Child Disability and Parental Labor Supply

João Garcia[*] Rafael Latham-Proença[†] Marcela Mello[‡]

Joana Silva [§]

October 6, 2025

Abstract

Having a child with a severe congenital disability deeply impacts family life, yet evidence on the economic effects remains limited. This paper examines the impact of a child health shock causing lifelong disability on household labor market outcomes and fertility in Brazil. Using an exogenous shock to disability incidence—the Zika virus epidemic, which caused a surge of microcephaly cases leading to lifelong disability in children—we find that mothers of affected children reduce labor force participation, including informal work, while fathers' informal employment is reduced . The shock increases the motherhood penalty by 66 percent, and informal employment does not buffer the effects on formal employment. Brazil's disability payment system supplements income but intensifies maternal employment losses. We find no effects on fertility or partner separation. Results suggest that child health shocks and pandemics have persistent economic impacts beyond immediate health conditions and that, in developing countries, informality and welfare programs shape these impacts.

[*]Universidad de Santiago de Chile. Email: joao.garcia@usach.cl.

[†]Open Philanthropy. Email: rafaelproenca@g.harvard.edu.

[‡]Universidad de los Andes. Email: mmello@uandes.cl

[§]Universidade Católica Portuguesa and World Bank. Email: joana.silva@ucp.pt

Acknowledgement: We thank Anna Aizer, Michela Carlana, Anders Jensen, Eliana La Ferrara, Dan Björkegren, Andrew Foster, John Friedman, Emily Oster, Vítor Possebom, Flávio Riva, Jon Roth, Bryce Steinberg, and Neil Thakral for helpful comments and discussions.

1 Introduction

Children's characteristics substantially influence parental labor market decisions, with severe, permanent disability being among the most influential factors (Currie & Stabile, 2003; Dobkin et al., 2018). Women traditionally bear the additional caregiving burden, leading to larger reductions in maternal labor supply when children require extra care. This creates a double burden: disabled children need both more time and attention from parents and greater financial resources for medical treatment and accommodations. While understanding these household adjustments has direct implications for social program design, rigorous causal evidence on parental labor market responses remains limited.

In developing countries, where social programs are less extensive and informal labor markets more pervasive, informal employment may provide critical flexibility that helps families balance caregiving and income-generation demands (Fields, 1975; Meghir et al., 2015). Yet causal evidence on whether and for whom informality serves this compensatory role remains scarce, leaving unresolved how disability impacts parental longer-run economic outcomes in settings where informal work dominates and safety nets are less comprehensive. Understanding these adjustments is critical for assessing the welfare implications of childhood disability shocks.

This paper addresses this gap by studying the impact of a child health shock causing lifelong disability on household labor market outcomes and fertility in Brazil. We estimate the causal effects of child disability on parental employment, household composition, fertility, and income by exploiting the 2015 Zika virus outbreak in Brazil. The outbreak caused several thousand children to be born with Congenital Zika Syndrome (CZS), a severe disability associated with microcephaly and other developmental complications. The sudden onset of this epidemic and the characteristics of Zika transmission significantly mitigate concerns about endogenous maternal health behaviors.

Research on child disability and maternal employment faces significant challenges from unobserved confounders. Mothers who follow preventive health recommendations, such as taking folate supplements or avoiding smoking, are less likely to have children with disabilities. These same mothers likely differ in unobservable characteristics that also affect labor market outcomes. This confounding invalidates standard event-study approaches around birth, such as those used by Kleven et al. (2019) to study general child penalties. While recent literature has identified and addressed this issue in the context of child health and developmental problems, existing approaches are difficult to apply to congenital disabilities. To the best of our knowledge, no existing work on congenital child disability has explicitly addressed this endogeneity concern.

Among a sample of relatively poor mothers in Brazil, we show that affected mothers had similar pre-birth labor market trajectories to matched control mothers. However, starting at the end of typical maternity leave, their employment and earnings decline much more than controls. From six months after childbirth, mothers of children with CZS are 49.1% (4 p.p.) less likely to have formal sector employment than mothers in the control group, with effects persisting for at least 36 months. Fathers' formal employment appears unaffected, but there is a reduction in informal employment.

Turning to informal labor markets—a sector that accounts for roughly twice as much employment as formal work in our population—we provide novel evidence that challenges conventional wisdom. Rather than serving as a flexible buffer that allows mothers to balance work and caregiving, we find large negative effects on informal employment as well. This finding has important implications for understanding labor market adjustment mechanisms in developing countries. We show that social security payments provide an important income source for these households, partially offsetting lost labor income. Households receiving these payments show even greater reductions in maternal labor supply, consistent with an income effect. Despite increased caregiving demands and financial pressures that might strain relationships and depress fertility, we find no significant differences in family structure between families with CZS children and controls, including equal rates of subsequent fertility and no differences in separation rates.

We leverage three administrative datasets to conduct this study. First, SINASC logs all births in Brazil, providing details on delivery municipality and date, mother's residence and date of birth, and whether the newborn has CZS, through the RESP-Microcefalia supplement. Second, the Annual Social Information Report (RAIS) allows us to track individual employment histories throughout the study period, observing monthly earnings and hours worked. We link these datasets using the Cadastro Único (Single Registry), a federal database of all social program recipients. The Single Registry also includes information on informal labor. Linking these datasets is challenging due to the lack of common individual identifiers, so we exploit the fact that both the births dataset and Single Registry include mothers' dates of birth, children's birth dates, and municipality of residence. Our results are limited to families in this linked sample, which has implications for external validity.

To isolate the causal effect of child disability, we compare the labor market trajectories of mothers with CZS children to a matched comparison group. The control group consists of mothers with identical observable characteristics who gave birth in the same month and municipality as mothers with CZS children. We then compare the average employment rate between these groups each month relative to childbirth. This matching approach yields causal estimates due to the particular characteristics of the Zika epidemic. The unexpected nature of the outbreak and mosquito transmission make selection bias unlikely—since anyone in affected areas could have been exposed, and the sudden introduction of the virus meant no one could have known to take preventive measures. Even after public health authorities identified the outbreak, prevention had limited effect because infections during the first trimester of pregnancy were most likely to cause disability.

Other potential threats to identification are addressed due to characteristics of the virus itself. Selective abortion is unlikely because Zika infection is asymptomatic in most cases, leaving women unaware of their exposure, and microcephaly is difficult to diagnose before birth. Since Zika has no lasting effects on adults, we can rule out direct effects on labor supply that are not causally mediated through effects on the child. The fact that we observe nearly identical pre-birth labor market trajectories between treatment and control groups further corroborates that exposure is effectively random conditional on observable characteristics.

The literature on child disability's effects on parental labor supply remains limited. Early work includes Powers (2001), Salkever (1982), and Wasi et al. (2012). More recent studies examine congenital disability impacts in Taiwan (Chen et al., 2023; Cheung et al., 2025), Denmark (Gunnsteinsson & Steingrimsdottir, 2019), Norway (Wondemu et al., 2022), and Chile (Martínez et al., 2023). Our paper contributes to this literature by examining an arguably exogenous increase in congenital disability risk. While previous studies can only control for observable characteristics related to disability, our use of an exogenous shock provides stronger causal identification. Our study is among the first to rigorously examine how child disability affects both formal and informal employment using administrative data. This distinction is crucial for developing countries where informal work often dominates, yet most existing research focuses exclusively on formal employment due to data limitations.

We also contribute to the broader literature on parental responses to adverse child health outcomes (Burton et al., 2017; Frijters et al., 2009; Laffére & Schmidpeter, 2021; Wolfe & Hill, 1995). Most previous research focuses solely on maternal labor supply using survey data with limited follow-up capacity. Recent work using longitudinal administrative data examines parental responses to various child health shocks (Adhvaryu et al., 2022; Breivik & Costa-Ramón, 2022; Eriksen et al., 2021; Vaalavuo et al., 2023), but our study benefits from focusing on a congenital condition caused by an exogenous event, making it unlikely to correlate with parental behaviors.

2 Background

The 2015 Zika outbreak in Brazil provides an exogenous shock to child disability rates with characteristics that help isolate its effects on parental employment. The sudden and widespread nature of the outbreak addresses concerns about selection driven by preventive behaviors. Selective abortion is unlikely due to difficult in utero diagnosis, asymptomatic adult infections, and Brazil's restrictive abortion laws. The lack of lasting adult symptoms also rules out direct effects of the virus on labor outcomes independent of child disability.

2.1 The Zika Virus and CZS

Zika is a flavivirus transmitted by the Aedes aegypti mosquito, the same vector that carries dengue and chikungunya—diseases affecting around 2 million Brazilians annually. Originally endemic to tropical areas of Africa, Asia, and Oceania, Zika had never been observed in Brazil or anywhere in the Americas before 2014. The mosquito vector is endemic to most of the territory of Brazil, although there is variation in susceptibility, with some areas being unfavorable (Kraemer et al., 2015).

Zika exposure during pregnancy, especially in the first trimester, can cause Congenital Zika Syndrome (CZS) in children, a severe, lifelong condition. The most distinctive symptom is microcephaly, characterized by underdevelopment of the brain and smaller than normal head circumference. Children with CZS require frequent medical and parental attention, often suffering from seizures, vision and hearing problems, intellectual disabilities, and difficulties with motor development, speech, and feeding. While Brazil's public healthcare system provides free medical care, families often face access barriers and need additional services not covered by the government.

2.2 The 2015 Outbreak

The virus was introduced to Brazil around 2014. The outbreak was first identified in late 2015 following a spike in microcephaly cases. For months, Brazilian researchers had observed a new dengue-like illness before identifying it as Zika, and the link to birth defects was unknown. In endemic areas, CZS is not observed because women typically develop immunity before pregnancy.

CZS cases increased rapidly but fell after several months. Figure 1 shows the epidemic timeline: during the second half of 2015, cases jumped from nearly zero to peak incidence in about three months. The subsequent decline was equally rapid, with a modest second wave in late 2016. This compressed timeline limited opportunities for preventive measures. Figure 2 shows CZS rates per 1,000 births across microregions in 2015-2016. While the Northeast had higher incidence rates, cases appeared in all states with no clear spatial pattern.

2.3 Zika Characteristics Relevant for Identification

Several characteristics of the Zika epidemic make it an ideal natural experiment, as they rule out or strongly mitigate key identification concerns. In this section we detail these characteristics.

Selection and Preventive Behavior A primary concern is whether unobserved characteristics correlated with labor market outcomes—such as health-conscious preventive behavior—influenced virus exposure. This mechanism is unlikely to cause bias given the outbreak's timeline. Zika and CZS were completely unknown in Brazil before the epidemic. Researchers first observed signs of a new disease in March 2015 and identified increased microcephaly rates in October 2015. The causal link between Zika and birth defects was only established in early 2016, so mothers could not have taken informed precautions before then.

Even after the public health emergency was declared, preventive measures would have limited immediate impact. Since Zika is most likely to cause CZS during the first trimester, effects remain undetected for months (Cauchemez et al., 2016; Johansson et al., 2016). A mother taking additional precautions after the emergency declaration would only affect births in the second half of 2016, when prevalence was already low. For instance, supposing a woman is three-months pregnant during the immediate follow up of the public health crisis warning, and started changing her behavior accordingly, she would deliver in the second half of 2016, already far past the peak of the number of cases. In effect, the infections responsible for most of the observed cases happened *before* the public was aware of the epidemic.[1]

Selective Fertility and Abortion Selective fertility responses to the outbreak are unlikely to bias our estimates for similar reasons. Any fertility decisions following widespread recognition of the outbreak's severity would only impact births at least nine months later, when cases had already fallen dramatically.

Differential abortion rates are also unlikely to threaten identification. First, CZS

[1] One important caveat is that, since Zika has the same vector as dengue, it could be that health-sensitive mothers were protected from Zika due to efforts to prevent dengue. While we cannot totally rule out this concern, we believe it is minimized by the fact that dengue is difficult to prevent at the individual level. Most prevention efforts are based on controlling the vector through community-based programs, biological or chemical methods (Khan et al., 2023; Murray et al., 2013).

is difficult to diagnose in utero, requiring expensive imaging with approximately 50% false positive rates (Chervenak et al., 1984; Leibovitz & Lerman-Sagie, 2018). Second, Zika infection is often asymptomatic or resembles dengue, making it difficult for mothers to know if they were infected. Third, even among infected mothers, the probability of developing microcephaly is relatively low (2.6 to 4%) (Ximenes et al., 2023). Finally, abortion is illegal in Brazil except for cases involving rape, serious maternal health risks, or fetal anencephaly.

Direct Effects on Maternal Health Unlike the severe effects on newborns, Zika infection has no lasting impact on adults and should not directly affect labor supply. About 80% of adult cases are asymptomatic, with others experiencing only mild fever and rashes lasting about a week (Haby et al., 2018).[2] Thus, we can rule out that any effects we observe are contaminated by direct effects to the mother not mediated by the effect on the child.

Infant Mortality Considerations Children with CZS have high mortality rates, though rates in our sample are considerably lower, possibly due to selection effects. Our main results do not adjust for this difference, meaning effects may partially reflect child mortality rather than solely permanent disability, though the direction of any bias is ambiguous. See Appendix C for a detailed description and analysis of the impacts of infant mortality to our results.

[2]One exception is that there have been reports of an increased chance of developing Guillain-Barré syndrome, a severe, potentially lethal condition. However, even this increased risk is extremely rare and would not have any relevant impact on our results.

3 Data

Our main data source for cases of CZS is RESP-Microcefalia, an administrative dataset compiled by public health authorities following all documented cases in the country. Formal employment links come from the Annual Social Information Report (RAIS), an administrative dataset made available by the Ministry of Labor. Finally, we use the Single Registry (*Cadastro Único)* to link these two datasets and as a source for informal labor. The Single Registry is an administrative dataset used to manage and coordinate various social programs, covering virtually all of Brazil's poor population.

CZS cases To identify the children affected by the Zika epidemic, we rely on RESP (*Registro de Eventos em Saúde Pública*), a publicly available surveillance registry of suspected cases of CZS in Brazil. These data were compiled and maintained in response to the Zika outbreak, starting in late 2015, but including retrospective cases.

The data include the municipality of birth, the mother's municipality of residence, date of birth, sex, mother's age, head circumference, and the presence of other anomalies. Health authorities follow cases and record eventual deaths. Cases are also classified on whether the diagnosis is confirmed as CZS (probable, confirmed, discarded or under investigation). We follow the standard practice and include all live births with a final classification of confirmed or probable CZS in our sample.

Our sample includes cases born from 2015 to 2017, and classified as confirmed or probable CZS, following the practice in the epidemiological literature.

Formal Labor Market To observe mothers' and fathers' formal labor market outcomes, we use RAIS, an administrative dataset covering all formal employment links in Brazil. It is collected by the Ministry of Labor in a compulsory survey of all firms and their registered workers, covering all formal labor links in the country. RAIS provides information on workers' demographics (age, gender, schooling, race), job characteristics (occupation, wage, contractual hours), hiring and termination dates, and personal tax ID (CPF). We use the RAIS extractions from 2013 to 2019, and construct monthly employment status from the hiring and termination dates.

If we find a person at least once in RAIS, we can re-construct her formal employment history. If we do not see her any year, then we know she has never worked in the formal sector. Our main measure of formal employment is an indicator of the person appearing in the RAIS dataset in that month with at least one job reporting a non-zero amount of hours per week.

Single Registry To link the household members, we use the Single Registry (*Cadastro Único*) to observe families' characteristics and link different family members to formal employment data. The Single Registry is a federal registry used for several social programs to verify eligibility and track recipients over time. It started exclusively as Bolsa Família's administrative database but became the primary federal dataset on poverty. More than 20 social programs use it, covering virtually all of Brazil's poor (Campello & Neri, 2013), and roughly the bottom 50% of the income distribution. Single Registry aims to include all households with income per capita below one-half of the minimum wage (R$255 in 2010). We use annual snapshots of the program from 2014 to 2019.

