The Effect of Cash Transfers on Maternal Health Seeking: Evidence from Ecuador

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Abstract

Many countries have adopted maternal benefit schemes intended to bolster facility-based birth rates and demand for antenatal care through conditionalities or improved financial accessibility. We study the impacts of one such program, a multi-faceted cash-transfer program, on maternal health-seeking and birth outcomes. Ecuador's Desnutrición Cero program sought to create incentives for care-seeking through cash transfers conditional on ante and postnatal care and lump-sum grants before delivery. We exploit a discontinuity created by the targeting method of a prioritized rollout to identify the causal impact of the program and do not find evidence of improvement in antenatal care-seeking, trained or facility-based delivery care, or birth outcomes. Our results show a marginal decrease in birth weights that is likely the result of selection into measurement. We can dismiss the possibility of large effects and show that our results are robust to several specification and sensitivity analyses. Our results contrast with evidence from other maternity benefit schemes, which often show large improvements in facility-based delivery care and child vaccination. We explore several weaknesses surrounding program targeting and implementation that may have limited effectiveness. [1]

Keywords: Cash Transfer; Maternal Health; Child Health

JEL Codes: I12; I15; O15

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1 Introduction

Work on the fetal origins hypothesis has shown that poor in-uterine health and fetal undernutrition can negatively impact a range of downstream health and economic outcomes (Almond & Currie, 2011; Barker, 1995). Put simply, fetal conditions seem to persist throughout the life cycle such that a mother's health during pregnancy is predictive of a child's health and well-being. If this relationship holds, a natural conclusion may be that "one can best help children (throughout their life course) by helping their mothers" (Almond & Currie, 2011, p. 167). In this paper, we study a program that took this conclusion seriously, looking to promote maternal health seeking with the stated goal of reducing childhood stunting in Ecuador.

Within South America, Ecuador has traditionally been a negative outlier in terms of childhood nutrition. Ecuador's stunting rate of 23.9 percent is more than triple the South American average of 7.9 (INEC, 2018; WHO, UNICEF, and World Bank Group, 2020). Even among the more tightly-defined Andean region,[2] Ecuador has the second-highest stunting rate and is again far above the average of 16.5 percent. Although Freire et al. (2014) estimate that stunting in children under five years of age has been decreasing over the past 30 years nationwide in Ecuador, it has remained stubbornly high among the highland rural areas that form the bulk of program areas in this study, at an estimated rate of 38.4 percent in 2012, the first full year of our study (Freire et al., 2014). Such high stunting rates likely have deeply negative consequences for both individuals and communities as chronic undernutrition has been linked to developmental delays (Cordero et al., 1993), decreased educational attainment (Alderman et al., 2006), and depressed wages (Maluccio et al., 2009).

One possible explanation for the country's high levels of stunting is poor in-uterine health. Within the Andean Area, Ecuador has the highest rate of low-weight births (12.8 percent), the lowest rate of antenatal care (79.5 percent of women attend 4+ visits), and the lowest rate of facility-based birth (77 percent)(PAHO, 2016). Suppose the mechanisms underlying the fetal origins hypothesis discussed above hold. In that case, Ecuador's poor indicators of maternal health coverage and poor birth outcomes are likely a contributor to its high malnutrition rates.

This paper focuses on a cash transfer program that, in response to the statistics above, is intended to improve in-utero conditions through improved maternal health, with the stated goal of reducing childhood stunting. *Desnutritión Cero* was a mixed-cash transfer program that sought to encourage maternal health-seeking behaviors in parroquias across ten provinces in Ecuador from 2012 to 2014. In our analysis, we employ a fuzzy regression dis-

[2] Defined by the PAHO as Bolivia, Chile, Colombia, Ecuador, Peru, and Venezuela.

continuity design that exploits the near-systematic assignment of Ecuadorian parroquias[3] to treatment based on levels of childhood stunting predicted by a government-led proxy means test (PMT). This identification strategy rests on two main assumptions, a discontinuity in the probability of treatment assignment and a lack of manipulation of the assignment by participants. We provide clear evidence in favor of the first and discuss why the second is mechanically valid in our context.

We use administrative birth records for the whole of Ecuador between 2012 and 2014 to test the program's impact on outcomes related to mothers' health-seeking during pregnancy, delivery care, and live births. We find evidence of a small decrease in birth weight that likely reflects selection into measurement due to the program. We do not, however, find evidence of a change in the prevalence of low birth weight or maternal health-seeking. In most estimations, we can dismiss the possibility of larger effects. For maternal health-seeking outcomes, we can dismiss the possibility of changes larger than 9-65 percent, depending on the outcome. Our results are more precise for birth outcomes, and we can dismiss the possibility of more than three percent changes in birth weight, birth length, or fetal mortality relative to 2011 levels. We hypothesize this is due to one of three explanations: i) the program did not target the correct constraint on in-uterine health; ii) the program did not target the correct population; or iii) the program had insufficient demand. While we do not have the data necessary to test which of these three explanations dominates, data from disbursements and the initial mapping exercise suggest a combination of all three. Overall, the hypotheses provide areas for further research and are discussed in more detail in Section 5.

Although there is already a large and growing literature on the effect of cash transfers on child health generally and on perinatal health specifically (see Owusu-Addo and Cross (2014) and Glassman et al. (2013), respectively, for reviews) and recent evaluations in developed (Cygan-Rehm & Karbownik, 2022; González & Trommlerová, 2022) and developing contexts (Celhay et al., 2021; Grépin et al., 2019) have shown that cash transfers can effectively promote health-seeking, there is limited evidence related to combined programs that aim to improve maternal and child outcomes throughout the lifecycle of pregnancy and resulting postnatal period. In the paper that relates most closely to ours, Celhay et al. (2021) show that Bolivia's Bono Juana Azurduy (BJA) program was able to decrease child mortality by between 3 and 14 percent by using very small conditional transfers for antenatal care and trained delivery care. Our evaluation contrasts these results from Bolivia, showing that a related program did not display similarly large improvements in Ecuador. That we can dismiss the possibility of similarly large effects to Celhay et al. (2021) highlights the role

[3]Within Ecuador, Parroquias (Parishes) are the third-level administrative unit and are comparable to municipalities or towns in many other settings. There are roughly 1,500 Parroquias in Ecuador.

context and implementation play in the success of social protection programs.

While the program we study here is similar to Bolivia's BJA program, our priors are also informed by a large literature studying India's Janani Suraksha Yojana (JSY) program. JSY was launched in 2005 and has become India's largest-ever cash transfer program. The program, also aimed at improving child growth patterns, offers pregnant women in marginalized groups a cash transfer that is conditional on facility-based child delivery (Haaren & Klonner, 2021). Although the program is generally consistent in showing that the program increased facility-based births (Rahman & Pallikadavath, 2018), Debnath (2021), Andrew and Vera-Hernández (2022) suggest that there may be negative externalities associated with these gains, dependent on the context of healthcare supply. They show that in contexts with low system capacity, JSY may have increased perinatal mortality due to decreased care quality in response to congestion. More generally, the literature is mixed regarding neonatal mortality. Lim et al. (2010) suggest that the program decreased neonatal mortality. However, their study was non-causal as it studied the impact of JSY payment receipt, which may be biased for several reasons. In contrast, Powell-Jackson et al. (2015) utilize a difference-in-difference approach and find no evidence of a change in mortality. This body of work relating to the JSY program is suggestive that Desnutritión Cero may have been effective, much like BJA. However, it also offers a word of caution to the potential negative externalities associated with maternal benefit schemes.

The rest of the paper is structured as follows: Section 2 provides information on the context, the program evaluated, and the data used. Section 3 describes the empirical strategy and tests its assumptions. Sections 4 and 5 present and discuss the results, and Section 6 concludes.

2 Policy context and data

2.1 *Policy context*

Desnutrición Cero was implemented in Ecuador in 2011 as a segment of the country's wider nutritional campaign, Acción Nutrición (Ministerio de Salud Pública del Ecuador, 2018). The program took a two-pronged approach, with one arm being a nationwide nutritional push that sought to increase the availability and intake of micronutrient supplements in children, the development of milk banks, and the distribution of infant measurement equipment. The other arm, and the one that we evaluate in this paper, was a geographically-targeted mixed-cash-transfer program designed to increase perinatal health-seeking behaviors among pregnant women and new mothers (Flores, 2020).[4]

The transfer arm, which we will refer to simply as Desnutrición Cero for the remainder of the paper, was implemented in 303 parroquias (the lowest administrative level above municipality) across ten provinces (the highest administrative level) in Ecuador. These parroquias were "prioritized" based on the predicted stunting rates estimated in a malnutrition mapping exercise conducted by León and Vera (2010)[5]. Based on this mapping, parroquias with greater than 35 percent predicted stunting received the transfer program (Vera, 2016). All pregnant women and mothers in the treatment parroquias were eligible for the cash transfer, which contained the following facets:

1. $10 USD for each antenatal care consultation during their pregnancy (Up to 5)

2. $60 USD a month before their expected delivery date to help cope with financial barriers to facility-based birth.

3. $10 USD for each postnatal care consultation in the child's first year of life (Up to 6)

The first and third components were conditional on the recipient woman attending a visit at a public healthcare facility; the second was unconditional but labeled for "institutional birth," to provide pregnant women with the resources necessary to travel to a qualified health facility (Flores, 2020). Although we do not have individual-level program take-up data, the Ecuadorian Health Ministry predicted an annual treated population of approximately 25,000 pregnant women and 25,000 infants ≤1 year of age based on a pilot conducted in 2010. These numbers correspond to roughly 40% of the corresponding populations of those groups in the 303 prioritized parroquias.

Desnutrición Cero lasted through 2014, at which point the cash transfer arm was discontinued and the nationwide nutrition package was rolled into another general nutrition program, Proyecto de Nutrición en el Ciclo de Vida (Flores, 2020).

Desnutrición Cero was designed to decrease stunting rates through multiple pathways. First, by increasing antenatal healthcare usage, the program intended to improve maternal health during pregnancy and thus improve in-utero conditions for the fetus. Second, the

[4] Although the nationwide assignment of the first arm resolves most worries about its potential to confound our estimates of the effect of the transfer arm, any complementarities in implementation of the two could still bias results. However, we do not believe this is the case, as the two arms were implemented by different sub-agencies until 2013, and were only retroactively grouped into the same policy strategy (Flores, 2020).

[5] The map was created based on an extension of poverty mapping techniques to chronic malnutrition estimation. The authors used household survey data from the 2006 round of Ecuador's ECV survey (the latest nationally-representative dataset with parroquia-level anthropometric measures) to create a predictive model for malnutrition that they then applied to the same/similar variables from the 2010 Census to produce the actual estimates.

large unconditional transfer before birth intended to increase facility-based birth and trained delivery care. Mothers who give birth in a dedicated facility are more likely to put the child to the breast within an hour of birth and have a higher frequency of breastfeeding during the first three days, a particularly important period when the woman's body is producing colostrum, a form of breast milk crucial for the mother-to-child transfer of learned immune responses (Karim et al., 2018). Third, the cash transfer after birth aims to increase healthcare usage during the child's first year. Finally, the cash transfer as a whole serves to relax the budget constraint of the family and increase consumption. If some of that consumption is on goods that are inputs to the child health production function, we should expect to see an improvement in child health and a decrease in stunting rates.

As described in the four pathways above, Desnutrición Cero aimed to reduce childhood stunting through improved in-utero conditions. While these pathways offer a clear theory of change, they also provide a useful backdrop upon which to discuss the empirical challenges associated with estimating the program's impact.

