

The Effect of Decentralization on Child Mortality: Evidence from a Public Health Reform*

Dominique Araya-Vergara

Fernanda Rojas-Ampuero

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Abstract

In the 1980s in Chile, a policy reform shifted the administration of public services from a centralized to a local system, placing local municipalities in charge of administering primary care establishments. We exploit the reform's staggered implementation and past utilization rates of establishments to study how differential exposure to this reform affects child mortality and birth outcomes. We combine birth records with archival data on the dates when each primary care establishment was transferred to a local municipality. Our results indicate that greater exposure to the reform leads to higher neonatal and infant mortality during the implementation period, with the effects disappearing afterward. The rise in child mortality is driven by perinatal conditions, consistent with worse birth outcomes such as low birth weight. Rural municipalities and those with a higher administrative burden experienced greater increases in child mortality.

Keywords: decentralization, local governments, child mortality, public health.

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1 Introduction

The decentralization of health systems is a common phenomenon (Manor, 1999). In 1987, the World Health Organization recommended decentralized healthcare systems (WHO, 1987), and by 1993 more than 80% of countries had embarked on decentralization reforms of various types (WB, 1993). Despite their popularity, the effects of these reforms are not well understood. On the one hand, decentralization aims to increase accountability, reduce information asymmetries, and increase the importance of individuals “voting with their feet” to reveal preferences for local goods (Besley and Case, 1995; Oates, 1993). On the other hand, inefficiencies may arise due to a loss of coordination, diminished economies of scale, or because local governments may be less capable of administering public services than the central government (Mookherjee, 2015). Moreover, while existing work has examined the efficiency gains of these reforms (Mahmood et al., 2024), less is known about their direct effects on health outcomes at the time of implementation.

In this paper, we study the effects of Chile’s 1980s decentralization reform, the Municipalization of Local Services (Municipalización de Servicios Comunes), on child mortality and birth outcomes. Our focus is the administrative decentralization of public primary care health centers (PCHCs), which were transferred from central to municipal control. The reform took place under the Pinochet dictatorship, a context that rules out the “voting with your feet” channel and thus allows us to examine the administrative mechanisms associated with decentralization. To evaluate the reform’s impact, we exploit its staggered implementation across municipalities and variation in past utilization rates of public PCHCs in 1975. In our empirical strategy, we estimate a difference-in-differences model that compares conception cohorts in municipalities with varying levels of past utilization rates before and after the first PCHC transfer.

To perform the analysis, we assemble a dataset that combines archival records with administrative data. We first digitize historical utilization rates of primary care services in 1975 and collect archival records of the date each public PCHC was transferred to a local municipality. Next, we combine these historical data and match them to individual birth and mortality records from 1985 onward to build a dataset at the con-

ception month and municipality level from 1985 to 1990. Although the decentralization process started in 1980, we focus on municipalities treated during the second wave of municipalization, which mainly occurred between 1987 and 1988, for two reasons. First, our administrative data are more granular after 1985. Second, the years of 1980 and 1981 correspond with the onset of multiple reforms in the country—including health, education, and pension reforms—making it more difficult to disentangle the effects of municipalization and other concurrent policies.

Our results show that increased exposure to local administration leads to higher neonatal and infant mortality. A cohort fully exposed to the municipalization reform during pregnancy and born in a municipality with 10% higher pre-utilization levels has 5.4% higher neonatal mortality and 4.2% higher infant mortality compared to a non-exposed cohort. Event-study estimates show that these effects are short-lived, lasting four to five quarters before becoming negligible. Importantly, at the time, Chile had relatively low child mortality rates compared to other developing countries, after decades of a steady downward trend since the 1960s. Hence, we interpret the increase in neonatal and infant mortality as a short-term pause in this trend.

We also find that the increase in child mortality is driven by increases in the shares of low-birth-weight (LBW) births (below 2,500 grams), very-low-birth-weight (VLBW) births (below 1,500 grams), and pre-term births (born before 37 weeks), as well as in the share of infant deaths due to pregnancy-related causes. Back-of-the-envelope calculations suggest that around one-third of the total effect of the reform on neonatal mortality is due to the increased risk of VLBW births, and about 50% of the total effect on infant mortality can be explained by perinatal causes of death. These results suggest that the rise in child mortality is partly due to reduced access to, or lower quality of, prenatal care in municipalities more exposed to the reform.