To be eligible for any government benefit that uses the Single Registry, families must have a valid registration (complete and up-to-date), updated at least every two years. They must undergo interviews with local government agents, including a standardized questionnaire on their earnings, living conditions, demographic and occupational characteristics, and personal tax ID (CPF). They are required to inform authorities of relevant changes to family size or income.

We are able to study informality by relying on self-reported employment in the Single Registry.[3] These results should be interpreted cautiously for two main reasons. First, it is self-reported, in a context where respondents may have an incentive to misreport their earnings downwards, to avoid losing benefits. Second, unlike with formal employment, we only observe these outcomes in years when the families are in the Single Registry, so there is a risk of selection bias. Nevertheless, this is valuable source of information on a crucial part of the Brazilian economy that is usually opaque.

Linking the Datasets The absence of personal identifiers in the RESP data precludes direct linkage to RAIS or the Single Registry. We deal with this challenge using the mothers' date of birth, municipality of residence, and date of childbirth, available on Single Registry. After identifying the control and treated mothers in the Single Registry, we use their tax ID to accurately match them with RAIS records.

Since we rely on the Single Registry to be able to link the datasets, our population of study is restricted to families in the Single Registry. This restriction means that our results should be understood to apply to lower-income individuals. It also implies that the variation in income and socioeconomic status in the sample is relatively small. Appendix A has further detail on the linkage process and sample definition.

4 Empirical Strategy

We exploit the sudden nature of CZS incidence during the Zika outbreak to identify causal effects on family labor market outcomes. Our identification strategy leverages the fact that, conditional on exposure to the epidemic environment, CZS occurrence

[3] Specifically, we code as informal labor the following categories: a) self-employed (*Conta própria*, b) employed informally, c) temporary agricultural works if they are not present in the RAIS, d) people who say they did work during the year, did not declare the category and are not present in the RAIS.

was largely unpredictable and unrelated to unobserved family characteristics that might independently affect labor market outcomes. We combine a matching strategy to select controls with careful examination of pre-trends to ensure a clean comparison group. We present three classes of results: the time series of treated and controls with their differences over time; the simple average difference after childbirth; and the Difference-in-Differences average.

Our approach builds on the unique characteristics of the Zika outbreak, which create ideal conditions for causal identification. We match mothers of affected children to unaffected mothers in the same municipality and month with similar observable characteristics, ensuring that spatial and temporal patterns of the epidemic, as well as individual risk factors, are balanced across treatment and control groups.

We rely on empirical and theoretical insights from the epidemiological literature to inform our matching procedure. Because the vector is common to dengue and chikungunya, we can draw on the epidemiological literature on these diseases to identify risk factors. The *Aedes aegypti* is more active during the rainy season, because its life cycle depends on stagnant water (Lowe et al., 2011). Therefore, the concentration of vectors that make infection possible varies with the physical conditions of each area and with seasonal climate. Other municipality-level risk factors include urbanization and population density (Wu et al., 2009). Individual-level risk factors identified in the literature include age and education level (Siqueira-Junior et al., 2008), employment (Teurlai et al., 2015), and socioeconomic status (Delmelle et al., 2016).

For our main results, we compare the outcomes for families of children born with CZS to matched control families with children without this anomaly. We match families to controls identical in a set of variables: year and month of birth of the child, place of birth, mother's age, mother's education (binary indicator of high school completion), self-identified white race and an indicator of primiparity. These variables were selected according to the results from the epidemiological literature and data-driven balance tests.

Place and time of birth are crucial variables to ensure the controls are exposed to the same environment as the treated. Age and race (as a socioeconomic proxy) are identified as relevant risk factors in the epidemiological literature and matter for labor market outcomes. Finally, parity is included because labor market effects are often different between firstborn and later children.[4] Table 1 shows the sample size from raw data, through linking with the Single Registry and matching. Appendix A gives further detail on the sample definition and matching procedure.

We form strata, S, groups of observations defined by having identical matching variables. That is, all observations of mothers who gave birth during the same month, in the same place and are identical in other relevant variables constitute one stratum. Denote $n_t(s)$ and $n_c(s)$ the number of treated and control units in stratum s, respectively. We weight treated units by 1, and control units by $w_s = \frac{n_t(s)}{n_c(s)}$, such that each stratum has the same weight on treated and control units. Weights are constant over time.

We estimate the treatment effect through a fully saturated cell-means model. Let time from childbirth be $k \in \kappa = (-18, ..., 36)$. The specification is:

$$y_{i\tau} = \sum_{k \in \kappa} \mu_k 1\{\tau = k\} + \sum_{k \in \kappa} \beta_k 1\{\tau = k\} \cdot T_i + \varepsilon_{i\tau} \tag{1}$$

[4] While income is arguably an important determinant of CZS during this period (Barbeito-Andrés et al., 2020; Lobkowicz et al., 2021; Souza et al., 2018), we do not include it as a matching variable. The first reason is that our sample is already restricted to families in the Single Registry, and therefore lower-income. The second is that, although income was not explicitly matched on, we show that the controls are identical to treated units in mothers' and fathers' earnings before childbirth. Lastly, we need to balance the goals of balancing the covariates and maintaining a large, representative sample; including too many control variables may reduce power and introduce bias.

where $y_{i\tau}$ is the outcome of interest of family i in period τ, and T_i denotes the treated group. Thus μ_k captures the average of the outcome at period k for control families and β_k captures the difference for treated families. Note that this is equivalent to comparing the weighted averages in each period, except that the regression model allows us to properly cluster standard errors, and account for serial correlation. We cluster the errors at the stratum level.[5]

From this model, we present two sets of estimates for the main result for each outcome: simple differences and Difference-in-Differences. The first corresponds to the average difference between treated and controls from months 9 to 36, i.e. $\frac{1}{28}\sum_{k=9}^{36}\beta_k$. We start at month 9 because we do not expect differences to start during the maternity leave, allowing a few months for the initial adjustment. For yearly outcomes, we present results from years 1 to 4: $\frac{1}{4}\sum_{k=1}^{4}\beta_k$.

Our identification hypothesis is conditional mean independence (CMI) of untreated outcomes within strata:

$$E[Y_{i\tau}^0|T_i = 1, S = s] = E[Y_{i\tau}^0|T_i = 0, S = s]$$

This approach effectively compares families that were equally exposed to the environmental and temporal conditions of the Zika outbreak but differed in whether their child was affected. As discussed in Section 2, the characteristics of the outbreak rule out several threats to identification, making it plausible that unobserved characteristics, such as mothers' behaviors, are not correlated to the chance of having a child with CZS. Given this hypothesis, β_k identifies the ATT: $E[Y_{ik}^1 - Y_{ik}^0|T_i = 1]$.

The second set of results are Difference-in-Differences estimates. These are con-

[5]Note that, since strata nest mothers/families, this is strictly more conservative than clustering at the mother/family level.

structed from the same underlying model by differencing out the baseline differences between treated and control (or equivalently, adding a treated group indicator and omitting β_k parameter corresponding to the baseline period): For monthly outcomes we use nine months before childbirth as the baseline reference: $(\frac{1}{28}\sum_{k=9}^{36}\beta_k) - \beta_{-9}$, while for annual outcomes we use the year before: $(\frac{1}{4}\sum_{k=1}^{4}\beta_k) - \beta_{-1}$. In this case, the key identification assumption is the standard conditional parallel counterfactual trends (with t_0 denoting the chosen baseline):

$$E[Y^0_{i,t} - Y^0_{i,t_0}|T_i = 1, S = s] = E[Y^0_{i,t} - Y^0_{i,t_0}|T_i = 0, S = s]$$

Ensuring that the baseline differences are indistinguishable from zero during the matching stage provides several advantages in this context. Under this condition, the two estimation strategies are equivalent. The CMI assumption is not more restrictive than parallel trends, provided we can show zero difference at baseline. Furthermore, for a variable with a limited range like employment, the parallel trends assumption may imply impossible values for the counterfactuals when there are baseline differences. By showing and testing for zero baseline differences, our approach is more transparent and robust to this issue. We detail these claims in Appendix B. For all our results, the simple difference and DiD estimates are within each others 95% confidence interval.

5 Results

We find that mothers of children with CZS experience substantially larger employment reductions than mothers of unaffected children. Formal sector employment falls by an additional 4 percentage points—representing a 66% increase in the standard motherhood penalty. This effect persists for at least three years and is mirrored by similar reductions in informal employment, challenging the conventional view that informal work provides flexibility for caregiving families.

Despite reduced parental labor supply, household incomes actually increase due to Brazil's social assistance program for disabled persons. More than 60% of affected families receive social security payments equivalent to one minimum wage by the fourth year. We find evidence that these transfers amplify the employment reduction, consistent with income effects reducing labor supply incentives.

Fathers show a reduction in informal employment, but little effect on earnings, and we find no effects on fertility or family stability. The burden of increased caregiving falls disproportionately on mothers, with limited household adaptation along other margins.

5.1 Summary and Balance

Table 2 shows summary statistics for families with CZS children and matched controls. The unweighted control sample shows substantial imbalances, particularly in maternal education (29% vs 37% high school completion) and age (23.1 vs 24.6 years), reflecting the greater availability of potential matches among younger, less educated mothers in the Single Registry population.

After reweighting controls to match the CZS sample, the matched characteristics are identical by construction. Importantly, unmatched characteristics also show good balance, with all p-values exceeding 0.16. Pre-birth employment rates and wages are nearly identical between groups, with CZS mothers showing slightly higher formal employment in both years before childbirth. This pre-treatment balance supports our identification strategy and suggests that conditional on the matching variables, exposure to CZS was effectively random.

5.2 Formal Labor

Figure 3 demonstrates remarkable pre-treatment balance in maternal employment between CZS and control families. Monthly employment rates are virtually identical in the 18 months before childbirth, with CZS mothers showing slightly higher rates if anything. This validates our matching strategy and rules out pre-existing differences in work patterns. The divergence begins after maternity leave ends. Control mothers experience the standard motherhood penalty, with formal employment falling 8.1 percentage points from pre-birth levels—a 57% reduction. Mothers of children with CZS face an additional 4 percentage point drop, representing a 66% larger penalty than controls. By 36 months post-birth, only 2.6% of CZS mothers remain formally employed compared to 8.8% of controls. These employment effects translate directly to earnings losses. Earnings patterns mirror employment closely, with the temporary spike around four months likely reflecting severance payments and unused vacation pay.

We find no adjustment on the intensive margin (Figure 4). Wages conditional on employment increase slightly for both CZS and control mothers who stay in formal work, while hours stay flat. Part-time employment offers limited flexibility in this context; 70% of employed mothers work the standard 44-hour week, with less than 10% working part-time schedules.

Fathers show no significant formal employment response to having a child with CZS. Figures 5 and 6 show similar pre-birth employment trajectories and post-birth patterns between fathers of CZS and control children. The sample for fathers is much smaller since fathers are present in only a fraction of households.[6] While we observe a small employment increase for employed fathers of CZS children after 30 months, overall employment and earnings effects are not statistically significant. We also see a suggestive

[6] We only include cohabiting fathers in our study.

immediate increase in conditional wages which is significant in the main specification but not robust to the DiD specification. The increased caregiving and financial burden falls entirely on mothers.

5.3 Informal Labor

The informal labor market represents a critical dimension often overlooked in studies of parental employment responses. In our sample, baseline informal employment rates are more than double those in the formal sector, highlighting the importance of this margin for low-income Brazilian families. Table 4 presents the summary findings.

Figure 7 presents our results. While control mothers experience only a modest and temporary reduction in informal work, consistent with its flexibility advantages, mothers of CZS children face large and persistent declines. The contrast with formal employment patterns, where the maternal penalty for controls is more persistent, is instructive: informal work appears to offer options for typical families navigating the standard motherhood penalty, but provides no buffer against the intensive caregiving demands of a severely disabled child.

By the first year post-birth, CZS mothers are 12 percentage points less likely to work informally than controls. This gap widens dramatically over time, reaching 27 percentage points by the fourth year. The trajectory suggests that, while it may play a role in the first few years of the infant's life, informal work is increasingly less important for affected families.

We find no evidence of adjustment on the intensive margin within informal work (Figure 8). Conditional on participation, CZS mothers show no significant changes in months worked per year or reported wages. This pattern mirrors our formal sector findings and reinforces that the primary adjustment occurs through complete exit from employment rather than reduced work intensity.

For fathers, in contrast with the results in formal employment, we find a strong reduction in the probability of working in the informal sector during the year (Figure 9). However, results on earnings are not statistically significant and intensive margin indicators are positive but imprecisely estimated (Figure 10). This suggest a composition effect, with fathers with just occasional, low earnings dropping out.

These results challenge the conventional wisdom that informal labor markets provide flexibility for families facing caregiving demands. In the context of severe child disability, neither formal nor informal work arrangements appear capable of accommodating the intensive care requirements, leading to wholesale withdrawal from market work.

5.4 Income and Expenses

Despite the substantial reduction in maternal labor supply, total household income for CZS families actually increases relative to controls. This counterintuitive finding reflects the offsetting effect of Brazil's disability benefits system. Figure 12 shows the effects on total household income over time, and on the probability of receiving the BPC. Total income increases every year for families of children with CZS, in conjunction with BPC recipient status. By the fourth year post-birth, more than 60% of CZS families receive BPC payments equivalent to one minimum wage monthly. The net effect is higher total household income for affected families compared to controls.

Figure 12 disaggregates the income effects. While household labor income falls due to reduced maternal employment, this decline is more than compensated by increases in social security payments. The increased income translates to higher household expenditures across multiple categories. Food expenses show the largest increase, consistent with the specialized nutritional needs of children with CZS who often require formula feeding due to swallowing difficulties. Medical expenses and rent also rise significantly, while other household bills show modest increases.

These expenditure patterns align with the documented care needs of CZS children (Fernandes et al., 2022). Beyond the direct medical costs, families face indirect expenses including specialized housing needs and increased food costs. The expenditure increases validate that the income transfers are meeting genuine additional needs rather than simply substituting for lost earnings.

The income and expenditure evidence provides important context for interpreting our employment results. The availability of substantial government transfers partially explains the large employment reductions, as families can maintain or even increase consumption while reducing work. However, the specific expenditure patterns confirm that these transfers address real additional costs associated with caring for severely disabled children.

5.5 Social Assistance Benefits

The interpretation of our results depends crucially on the provision of social security payments for families with disabled children. ”We find that negative employment effects are present for both recipients and non-recipients of social assistance, though effects are substantially larger for recipient families. Importantly, this heterogeneity appears related to the benefit itself rather than pre-existing characteristics of recipient families.

The role of social assistance is a first-order concern, since Brazil has a relatively well developed social safety net. In particular, low-income households with a disabled member can be entitled to the *Benefício de Prestação Continuada* (BPC), which pays the equivalent of one minimum wage.[7] This fact raises the question of how much our estimates are driven by these payments.