In the next section, we will discuss in greater detail that Desnutrición Cero was implemented in parroquias with the greatest forecasted stunting rates. This creates the potential that birth outcomes and program assignment are simultaneously determined if maternal healthcare and birth outcomes impact growth potential and introduces the possibility of omitted variable bias if the variables used in the PMT regressions are not introduced into a naive regression of birth outcomes on program assignment.

When taken together, these causal challenges imply that a naive estimation of the impact of Desnutrición Cero on care or birth outcomes would be downward biased. In fact, given that the program was targeted at the communities with the greatest need, these estimations may very well imply that the program *increased stunting rates*. Section 3.1 discusses how we exploit a natural experiment created by the program assignment rule to meet these causal challenges and the assumptions we must make as a result.

2.2 *Program assignment and malnutrition mapping*

Our identification relies on the systematic assignment of parroquias to the Desnutritión Cero program based on a malnutrition mapping exercise conducted by León and Vera (2010) in conjunction with the Ecuadorian government. As such, it is worth taking the time here to discuss their methodology.

In 2010, to improve public policy targeting, the Ecuadorian Ministry for Social Coordination released a mapping of chronic malnutrition in Ecuador in conjunction with the World Food Programme. Based heavily on the small areas version of the proxy means test by Elbers et al. (2003), the mapping produced stunting estimates for each parroquia in the country. In the small areas methodology, practitioners begin with a rich dataset, such as a household consumption survey, and estimate the joint distribution of the targeted outcome (consumption, stunting, etc.) and a set of predictive covariates X_i within a given administrative unit. By restricting the set of predictive covariates to variables only found in a nationwide census, practitioners can use their estimated joint distribution to predict and map outcomes at lower administrative levels.

In their mapping, León and Vera (2010) use the 2006 Encuesta de Conditiones de Vida (ECV) survey to estimate a linear approximation of the conditional distribution of stunting rates within a given province:

$$y_{p,h,i} = E[y_{p,h,i}|\mathbf{X}^T_{p,h,i}] + \mu_{p,h,i} = \mathbf{X}^T_{p,h,i}\beta + \mu_{p,h,i} \tag{1}$$

Where $y_{p,h,i}$ is the height-for-age Z-score of child i in household h in province p, and $X_{p,h,i}$[6] is a vector of characteristics found in both the 2006 ECV survey and the 2010 census. $\mu_{p,h,i}$ is a stochastic error term assumed to follow a normal distribution $N \sim (0, \Sigma)$. The authors further decompose the error term into its different levels:

$$\mu_{p,h,i} = n_p + e_h + \delta_i \tag{2}$$

From this decomposition, the authors then simulate province, household, and individual level shocks, $\tilde{n}_p$, $\tilde{e}_h$, and $\tilde{\delta}_i$, from the respective sampling distribution of each term.

Based on this estimation and simulation, the authors then apply their model to the 2010 Ecuadorian Census and predict the height-for-age Z-score such that:

$$\hat{y}_{p,h,i} = \mathbf{X}^T_{p,h,i}\hat{\beta} + \tilde{n}_p + \tilde{e}_h + \tilde{\delta}_i \tag{3}$$

and consider a given child to be stunted if $\hat{y}_{p,h,i} \leq -2$. Finally, the authors aggregate these predicted stunted rates to any given administrative unit to create their malnutrition mapping.

This paper exploits the assignment of Desnutrición Cero using this malnutrition mapping. Specifically, Desnutrición Cero was implemented in 303 ”prioritized” parroquias in 10 of Ecuador's 24 provinces that were chosen because they had a predicted stunting rate of over 35 percent. Figure 2 compares the Desnutrición Cero treatment area to the original malnutrition mapping by León and Vera (2010). There are two main takeaways from this

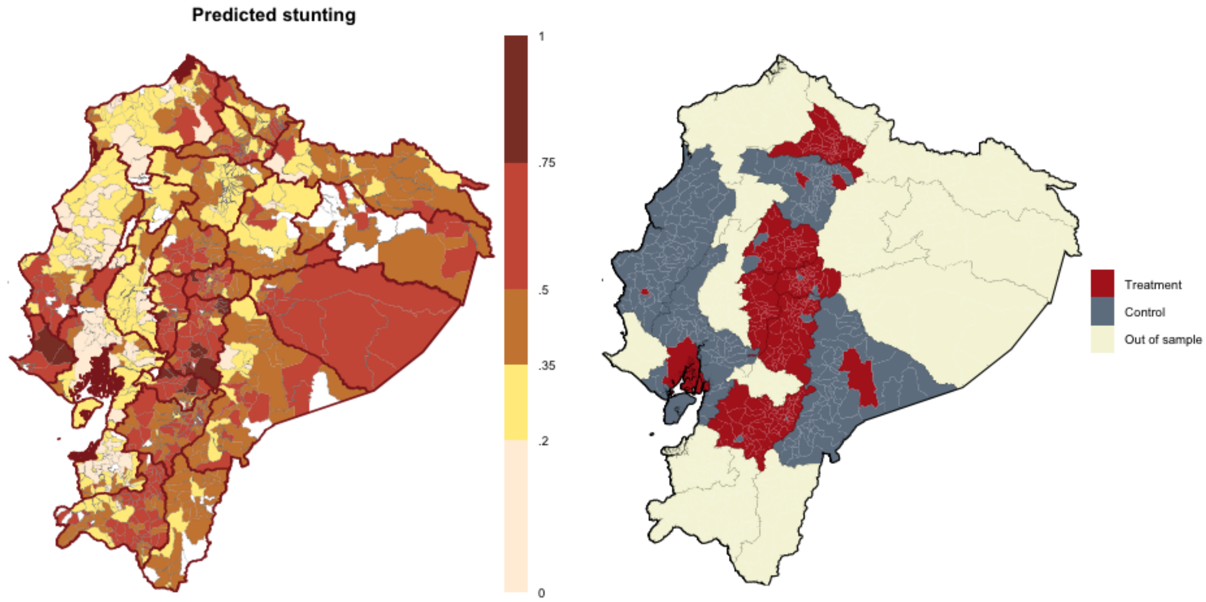
[6]The list of covariates used to predict height-for-age z-score within each province can be found in appendix 1 of León and Vera (2010)

figure. First, we see that both stunting and program assignment are largely focused in Ecuador's central mountainous region. This aligns with our earlier discussion of stubbornly high stunting rates in South America's highlands. Second, treatment assignment largely follows the 35 percent predicted-stunting rule. However, it should be clear to the naked eye that some exceptions exist.

Fig. 2: Comparing Desnutricion Cero Treatment Areas and Predicted Stunting Rates

(a) Based on León and Vera, 2010 Malnutrition Mapping

(b) Desnutricion Cero Treatment Areas



Notes: The left figure reports the predicted stunting rates from León and Vera, 2010 at the parroquia level. The figure on the right displays the Desnutrición Cero treatment of status of parroquias within the relevant provinces.

2.3 *Data*

We use data from Ecuador's Vital Statistics Database, which contains publicly-available and census-level birth data from 1992 to the present (INEC, 2020). For our estimates we use data on all births from 2004 to 2015.[7] We drop from our sample parroquias from the 14 provinces that did not contain any treatment parroquias, and the urban parroquias of Quito and Guayaquil, as the program assignment strategy differed in densely urban settings (Vera, 2016).[8] Finally, following León and Vera (2010), for parroquias near an urban center we

[7]Although data is available for years previous to 2004, we exclude it as there were serious data quality issues (exemplified by a large degree and count of outliers). Starting in 2008 a program was implemented to improve the database that resulted in collection responsibility being passed to Ecuador's Insitutio Nacional de Estadistica y Censos (INEC) in 2012.

[8]In these areas treatment assignment was restricted to mothers that were already enrolled in a different transfer program, the Bono de Desarollo Humano. We only have treatment assignment data at the parroquia

aggregate those classified as "urban" into one parroquia representing the urban area and leave those classified as "rural" disaggregated.

Table 1: Summary statistics and balance at baseline (2011)

	Full sample	Treatment	Control	Difference
Mother's age at birth	25.34	25.67	25.02	0.65***
	(6.76)	(6.92)	(6.59)	[0.17]
Any antenatal care	0.96	0.94	0.97	-0.03***
	(0.20)	(0.23)	(0.17)	[0.01]
C-Section	0.82	0.79	0.84	-0.04***
	(0.39)	(0.40)	(0.37)	[0.02]
Home birth	0.29	0.36	0.21	0.15***
	(0.45)	(0.48)	(0.41)	[0.03]
Birth in a public facility	0.52	0.51	0.52	-0.01
	(0.50)	(0.50)	(0.50)	[0.03]
Trained care at birth	0.78	0.70	0.86	-0.15***
	(0.41)	(0.46)	(0.35)	[0.03]
Mother literate	0.97	0.96	0.98	-0.02***
	(0.17)	(0.20)	(0.14)	[0.00]
At least high school education	0.46	0.38	0.55	-0.17***
	(0.50)	(0.49)	(0.50)	[0.02]
Child's biological sex	0.51	0.50	0.51	-0.01**
	(0.50)	(0.50)	(0.50)	[0.00]
Weeks pregnant at birth	38.85	38.87	38.83	0.04
	(1.56)	(1.54)	(1.57)	[0.04]
Length (in cm)	48.48	48.36	48.59	-0.23***
	(2.47)	(2.54)	(2.40)	[0.07]
Weight (in grams)	3077.75	3071.78	3082.67	-10.89
	(445.29)	(448.87)	(442.26)	[14.83]
Geographic density of public health facilities (std.)	0.00	-0.02	0.02	-0.04
	(0.99)	(0.83)	(1.13)	[0.22]
Observations	62601	30408	32193	62601

$***p < 0.01, **p < 0.05, *p < 0.1$

Notes: For all columns, the unit of observation is a birth event. 95% Confidence Intervals are in parentheses. Sample restricted to the 10 provinces that contained treatment parroquias: Azuay, Bolivar, Chimborazo, Cotopaxi, Imbabura, Manabi, Morona Santiago, Pichincha, and Tungarahua. Standard deviations in (), standard errors in []. Difference is treatment - control.

In order to construct our analysis dataset, we match the births to the predicted stunting levels estimated in León and Vera (2010) and the list of parroquias that received treatment, so that each observation is at the birth-level and contains the predicted-stunting and treatment assignment of the mother's parroquia of residence. Overall our dataset contains over 760,000 births. The birth registry data contains basic anthropometric measures of the in-

level, and so are not able to include them in our analyses.

fant, administrative details of the birth, and demographic information on the mother (INEC, 2020). Summary statistics of relevant variables for the total sample and the treatment and control groups over the time period studied are presented in Table 1. We see that within the 10 relevant provinces, mothers in treatment areas were less educated, more likely to give birth at home, and less likely to have trained delivery care. The summary statistics surrounding antenatal care suggest that on the extensive margin a large majority of women, 96 percent, receive some sort of antenatal consultation. However, on the intensive margin, we observe large differences between groups: 86 percent of women in treatment areas received trained care at birth, compared to 70 percent in excluded parroquias; similarly rates of home birth in treatment areas are 36 percent compared to the 21 percent in control areas (Tunçalp et al., 2017). Taken together, the statistics reported in Table 1 give the sense that there was at least some supply of maternal healthcare in the areas where the program operated. However, given the administrative nature of our data, we are unable to make statements about the quality of this care or the crowding of facilities, which Andrew and Vera-Hernández (2022) suggest may influence the impacts of the program. In contrast, these figures imply that Desnutrición Cero indeed targeted a legitimate disparity in demand. We should note, however, that despite the moderately sized differences in usage of care, we do not observe evidence of differences in birth weight. As we will discuss in Section 5, this result may help explain our results insomuch that Desnutrición Cero potentially targeted a non-binding constraint on fetal growth and child health.