To understand where the effects come from, we explore heterogeneity by mothers' demographics at birth. We first examine marital status and schooling. We find slightly larger (more negative) effects for married mothers and for women with a high school education or more, though we cannot reject that the effects differ for single mothers or women with lower education. In contrast, we find statistically significant differences by maternal age at birth. The women most affected by the reform are those under 20

and above 34. For them, the likelihood of having an LBW or pre-term birth is twice as high as the average mother in the sample. We interpret these results as evidence that the reform more negatively affects the birth outcomes of children born to women with a higher pregnancy risk at baseline.

Finally, we evaluate the mechanisms behind the reform’s effects on child mortality. Qualitative evidence suggests that municipalities were unprepared to implement the reform in the short term due to a lack of expertise in the health sector ([Colegio Médico de Chile, 1983](#)) and because local authorities prioritized other public services, such as schools, over primary healthcare ([Carmona, 1992](#)). Motivated by this evidence, we examine mechanisms related to the financial and administrative characteristics of municipalities at baseline. We find that the negative effects of the reform on birth outcomes are larger in municipalities with higher per capita deficits, defined as the difference between per capita spending and revenue. We also find more negative effects on child mortality in municipalities with a larger share of rural establishments, likely because they had fewer resources and were located farther away from major city centers. In exploring administrative burden, we do not find that the number of PCHCs explains the effects. Instead, the speed of the municipalization process matters: municipalities where all establishments were transferred within two months experienced more negative effects on mortality and worse birth outcomes, while those that transitioned more gradually—allowing time to learn and adjust—experienced milder or no effects. Thus, the effects are not only driven by financial strain but also by limited administrative learning, which is consistent with the short-term nature of the results.

This paper contributes to several strands of literature. First, it contributes to the literature on the effects of decentralization reforms on health outcomes.¹ Previous studies have examined the effects of decentralization on child mortality ([Rubio, 2011](#); [Cantarero and Pascual, 2008](#); [Uchimura and Jütting, 2009](#); [Asfaw et al., 2007](#)), generally finding reductions in this outcome. Other studies have examined the gradual

¹See [Mookherjee \(2015\)](#) for a general review of the literature, and [Abimbola et al. \(2019\)](#) and [Cobos Muñoz et al. \(2017\)](#) for reviews on health outcomes. More recent evidence in economics of successful decentralization reforms include [Bianchi et al. \(2023\)](#), who study the effects of fiscal decentralization on labor market outcomes in Italy, and [Jackson \(2025\)](#) who examines the positive effects of a decentralization reform in Chicago on student outcomes.

administrative decentralization of healthcare (e.g., in Spain after 1981), also finding positive effects ([Jiménez-Rubio and García-Gómez, 2017](#); [Antón et al., 2014](#)). In contrast to this literature, we find negative effects (i.e., increases in child mortality). This is likely because we focus on a single aspect of the reform that is directly associated with administrative burden and lack of expertise, as opposed to political preferences, given the dictatorship context ([Faguet, 2004](#); [Perez et al., 2019](#)).

This paper also contributes to the extensive literature studying the effects of prenatal shocks on children’s health outcomes.² More related to our research, studies have found that increases in access to early prenatal care can improve birth weight ([Kose et al., 2024](#)) and lifespan ([Bailey and Goodman-Bacon, 2015](#)). Instead, our paper contributes by examining how disruptions to primary care operations from decentralization affect short-term birth outcomes.

Finally, the paper also contributes to a recent literature studying the relationship between managerial practices and health outcomes ([Muñoz and Otero, n.d.](#) in Chile; [Janke et al., 2024](#) in the UK; [Card et al., 2023](#) in California; [Hollingsworth et al., 2024](#) in North and South Carolina). Notably, all of these studies focus on the administration of hospitals. In contrast, this paper examines PCHCs, and the results suggest that shocks to the administration of PCHCs may have substantial short-term impacts on health outcomes.