To analyze this question, we identify families who are receiving the BPC two years after the year of childbirth.[8] We present regressions of the form:

$$Y_{it} = \alpha + \beta CZS_i + \gamma BPC_s + \delta CZS_i \cdot BPC_s + \varepsilon_{it} \tag{2}$$

Where CZS_i is an indicator of the family of a child with CZS and BPC_s being the stratum-level indicator of families who receive the BPC by the second year after childbirth. Note that BPC_s is defined at the stratum level: it denotes strata where the treated units receive the BPC. This specification preserves balance between comparison units. It is possible that BPC recipients had lower employment at baseline; a specification that simply includes an individual level indicator may confuse the differential effect of CZS for recipients with this lower baseline level. By adding the stratum-level indicator, we control for baseline differences. Therefore, δ identifies the differential effect for recipients:

$$E[Y^1 - Y^0|T = 1, BPC(s) = 1] - E[Y^1 - Y^0|T = 1, BPC(s) = 0]$$

To assess the validity of the strategy, we employ two falsification tests. First, we estimate a placebo, consisting of the same specification using employment one year before

[7]To be eligible, the household income must be less than one quarter of the minimum wage per household member. The recipient must also prove their physical or mental disability precludes normal independent participation in society, including labor. This benefit is also available for people over 65 in low-income households.

[8]We focus on two years after childbirth because effects are not fully manifested in the first year, and for the later part of our sample, born in 2017, we don't have data for three years after childbirth.

childbirth as the outcome. If we do find significant differences, this would indicate some imbalance at baseline.

Second, we flexibly control for the propensity score of receiving the BPC and its interaction with CZS. This ensures that the heterogeneity we are identifying is indeed associated with the BPC itself, and not with demographic differences that predict BPC recipient status. For instance, we show that severe microcephaly is a significant predictor of BPC recipient status. Suppose families of children with severe microcephaly reduce their labor supply even more than other CZS cases. This would appear as BPC-related heterogeneity. By controlling for the propensity score and propensity score interacted with the treatment, we ensure the heterogeneity is not associated with this type of characteristic. For the same reasons we included BPC at the stratum-level, we also include the Propensity Score at the stratum-level and add its interaction with CZS. See the Appendix D for details on the propensity score estimation.

Table 6 presents the results on formal employment. In the first panel, the outcome variable is formal employment two years after childbirth. In column 1 we find that non-recipients have employment about 7.7 p.p. lower than their respective controls. The difference in recipients is larger by 3.7 p.p. relative to their controls, but the interaction is not statistically significant. Columns 2 and 3 include the propensity score and its interaction with CZS; both terms are added with the propensity score centered around its mean (37%), such that the coefficient on CZS corresponds to effect at the mean propensity score. The results for nonrecipients are essentially unchanged, but the interaction effect becomes stronger and statistically significant at 5%, indicating a 6 p.p. additional reduction in employment for mother of children with CZS who receive the BPC. In practical terms, mothers of children with CZS who do not receive BPC experience a 6.9 percentage point reduction in formal employment. For BPC recipients, the total effect is approximately 13 percentage points—nearly double the effect for non-recipients.

This substantial difference persists even after flexibly controlling for characteristics that predict BPC receipt.

In column 4 we present the most flexible specification, controlling for ventiles of the PS and their interactions with CZS. In this case, the base CZS effect corresponds to the average slope of CZS across the 20 groups. Both the base effect and the interaction of CZS and BPC are basically unchanged.

The fact that the interaction effect becomes more negative when we control for the PS means that the main effect is biased towards zero. This suggests that groups more likely to receive the BPC also have a smaller effect on their employment due to their characteristics. A possible explanation is that the families most likely to receive disability payments are ones where the mother had a low probability of employment at baseline. We can see this in the coefficient on BPC in column 1, Panel B. Because they already are less likely to work, their reduction is smaller due to the limited range of the variable. The PS strategy adjusts for this difference, hence the smaller and statistically not significant coefficients in BPC in columns 2 to 4.

In the bottom panel we show the placebo test. As expected, the effects employment before childbirth of CZS and its interaction with the BPC are not statistically significant and closer to zero. The base effect of the BPC itself is strongly negative, indicating the recipients tend to have lower employment at baseline.

In Table 7 we present the results for informal employment. The results are qualitatively similar to the ones on formal employment. There is a statistically significant effect of CZS per se and a significant interaction with the BPC. However, while the effects on formal employment are roughly of the same magnitude, in the informal market the interaction is about double the main effect. Therefore, the total effect for BPC recipients (-28.8 pp) is approximately three times larger than for non-recipients (-9.8 pp).

This is consistent with the patterns of the effect over time: informal employment for affected mothers falls roughly linearly over time, as BPC coverage increases. As before, the placebo tests using previous employment are not statistically significant.

We stress that the interpretation of these results is, strictly speaking, not causal, since we cannot rule out selection on unobservables. Furthermore, we cannot speak of why the BPC reduces employment for mothers of children with CZS. Our results are consistent with an important income effect depressing labor supply or a means-test cliff effect.

5.6 Fertility and Family Structure

The intensive caregiving demands and financial pressures associated with child disability might be expected to strain family relationships and influence fertility decisions. However, we find no evidence that families with CZS children adapt along these margins. Broadly speaking, we can divide the families in our sample in 4 groups: single mothers living without any other adult (55%), nuclear families with mother and father (20%), households where we mother lives with her parents (20%), and a small group of other arrangements, including living with siblings and unrelated adults (5%).[9] We find only small movements in these categories over time.

Figure 13 shows that about a large share of mothers mothers in the sample live as the only adult in a household (50% before childbirth, 55% after), and there is no difference between those affected by CZS and controls. At the same time, cohabitation with a spouse (usually the father) goes from 25% to 30%. Here, we see a slight imbalance in the controls at baseline: cohabitation is about 5 p.p. more likely in the treated the year

[9] Living with the grandparents is not mutually exclusive with living with the spouse, but in practice these categories rarely overlap.

before childbirth (p-value: 9%). We see a statistically significant difference that vanishes in the DiD specification.

Figure 14 shows the probability of the mother living with her parents and of subsequent fertility. In grandparent cohabitation we see the mirror image of spouse co-habitation: a significant negative difference that vanishes in the DiD specification. We also find that subsequent fertility is low and not different between groups. The confidence intervals are tight around zero, indicating that families do not reduce their childbearing in response to having a child with severe disabilities. This finding is important for interpreting our employment results; the lack of fertility response means that our results are not driven by family size differences.

These null results on family structure are meaningful in several ways. First, they suggest that Brazilian families a child's with severe disabilities do not limit future fertility, although the vulnerable population in our sample seems to have limited fertility. Second, the lack of an effect on the stability of relationships may reflect their underlying vulnerability: only one quarter to one third of households include the father, but when they are present they seem resilient to the challenge of raising a disabled child.

Heterogeneity by household structure: We examine whether our estimated effects on employment differ by household structure. Figure 15 presents the results. We compare the effects between mothers living with no other adult and mothers co-living with spouses, parents or other adults. We find no difference in formal employment. This is not surprising, since we find formal employment after childbirth is almost zero for treated mothers; consequently, there is very little scope for meaningful heterogeneity. However, we do find significant heterogeneity in informal employment. Mothers living alone see a greater reduction in informal employment, and it manifests sooner. This result highlights the importance of observing informality in this context.

5.7 Spillover Effects on Fertility

Following the news establishing the link between the Zika virus outbreak and CZS, there were reports of women afraid of conceiving new children. This reaction may be reflected in local spillovers to fertility because people infer the risk is higher in an area where cases of CZS were reported, or because proximity raises the salience of the risk. In this section we investigate whether there were significant local negative spillovers to subsequent fertility in mothers not directly affected.

To estimate the spillover effects on fertility, we compare the fertility rate in municipalities with at least one case of CZS to places with no incidence. In this analysis, we include every municipality in the country. We identify the spillover effects by estimating the following DID equation:

$$fertility_{mt} = \sum_{\tau \in T} \gamma_\tau \cdot Incidence_m \cdot \mathbf{1}_{t=\tau} + \delta_m + \delta_t + \epsilon_{it} \qquad (3)$$

where $fertility_{mt}$ is the total number of babies born in a municipality m at year t per 1,000 inhabitants and $T = (\tau \in N, 2010 \leq \tau \leq 2020, \tau \neq 2014)$. $Incidence_m$ equals one if there was a CZS case during the Zika virus outbreak period in municipality m. We control for the municipality and year fixed effect, δ_m and δ_t, respectively. Standard errors are clustered at the municipality level.

Our parameters of interest are γ_t, which captures the effect on fertility in each year t. Here, we must rely on a parallel trends assumption instead of conditional independence. This is because municipality-level incidence of the virus was determined by several factors that are correlated with the level of fertility, such as local climate. Therefore, our identification assumption is that fertility would have followed parallel paths in affected municipalities relative to unaffected ones.

Our analysis reveals a decline in overall fertility rates in affected municipalities following the confirmation of the link between the Zika virus and this condition. Figure 16 shows the main results. Prior to the outbreak (2010-2014), fertility rates were similar across all areas, with perhaps a positive trend in affected municipalities. In 2015, the first year of the outbreak and when the link between Zika and CZS was confirmed, we observe no significant changes in fertility rates. This is to be expected, as there was no time for fertility decisions to affect the number of births. However, in 2016, we note a decrease of 0.16 births per thousand inhabitants in affected areas. This effect persists through 2019.

6 Conclusion

This paper examines how severe child disability affects parental labor market outcomes, household composition, and economic well-being, using Brazil's 2015 Zika outbreak as a source of exogenous variation. The unique characteristics of the epidemic—its sudden onset, mosquito transmission, and lack of preventive knowledge—allow us to credibly isolate the causal effects of child disability from confounding factors that typically plague this literature.

Our findings reveal substantial and persistent effects concentrated entirely on mothers. Maternal formal employment falls by an additional 4 percentage points beyond the standard motherhood penalty—a 66% increase in the employment reduction typically associated with childbirth. Contrary to conventional wisdom about labor market flexibility in developing countries, we find similarly large negative effects in the informal sector, where baseline employment rates are twice as high. This challenges the view that informal work provides meaningful accommodation for caregiving demands.

The employment reductions occur alongside increased household income due to

Brazil's disability transfer program, with over 60% of affected families receiving monthly payments equivalent to the minimum wage. We provide evidence that these transfers amplify employment reductions through income effects, while substantial caregiving demands persist even among non-recipient families. The increased income supports higher expenditures on food, medical care, and housing adaptations required by children with severe disabilities. Notably, we find no adjustments along other household margins. Fertility rates and partnership stability remain unchanged, suggesting that families maintain their demographic intentions while mothers exit the labor market.

These findings contribute to several literatures. For research on child disability and parental outcomes, we provide the first causally identified estimates using an exogenous health shock. For the broader literature on motherhood penalties, we demonstrate how these effects are dramatically magnified by severe child disability. For development economics, we provide rare evidence that informal labor markets do not buffer families against intensive caregiving demands.

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Tables

Table 1: Sample Construction and Matching Flow

Stage	Cases
A. Initial Sample Construction	
RESP records (probable/confirmed CZS)	3,594
After dropping missing birth dates	2,946
After restricting to 2015 – 2017	2,431
After de-duplication	2,411
After SR-2017 linkage	940
After SR-2019 linkage	1,221
Cases available for matching	1,200
B. Sequential Matching Process	
Stage 1: Same Municipality	669 (6,009 controls)
Stage 2: Same Microregion	198 (1,480 controls)
Final sample (cases)	**867**
Final sample (controls)	**7,489**

Notes: This table shows the sequential sample construction process. In Panel A, each row represents the remaining sample after applying the exclusion criterion. Panel B shows the resulting sample from two rounds of matching: the first with strict municipality equality, the second requiring the same microregion and a similar population. Cases are matched exactly on child's month of birth, mother's year of birth, maternal education (high school completion), race (White vs. Non-White), and birth order (firstborn indicator). Controls cannot be reused across matching stages. See Appendix 1 for details.

Table 2: Summary Statistics

	CZS	Control Unweighted	Control Weighted	p-value
Completed HS (%)	37.37	29.06	37.37	.
Mother's age	24.64	23.08	24.64	.
Race (%):				
White	16.84	11.20	16.84	.
Black	11.53	16.33	12.96	0.254
Yellow	0.58	0.51	0.66	0.788
Pardo	70.47	71.76	69.19	0.316
Indigenous	0.58	0.21	0.35	0.344
Parity (%):				
Firstborn	57.44	60.46	57.44	.
Second	27.22	24.05	26.87	0.767
Third	9.57	10.60	10.18	0.580
Higher	5.77	4.89	5.51	0.765
Mother's Labor Trajectory:				
Employment y-1	14.92	13.17	13.87	0.371
Employment y-2	16.29	14.22	14.70	0.171
Wage y-1	1083.63	990.32	1044.88	0.552
Wage y-2	1062.34	952.13	987.27	0.163
N	867	7,489	867	

Notes: This table shows sample means for the CZS and control samples. Control (raw) refers to the unweighted control sample, while Control (weighted) reweighs to match the characteristics of the CZS sample. The p-value refers to the comparison between CZS and weighed controls. Variables with missing p-values are used for matching, and are identical by construction. The last row shows the effective sample size, reflecting that the weighted controls have the same effective sample size as the treated.

Table 3: Results Summary - Formal Labor Market

Mother's Formal Labor Market

Variable	SD	DID	Baseline	N	Clusters
Employment	-0.040***	-0.051***	0.149	509,716	852
	(0.007)	(0.013)			
Earnings	-39.47***	-52.05***	155.4	509,716	852
	(8.92)	(14.89)			
Monthly wages if employed	83.29	79.47	1043.7	54,233	596
	(72.17)	(77.54)			
Weekly hours	-2.02***	-2.41***	6.68	509,716	852
	(0.32)	(0.56)			
Weekly hours if employed	-0.68	0.72	42.6	59,659	599
	(0.83)	(1.09)			

Father's Formal Labor Market

Variable	SD	DID	Baseline	N	Clusters
Employment	0.014	0.011	0.29	39,650	173
	(0.033)	(0.041)			
Earnings	78.10	33.77	387.4	39,650	173
	(64.906)	(59.171)			
Monthly wages if employed	230.79*	92.01	1334.3	10,943	156
	(124.080)	(113.839)			
Weekly hours	0.421	0.588	13.13	39,650	173
	(1.562)	(1.836)			
Weekly hours if employed	-0.417	-0.357	44.29	11,492	156
	(0.949)	(0.837)			

Notes: This table shows the average estimated ATT corresponding to months 9 to 36 after childbirth. The first column corresponds to average Simple Differences in the post period (from month 9 to 36), the second column is a Differences-in-Differences average over the same interval, normalizing $\beta_{-9} = 0$. Baseline mean refers to the mean of the control group nine months before childbirth

Table 4: Results Summary - Informal Labor and Total Income

Variable	SD	DID	Baseline	N	Clusters
Mother's informal labor					
Employment	-0.204***	-0.190***	0.399	29,979	852
	(0.015)	(0.041)			
Earnings	-384.6***	-493.575***	690.9	29,979	852
	(45.9)	(132.7)			
Conditional months of work	0.010	0.641	7.814	9,984	647
	(0.473)	(0.663)			
Conditional earnings	55.5	-870.4	1779.3	9,984	647
	(259.1)	(779.4)			
Father's informal labor					
Employment	-0.192***	-0.252***	0.499	2,196	173
	(0.047)	(0.087)			
Earnings	-342.0	-457.1	1256.3	2,196	173
	(245.3)	(407.8)			
Conditional months of work	1.059*	1.198	8.139	1,168	157
	(0.629)	(1.262)			
Conditional annual earnings	563.6	609.9	2579.4	1,168	157
	(457.8)	(758.1)			

Notes: This table shows the average estimated ATT corresponding to years 1 to 4 after childbirth. The first column corresponds to average Simple Differences in the post period (from month 9 to 36), the second column is a Differences-in-Differences average over the same interval and normalizing $\beta_{-1} = 0$. Baseline refers to the mean of the control group one year before childbirth.