3 Empirical Strategy

3.1 *Regression Discontinuity*

In order to estimate the effect of the program on our set of outcomes we exploit the assignment methodology described in section 2.2 and estimate a fuzzy regression discontinuity model using the predicted stunting rate from León and Vera (2010) as the running variable and the 35 percent stunting threshold as the cutoff. This methodology is in line with Buddelmeyer and Skoufias (2004) who show that regression discontinuity designs perform well compared to randomized experiments when estimating the effects of cash transfer programs.[9]

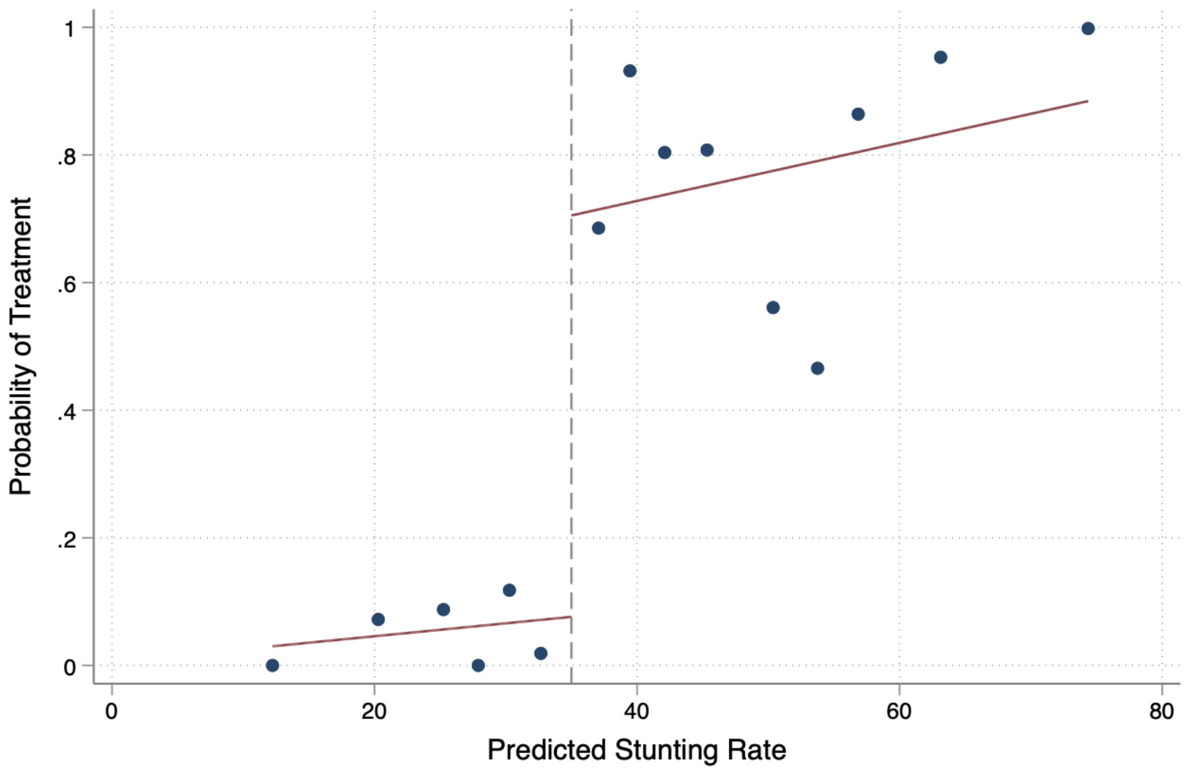
For our regression discontinuity design to be valid, two assumptions must hold. First, the probability of treatment given the value of the running variable, $E[T_p|R = r]$, must be discontinuous at the cutoff value, $R = c$; second, individuals must be unable to perfectly

[9]Several studies on cash transfer programs have relied on difference-in-difference estimators; we explore this but did not judge it feasible due to significant differences in pre-trends in our outcomes. A thorough exploration can be found in Appendix A.

manipulate their position around the cutoff. Figures 4 and 5 present suggestive evidence that these assumptions hold. Figure 4 presents estimates of $E[T_p|R = r]$ on both sides of the 35 percent stunting cutoff. The figure clearly shows that the probability of treatment assignment jumps at the cutoff, leading to the conclusion that there is a valid treatment discontinuity at the 35 percent cutoff. Figure 5 presents the distribution of births in both treatment and control areas to explore the imperfect control assumption. In this figure, we do not observe evidence of bunching directly above or below the cutoff, nor do we expect individuals to be able to perfectly control where they fall in the distribution given the malnutrition mapping was conducted multiple years before the program's rollout. Figures 4 and 5 also show that the 35 percent assignment rule did not hold perfectly in the rollout of Desnutrición Cero. Indeed, during conversations for this project, program administrators confirmed that there was some wiggle room in the program assignment and that the 35 percent cutoff was more of a guideline than an absolute rule (Flores, 2020). Due to this "leakage" in program assignment, we estimate a fuzzy regression discontinuity design (RDD) rather than a sharp RDD.

In addition to the assumptions around the validity of the assignment, we must also assume that there is no underlying discontinuity in the determinants of our outcomes of interest. Table 3 sheds some light on this assumption by testing for discontinuities in the covariates we will include in our estimation. For estimations on care-seeking behavior, we test for discontinuities in maternal age and the geographic density of public health facilities in the parroquia. We also control for the biological sex of the fetus to account for the fragile male hypothesis, the observed fragility of potentially male fetuses when compared to potentially female (Kraemer, 2000). With all p-values $>.15$ in our tests of discontinuity in Table 3, we do not see evidence of a discontinuity in any of our covariates. This is confirmed visually in Figure 6, which plots the three covariates around the cutoff in the years before and during the intervention. Combining this conclusion with our conclusions from Figures 4 and 5 leads us to conclude that our discontinuity is valid.

Fig. 4: Conditional probability of treatment



Notes: The figure reports the proportion of births that take place in treatment parroquias versus the León and Vera (2010) predicted stunting rate. The vertical dotted line represents the 35 percent assignment rule for Desnutrición Cero. Figure based on author's calculations.

Although we show in Appendix A that a difference-in-difference design is not suitable for our context, we leverage the existing data from before the intervention's rollout by estimating a fuzzy regression discontinuity model using a non-parametric local linear regression where the first stage estimation is given by:

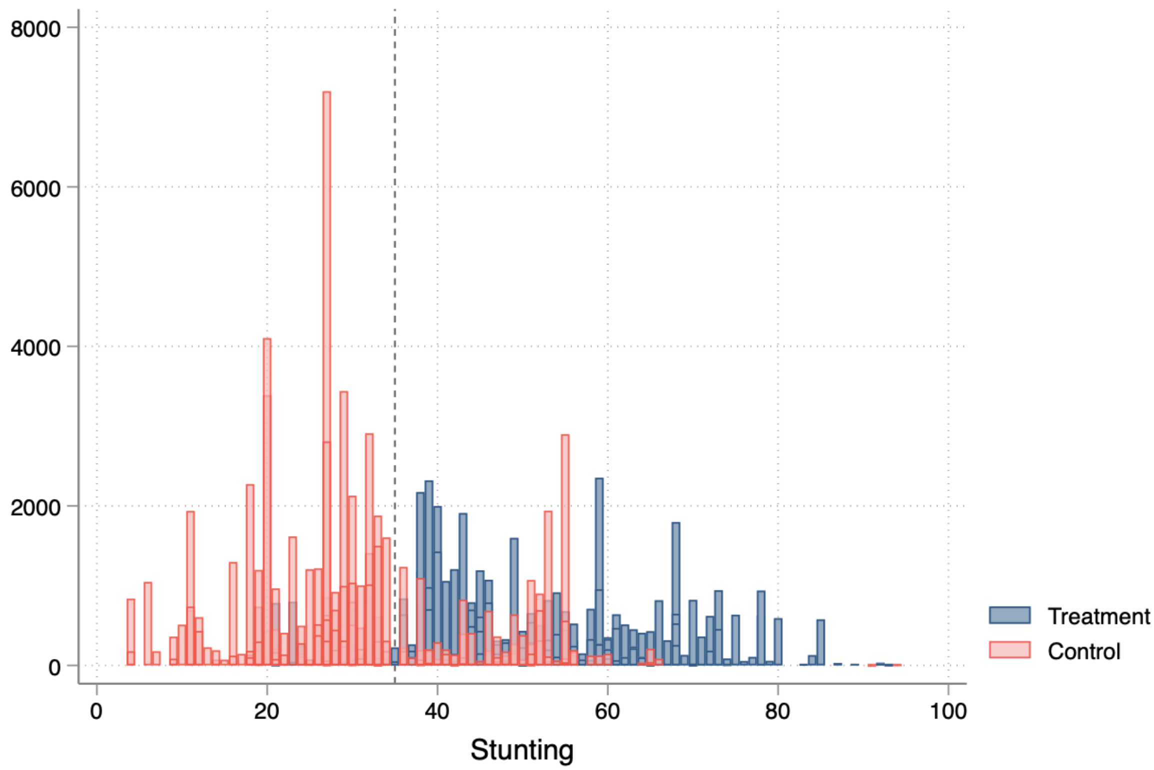
$$T_{pt} = \alpha_0 + \alpha_1 \mathbf{1}(S_p \geq C) * \gamma_t + \alpha_2 (S_p - C) + \alpha_3 \mathbf{1}(S_p \geq C) * (S_p - C) + \gamma_t + \eta_p + \alpha_4 X_{i,p,t} + \nu_{i,p,t} \quad (4)$$

and the second stage:

$$Y_{i,p,t} = \beta_0 + \beta_1 \hat{T}_{pt} + \beta_2 (S_p - C) + \beta_3 \hat{T}_{pt} * (S_p - C) + \gamma_t + \eta_p + + \beta_4 X_{i,p,t} + \varepsilon_{i,p,t} \quad (5)$$

Where $Y_{i,p,t}$ is the outcome variable of interest for birth event i in parroquia p and year t. S_p and T_{pt} are the predicted stunting rate and treatment status of parroquia p in year t,

Fig. 5: Treatment Group Distribution



Notes: The figure reports the total number of births that take place in treatment parroquias versus control parroquias at each level of the León and Vera (2010) predicted stunting rate. The vertical dotted line represents the 35 percent assignment rule for Desnutrición Cero. Figure based on author's calculations.

respectively. γ_t is a year fixed effect, η_p is a regional fixed effect,[10] and $X_{i,p,t}$ is a vector of birth-event-level covariates all included in the estimation.[11] By leveraging the panel nature of our data and allowing treatment status to vary over the years, we identify the change in the discontinuity as a result of the program.[12]