The rest of the paper is organized as follows. Section 2 describes the reform and historical background. Section 3 describes the data, and Section 4 explains the empirical methodology. Section 5 presents the main results, heterogeneity analyses, threats to identification, and robustness checks. Section 6 explores mechanisms, and Section 7 concludes.

2 Historical background

During the first decade of the Pinochet dictatorship, multiple reforms changed the health insurance system and the healthcare administration. Among these, a 1980 reform transferred the administration of public services from the central government to

²See [Almond et al. \(2018\)](#) for a review.

municipalities, giving them responsibility for PCHCs and public schools, including their budgets, infrastructure, and personnel.³ This process is known as the “municipalization of public services.”

The reform had several objectives. First, the government aimed to decentralize the execution of health services to the municipality level to better address the diverse characteristics and needs of local populations. Second, it sought to improve the supervision of PCHCs that were physically distant from the national administration. Third, the reform intended to channel municipal funds toward improving the infrastructure and operations of PCHCs. Finally, it aimed to increase community participation and to integrate the health sector with other areas under municipal control, such as education and housing (Miranda et al., 1990).

A key aspect of the reform was its change to municipal funding. PCHCs were financed through two sources: a monetary transfer from the central to the local government, called Facturación por Atención Prestada en Establecimiento Municipal (FADEM), and a municipality transfer from each municipality’s own budget. FADEM was a nationally set transfer per health service but capped according to regional budgets. According to Gideon (2001), these budgets were set using historical and discretionary criteria, which in practice meant that municipality resources were used to cover the gap between the cost of operating local health services and the FADEM transfer (Heyermann, 1995). This new system created disparities in the quality of services provided by PCHCs, depending on the resources each municipality allocated to its health budget.

In addition to changes in funding, the reform required local municipalities to administer all infrastructure and personnel previously managed by the national and regional health services. Among other responsibilities, municipalities became responsible for hiring personnel, paying wages, and making acquisitions (Carmona, 1992). In particular, the personnel, which included doctors, nurses, and social workers, became municipal employees (Castañeda, 1992), meaning that medical workers lost the possibility of pursuing a civil servant career and forfeited the benefits associated with being public health

³A municipality is similar to a county in the United States, but in Chile, municipalities elect a mayor and administer local services. During the dictatorship, however, mayors were appointed by the central government.

workers ([González, 1992](#)).

The municipalization process occurred in waves. The first wave of transfers took place between 1981 and 1982, during which 28% of public health establishments were transferred to municipal control. The process was then paused until 1987, when a second wave of municipalization began. The two most likely reasons for stopping the process were the financial crisis of 1982 and significant opposition from physicians to the reform ([Miranda et al., 1990](#); [Heyermann, 1995](#)).

Later evaluations of the reform reported negative consequences and increased opposition from health professionals. For example, the Chilean Medical Association argued that “health professional salaries have not improved, and poorly qualified municipal bureaucracies have been established to oversee medical work. Prevention and health promotion efforts tend to be reduced by municipalities, with emphasis being placed on curative activities” ([Colegio Médico de Chile, 1983](#)). These problems became even more salient due to low budgets and poor management. In fact, the government recognized some of these issues in 1986, listing the main ones as financial problems resulting from arbitrary ceilings set for different municipalities, the loss of civil service careers, and the progressive weakening of coordination mechanisms between local health services and the national Ministry of Health. These problems persisted even after the end of the dictatorship. The Ministry of Health reported that they continued after 1990, specifically citing the lack of integration between local and regional health services, the absence of training plans for health professionals, and difficulties in retaining doctors and nurses in local services ([Heyermann, 1995](#)).

3 Data

We construct a novel dataset that combines archival records of the dates on which establishments were transferred to local municipalities with vital statistics records at the individual and municipality levels.

3.1 Archival data

3.1.1 Dates of establishment transfers

We obtain information on the transfer of each establishment to municipalities from excerpts of government decrees published in [Diario Oficial de la República de Chile \(2024\)](#) between 1981 and 1989.⁴ We attempt to find all decrees related to an establishment transfer during the years the reform was implemented. The *Diario Oficial* is published daily in Chile (except Sundays and holidays). After manually searching each issue, we find 36 decree excerpts.