Table 5: Results Summary - Family Structure and Income

Variable	SD	DID	Baseline	N	Clusters
Family Income					
Total income	4907.7***	4498.7***	2757.4	29,979	852
	(254.3)	(344.9)			
Labor earnings	-375.8**	-744.0**	1910.8	29,979	852
	(171.8)	(291.5)			
Family Structure					
Single household	0.005	0.014	0.504	26,782	844
	(0.020)	(0.034)			
Spouse present	0.051***	-0.004	0.253	26,782	844
	(0.018)	(0.031)			
Grandparent present	-0.044***	-0.014	0.178	26,782	844
	(0.011)	(0.018)			
Subsequent fertility	0.009	0.009	0	26,782	844
	(0.010)	(0.010)			

Notes: This table shows the average estimated ATT corresponding to years 1 to 4 after childbirth. The first column corresponds to average Simple Differences in the post period (from month 9 to 36), the second column is a Differences-in-Differences average over the same interval and normalizing $\beta_{-1} = 0$. Baseline mean refers to the mean of the control group one year before childbirth.

Table 6: Effects on Formal Employment by Social Security Recipient Status

	(1)	(2)	(3)	(4)
	Panel A			
	Outcome: Formal Employment 2 Years After Childbirth			
CZS	-0.077***	-0.069***	-0.066***	-0.068***
	(0.016)	(0.016)	(0.019)	(0.016)
BPC	-0.017	0.006	0.006	0.010
	(0.017)	(0.020)	(0.020)	(0.020)
CZS x BPC	-0.037	-0.060*	-0.060*	-0.063*
	(0.022)	(0.024)	(0.024)	(0.025)
PS Controls	None	Linear	Cubic	Ventile FE
PS Controls x CZS	-	Yes	Yes	Yes
Observations	8,356	8,354	8,354	8,354
Clusters	852	851	851	851
	Panel B			
	Outcome: Formal Employment 1 Year Before Childbirth			
CZS	-0.017	-0.016	0.012	-0.020
	(0.020)	(0.020)	(0.024)	(0.020)
BPC	-0.061**	-0.031	-0.030	-0.024
	(0.021)	(0.023)	(0.023)	(0.022)
CZS x BPC	0.035	0.030	0.026	0.027
	(0.031)	(0.033)	(0.033)	(0.032)
PS Controls	None	Linear	Cubic	Ventile FE
PS Controls x CZS	-	Yes	Yes	Yes
Observations	8,356	8,354	8,354	8,354
Clusters	852	851	851	851

Notes: This table shows the results of Equation (2), with increasingly flexible controls for the propensity score of receiving the BPC and the interaction of the propensity score and CZS. The first column includes no controls, the second column includes the PS and PS times CZS, the third adds the square and cube of the PS and their interactions with CZS, and the fourth includes 20 dummies for the ventiles of the PS, as well as their interactions with CZS. PS Controls are included after subtracting their average, such that the coefficient on CZS_i can be interpreted as the parameter at the average PS. In column 4, the model is parametrize such that the coefficient on CZS return the average of the slope coefficients between each of the 20 bins.

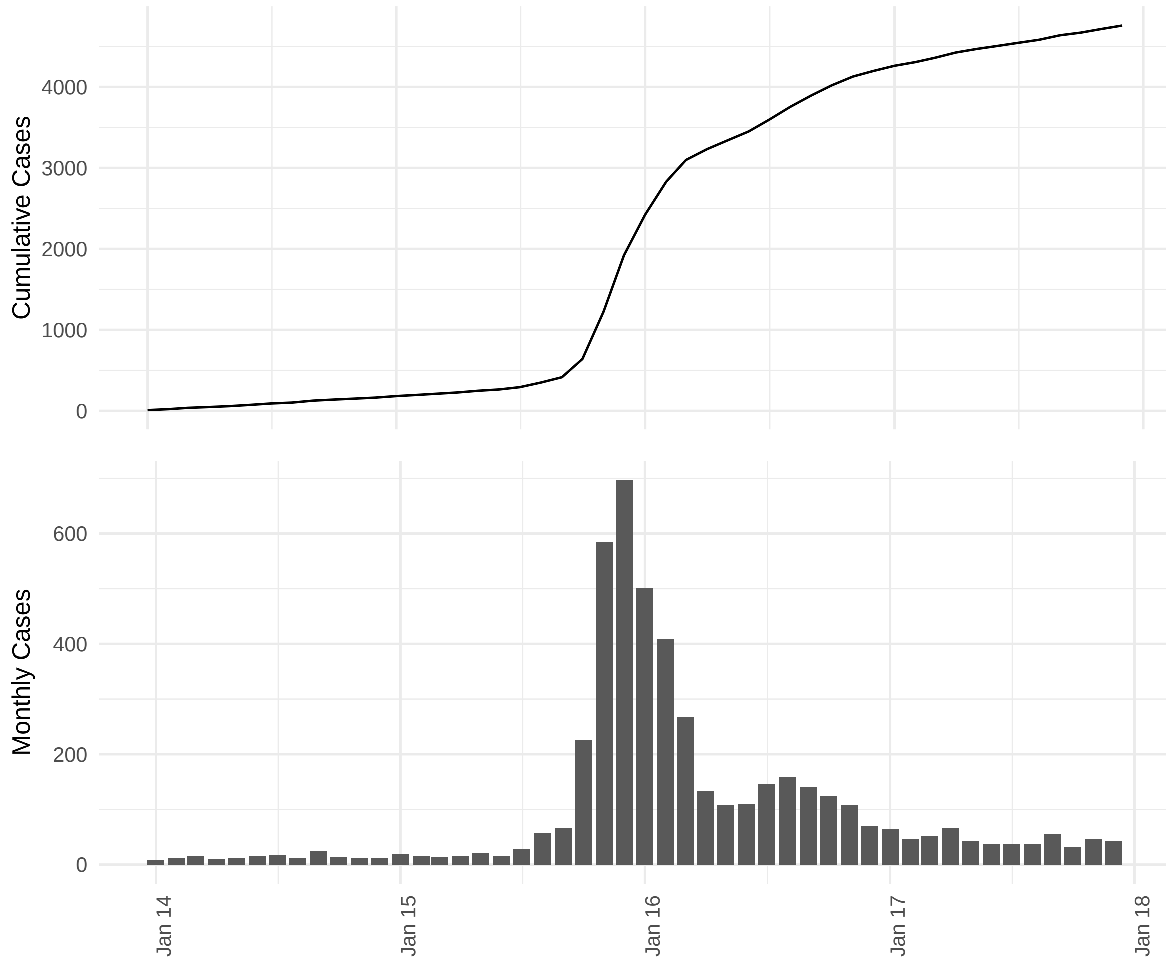
Table 7: Effects on Informal Employment by Social Security Recipient Status

	(1)	(2)	(3)	(4)
	Panel A			
	Outcome: Informal Employment 2 Years After Childbirth			
CZS	-0.098***	-0.100***	-0.075*	-0.096***
	(0.024)	(0.024)	(0.030)	(0.024)
BPC	0.016	-0.001	0.001	0.002
	(0.023)	(0.025)	(0.025)	(0.026)
CZS x BPC	-0.190***	-0.183***	-0.188***	-0.199***
	(0.031)	(0.034)	(0.035)	(0.035)
PS Controls	None	Linear	Cubic	Ventile FE
PS Controls x CZS	-	Yes	Yes	Yes
Observations	7,494	7,492	7,492	7,492
Clusters	845	844	844	844
	Panel B			
	Outcome: Informal Employment 1 Year Before Childbirth			
CZS	0.009	0.011	0.037	-0.016
	(0.056)	(0.056)	(0.072)	(0.055)
BPC	-0.016	-0.042	-0.039	-0.047
	(0.046)	(0.048)	(0.048)	(0.049)
CZS x BPC	-0.054	-0.057	-0.060	-0.044
	(0.083)	(0.089)	(0.089)	(0.091)
PS Controls	None	Linear	Cubic	Ventile FE
PS Controls x CZS	-	Yes	Yes	Yes
Observations	1,921	1,921	1,921	1,921
Clusters	365	365	365	365

Notes: This table shows the results of Equation (2), with increasingly flexible controls for the propensity score of receiving the BPC and the interaction of the propensity score and CZS. The first column includes no controls, the second column includes the PS and PS times CZS, the third adds the square and cube of the PS and their interactions with CZS, and the fourth includes 20 dummies for the ventiles of the PS, as well as their interactions with CZS. PS Controls are included after subtracting their average, such that the coefficient on CZS_i can be interpreted as the parameter at the average PS. In column 4, the model is parametrize such that the coefficient on CZS return the average of the slope coefficients between each of the 20 bins.

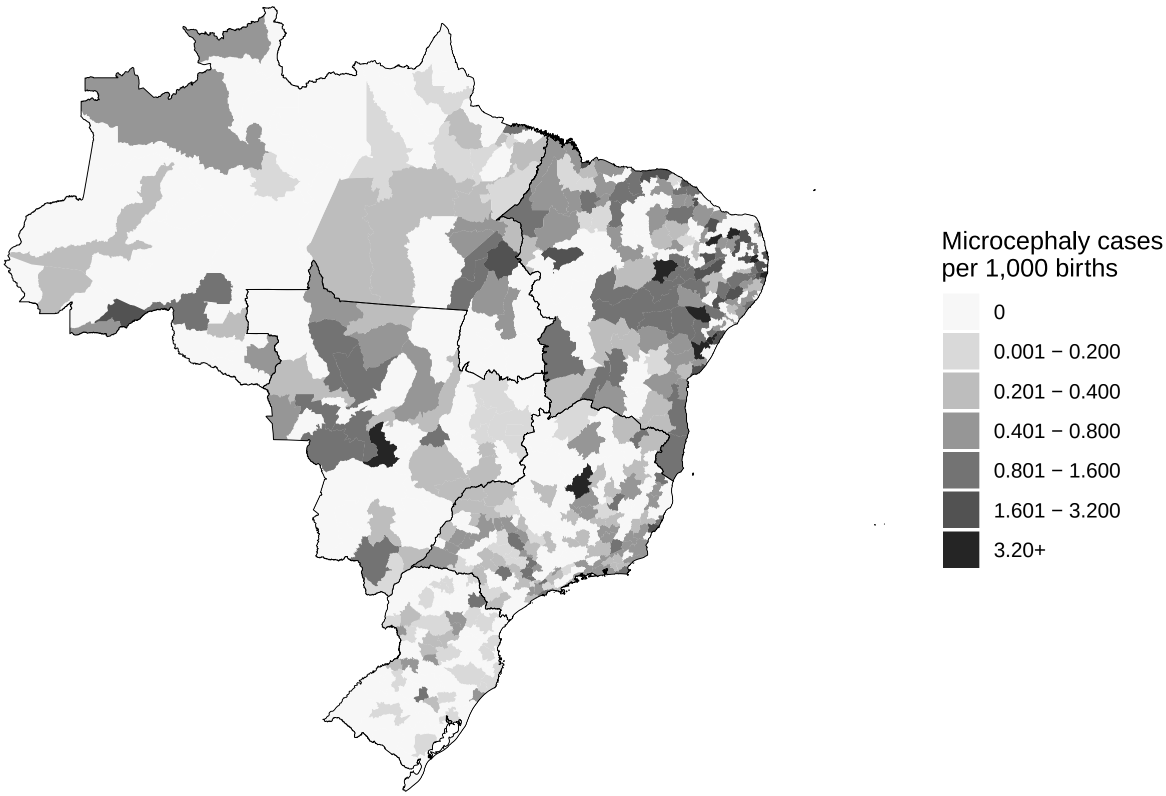
Figures

Figure 1: Microcephaly Cases by Month



Notes: These figures show the evolution in the total number of cases of microcephaly. The top graph shows cumulative cases, while the bottom shows monthly incidence. The data is from SINASC/SUS.

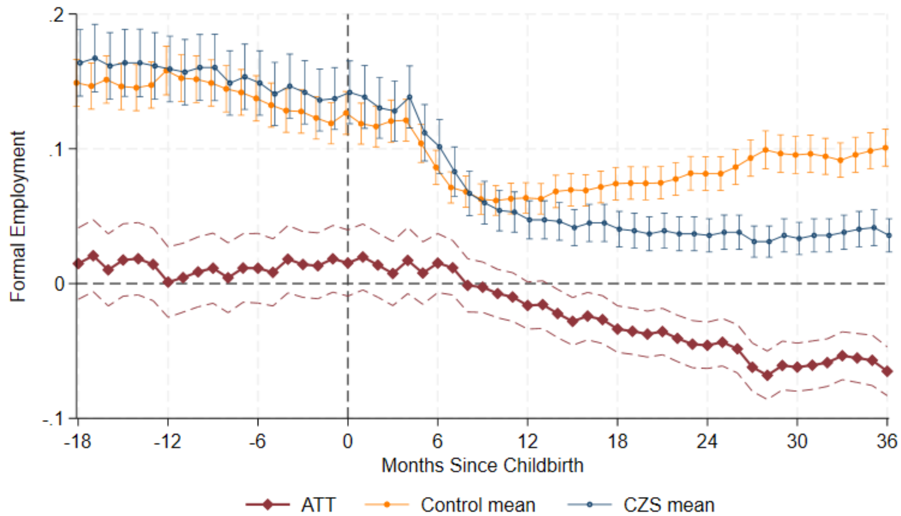
Figure 2: Geographic Variation on the Number of Microcephaly cases per 1000 Births



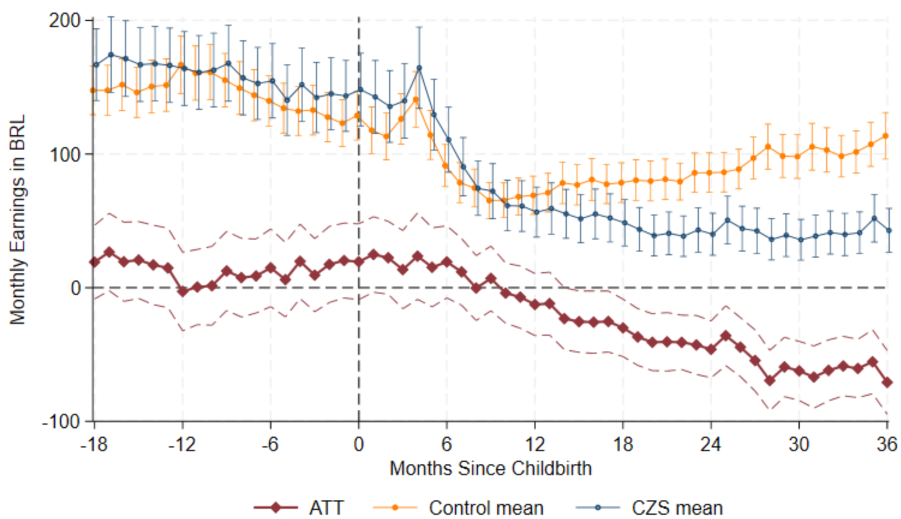
Notes: This figure illustrates the geographic variation on the number of microcephaly cases per thousand births in 2015 and 2016. Each polygon is a micro-region, comprising on average about 10 municipalities. Micro-regions with zero births in the period are assigned to the zero cases per 1,000 births category. The total number of births and cases of microcephaly is available from SINASC/SUS.