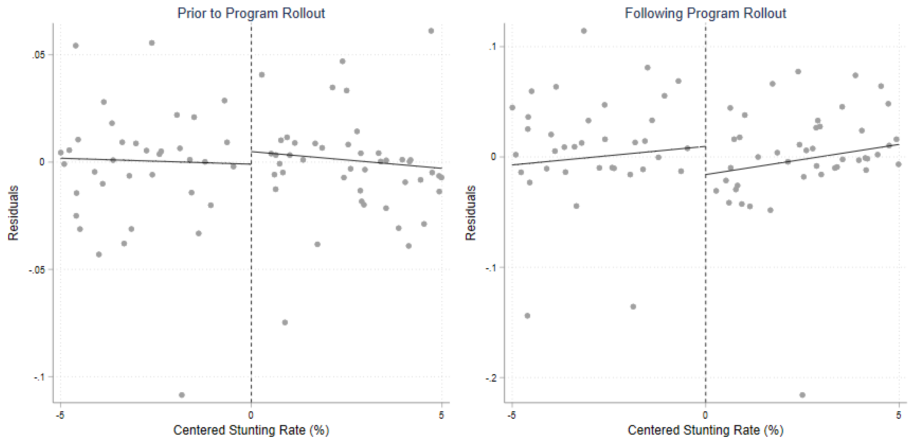
We estimate equations (5) and (6) over an estimation window, h, defined by the mean-squared error optimal bandwidth defined in Imbens and Kalyanaraman (2012). Additionally, since the running variable is defined at the parroquia level, it is discrete at the birth-event level. As such, to ensure proper coverage, we follow the Lee and Lemieux (2010) recommendation and cluster our standard errors by the running variable, which in this case is

[10]This fixed effect is at the Canton level, which in Ecuador is the administrative level above the Parroquia. We cannot include a parroquia-level fixed effect as the program was assigned at the program level

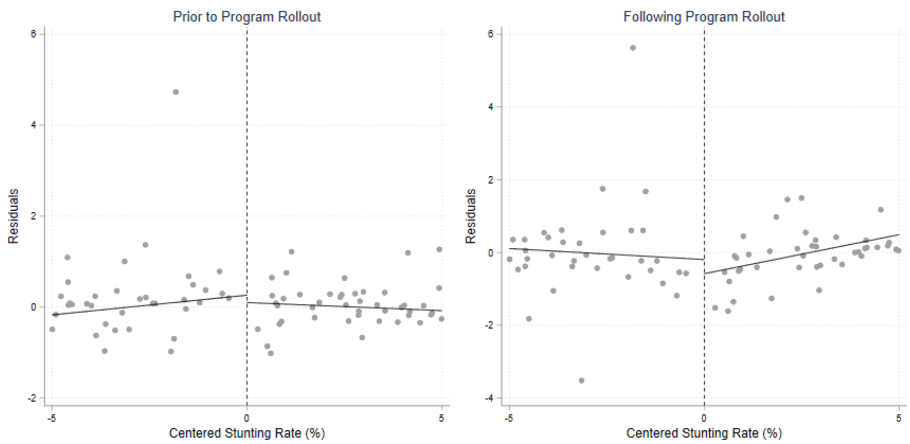
[11]These covariates include maternal age, the child's sex, and the standardized density of public health facilities at the parroquia level

[12]By choosing not to interact our standardized running variable with the year fixed effect, we are imposing the assumption that the slope of our outcome on either side of the cutoff is constant across years

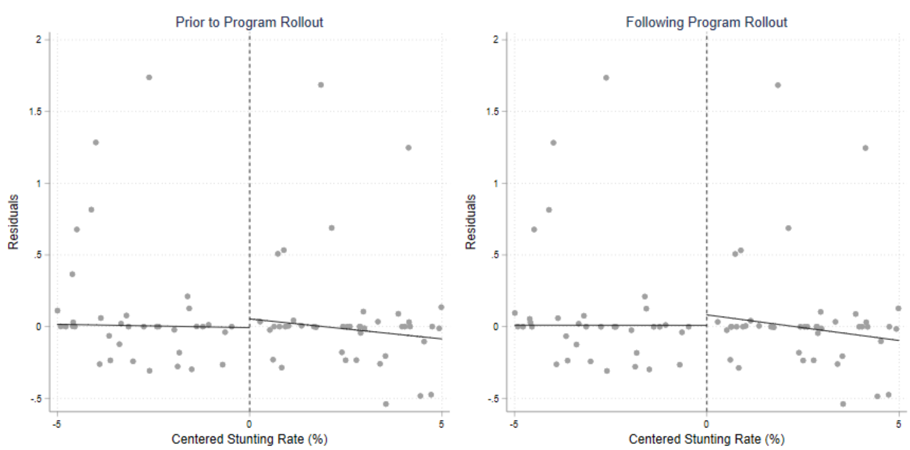
Fig. 6: Continuity of Covariates



(a) Child's Biological Sex (1=Male)



(b) Mother's Age



(c) Density of Public Health Facilities

Notes: Figures are created using a mean squared error minimizing bandwidth, a triangular kernel, and evenly spaced variance mimicking bins defined in Calonico et al., 2015. Points represent the residuals of a regression of the relevant characteristic on canton and year fixed effects.

Table 2: Continuity of covariates

	Child Sex (1=Male)	Mother's Age	Density of Public Facilities
P=1	-0.0116	-0.0190	0.00370
	[-0.0342, 0.0109]	[-0.596, 0.558]	[-0.0240, 0.0314]
P=2	-0.0116	-0.0211	0.00133
	[-0.0342, 0.0109]	[-0.597, 0.555]	[-0.0233, 0.0259]
P=3	-0.0117	-0.0171	0.00271
	[-0.0342, 0.0109]	[-0.589,0.555]	[-0.0166, 0.0220]
Control Mean (2011)	0.52	24.79	-0.51
Bandwidth	5.00	5.00	5.00
First Stage F-Statistic (P=1)	20.02	31.18	20.02
First Stage F-Statistic (P=2)	20.34	31.33	20.34
First Stage F-Statistic (P=3)	20.46	30.82	20.46
Observations			
Prior to Intervention			
Left of cutoff			
Birth Events	62296	53821	62296
Parroquias	37	37	37
Right of cutoff			
Birth Events	61233	54615	61233
Parroquias	45	45	45
During Intervention			
Left of cutoff			
Birth Events	27548	27047	27548
Parroquias	37	37	37
Right of cutoff			
Birth Events	27421	27029	27421
Parroquias	45	45	45

$***p < 0.01, **p < 0.05, *p < 0.1$
Notes: For all columns, the unit of observation is a birth event. 95% Confidence Intervals are in parentheses. Sample restricted to the 10 provinces that contained treatment parroquias: Azuay, Bolivar, Chimborazo, Cotopaxi, Imbabura, Manabi, Morona Santiago, Pichincha, and Tungarahua. All estimates use an estimation window defined by the MSE-optimal bandwidth and a triangular kernel.

equivalent to clustering at the parroquia level.

4 Results

4.1 Birth Outcomes

As discussed earlier, the stated goal of Desnutrición Cero was to reduce stunting rates, and the program intended to do so, in part, by improving in-utero conditions. We can identify changes in these conditions in our data by studying the program's impact on birth outcomes. To test this, Figure 7 plots birth weights, the prevalence of low birth-weight,[13] birth length and mortality across the discontinuity before the program rollout and during the years of implementation. Additionally, to address selection concerns associated created by improvements in delivery care, we also display the discontinuity in the proportion of births that have anthropometric data recorded.[14] As described in Section 3.1, our main estimation strategy includes a set of related covariates, year fixed effects, and canton fixed effects. As such, Figure 7 displays the residuals of the regression of our outcome on this set of covariates. Upon visual inspection, we observe no evidence of a treatment effect on any of our outcomes within the MSE-optimal bandwidths.

We test our visual intuition more formally in Table 4, which displays the estimation of equations (5) and (6) with a polynomial order of up to three. Our visual interpretation is mostly confirmed by our formal estimation, inasmuch as our coefficients are small relative to the outcome means and precisely measured, so we can dismiss the possibility of large effect sizes in either direction. We observe that most of our treatment effect estimates do not cross the traditional threshold of statistical significance. However, contrary to our priors, we observe a statistically significant decrease in children's birth weights. With this, we must make a note of two further observations. First, the result is small in magnitude; the point estimate represents just a 1.7 percent change, and the precision of the estimate allows us to rule out decreases of more than 3.4 percent. Second, despite being imprecisely measured, the point estimate for the proportion of births with birth weights recorded suggests a 1.5 percent increase. As we will discuss in the next section, this may be a result of a small cohort of women substituting facility-based birth for home-based birth. While data limitations prevent us from describing the socio-economic characteristics of the women in our sample, it may be reasonable to assume that these "marginal women" are of lower socio-economic status, have worse in-utero conditions, and would likely give birth to smaller children absent of any intervention. The small decrease in birth weight that we observe in our estimates may then reflect a selection into measurement rather than the deleterious effects of the program.

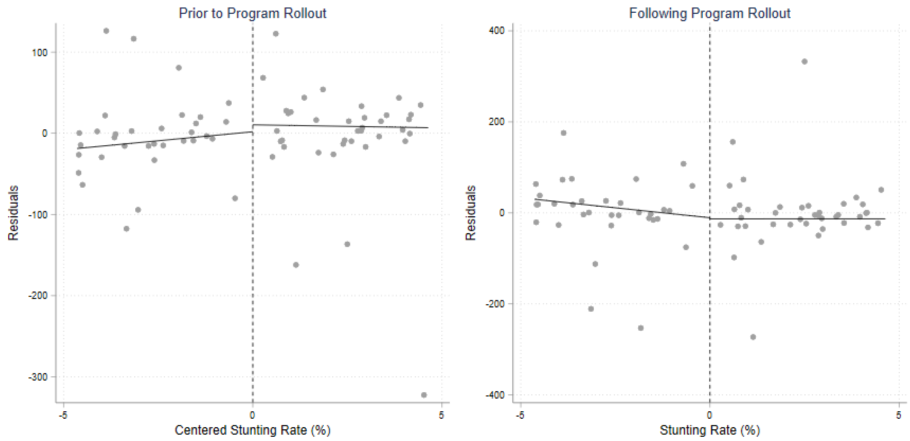
4.2 *Care-Seeking Results*

Although we do not find evidence of large effects on children's birth outcomes, it is possible that these outcomes are too far down the causal chain and too difficult to change for us to detect small effect sizes. We may nonetheless expect to see movement on the behaviors that Desnutrición Cero directly incentivizes. The logic here is simple: for antenatal and postnatal

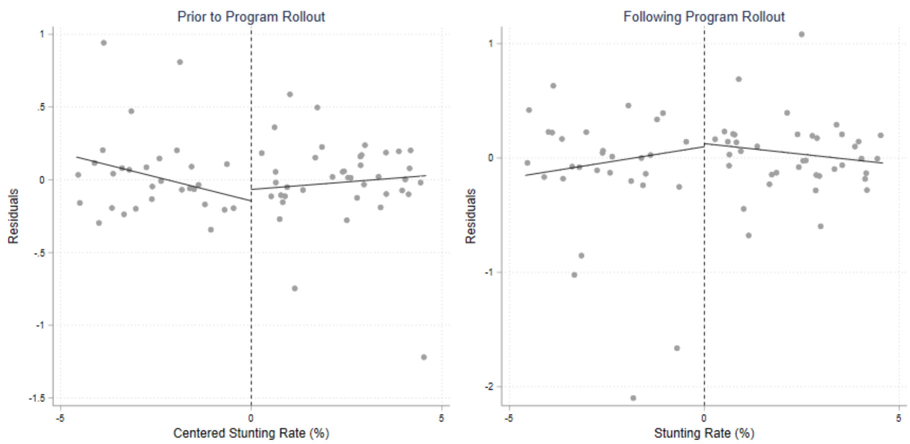
[13] Low birth-weight is defined as a birth-event resulting in a neonate weighing less than 2,500 grams.