These excerpts contain the exact date on which a municipality assumes control of a given establishment, along with the type and name of the establishment and the specific decree they reference. The dates provided include the decree signing date and the publication date in the *Diario Oficial*. Each decree takes effect on the first day of the month following its publication. Figure A.1, panel (a) presents a sample of the government excerpts. PCHCs are classified in three different categories based on the population they serve and the complexity of the medical care. We refer to all types as primary care health centers.

Municipalities do not receive control of all their health establishments at once. When they receive control of new establishments, an amending decree is issued (its excerpt published in the *Diario Oficial*) at a later date, listing the newly transferred establishments. Figure A.1, panel (b) shows an example of these amending decrees. It contains the same information as the initial excerpts and also lists the original decrees they modify.⁵ Using the municipality name, we match each establishment’s transfer information with our other data sources.

We collect data on 1,880 establishments in the country: 1,035 are small rural health-care facilities that provide very basic medical services (called *postas rurales*), 579 are even smaller rural facilities (*estaciones médico rurales*), and 266 are PCHCs.

⁴The digital version of the *Diario Oficial de la República de Chile* is available [here](#) and in physical form at Chile’s National Library.

⁵Sometimes the amendment is not the transfer of a new establishment but another change in the decree. We also record the date of these changes. In general, they refer to adjustments in the price of services.

We validate our final sample of transferred establishments using data provided by [Miranda et al. \(1990\)](#), who report the total number of establishments transferred to municipal control by type and year between 1981 and 1988. We complement the data for 1989 using figures reported by [Heyermann \(1995\)](#).⁶ Appendix Table [A.1](#) presents the breakdown of our collected data by period and establishment type and compares them to the data from [Miranda et al. \(1990\)](#) and [Heyermann \(1995\)](#). The years of transfers are similar but do not coincide exactly with those sources. Moreover, both sources are incomplete relative to what we find and are not fully consistent with each other. Compared with [Heyermann \(1995\)](#), 99% of *postas rurales* transfers, 93% of *estaciones médico rurales* transfers, and 83% of *consultorios* transfers are captured in our data.⁷ Hence, we are confident that our sample is representative of the transfers that occurred as part of the decentralization reform.

3.1.2 Primary care establishments visits

To measure utilization of primary care establishments, we digitize yearbooks containing information on healthcare visits in 1975 ([Servicio Nacional de Salud, 1997](#)). The data are available at the establishment level for large PCHCs and are aggregated at the health service area (HSA) for small rural establishments. An HSA is a group of municipalities used by the Ministry of Health in 1975 to administer health services from the central to the local level.

We use the crosswalk available in [Livingstone \(1976\)](#) to assign each municipality in the country to an HSA in 1975 based on its name.⁸ This process leads to 96% of municipalities being matched to an HSA. For the 4% of municipalities not matched in the initial process, we match them to an HSA based on the HSA of other municipalities within the same higher-level administrative division (*departamento*) in 1975, with the restriction that all other municipalities in the same *departamento* must belong to the same HSA. After this step, only four municipalities are not matched: Algarrobo, Casablanca, Santiago, and Porvenir. Except for Santiago, the capital, the other mu-

⁶We are unable to locate the original sources cited in these papers.

⁷*Consultorios* can serve both urban and rural populations.

⁸The crosswalk indicates that municipalities in the province of San Antonio are part of the province of Santiago. We manually correct this.

municipalities do not have large populations. Specifically, the municipality of Santiago cannot be assigned to a unique HSA in 1975, as different PCHCs depended on different HSAs. Therefore, we exclude Santiago (as defined in 1975) as well as any municipalities created after 1975. We discuss how these restrictions affect the comparability of our sample to all municipalities in Chile in Section 3.5.

3.1.3 Population counts

We digitize demographic yearbooks compiled by Chile’s National Bureau of Statistics (INE) from 1970 to 1990. These yearbooks contain vital statistics information on births, deaths, marriages, and population counts, organized by municipality. We use the municipal population counts from 1975 to generate a utilization measure relative to each municipality’s population count. Further details are provided later in Section 3.3.