Figure 3: Effects on Mothers: Employment and Earnings

Employment



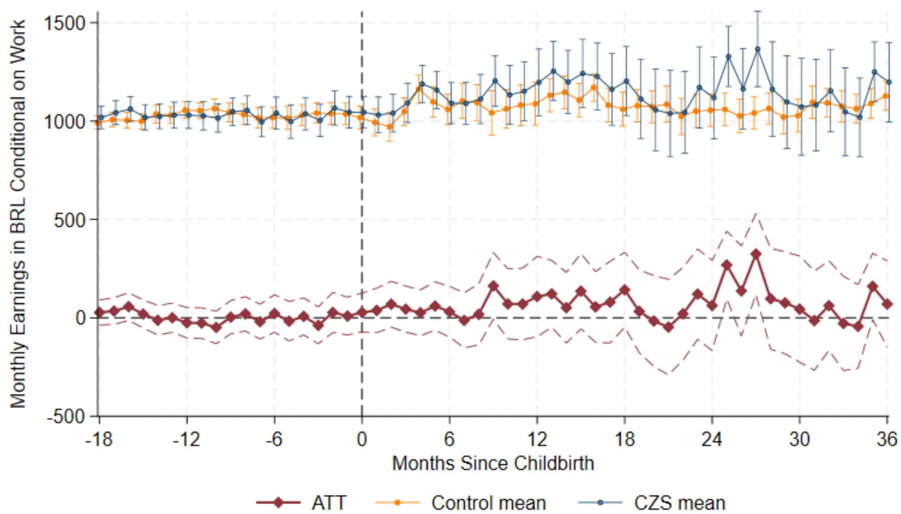
Earnings



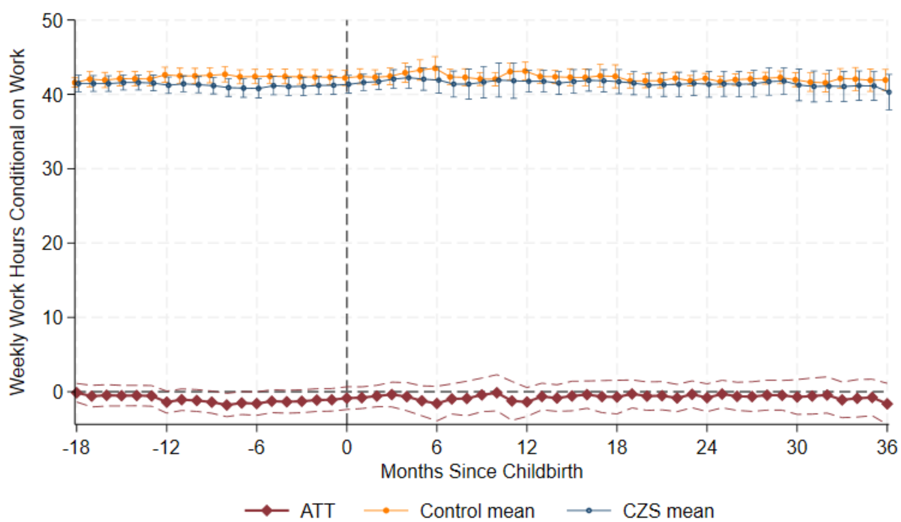
Notes: This figure shows the employment rate (above) and earnings (below) of mothers in the formal sector. The CZS Group consists mothers of children diagnosed with Congenital Zika Syndrome, while the Control Group consists of mothers of children without the condition matched on a set observed variables. Earnings are in BRL, and the error bars represent 95% confidence intervals, with errors clustered at the stratum level.

Figure 4: Effects on Mothers: Conditional Wages and Hours

Monthly Wages



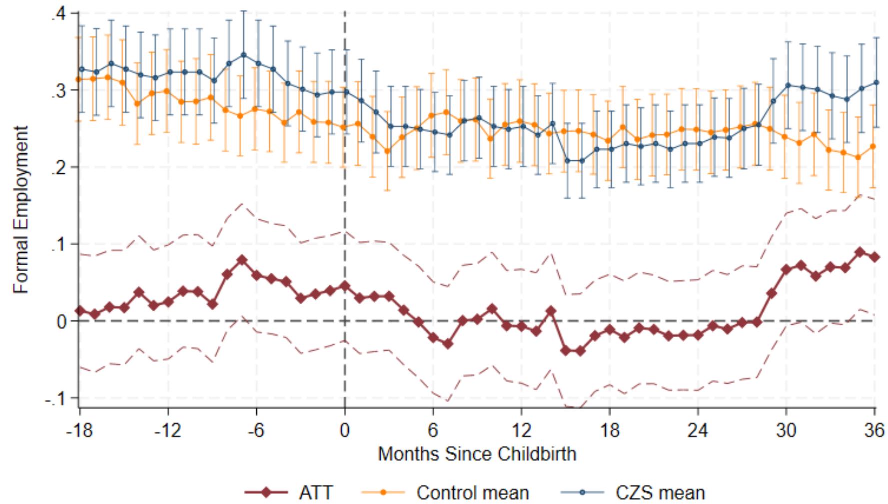
Weekly Hours



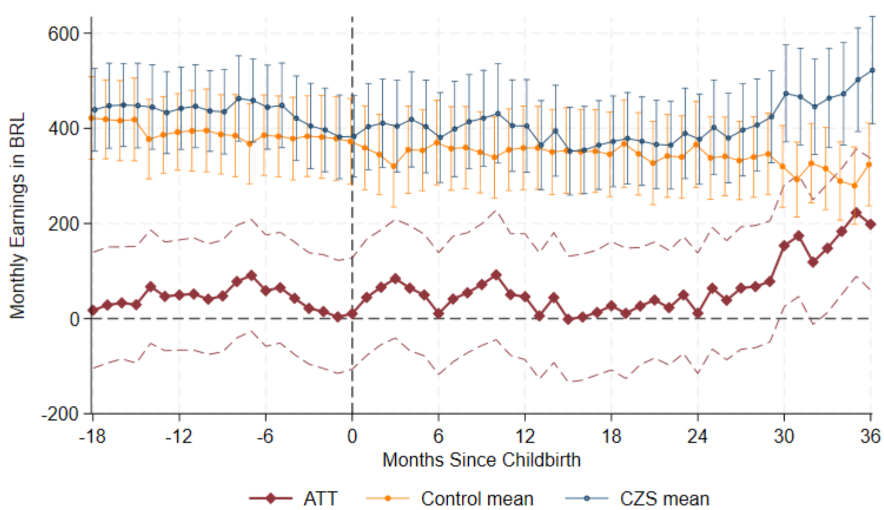
Notes: This figure shows the monthly wages (above) and contractual hours of work (below) of mothers in the formal sector, both conditional on working. The CZS Group consists mothers of children diagnosed with Congenital Zika Syndrome, while the Control Group consists of mothers of children without the condition matched on a set observed variables. Wages are in BRL, and the error bars represent 95% confidence intervals, with errors clustered at the stratum level.

Figure 5: Effects on Fathers: Employment and Earnings

Employment



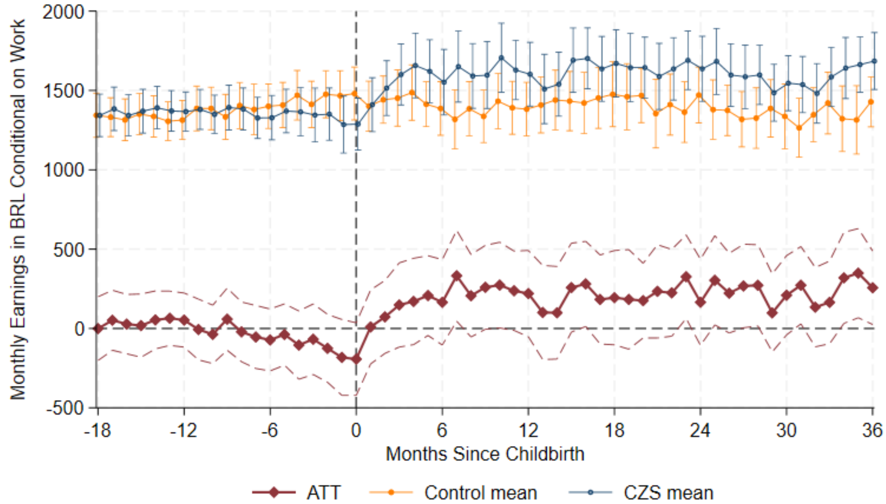
Earnings



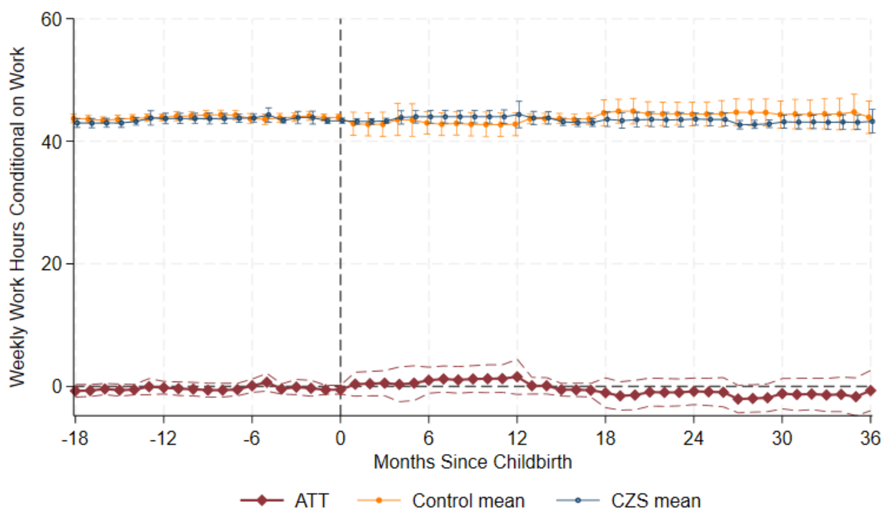
Notes: This figure shows the employment rate (above) and earnings (below) of fathers in the formal sector. The Microcephaly Group consists fathers of children diagnosed with Congenital Zika Syndrome. The Control Group consists of fathers of children without the condition matched on a set observed variables. Earnings are in BRL, and the error bars represent 95% confidence intervals, with errors clustered at the stratum level.

Figure 6: Effects on Fathers: Conditional Wages and Hours

Monthly Wages



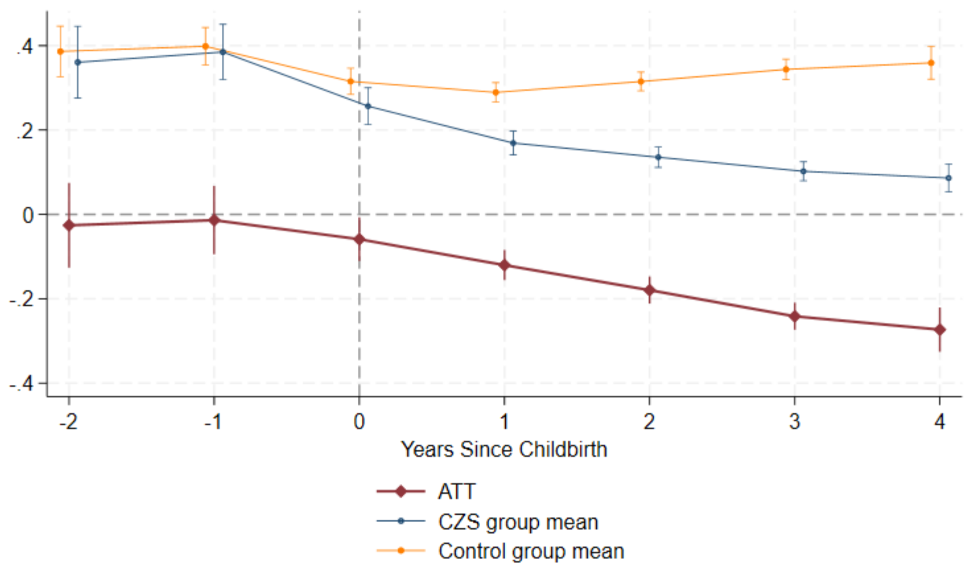
Weekly Hours



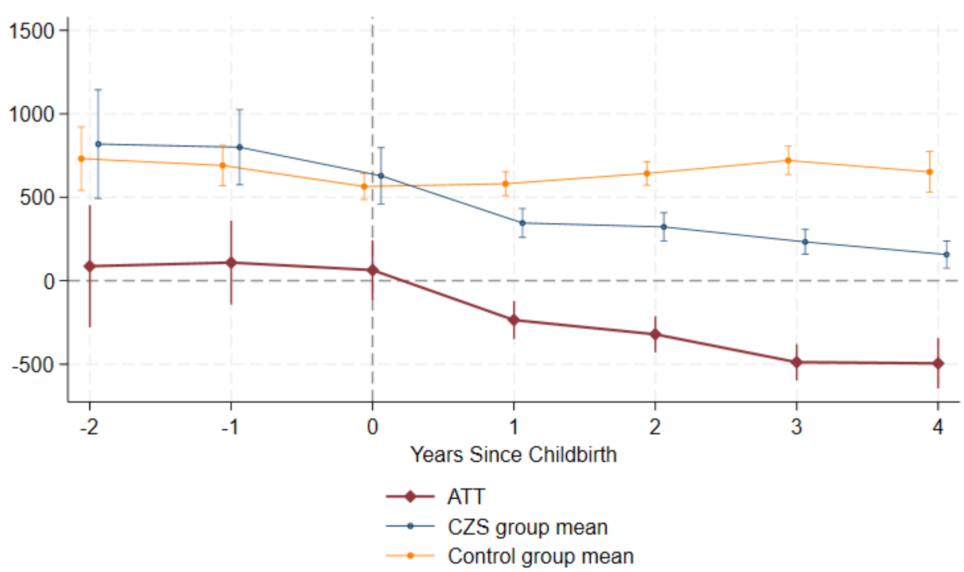
Notes: This figure shows the monthly wages (above) and weekly hours (below) of fathers who work in the formal sector. The Microcephaly Group consists fathers of children diagnosed with Congenital Zika Syndrome. The Control Group consists of fathers of children without the condition matched on a set observed variables. Earnings are in BRL, and the error bars represent 95% confidence intervals, with errors clustered at the stratum level.

Figure 7: Effects on Mothers' Informal Work

Informal Employment



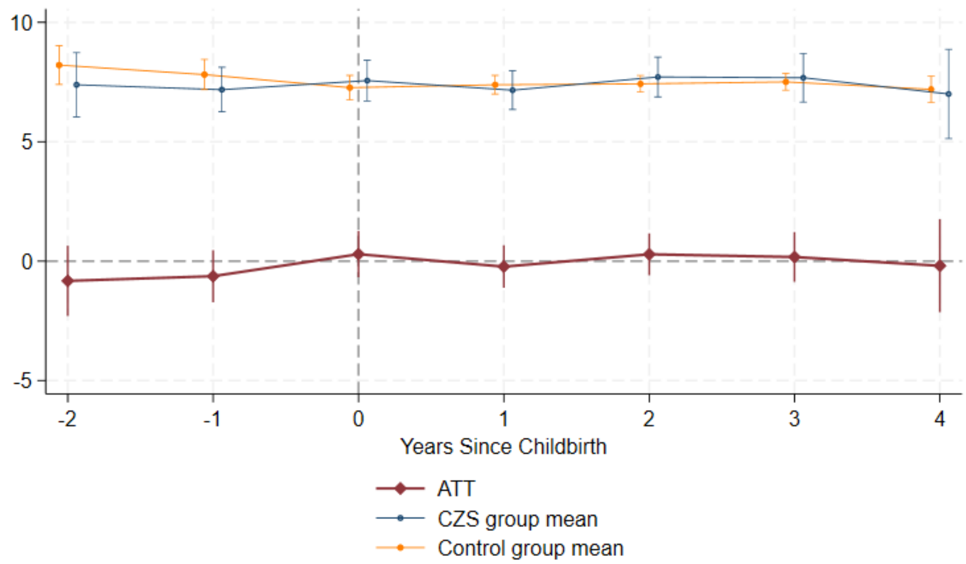
Informal Earnings



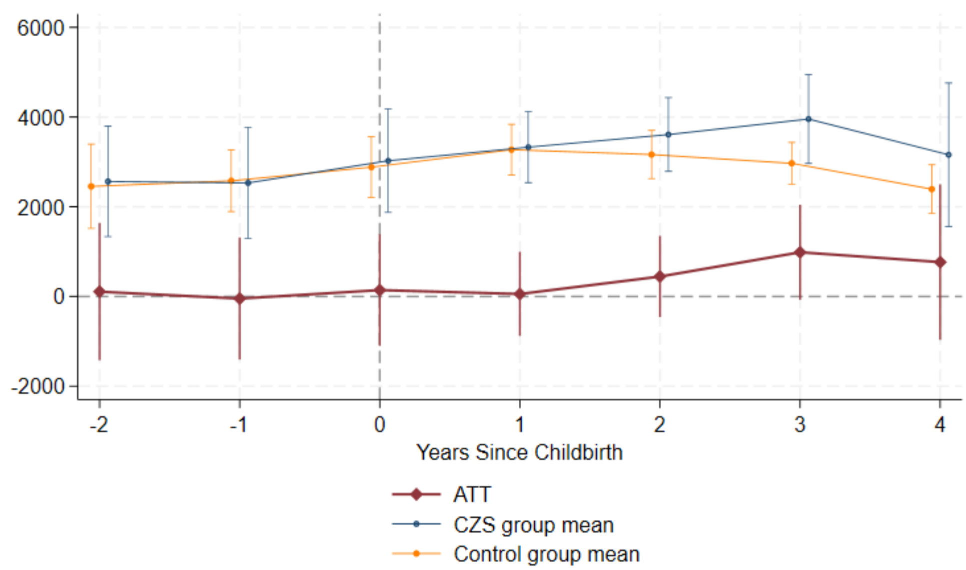
Notes: This figure shows the estimated effect on months of work (top) and monthly wages (bottom). The CZS Group consists mothers of children diagnosed with Congenital Zika Syndrome, while the Control Group consists of a matched group of mothers of children without this condition. Error bars represent 95% confidence intervals, with errors clustered at the stratum level.