[14] We observe that 88 percent of home-based births are missing birth weights, while that number is roughly 10 percent for births in that take place in medical facilities

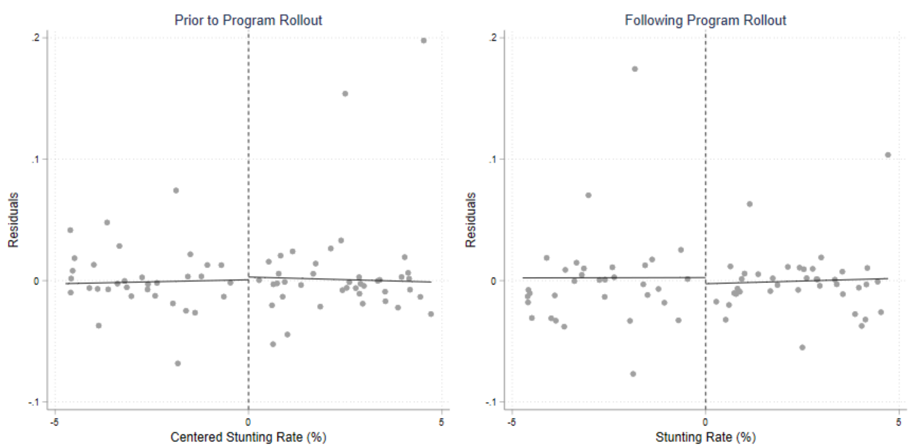
Fig. 7: RDD Plots: Birth Outcomes



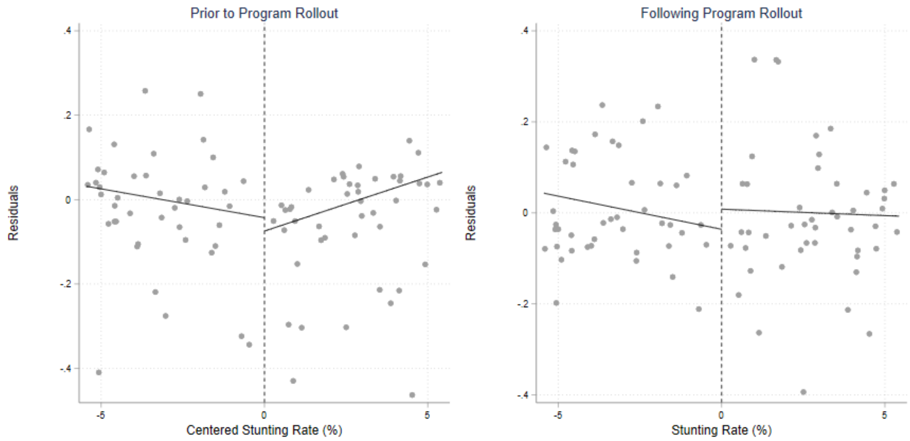
(a) Birth Weight



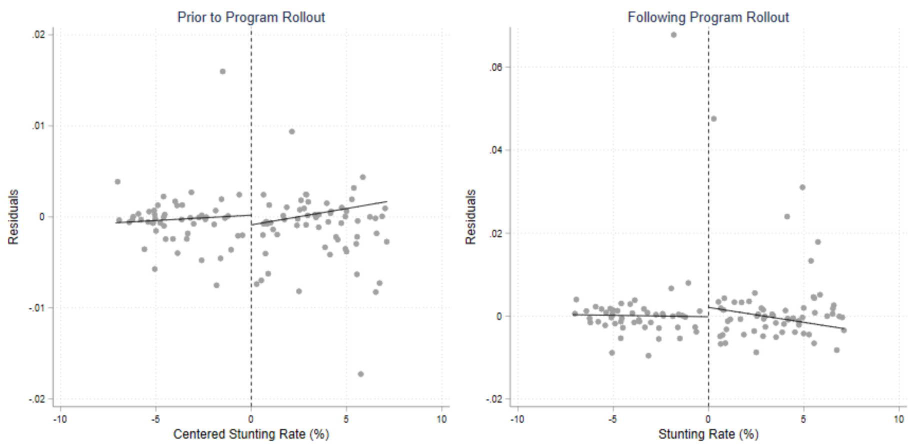
(b) Birth Length



(c) Low Birth Weight



(d) Has Birth Weight



(e) Neonatal Mortality

Notes: Notes: Figures are created using a mean squared error minimizing bandwidth, a triangular kernel, and evenly spaced variance mimicking bins defined in Calonico et al. (2015). Points represent the residuals of a regression of the relevant characteristic on canton and year fixed effects.

care, the conditionality of the cash transfer creates an incentive to increase usage. For delivery care, the unconditional transfer should relax the financial barriers to facility-based birth. As such, in Figure 8, we present discontinuity plots parallel to Figure 7 for outcomes related to mothers' health-seeking behaviors. First, we observe higher levels of variation across parroquias than we did for birth outcomes. Second, while visual inspection of the fitted lines suggests that there may be some effect on home-based birth and public-sector delivery, it is not clear from the underlying data points that there is a true discontinuity.

Accompanying these plots, we again estimate equations (5) and (6) on antenatal care usage, the prevalence of facility-based birth, and the use of trained delivery care.[15] We also estimate the equations on number of births in public and private facilities. We include this

[15] We define a birth event to have used trained delivery care if it was attended by a doctor, nurse, or certified midwife.

Table 3: RDD Results: Birth outcomes

	Birthweight	Birth Length	Low Birthweight	Has Birthweight	Mortality
P=1	-53.38**	0.112	-0.00715	0.0151	-0.000478
	[-106.3, -0.447]	[-0.289, 0.514]	[-0.0197, 0.00543]	[-0.0821, 0.112]	[-0.00294, 0.00198]
P=2	-53.45**	0.111	-0.00712	0.0144	-0.000516
	[-106.4, -0.530]	[-0.290, 0.512]	[-0.0197, 0.00546]	[-0.0825, 0.111]	[-0.00298, 0.00195]
P=3	-53.10**	0.107	-0.00705	0.0165	-0.000634
	[-105.9, -0.319]	[-0.294, 0.508]	[-0.0196, 0.00550]	[-0.0816, 0.115]	[-0.00309, 0.00183]
Control Mean (2011)	3127.30	48.75	0.07	0.78	0.00
Bandwidth	4.63	4.58	4.72	5.44	7.10
First Stage F-Statistic (P=1)	59.34	56.82	58.43	29.76	26.35
First Stage F-Statistic (P=2)	58.36	55.98	57.82	29.44	25.55
First Stage F-Statistic (P=3)	58.44	56.65	57.56	29.15	31.78
Observations					
Prior to Intervention					
Left of cutoff					
Birth Events	33783	32089	33783	69572	87529
Parroquias	33	30	33	44	52
Right of cutoff					
Birth Events	35333	35256	35517	66660	79062
Parroquias	39	39	40	47	61
During Intervention					
Left of cutoff					
Birth Events	20772	19700	20772	30753	38185
Parroquias	33	30	33	44	52
Right of cutoff					
Birth Events	21088	21097	21142	29306	34604
Parroquias	39	39	40	47	61

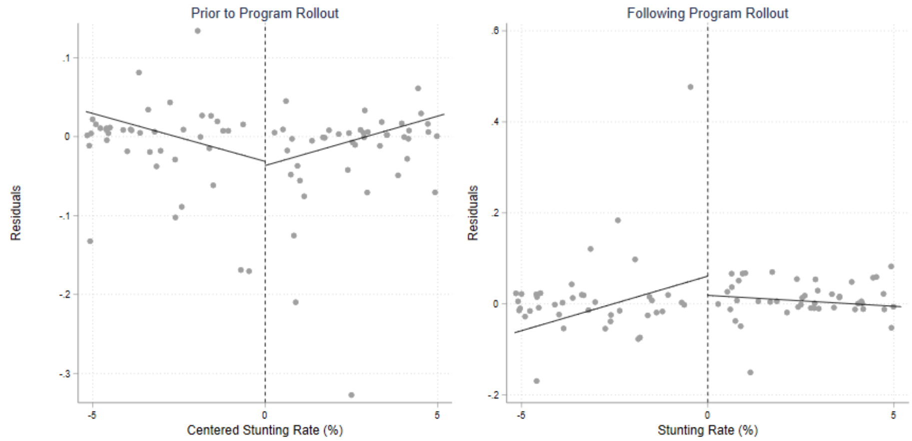
$***p < 0.01, **p < 0.05, *p < 0.1$

Notes: For all columns, the unit of observation is a birth event. 95% Confidence Intervals are in parentheses. Sample restricted to the 10 provinces that contained treatment parroquias: Azuay, Bolivar, Chimborazo, Cotopaxi, Imbabura, Manabi, Morona Santiago, Pichincha, and Tungarahua. All estimates use an estimation window defined by the MSE-optimal bandwidth and a triangular kernel.

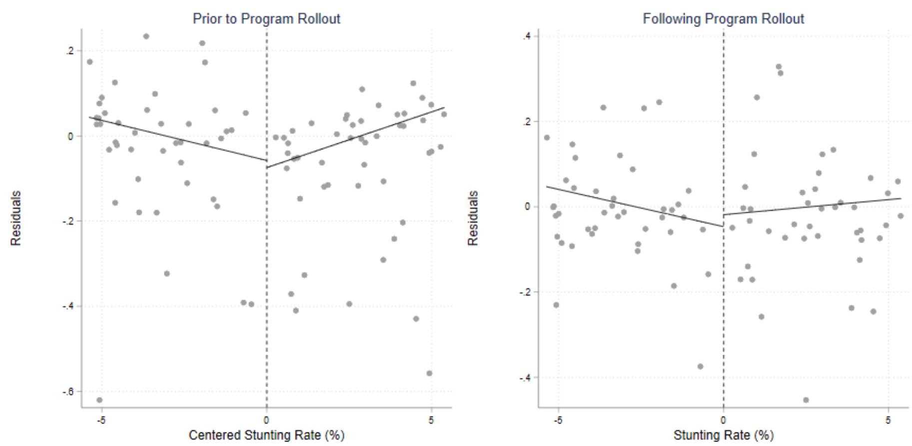
outcome because Ecuador has a health system that is partially public and partially private. Because Desnutrición Cero only provided transfers for consultations at public facilities, it is possible that the program better integrated mothers into the public health system and increased usage of public facilities (Flores, 2020).

