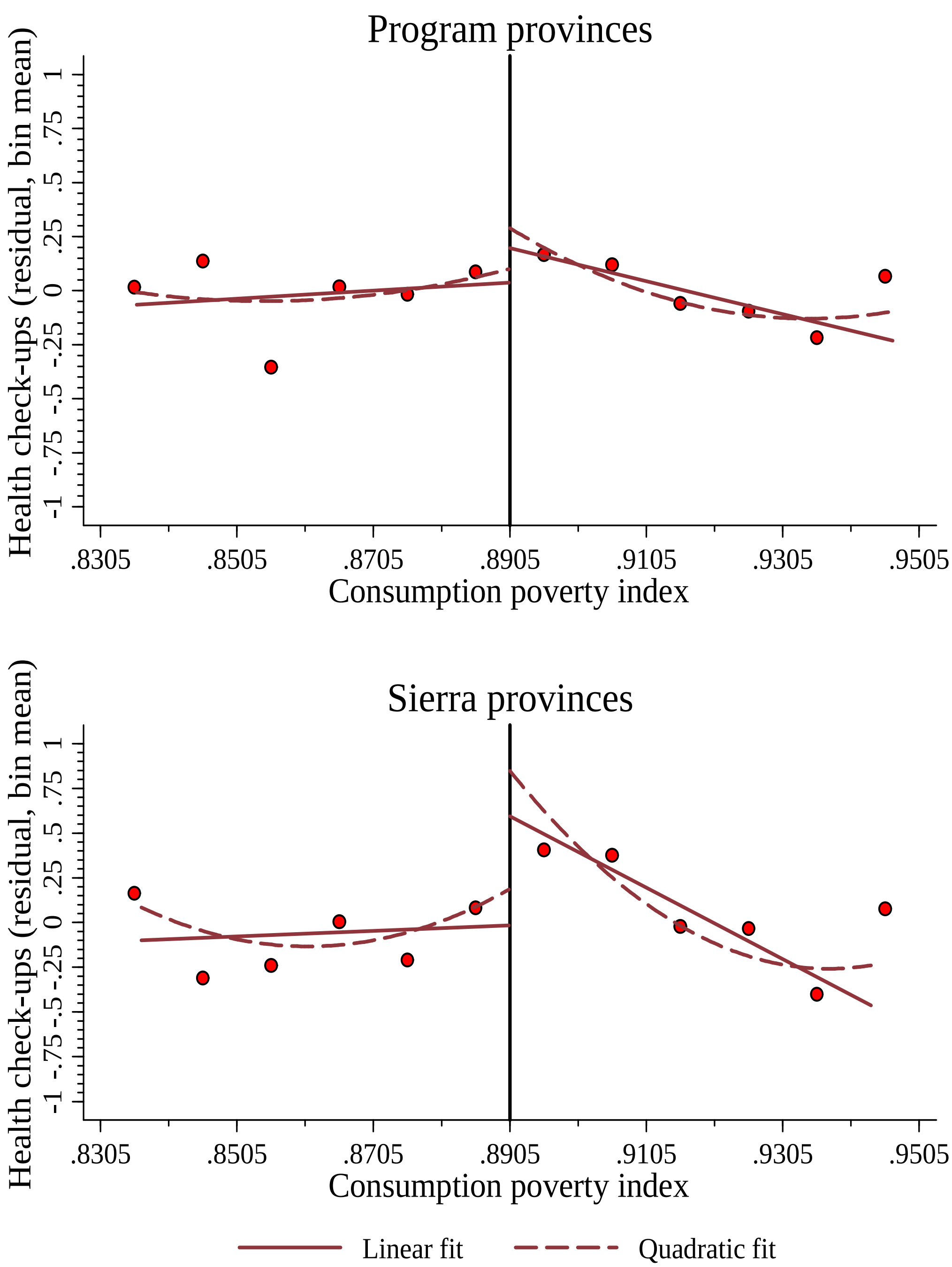


Figure 4: Impact on child mortality, maximum differential exposure cohorts, h=6



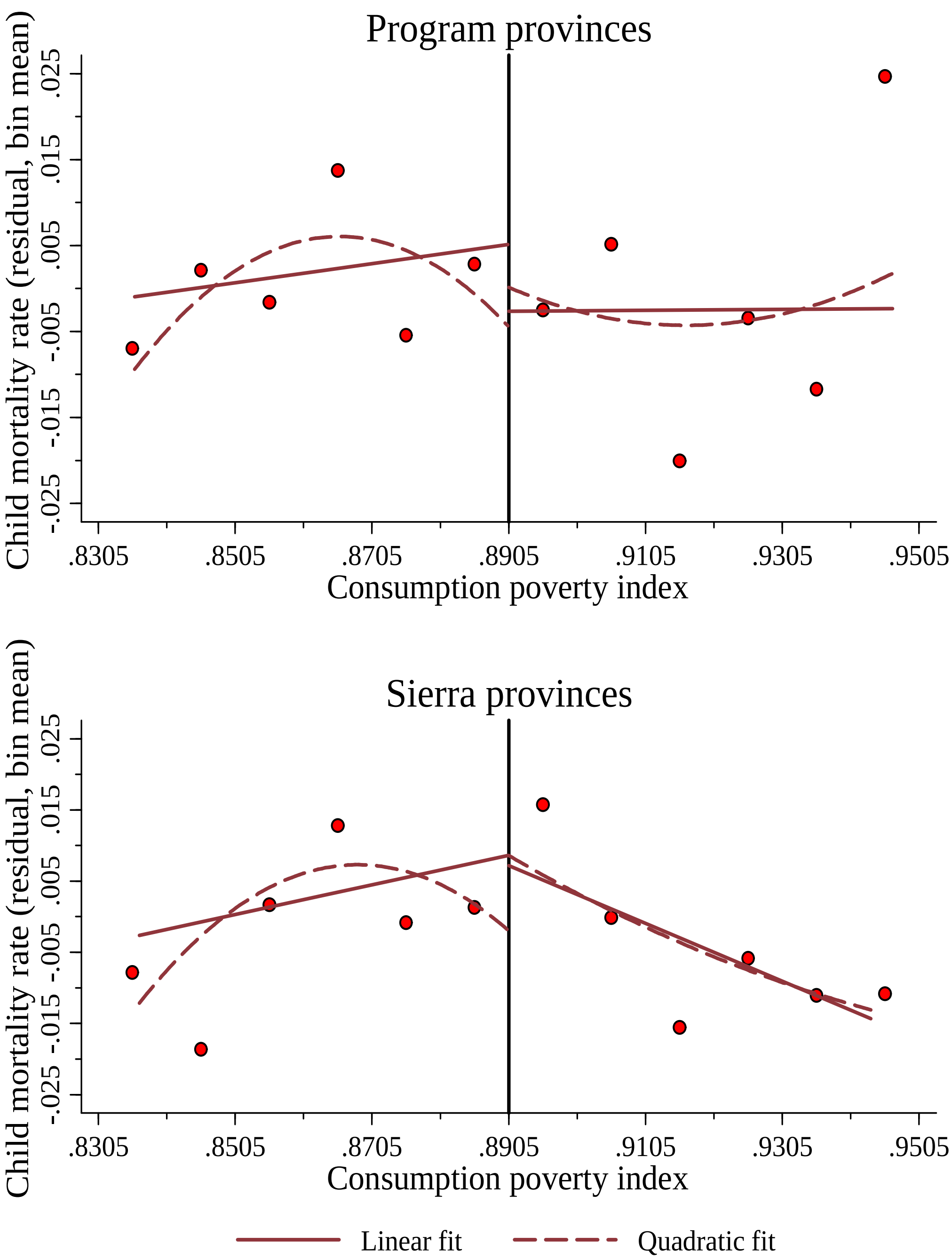
Notes: The unit of observation is the rural *parroquia*. The sample is restricted to *parroquias* within 6 percentage point distance to the cutoff. The residuals are computed from regressions with the control variables listed in Table 3. Program provinces are Bolívar, Chimborazo, Imbabura, and Manabí.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