Figure 8: Effects on Mothers' Informal Work

Months of Work



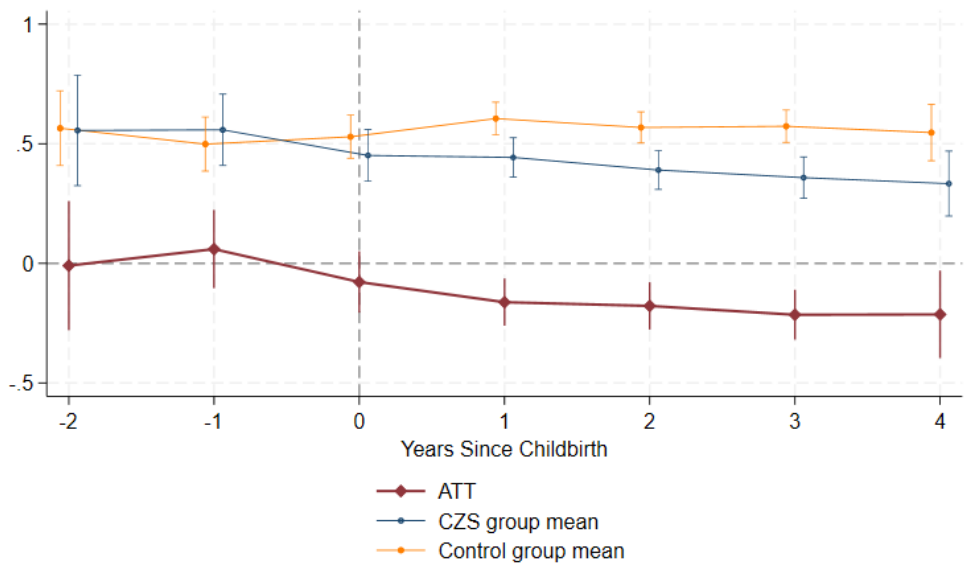
Conditional Yearly Wages



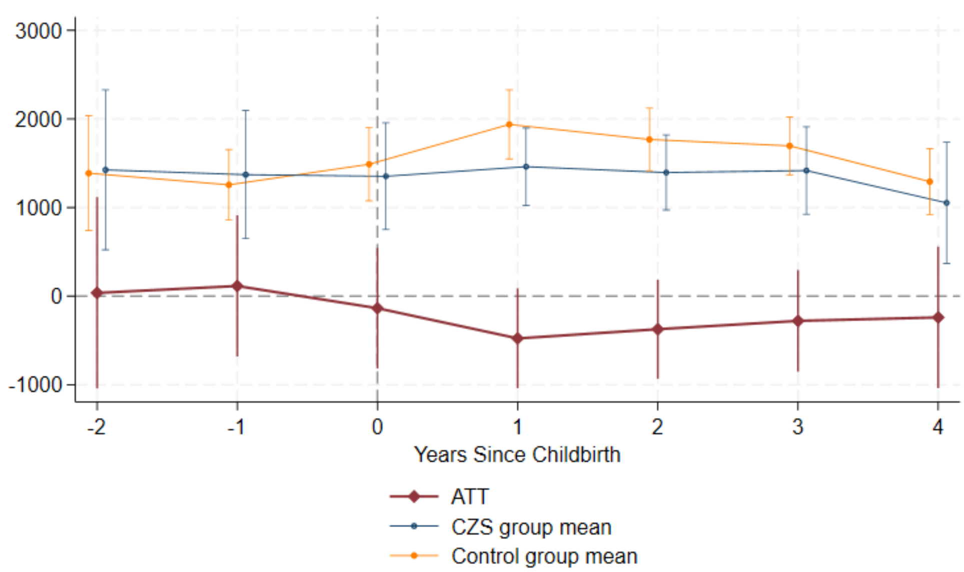
Notes: This figure shows the estimated effect on the number of months worked in the year (top) and monthly wages (bottom) for mothers working in the informal sector. The CZS Group consists mothers of children diagnosed with Congenital Zika Syndrome, while the Control Group consists of a matched group of mothers of children without this condition. Error bars represent 95% confidence intervals, with errors clustered at the stratum level.

Figure 9: Effects on Fathers' Informal Work

Informal Employment



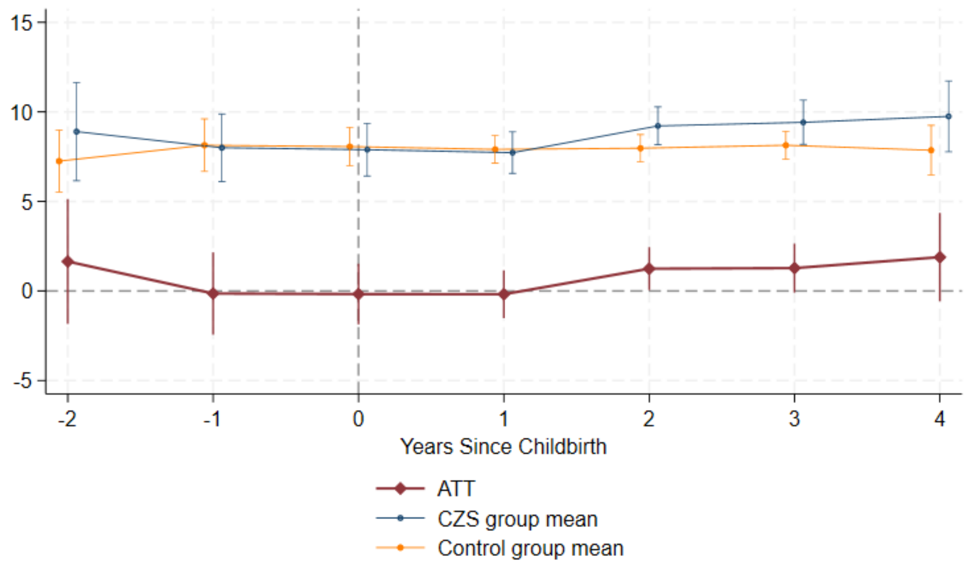
Informal Earnings



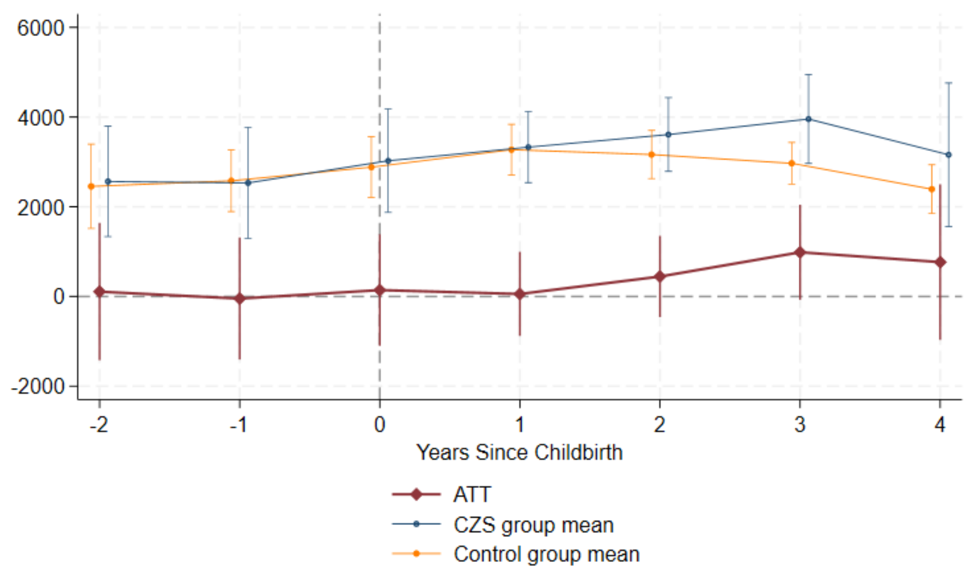
Notes: This figure shows the estimated effect on months of work (top) and monthly wages (bottom). The CZS Group consists fathers of children diagnosed with Congenital Zika Syndrome, while the Control Group consists of a matched group of fathers of children without this condition. Error bars represent 95% confidence intervals, with errors clustered at the stratum level.

Figure 10: Effects on Fathers' Informal Work

Months of Work



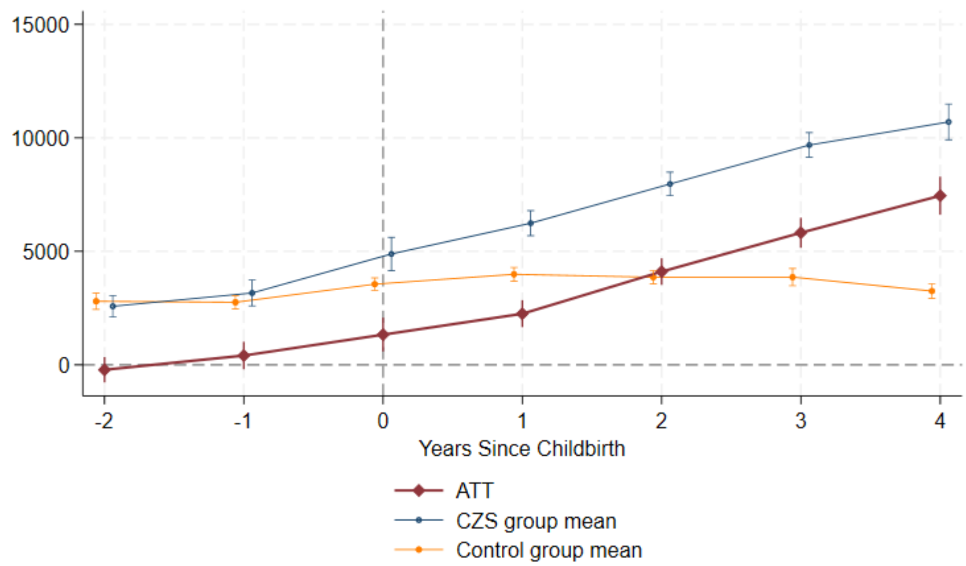
Conditional Yearly Wages



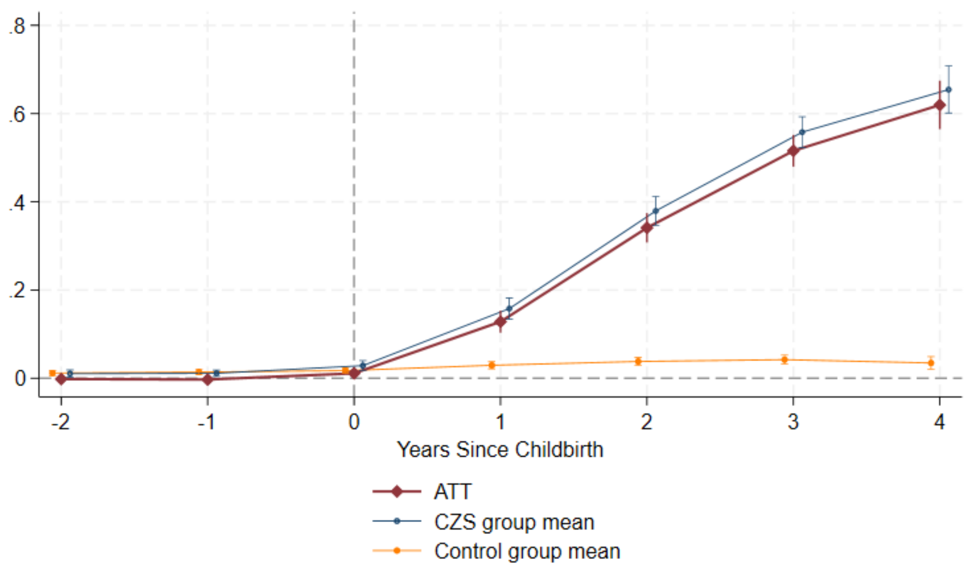
Notes: This figure shows the estimated effect on the number of months worked in the year (top) and monthly wages (bottom) for fathers working in the informal sector. The CZS Group consists fathers of children diagnosed with Congenital Zika Syndrome, while the Control Group consists of a matched group of fathers of children without this condition. Error bars represent 95% confidence intervals, with errors clustered at the stratum level.