The results of our estimations are presented in Table 5. Taken together, we do not find strong evidence of positive program effects on care-seeking outcomes, and most of the estimated coefficients are near zero. Although our estimates are somewhat imprecise, we can exclude the possibility of effect sizes greater than 20 percent on antenatal care, trained delivery care, public-sector delivery, and private-sector delivery. Despite our estimates' imprecision, visual inspection of the associated plots shows little movement at the 35 percent

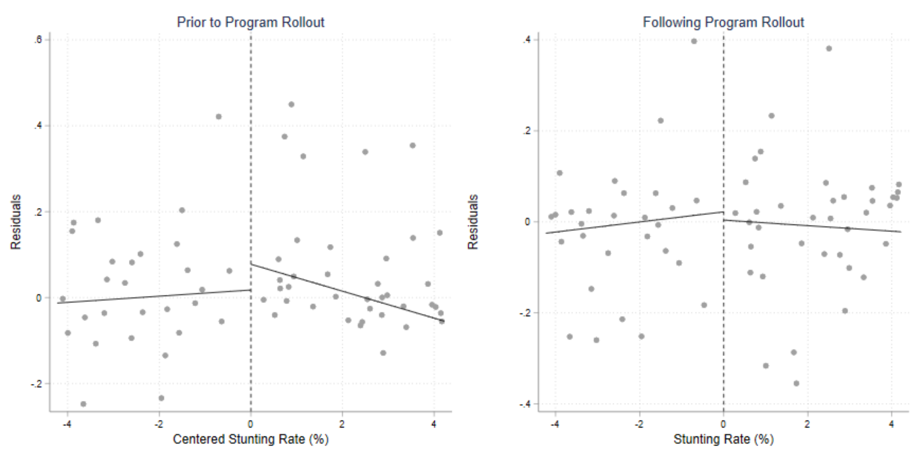
Fig. 8: RDD Plots: Care Outcomes



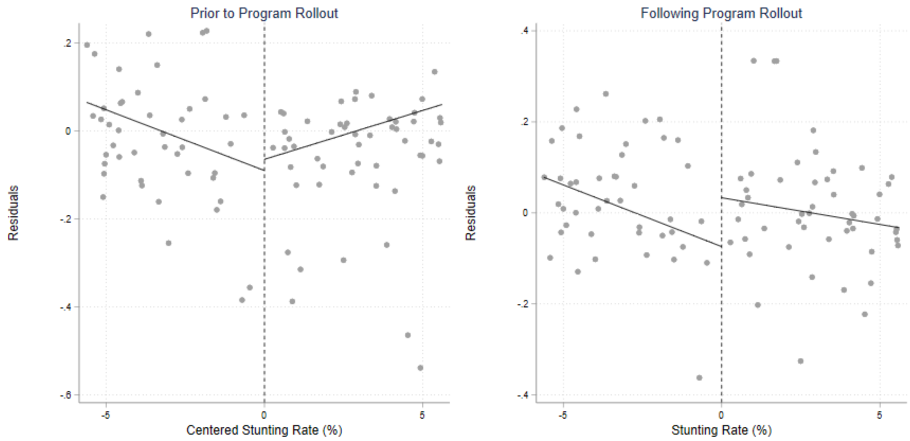
(a) Any Antenatal Care



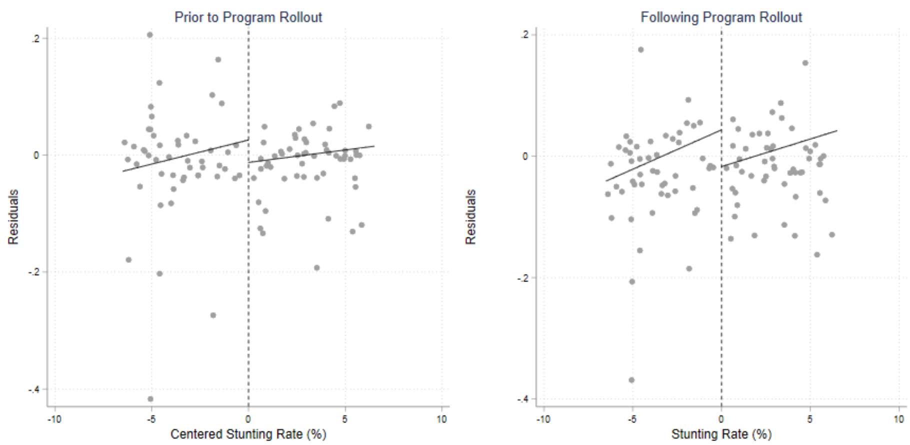
(b) Trained Delivery Care



(c) Home Based Birth



(d) Public Sector Delivery



(e) Private Sector Delivery

Notes: Figures are created using a mean squared error minimizing bandwidth, a triangular kernel, and evenly spaced variance mimicking bins defined in Calonico et al., 2015. Points represent the residuals of a regression of the relevant characteristic on canton and year fixed effects.

stunting cutoff after the program's initiation.

Our estimates on home-based births are less precise. Here, we cannot dismiss up to a 65 percent decrease in home-based births. However, since we can dismiss the possibility of large compensating shifts in facility-based births, it is unlikely that there are large effects. But it is possible that the program caused a moderate decrease in home-based births, which may help explain why we observe a slight decrease in children's birth weights, as children born in a facility are more likely to be measured. Presumably, among those who are measured, those born in a facility may also be more likely to be weighed directly after birth and to be weighed accurately.

Table 4: RDD Results: Care outcomes

	Antenatal	Trained Delivery Care	Home-Based Delivery	Public Sector Delivery	Private Sector Delivery
P=1	0.0135	0.00853	-0.0374	0.00429	0.00525
	[-0.0704, 0.0974]	[-0.0915, 0.109]	[-0.157, 0.0823]	[-0.109, 0.118]	[-0.0470, 0.0575]
P=2	0.0135	0.00800	-0.0385	0.00456	0.00421
	[-0.0704, 0.0974]	[-0.0918,0.108]	[-0.158, 0.0815]	[-0.109, 0.118]	[-0.0476, 0.0561]
P=3	0.0141	0.00953	-0.0459	0.00521	0.00637
	[-0.0696, 0.0979]	[-0.0907, 0.110]	[-0.167, 0.0750]	[-0.108, 0.118]	[-0.0476, 0.0603]
Control Mean (2011)	0.97	0.85	0.23	0.56	0.21
Bandwidth	5.19	5.38	4.22	5.61	6.50
First Stage F-Statistic (P=1)	27.28	21.49	34.94	28.87	27.26
First Stage F-Statistic (P=2)	26.91	20.95	34.71	28.59	27.35
First Stage F-Statistic (P=3)	26.77	20.65	35.04	28.65	27.55
Observations					
Prior to Intervention					
Left of cutoff					
Birth Events	56056	57567	41676	62227	76228
Parroquias	43	43	28	45	50
Right of cutoff					
Birth Events	53409	58023	45815	65746	66499
Parroquias	45	47	37	51	54
During Intervention					
Left of cutoff					
Birth Events	28928	29427	20423	30891	36715
Parroquias	43	43	28	45	50
Right of cutoff					
Birth Events	27421	29088	22059	31246	31564
Parroquias	45	47	37	51	54

$***p < 0.01, **p < 0.05, *p < 0.1$

Notes: For all columns, the unit of observation is a birth event. 95% Confidence Intervals are in parentheses. Sample restricted to the 10 provinces that contained treatment parroquias: Azuay, Bolivar, Chimborazo, Cotopaxi, Imbabura, Manabi, Morona Santiago, Pichincha, and Tungarahua. All estimates use an estimation window defined by the MSE-optimal bandwidth and a triangular kernel.

4.3 *Bandwidth Sensitivity Analysis*

Up to this point, the reader may have noticed that the Imbens and Kalyanaraman (2012) optimal bandwidths we present are large, although they are widely consistent across outcomes. This is likely because of our data's high level of dispersion and the choice to cluster our standard errors by the running variable. While we believe this is the correct methodology, it may be possible that we would detect treatment effects using a different bandwidth selection method.

To ease this concern, we conduct a cross-validation of our bandwidth selection, estimating equations (5) and (6) for our outcomes of interest using an array of different estimation windows. Figure 9 graphs the results of this cross-validation for our main outcomes. For care-seeking outcomes, we observe a moderate sensitivity of the point estimate to the choice of bandwidth. In each case, the estimate is higher in magnitude at smaller bandwidths. However, we note that no estimates are statistically significant, regardless of the estimation window. For birth outcomes, we observe a much lower sensitivity of the point estimate and, by extension, very little change in the overall interpretations. However, we make note of two findings. First, we observe that the result on birth weights holds for most estimations but loses significance at low bandwidths, likely reflecting a lack of statistical power. Second, we observe that at low bandwidths, there is a large and statistically significant increase in the probability that a birth has a birth weight reported. Although this result is not robust to changes in the bandwidth, this finding may give credence to our earlier explanation of the negative effect on birth weights. Overall, this cross-validation shows that our results are robust to various estimation windows.

4.4 *Sierra Region Robustness Check*

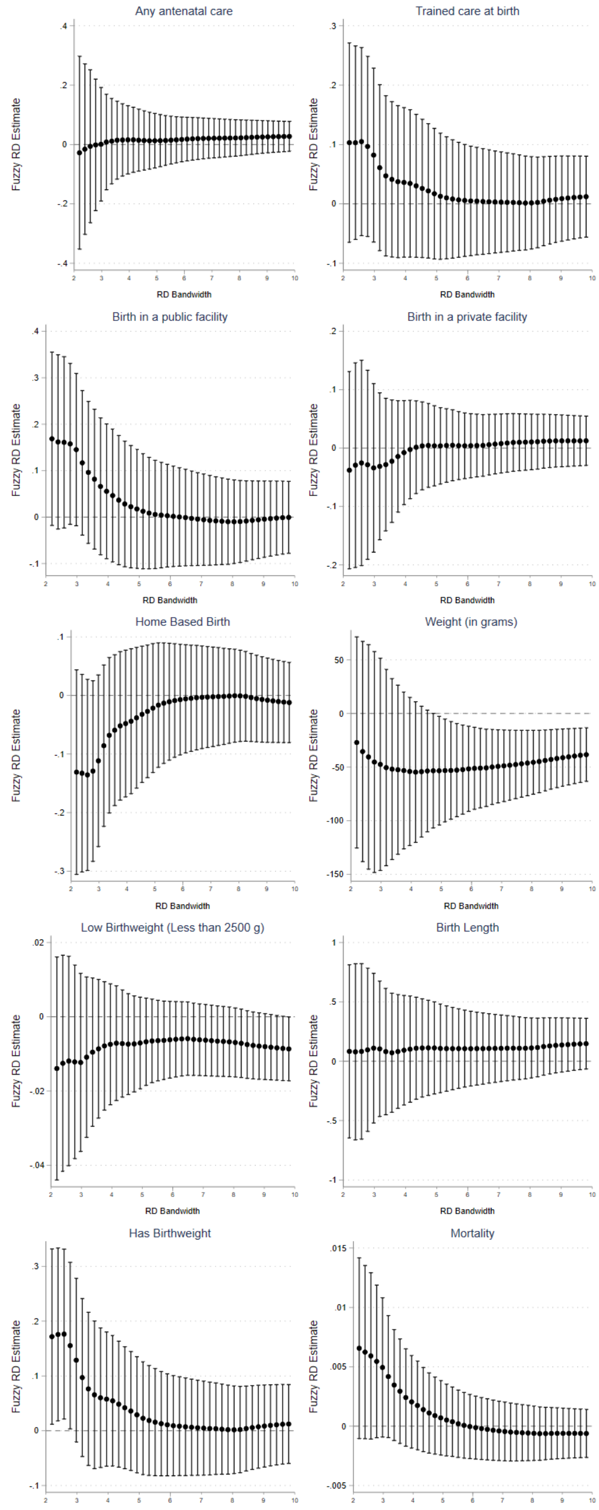
Figure 2 shows that the program is heavily concentrated within Ecuador's Sierra region. We also know that our outcomes of interest are highly clustered by region (more on this in section 5). This means that, in the spirit of LaLonde (1986), the choice of control parroquias in our estimation may have a large influence over our results. In particular, if birth weights and geographic accessibility to care are systematically lower in mountainous regions, including control parroquias from Coastal and Amazon regions may downward bias our estimates of treatment effects.

To estimate the potential severity of this bias, we restrict our sample to include only the parroquias in Ecuador's Sierra region and estimate our fuzzy regression discontinuity estimates. Figure 10 shows these results for our main outcomes across various bandwidths. We observe that, where we had not previously found results on rates of low birth weight and birth length, we do find statistically significant improvements that may be reflective of an improvement in in-utero conditions, despite the continued negative result on birth weights. While these results are promising, we must note that, despite their significance, the changes that we observe are very small relative to the outcome means. Thus, although the finds from this robustness check are discordant with our main findings in terms of statistical significance, we conclude that the inclusion of the coastal and amazonian regions in our analysis does not change the overall conclusion of this paper, that any evidence of program effects of Desnutrición Cero is weak at best. This signals that our overall conclusions are not as sensitive to choice of control as one may assume.