3.2 Individual-level data

Starting in 1985, we have access to individual-level data on birth and death records provided by the INE. The birth records contain information on date of birth, birth weight, gestational age in weeks, municipality of birth, and parents’ demographics. The death records contain information on date of birth and death, municipality of death, and causes of death. For infant deaths, we also observe mothers’ characteristics.

3.3 Primary care utilization in 1975

Using 1975 population counts at the municipality level, we create utilization measures in 1975 at the HSA level by dividing the number of children’s PCHC visits in an area by its population. We define a utilization measure U_a as follows:

$$U_{a,1975} = \frac{PCV_{a,1975}}{Population_{a,1975}} \quad (1)$$

where a denotes one of the HSAs in the country in 1975, $PCV_{a,1975}$ is the area-specific count of children’s visits to a primary care establishment in 1975, and $Population_{a,1975}$

is the HSA-specific population.⁹

3.4 Sampling restrictions

We restrict the analysis to live births and infant deaths that occurred in municipalities whose first establishment transfer occurred during the second wave of the reform between 1987 and 1990, and whose month of conception is between January 1985 and December 1990. Because we cannot link live births and infant deaths at the individual level, we collapse the data into conception-month $\times$ birth-municipality cells. Birth-municipality is defined as the mother’s municipality of residence at the time of the child’s birth or death.

Before constructing the cells, we further restrict the sample to live births to mothers aged 15–49 and trim outliers for birth weight and gestational age. We drop 0.5% of the smallest and largest infants and 1% of the longest gestational ages, as they could represent outliers in small municipalities or coding errors. To avoid outliers in small municipalities with only a few births and multiple zeros, we further restrict the sample to municipalities with at least 11 live births on average over the time period. As previously mentioned, we also restrict the sample to municipalities that existed in 1975 and drop Santiago, as it cannot be assigned to a unique HSA. Finally, we restrict the sample to municipalities with revenue and spending data in 1985, which we obtain from [González et al. \(2021\)](#).

To construct the variable for month of conception, we use data on the exact date of birth and gestational age (measured in weeks). If data for either are missing, we treat the month of conception as missing in the live births data. For the death records, we use data on the date of death and age at death to assign a month of birth to the observation. When gestational age is missing from the death records, we assign the average gestational age of deceased children in the same quarter-municipality with non-missing gestational age.¹⁰ We do this because the number of deaths in each cell

⁹For more information on the data sources, see chapter 3 in [Araya-Vergara \(2024\)](#).

¹⁰As a robustness check, we construct bounds for the results as follows. When the gestational age is missing from the death records, we assign months of conception to these observations. For the upper bound, we assume a full-term pregnancy lasting 40 weeks; for the lower bound, we assume a very pre-term pregnancy lasting 27 weeks.

is relatively small, and there is a non-insignificant number of missing gestational ages. Thus, treating these data as missing tends to inflate the number of cells with zero deaths.

To assign birth dates to observations with missing birth dates from the death records, we assume that infants who died within hours were born the same day as their death. For those who died within days of birth, we calculate the day of birth by subtracting the age in days from the date of death. Last, for infants who died within months of birth, we calculate the month of birth by subtracting the age in months from the date of death.

3.5 Summary statistics

Table 1 presents summary statistics for municipalities in the estimation sample in 1985, before the second wave of the reform occurred. For comparison, column (1) presents summary statistics for all municipalities in Chile in 1985, while column (2) presents them for municipalities in the estimation sample. Columns (3) and (4) present summary statistics by levels of past utilization rates, that is, for municipalities with child utilization rates in 1975 below the sample median (low utilization) and above the sample median (high utilization), respectively.

There are 126 municipalities in our sample, from a universe of 227 with utilization data in 1975.¹¹ The municipalities in our sample have higher birth counts because the estimation sample includes larger municipalities (Panel A), but birth outcomes, mothers' characteristics, and municipality characteristics are similar across samples in columns (1) and (2) (Panels B, C, and D).