Figure 11: Total Family Income and Social Security

Total Annual Family Income



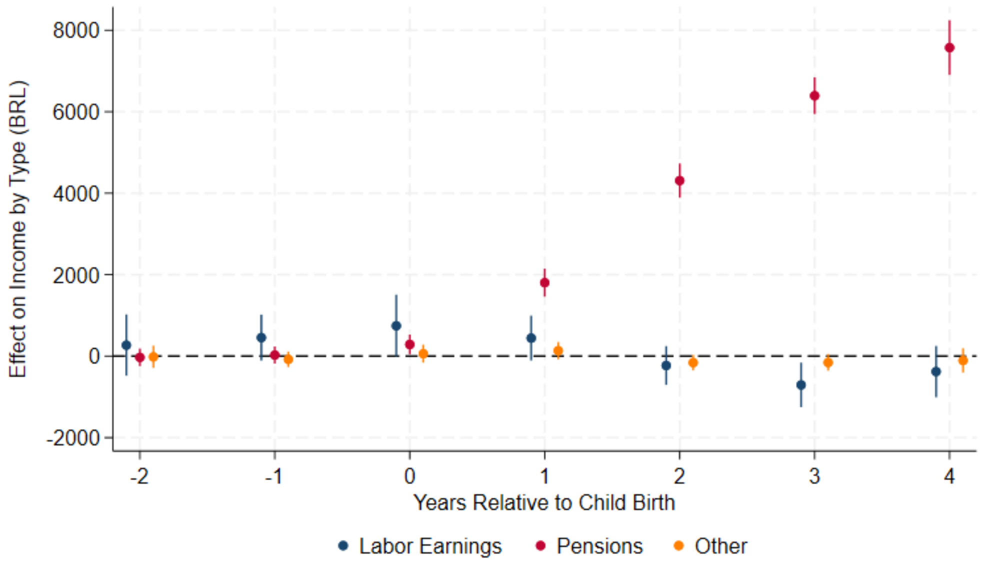
Receives Social Security



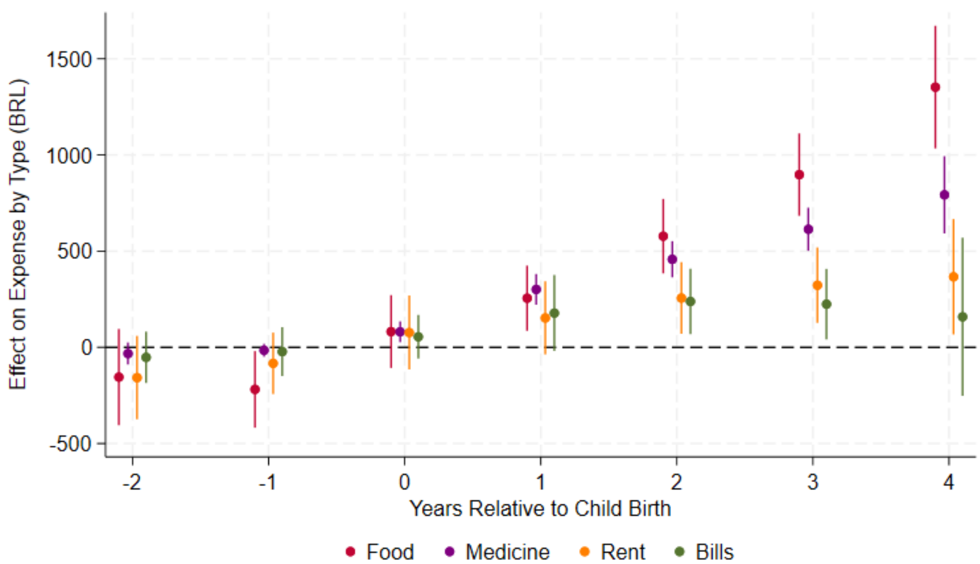
Notes: This figure shows the estimated effect on the total annual family income (top) and the probability the family is a recipient of the BPC (bottom) for mothers working in the informal sector. The CZS Group consists mothers of children diagnosed with Congenital Zika Syndrome, while the Control Group consists of a matched group of mothers of children without this condition. Error bars represent 95% confidence intervals, with errors clustered at the stratum level.

Figure 12: Family Income and Expenses

Income Sources



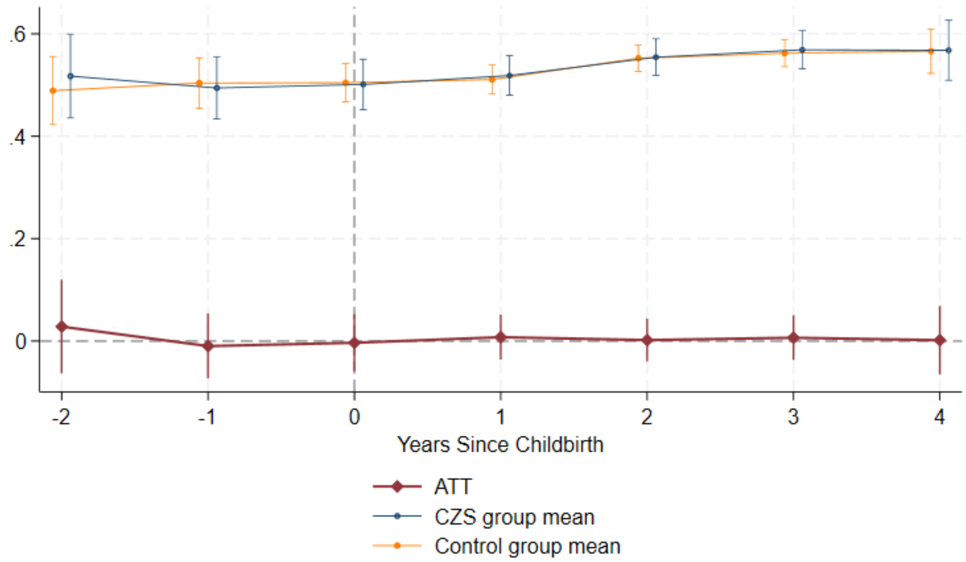
Expenses



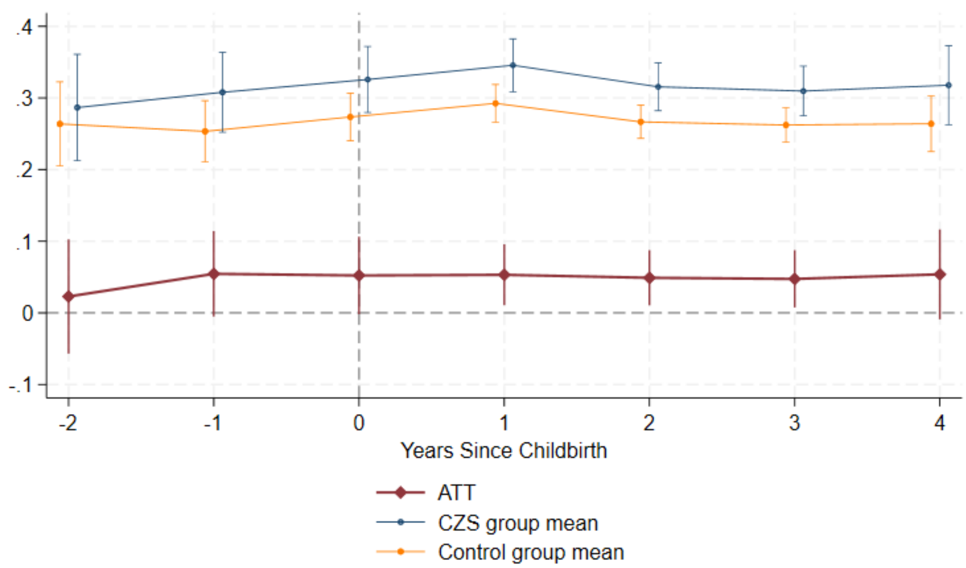
Notes: This figure shows the estimated effect on the families' income by source and the estimated effect on four categories of expenditures. All figures are in current BRL, based on self-reports in the Single Registry. Error bars represent 95% confidence intervals, with errors clustered at the stratum level.

Figure 13: Effects on Family Structure

Single household



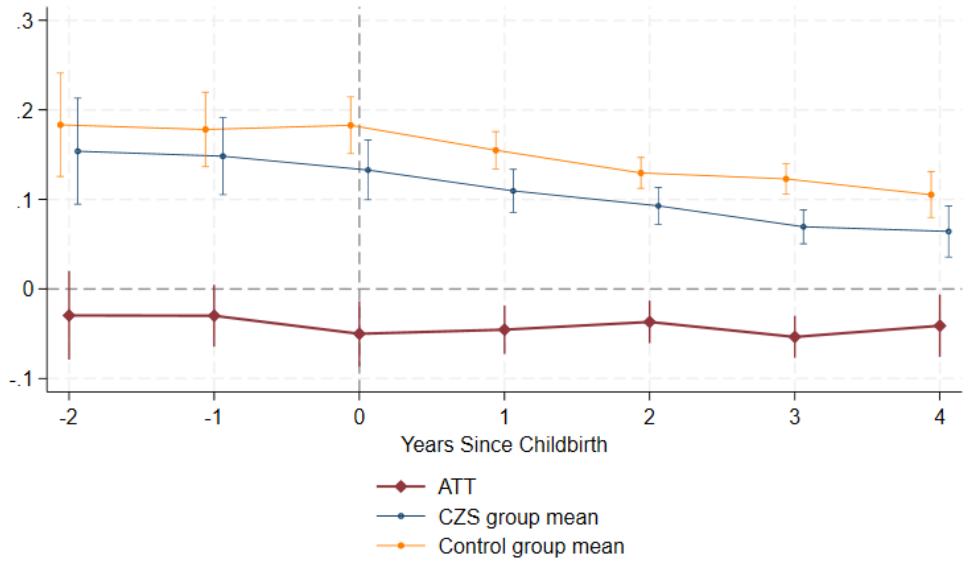
Spouse present



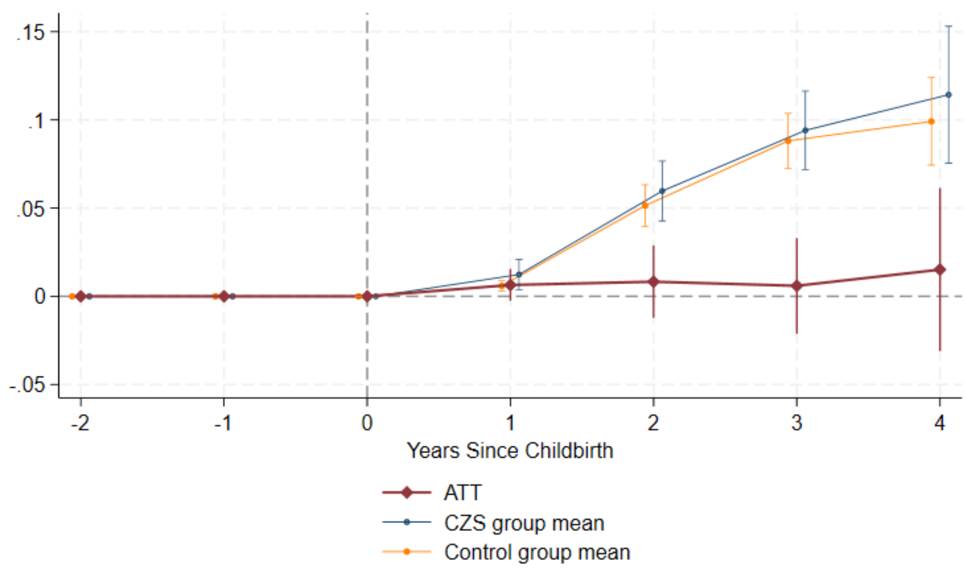
Notes: This figure shows the estimated effect on the probability the mother is the only adult in the household (top) and of there being a spouse present (bottom), based on Single Registry data. Error bars represent 95% confidence intervals, with errors clustered at the stratum level.

Figure 14: Effects on Family Structure

Grandparents present



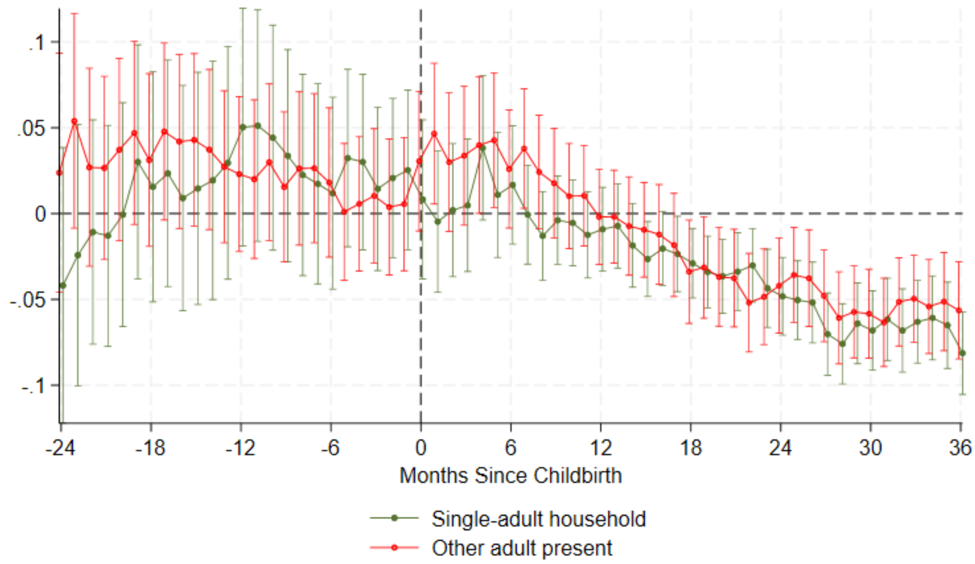
Subsequent Fertility



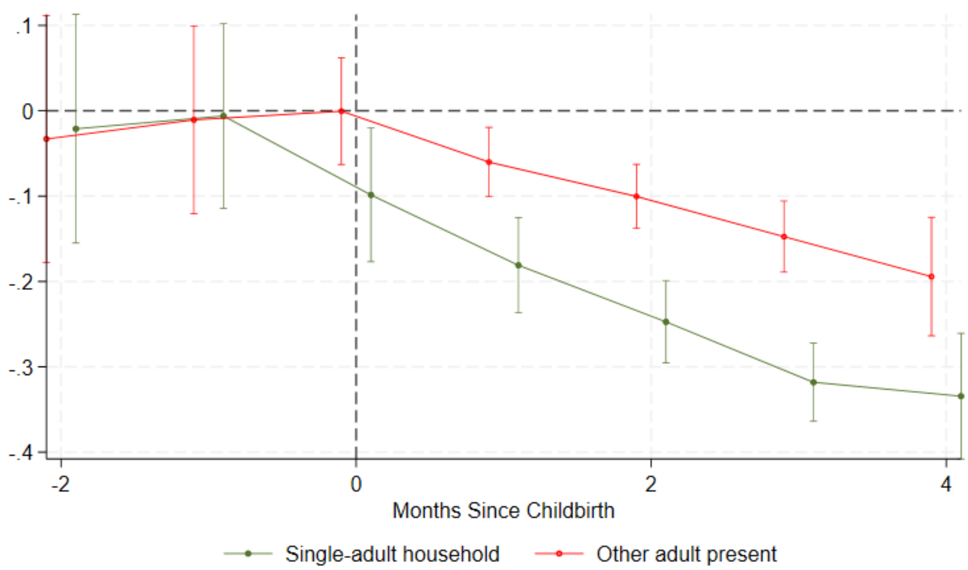
Notes: This figure shows the estimated effect on the probability of grandparents being present in the household (top) and in subsequent fertility (bottom), based on Single Registry data. Error bars represent 95% confidence intervals, with errors clustered at the stratum level.

Figure 15: Heterogeneity by Family Structure

Effect on Mother's Formal Employment

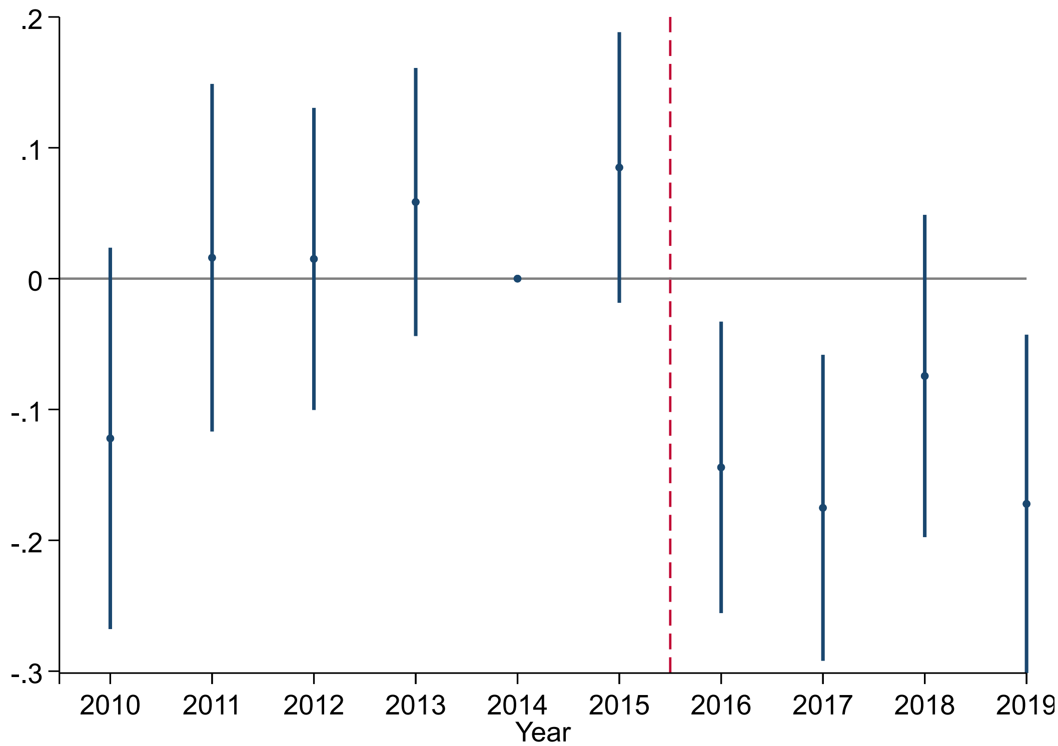


Effect on Mother's Informal Employment



Notes: This figure shows the estimated effect on the probability of employment in the formal (top) and informal (bottom) sectors. The sample is divided between mothers living with no other adult and mothers living in households with at least one other adult. Error bars represent 95% confidence intervals, with errors clustered at the stratum level.