5 Discussion

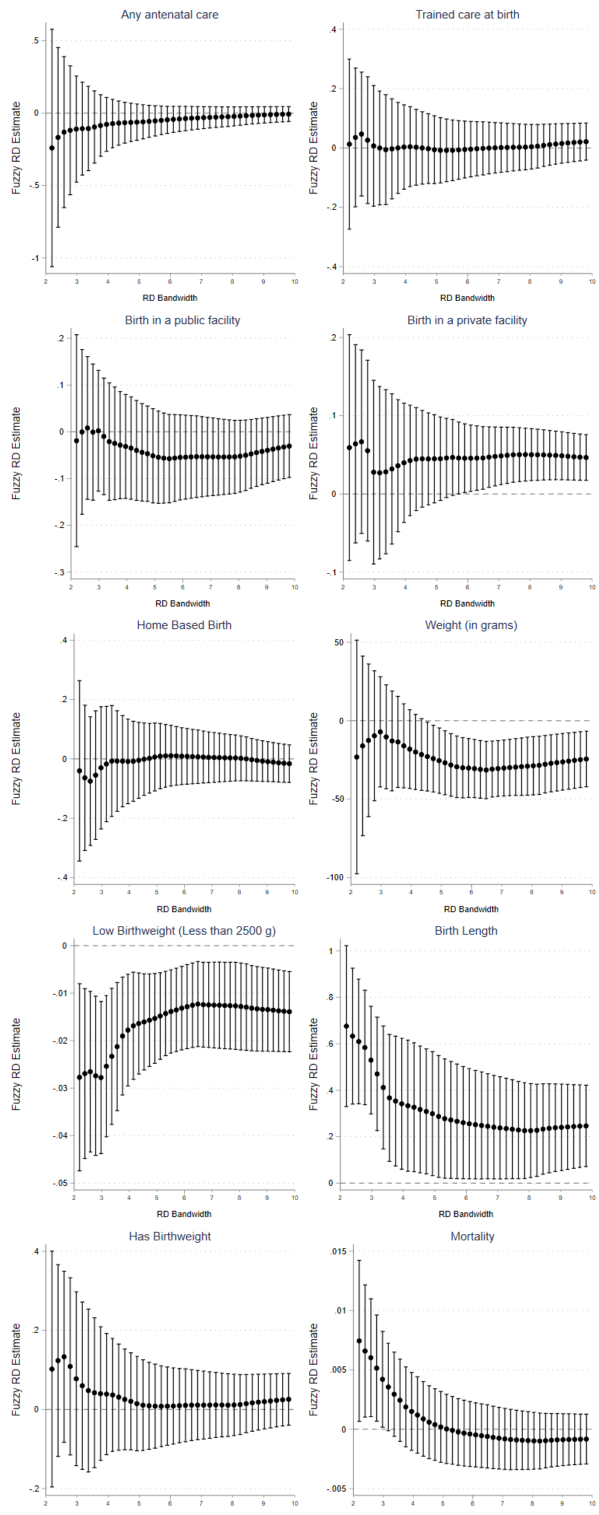
In our analysis, we do not find consistent evidence of a program effect on the tested pathways. The evidence we do find is instead suggestive of small improvements in birth outcomes.

Fig. 9: Bandwidth Cross Validation



Notes: Each panel displays the point estimate and 95% confidence interval for the relevant outcome at for a range of different bandwidths used for the local linear regression. All estimations are conducted using a triangular kernel and include year and region fixed effects.

Fig. 10: Sierra robustness check



Notes: Each panel displays the point estimate and 95% confidence interval for the relevant outcome at for a range of different bandwidths used for the local linear regression. All estimations are restricted to parroquias that make up Ecuador's Sierra Region. All estimations are conducted using a triangular kernel and include year and region fixed effects. Estimates at bandwidths 1.6, 2, and [3,4] were removed due to a locally weak instrument in order to increase readability

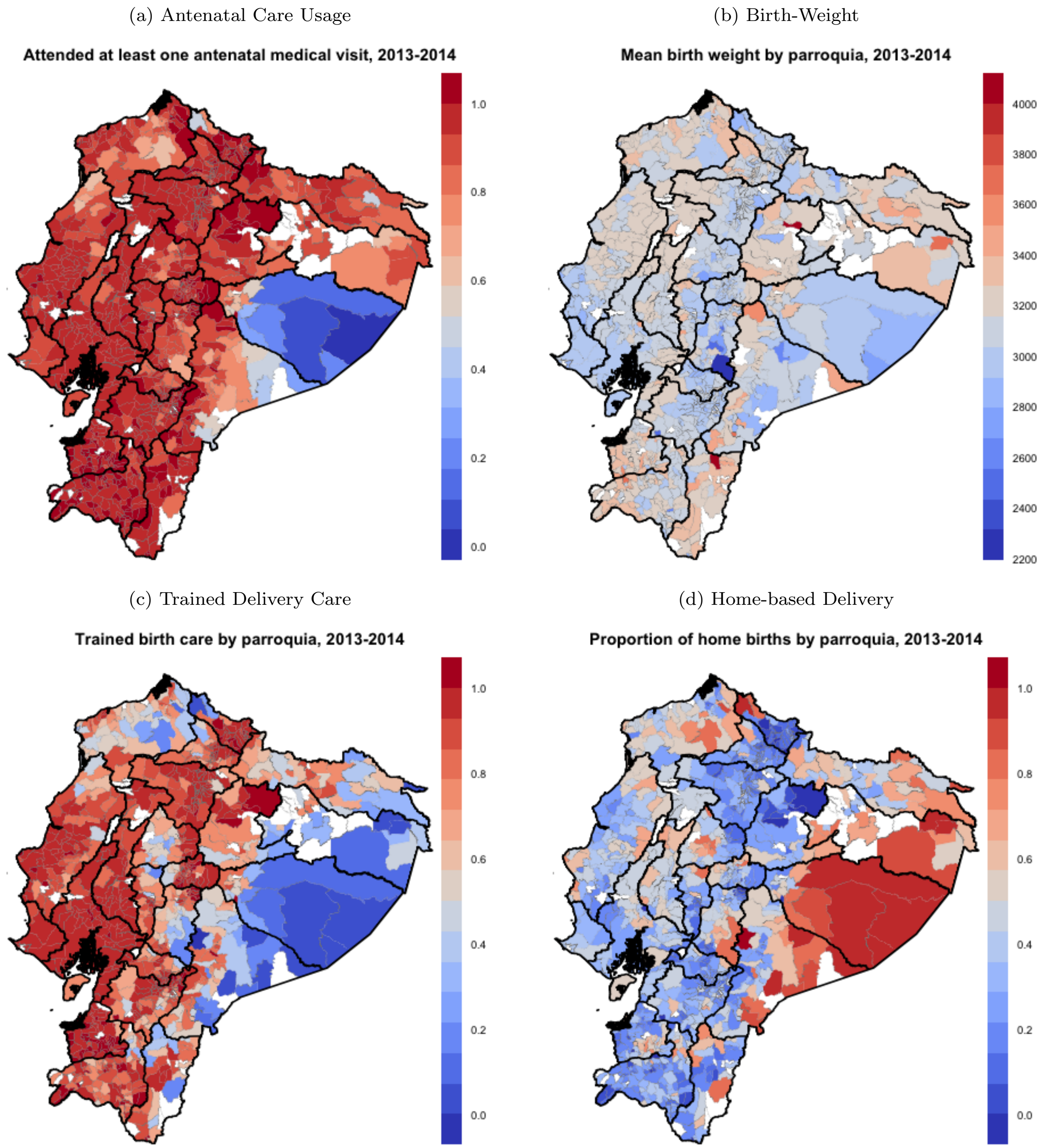
While this does not necessarily mean the program was not effective at decreasing childhood stunting, it does provide cause for measured skepticism. Before delving into further analysis of our results, we must first acknowledge that results from a regression discontinuity design are local by their very nature. This means that, while we believe our results are internally valid, they only measure the impact of the program on parroquias with a predicted stunting rate in the neighborhood of 35 percent. It is entirely possible, and perhaps likely, that parroquias with predicted stunting rates within the 50s and above face a systematically different set of challenges than those in our estimated sample. For these parroquias, perhaps the program displays larger effects. Therefore, we must practice caution before extending our results.

As our results fall within the null, we are unable to say for certain that the program was ineffective at increasing positive health-seeking behaviors and improving birth outcomes. For instance, it is possible that treatment effects are heterogeneous across predicted stunting levels. For example, we provide suggestive evidence of the program's incentives causing switching from home-based births to births in public facilities near the cutoff; it is possible the program incentivized a similar switch for villages with extreme stunting rates and that effect sizes are more exaggerated, causing greater improvements in birth outcomes. By definition, these villages are excluded from a regression discontinuity design, and any effects on them would not be detected in our analysis. Nevertheless, at the beginning of this paper, we introduced three possible reasons why we do not find evidence of a treatment effect, and these warrant further discussion.

First, as mentioned in Section 4.3, maternal outcomes in Ecuador are highly correlated by region. Figure 11 provides a mapping of our main outcomes by parroquia during the program period (2013-2015). These maps show that the main behaviors that Desnutrición Cero targeted, namely antenatal and delivery care usage, are quite high in Ecuador's Sierra region. We see that poor maternal care seeking indicators are centered in the Amazon region, where Desnutrición Cero is largely absent. Also, notice that the lowest birth weights in Ecuador are clustered in the Sierra region, where antenatal and delivery care usage is high. This may, in part, explain the negative coefficients on our estimations, but more importantly, it provides suggestive evidence that the barriers to improved birth weights in Ecuador lie somewhere other than maternal care. Indeed, this is in line with a recent Lancet series that argues there is a woeful under-focus on maternal nutrition as a determinant of birth and child health outcomes (Shekar et al., 2021; Victora et al., 2021).

Second, it is possible that the program is mistargeted. The efficacy of proxy-means tests has long been the target of skepticism (see Brown et al. (2016) for a study of PMT effective-

Fig. 11: Geographic Distribution of Main Outcomes

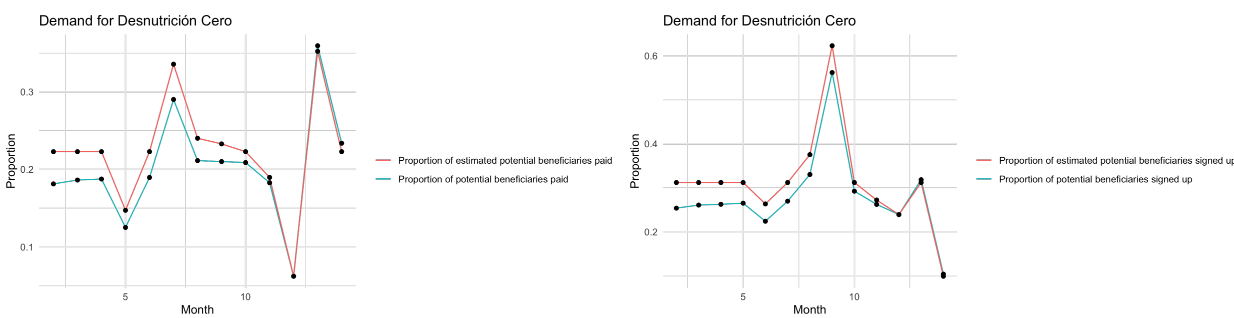


Notes: Each panel maps one of the main outcomes in this paper. Data for this figure is taken from administrative birth records. Based on the Authors' calculations.

ness). In the small areas version of the proxy means test employed by León and Vera (2010), the accuracy of the predicted stunting rates is largely determined by the fit of equation (1). In the estimations used to calculate the stunting rates for program selection here, the within-province R^2 values of equation (1) range from 0.5232 (Imbabura) to 0.2361 (Cotopaxi). This means that within Imbabura, their conditional expectation can explain roughly 52 percent of the variation in height-for-age Z-scores but can only explain 23 percent of the variation within Cotopaxi. With coefficients this low, it is entirely possible that the predictive accuracy of the 2010 malnutrition mapping used for program assignment is low. While this possibility improves our case for identification by implying that the predicted stunting rates we use are only somewhat correlated with true stunting rates, meaning which side of the cutoff a household falls on is somewhat random, it also implies that geographic mistargeting could certainly be an important contributor to our failure to detect any treatment effects. Finally, this study's main limitation is that we could not obtain granular administrative data on participation in and payments from Desnutrición Cero (Nacif, 2016). Figure 12 plots data for 2013-2014 on aggregate payouts and signups and suggests that the program signed up roughly 35 percent of the estimated potential beneficiaries noted in section 2 and that around 25 percent of these women and children received payment. It is possible that this takeup was either too low or concentrated among women who would not benefit the most from the transfer and incentives. However, without individual-level treatment data, it is difficult to make definitive statements about program efficacy beyond the lack of treatment effects we see in our analysis.