When comparing the characteristics of municipalities between high and low past utilization rates, Panel A shows that high-utilization areas account for a larger share of births, consistent with a higher utilization of health services and larger populations in those areas. Panel B shows that children in the estimation sample are born at 39 weeks of gestation and weigh 3,236 grams. The proportion of LBW births is 6.2%, and it is slightly higher in low-utilization areas. Additionally, 0.4% of births in 1985 are VLBW,

¹¹According to the 1982 Population Census, there were 324 municipalities in Chile, but 227 existed in 1975 or had utilization data in 1975.

and this rate is the same in low- and high-utilization areas. Finally, 5.4% of births are pre-term (before 37 weeks), with similar rates across low- and high-utilization areas but lower in high-utilization areas.

Panel C shows that mothers in the sample are 25.4 years old on average, and 33% are single. These numbers are similar across low- and high-utilization areas. However, mothers in high-utilization areas have more education than those in low-utilization areas.

Panel D reports municipality characteristics. As expected, utilization rates are higher in high-utilization areas—0.37 child visits per person versus 0.22 in low-utilization areas. These municipalities have three fewer establishments, which are likely larger given the larger populations they serve. Additionally, 70.3% of municipalities in high-utilization areas transferred all their establishments to local administration within two months, compared with 57.4% in low-utilization areas. Finally, high-utilization areas have both lower per capita revenue and spending than low-utilization areas, likely reflecting larger populations, but the municipal deficit—defined as the ratio between total spending and total revenue in a given year—is greater in low-utilization areas.

Figure 1 shows that child mortality varies by utilization rates after the municipalization process begins. The figure plots the relation between average neonatal and infant mortality and the calendar month of conception, separately for municipalities with high and low utilization in 1975. Each dot represents the utilization-specific mean of child mortality in each conception-month bin, weighted by the number of live births in that cell. The means are constructed using the approach developed in [Cattaneo et al. \(2025\)](#). The vertical dashed line indicates the start of the second wave of municipalization, and the vertical solid line indicates the first cohort whose pregnancy was affected by the municipalization process.

Mortality rates follow similar trends in high- and low-utilization areas for cohorts conceived before the start of the second wave, with low-utilization area having worse outcomes on average. After the start of the second wave, however, cohorts in high-utilization areas experience worse health outcomes, converging toward the levels of low-utilization areas. This suggests that the downward trend of child mortality stops in the short term and is more pronounced in high-utilization areas. Appendix Figure

A.2 presents the same plots for the outcomes of birth weight and gestational age, suggesting very similar short-term patterns.

There are several reasons why differences in utilization rates can reflect differential exposure to the reform. First, municipalities with higher utilization may be more exposed because of the larger administrative burden associated with treating populations with a greater demand for services. Second, a larger share of the population in these municipalities is directly affected by the decentralization. Our utilization measure captures both components: population exposure to the reform and the intensity of children’s utilization of public services in a given municipality, proxied by child visits.

4 Methodology

To estimate the effects of the reform on mortality and birth outcomes, we exploit variation in pre-municipalization utilization rates of primary care establishments, combined with prenatal cohort-level variation relative to the date of a transfer in the corresponding municipality. Specifically, we compare cohorts conceived in municipalities within HSAs with high utilization in 1975 to those conceived in municipalities with low utilization in 1975, before and after the start of the municipalization process in the municipality of birth.

To estimate the reform’s total effect, we run a difference-in-differences model where we interact the utilization measure with the proportion of expected gestation months occurring under the new municipal administration:

$$Y_{cy(k)} = \gamma_c + \delta_{y(k)} + \beta U_{a(c),1975} \times Share_{cy(k)} + \Gamma_1 X_{cy(k)} + \epsilon_{cy(k)}, \quad (2)$$

where $Y_{cy(k)}$ is the average outcome (e.g., mortality rate) for the cohort conceived in month k of year y in municipality c . $U_{a(c)}$ is the utilization rate of PCHCs in 1975 in HSA a where municipality c is located. We measure utilization as described in Section 3.3. $Share_{cy(k)}$ is the share of the expected months of pregnancy that a mother spends under the new municipal administration, defined as the difference between ten months and the conception month. We control for birth-municipality fixed effects, γ_c , and month-