Figure 16: Spillover Effects on Fertility



Notes: This figure shows the estimated spillover effects on fertility. The dependent variable is the fertility rate (per thousand). The independent variables are leads and lags of an indicator of having any case of microcephaly between 2015 and 2017.

A Appendix: Sample Definition and Matching

A.1 Initial Sample Construction

The analysis begins with data from RESP (Registro de Eventos em Saúde Pública), specifically the Microcefalia/CZS module. We keep only records with a final classification of **probable** or **confirmed CZS** and restrict the sample to births occurring between 2015–2017. Observations missing either the child's date of birth or mother's date of birth are excluded, yielding 2,431 initial cases.

We link these data to the Single Registry to obtain maternal sociodemographic characteristics. Before linking, we eliminate RESP records that cannot be uniquely identified by the tuple (*child date of birth*, *mother date of birth*, *municipality of residence*), which reduces the sample to 2,411 cases. The fact that there are only 10 pairs of cases that need to be dropped due to ambiguous identification is an encouraging sign for match quality.

The linkage procedure follows a sequential approach across two Single Registry vintages. we first attempt linkage to SR-2017 using exact agreement on (child date of birth, mother date of birth, municipality of residence). Cases with no SR match or multiple SR matches are excluded, leaving 940 successfully linked cases. To recover additional linkages, I repeat this procedure using SR-2019 for the remaining unlinked RESP cases, applying identical matching criteria and exclusion rules. This second pass yields 281 total linked cases. Finally, I exclude linked observations with missing maternal education information in the Single Registry data (21 cases), producing the final case sample of 1,200 observations available for matching. During the process there are 8 cases of ambiguous linkage in the SR-2017 and 2 in SR-2019, indicating rates of erroneous linkages on the order of 1%. The final linkage rate of about 50% roughly corresponds to the coverage of the Single Registry.

A.2 Construction of Matched Control Groups

The matching strategy employs exact matching within cells defined by key maternal and child characteristics. I retain all treated observations that can be matched to at least one control unit within their respective matching cell.

The exact-matching procedure uses the following variables:

- Month of birth of the child
- Year of birth of the mother
- Maternal education (binary): completed high school or higher vs. less than high school
- Race (binary): White vs. Non-White
- Birth order (binary): firstborn vs. higher-order birth
- Geography: either municipality (in stages 1 and 2) or microregion with a population constraint (stages 3 and 4)

The matching follows a two-stage sequential process, progressing from restrictive to more flexible geographic criteria:

Stage 1: Municipality-level matching in a combination of SR-2017 and SR-2019. For each treated case in the N5 sample, I identify all SR-2017 and SR-2019 individuals who are identical on all matching variables and reside in the same municipality. This stage yields 691 successfully matched treated cases and 6,009 corresponding controls.

Stage 2: Microregion-level matching in a combination of SR-2017 and SR-2019. For treated cases unmatched in Stage 1, I relax the geographic restriction to the microregion level while imposing an additional constraint: treated and control municipalities must belong to the same population-size quintile. This restriction prevents inappropriate matches between small rural municipalities and large urban centers within the same microregion. This stage adds 198 treated cases and 1,480 controls to the matched sample.

Certain details of the matching design merit emphasis. First, there is no upper limit on the number of controls matched to each treated unit. Because matching is exact within cells, a single control observation may be matched to multiple treated units that share identical characteristics within the same matching cell and stage. Second, controls are not reused across matching stages: individuals serving as controls in earlier stages are excluded from the pool of potential controls in subsequent stages, ensuring clean temporal and geographic stratification.

The final analytical sample comprises 867 treated observations (all cases with at least one matched control) and 7,489 total control observations. A balance table presenting summary statistics for key covariates across the treated and control samples appears in Table 1 of the main text.

B Appendix: Methods

This section presents methodological considerations for our choice of main strategy, consisting of matching on observables and presenting weighted differences of means, while checking for zero differences in baseline.

CMI vs Parallel Trends: First, we claim that, if there are no differences at baseline, the assumption of conditionally parallel counterfactual trends implies conditional mean independence. To see this, simply recall that the conditional parallel trends assumption can be expressed as:

$$E[Y^0_{i,t} - Y^0_{i,0}|T_i = 1, S = s] = E[Y^0_{i,t} - Y^0_{i,0}|T_i = 0, S = s]$$

with $t = 0$ denoting the baseline period. If the treated and control group have no differences in baseline: $E[Y^0_{i0}|T_i = 1, S = s] = E[Y^0_{i0}|T_i = 0, S = s]$. Thus, we can eliminate the terms from both sides and simplify to:

$$E[Y^0_{i,t}|T_i = 1, S = s] = E[Y^0_{i,t}|T_i = 0, S = s]$$

which is CMI. This shows that the main identification assumption for our strategy is not stronger than parallel trends, provided we can establish zero baseline difference.

Bounded Outcomes and Parallel Trends: or outcomes with a limited range like employment, relying on the standard DiD with additive parallel trends can be dangerous. If one group is close to the bounds of the variable's domain (as is the case in this paper) and there are nonzero baseline differences, the parallel trends assumption may imply out-of-bounds counterfactuals. For a simple illustration, suppose the treated mothers had 10% employment rate the year before childbirth, and controls had 15%. Suppose further that employment of controls falls to 2%. In this case, the implied counterfactual employment rate of the treated is -3%. Even if the observed ex-post employment rate of the treated is 0, the resulting DiD estimate of the treatment effect would be +3 p.p.

One possible solution to this problem is to change the hypothesis to a multiplicative parallel trends, and estimate the model in logs. While this may be a viable alternative in some contexts, several issues arise here. First, log(0) is undefined and cannot be applied to the employment variable without more careful modeling, such as with a Poisson regression model or latent variable model. More fundamentally, unless pre-trends are flat or identical, we cannot have parallel trends both in levels and in logs.

We show that, after matching, the baseline gap in most of our estimates is close to zero, so the bound issue does not arise. Our preferred estimator keeps that gap visible and testable, guaranteeing robustness against the issue. Furthermore, by testing not only equality in pre-trends but also in levels, our test of whether the method adequately controls for selection is stricter: we are ensuring not only equality of pre-trends but also levels.

C Appendix: Mortality

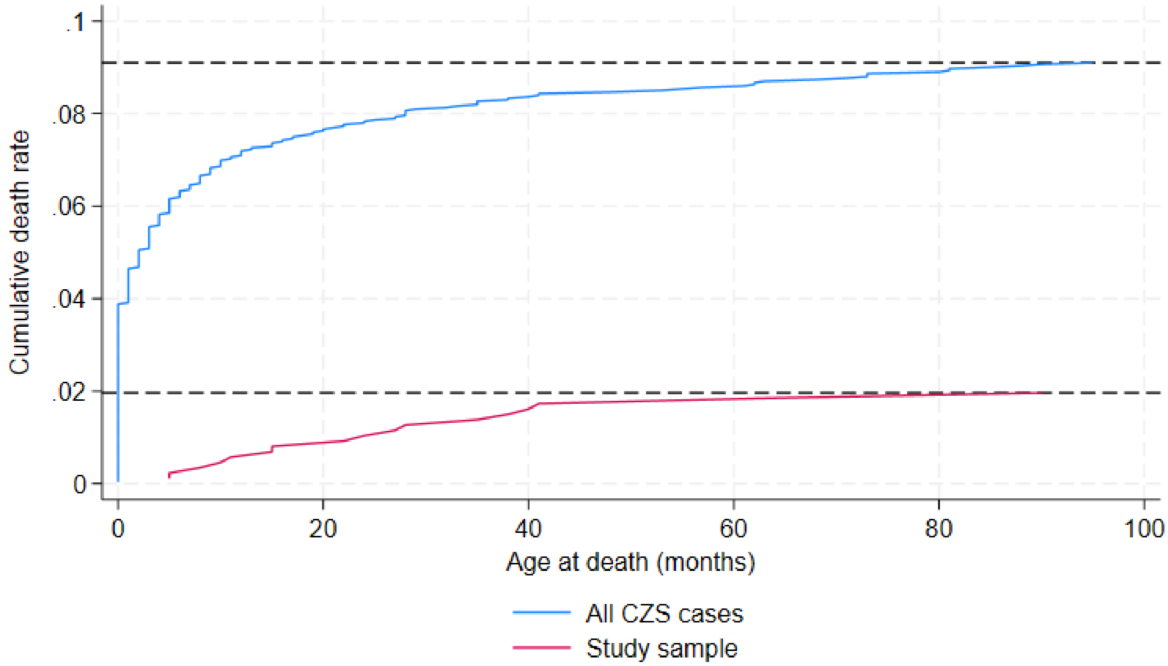
In this section we describe the child mortality patterns in the data and what they imply for our results.

Because our sample is limited to the Single Registry, there is some degree of selection against child mortality. Figure C1 shows the cumulative rate of death for our sample compared to the overall death rate for CZS cases based on the full RESP data. While the overall mortality rate at 36 months is about 8%, it is roughly 1.5% (12 cases of 867) for our sample. The difference is especially pronounced for very early deaths: a full 6% of cases die before 6 months of age, but only 1 of these cases is in our sample (0.17%).

This strong selection is possibly because many families qualify for government programs after having a child. If the child dies in early infancy, the parents may never qualify and, therefore, not be included in the Single Registry. This means that our estimates are much more representative of surviving cases, and therefore more informative about *child disability* itself, as opposed to health shocks that include a high probability of death.

To gauge the sensitivity of our results to mortality we calculate Lee bounds for our main estimates. For every case of death before 2020, we re-estimate the model under two assumptions: the outcome for these families would have been equal to the minimum outcome in the sample, or to the maximum outcome in the sample. We impute these values in the month of death of the child and keep it until the end of the sample interval. Table C1 shows the results. Overall, the bounds are relatively tight. For employment, earnings and hours the bounds exclude zero.

Figure C1: Cumulative Deaths



Notes: This figure shows the cumulative mortality rate by months of age for two groups: the overall sample of CZS cases from RESP, and our sample, after cleaning, linking to the Single Registry and matching to controls.

Table C1: Sensitivity Analysis: Mortality

	Min	Max
Employment	-0.041	-0.032
Earnings	-40.190	-21.812
Monthly wages if employed	-130.401	251.871
Weekly hours	-2.069	-0.819
Weekly hours if employed	-7.993	15.774

Notes: This table shows Lee bounds for the main effects. We re-estimated the main model assuming each mortality case among treated had either the maximum or minimum outcome in the sample in any period after the date of death.

D Appendix: BPC Propensity Score Estimation

This section describes the estimation of the propensity score used in the analysis of the BPC. We focus on identifying the probability of being a recipient in 2018, following the standard approach. We start with a probit model, including the variables used to find matched controls: race, mother's age, mother's education, place and time of childbirth. We also include an indicator of severe microcephaly, i.e., the child's head circumference being less than 3 standard deviations for its gender. Given the estimated propensity score, we test whether each covariate is balanced after controlling for the PS. If not, we include additional terms for cross products and higher order polynomials. The test is done with linear, cubic and ventile dummies of the PS; if any reject balance, we proceed to add more terms to the model.

The final estimated model includes the following terms:

1(Severe Microcephaly)

1(White)

1(Mother completed high school)

Mother's age

1(Mother formally employed in previous year)

Household income per capita in previous year

1(Family not in the Single Registry in previous year)

Child's year-quarter of birth

Birth order (5 dummies)

State

Mother formally employed in previous year x 1(Severe Microcephaly)

Child's year-quarter of birth x 1(Severe Microcephaly)

To avoid dropping categories for perfect prediction in a cell, two small states, Acre and Distrito Federal were pooled with Amazonas and Goiás, respectively and birth order is top coded at 5.

Table A1 shows the Average Marginal Effect of each variable. On average, severity of microcephaly and period of birth are the most predictive variables. Table A2 shows that the estimated propensity score is capable of balancing the covariates, even in flexible specifications. As to the predictive power, the area under the ROC curve is 71.7% (CI: (68.2%, 75.1%))[10], and we do not reject the hypothesis of good calibration with a Pearson goodness-of-fit test (p-value 48.7% with 20 groups).

[10]This can be interpreted as: given two random units, one success and a failure, the model assigns a higher probability of success to the successful unit 71.7% of the time.

Table D2: Propensity score estimation: Average Marginal Effects

Severe Microcephaly	0.106***
	(0.032)
Race: White	-0.014
	(0.046)
Mother completed high school	-0.037
	(0.038)
Mother's year of birth	-0.001
	(0.003)
Mother had a formal job in y-1	0.001
	(0.041)
Income per person in y-1	-0.000
	(0.000)
Not in Single Registry in y-1	0.046
	(0.044)
Quarter of birth:	
2015Q3	0.027
	(0.066)
2015Q4	0.309***
	(0.071)
2016Q1	0.388***
	(0.088)
2016Q2	0.327***
	(0.083)
2016Q3	0.089
	(0.082)
2016Q4	0.177
	(0.107)
2017Q1	0.053
	(0.119)
2017Q2	0.028
	(0.110)
2017Q3	-0.013
	(0.100)
Birth Order:	
Second	0.074
	(0.043)
Third	0.064
	(0.063)
Fourth	0.044
	(0.085)
Fifth or higher	0.229
	(0.155)
Observations	866

Notes: This table shows the average marginal effect for each variable. The omitted category for quarter of birth is 2015Q2 or earlier.

Table D3: Propensity Score Balance

Variables	PS Linear	PS Cubic	PS Ventiles
Family's income per person in y-1	0.979	0.981	0.982
Mother had a formal job in y-1	0.977	0.959	0.781
Severe microcephaly	0.915	0.747	0.691
Race: white	0.991	0.992	0.837
Mother's year of birth	0.495	0.512	0.428
Mother completed high school	0.953	0.901	0.978
Birth order	1.000	0.999	0.999
State	1.000	1.000	1.000
Child's month-year of birth	0.798	0.849	0.699

Notes: This table shows the estimated p-values of balance tests from regressions of an indicator of BPC recipient status in 2018 on the propensity score and each set of variables. Each line corresponds to a separate regression. The first column corresponds to regressions controlling for the propensity score, the second adds the square and cubes, and the third includes 20 ventile indicators. Each p-value corresponds to the test of the null that the variable does not predict the outcome after controlling for the propensity score. All variables are tested as indicators, except mothers age and child's month-year of birth, which are tested as decile indicators.