Figure 5: Impact on health check-ups in 2000



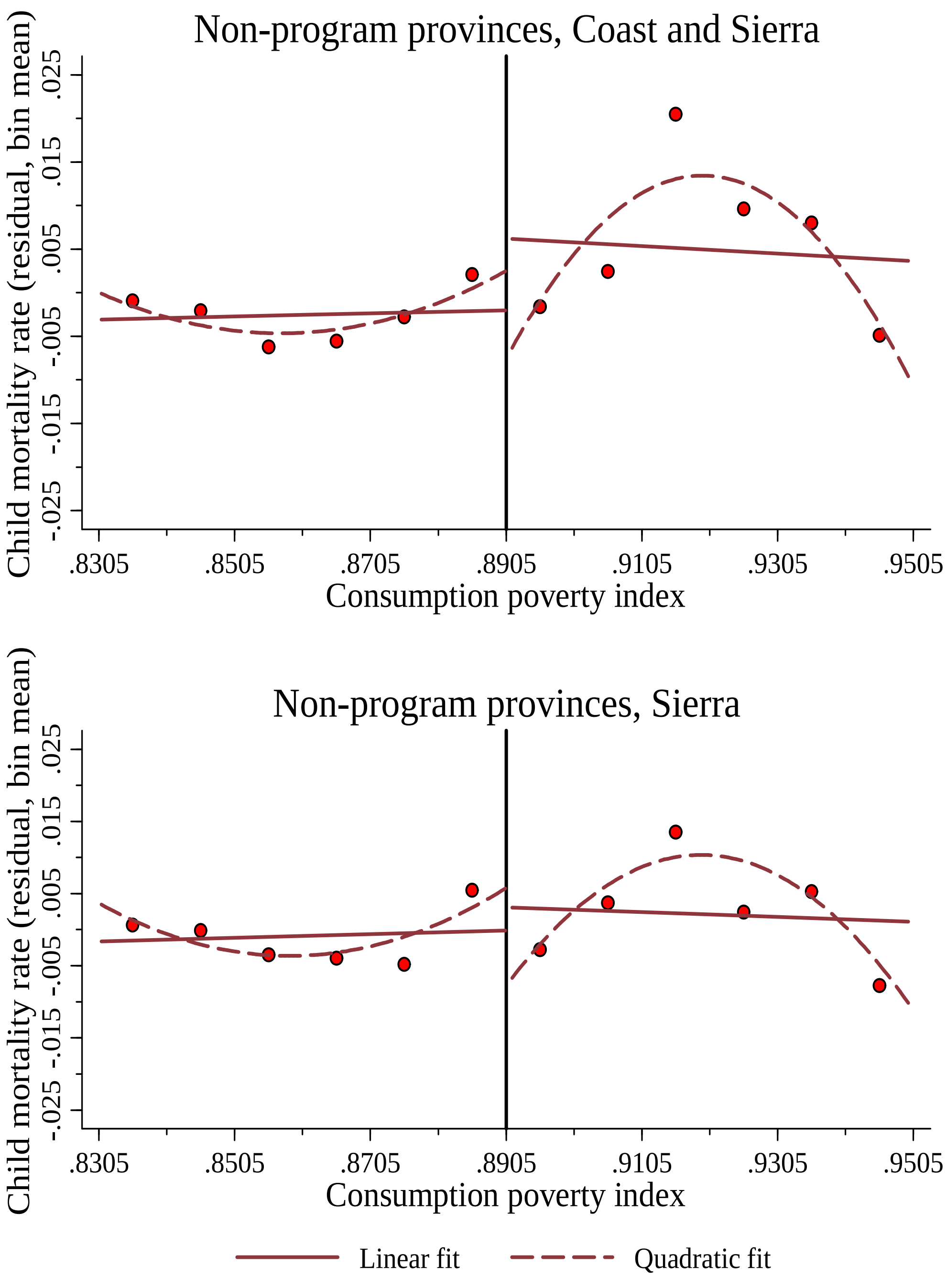
Notes: The unit of observation is the rural *parroquia*. The sample is restricted to *parroquias* within 6 percentage point distance to the cutoff. The residuals are computed from regressions with the control variables listed in Table 7. Program provinces are Bolívar, Chimborazo, Imbabura, and Manabí.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

Figure 6: Impact on child mortality, 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. The residuals are computed from regressions with the control variables listed in Table 8. Program provinces are Bolívar, Chimborazo, Imbabura, and Manabí. Sierra provinces are Bolívar, Chimborazo, and Imbabura.

Figure 7: Impact on child mortality, maximum differential exposure cohorts, non-program provinces



Notes: The unit of observation is the rural *parroquia*. The sample is restricted to *parroquias* within 6 percentage point distance to the cutoff. The residuals are computed from regressions with the control variables listed in Table 9. Coastal provinces are El Oro, Esmeraldas, Guayas, and Los Ríos. Sierra provinces are Azuay, Carchi, Cañar, Pichincha, and Tungurahua.