Fig. 12: Program disbursements and signups, 2013-2014

(a) Program disbursements (b) Program signups



Notes: The left panel displays the temporal distribution of Desnutrición Cero disbursements, while the right panel displays the number of program registrations. "Estimated potential beneficiaries" refers to the program demand estimates discussed in section 2.1. "Proportion of potential beneficiares" is the estimated number of births and pregnancies in treatment parroquias over the time period of the treatment. Figure based on administrative data from Nacif, 2016 and Authors' calculations.

6 Conclusion

This paper studies the impacts of Desnutrición Cero, a multi-faceted cash transfer program, on maternal health seeking and birth outcomes. We exploit a discontinuity created by a prioritized rollout and do not find evidence of improvement in care-seeking or birth outcomes beyond suggestive evidence of small improvements in birth outcomes. We show that our results are robust to several specifications and sensitivity analyses. These results contrast sharply with other evidence on maternity benefit schemes. However, rather than interpreting our results as a refutation of the previous evidence, our results highlight the importance of targeting and implementation in the efficacy of social protection schemes.

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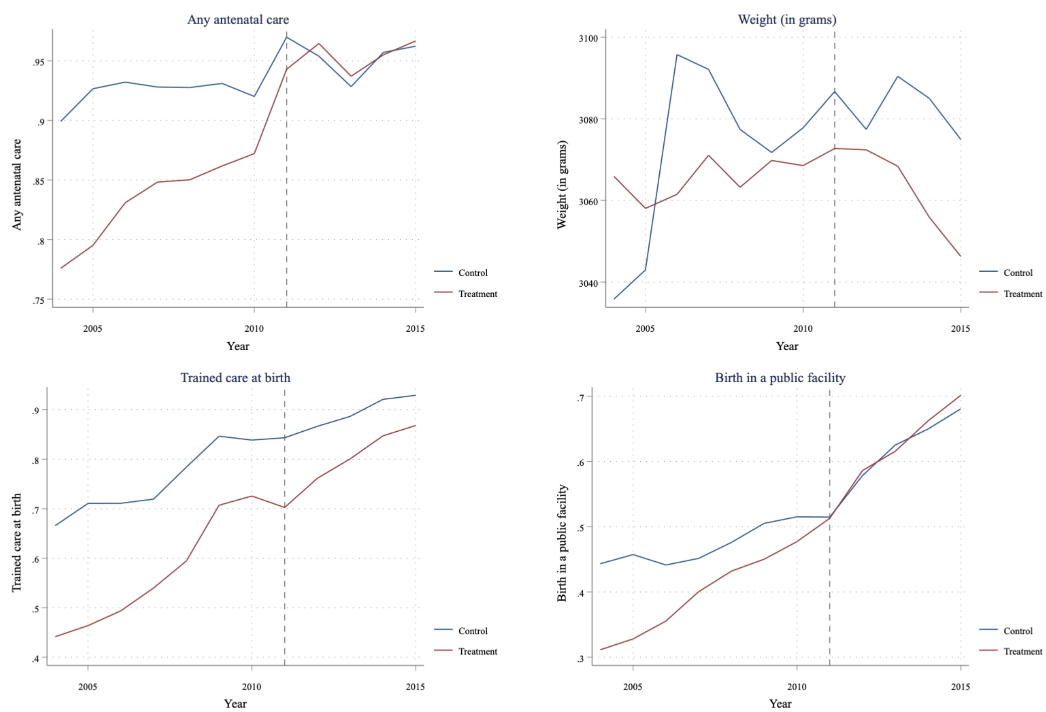
Appendix A: Pre-trends results

Several studies on cash transfer programs have relied on difference-in-difference estimators to establish the causal effects of social programs (see Schultz (2004) for a well-known example). To explore the feasibility of using a difference-in-difference estimator in the present context, we test pretrends using the same dataset as our main analysis (Ecuadorian Birth Register from 2005 to 2016). A key assumption of the difference-in-difference framework is that, if not for the intervention, outcomes for treatment and control areas would follow the same trends. While this assumption is fundamentally untestable, we may obtain a sense of the validity of the assumption by studying the trends of treatment and control areas prior to program assignment.

Figure A1 plots the trends in our main outcomes of interest between program and non-

Fig. A1: Trends in main outcomes within treated provinces



Notes: Figure displays the average trend in birth and care-seeking outcomes in study control areas versus treatment areas. Blue lines represent the average outcome across all relevant control parroquias. Red lines represent the average outcome across all Desnutrición Cero treatment areas.

program parroquias. We see that at the beginning of the panel, there seems to be large disparities in care between control and treatment parroquias that significantly narrows in the years immediately preceding Desnutrición Cero. This is particularly true for our main outcomes of antenatal-care usage and trained-delivery care. While these trends are promising in terms of health equity in Ecuador, they raise concerns about the use of a difference-in-difference estimator to evaluate Desnutrición Cero.

Although these figures hint that the parallel trends assumption may not hold in our context, we also test the narrowing of these differences empirically. Specifically we estimate the following model:

$$y_{i,p,t} = \sum_{t=2001}^{2011} \beta_t (T_p \times Year_t) + \beta_1 T_p + \gamma_t + X_{i,p,t} + \varepsilon \tag{6}$$

Where $y_{i,p,t}$ is the outcome of interest for birth-event i in parroquia p and year t. T_p is the treatment status of parroquia p, γ_t is a year fixed effect, and $X_{i,p,t}$ is a vector of birth-event level covariates. We utilize data from the beginning of our sample in 2004, until the year of program implementation start, 2011. In this estimation, significant values of β_t would signify that trends across treatment groups do not move in unison prior to program assignment.

Table A1: Pre-trend test

	(1) Antenatal care	(2) Birth weight	(3) Trained care	(4) Public-sector delivery
Treatment	-0.09***	-50.77***	-0.11***	-0.09***
	(0.01)	(10.62)	(0.01)	(0.01)
2004 X Treatment	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
2005 X Treatment	-0.01*	-11.36	-0.02***	-0.01
	(0.00)	(7.77)	(0.00)	(0.01)
2006 X Treatment	0.01**	-68.17***	-0.00	0.02***
	(0.00)	(7.68)	(0.00)	(0.01)
2007 X Treatment	0.02***	-56.03***	0.03***	0.05***
	(0.00)	(7.51)	(0.00)	(0.01)
2008 X Treatment	0.03***	-50.46***	0.03***	0.07***
	(0.00)	(7.19)	(0.00)	(0.01)
2009 X Treatment	0.04***	-40.38***	0.06***	0.06***
	(0.00)	(7.04)	(0.00)	(0.01)
2010 X Treatment	0.05***	-54.66***	0.07***	0.06***
	(0.00)	(7.04)	(0.00)	(0.01)
2011 X Treatment	0.08***	-56.26***	0.05***	0.09***
	(0.00)	(6.99)	(0.00)	(0.01)

$***p < 0.01, **p < 0.05, *p < 0.1$

Notes: For all columns, the unit of observation is a birth event. Sample restricted to the 10 provinces that contained treatment parroquias: Azuay, Bolivar, Chimborazo, Cotopaxi, Imbabura, Manabi, Morona Santiago, Pichincha, and Tungarahua.

Table A1 presents the results of the estimation of equation (4). The results from this support the intuition from Figure A1, suggesting that the parallel trends assumption is not satisfied. While the coefficients for birth weight are generally strongly significant, there is some variation in magnitude, making the results somewhat difficult to interpret. On the contrary, the results for antenatal care, trained delivery care, and public sector health seeking are much more clear. In these estimations, the point estimates of β_t are strongly significant and grow in magnitude across years. These results suggest that there were large gaps in outcomes during the year 2004 but these gaps narrowed substantially in the years preceding the roll out of Desnutrición Cero. These results show that inequalities in access to care within Ecuador were decreasing in the years prior to Desnutrición Cero, a trend that likely would have continued regardless of program rollout. As such, using a difference-in-difference estimator to measure of the effect of Desnutrición Cero on antenatal care or delivery care would likely be biased by these preexisting trends, leading us to overestimate the effects of the program.

Appendix B: Reduced form comparison

As another test of the strength of our first stage we display below our estimates from our main fuzzy RD models compared to results from equivalent sharp RD models. If we truly had a weak IV then we would expect little correspondence between the two sets of estimates. This is not the case, with the confidence intervals for the sharp estimates largely falling within those for the fuzzy estimates. This, combined with the first stage f-values presented in Tables 4 and 5 above, provides string evidence against a weak first stage.

It should also be clear when comparing the results from the fuzzy estimation to the sharp estimation, which in this case represents a reduced form estimation, that the point estimates from the sharp estimation are all strictly attenuated and more precisely measured. This result strengthens the claim that we have made throughout this paper, that there is limited to no evidence of an improvement in care or birth outcomes as a result of Desnutrición Cero, at least within the neighborhood of the program assignment cutoff.

Table B1: Sharp vs. fuzzy estimates

	Fuzzy		Sharp	
	RD Estimate	CI	RD Estimate	CI
Antenatal care visits	-0.27	[-1.33, 0.80]	-0.09	[-0.62, 0.43]
Trained care at birth	-0.01	[-0.13, 0.11]	-0.01	[-0.06, 0.04]
Home birth	0.01	[-0.12, 0.15]	0.01	[-0.05, 0.06]
Birth in a public facility	0.17	[-0.04, 0.38]	0.10	[-0.00, 0.21]
Birth in a private facilitiy	-0.18*	[-0.37, 0.01]	-0.09	[-0.19, 0.00]
Weight (in grams)	-70.17	[-221.98, 81.63]	-43.51	[-107.50, 20.47]
Low birthweight (¡2500 g)	0.02	[-0.02, 0.06]	0.01	[-0.01, 0.03]
Length (in cm)	0.31	[-0.44, 1.06]	0.16	[-0.10, 0.42]

$***p < 0.01, **p < 0.05, *p < 0.1$

Notes: For all columns, the unit of observation is a birth event. 95% Confidence Intervals are in parentheses. Sample restricted to the 10 provinces that contained treatment parroquias: Azuay, Bolivar, Chimborazo, Cotopaxi, Imbabura, Manabi, Morona Santiago, Pichincha, and Tungarahua. All estimates use an estimation window defined by the MSE-optimal bandwidth and a triangular kernel.