of-conception fixed effects, $\delta_{y(k)}$. For precision, we also control for municipality-cohort-level covariates X_{cyk} that include the following: mothers' average characteristics (such as age at birth, share of single mothers, share with completed high school, and share with some college education) in each municipality and month-of-conception cell. We also control for per capita municipal revenue and spending per year, and the municipal deficit per year, which control for municipalities' total financial burden. Standard errors are clustered at the municipality level, and we weight observations by the number of children born in each municipality-conception-month cell.

In this equation, the coefficient of interest is β , which captures the effect of full exposure to the reform during pregnancy in high- versus low-utilization areas on birth outcomes and infant mortality. This model allows us to capture the effect of being partially exposed to the municipalization process during pregnancy.

To estimate the effects per month relative to the reform, we expand our model and estimate an event-study specification using the following equation:

$$Y_{cy(k)} = \gamma_c + \delta_{y(k)} + \sum_{\tau} \beta_{\tau} U_{a(c),1975} \mathbf{1}(\tau = k - Month_c) + \Gamma_1 X_{cy(k)} + \epsilon_{cy(k)}, \quad (3)$$

where all variables are defined as before, and event time τ is defined as the difference between the conception month for cohort $y(k)$ and the month of the first transfer of a PCHC to municipality c , $Month_c$. The coefficients of interest in this model are β_{τ} s, which capture the effect of the change in administration on cohorts conceived τ months after the start of the municipalization process in high- versus low-utilization areas.

5 Results

In this section, we estimate the reform's effects on death and birth outcomes. We present results at both the aggregate and individual levels and examine which groups of mothers are most affected.

5.1 Exposure to the reform and child mortality

We begin by presenting estimates for the difference-in-differences model in Table 2, for both neonatal mortality (28 days) in Panel A and infant mortality (one year) in Panel B. The estimates are presented under three different models: column (1) does not control for any individual- or municipality-time-level characteristic, column (2) adds average mother characteristics as controls, and column (3) adds municipality revenue and spending per capita controls. The coefficients on the interaction between past utilization and the share of a pregnancy exposed to the reform remain stable after adding controls; therefore, column (3) is our preferred specification.

The results in column (3) imply that a cohort with 10% higher past utilization and full exposure to the local administration during pregnancy experiences 5.4% higher neonatal mortality and 4.2% higher infant mortality compared to a cohort with no exposure.

Figure 2 presents the results of the event-study model. Although noisy, the estimates suggest that the mortality rates of cohorts in high- versus low-utilization areas conceived after the first PCHC transfer increase for both neonatal and infant mortality. Importantly, the higher infant mortality rate in panel (b) appears three months before time zero, implying that cohorts partially exposed to the local administration during pregnancy may be affected by the policy; however, the estimates are noisy and not statistically different from zero. The effects peak at quarter 4, or one year after the first transfer, and then decrease, consistent with Figure A.1: the municipalization reform has short-term effects on child mortality that fade after one year, reducing the downward trend in neonatal and infant mortality more in areas with higher past utilization rates.

The administration change of a PCHC could have impacted child mortality at two points: the care children already born received from primary care doctors or the care expecting mothers received during pregnancy. To explore these two hypotheses, we examine infant mortality by cause of death. Table 3 presents the results. A cohort with 10% higher past utilization that was fully exposed to the local administration has a 6.8% higher share of deaths related to perinatal conditions and an 11.7% higher

share of deaths related to infections, compared to non-exposed cohorts. We do not find significant changes in deaths due to respiratory, congenital, injuries, poisoning, or nervous-system-related causes.

Taken together, our results imply that greater exposure to decentralization increases child mortality, partly through worsened prenatal care received by expecting mothers. In our sample, 36% of infant mortality is due to perinatal conditions; therefore, the increase in mortality from perinatal causes explains about 50% of the overall increase in infant mortality.

If a higher share of deaths stem from perinatal conditions, we should expect worse outcomes at birth. To explore this, we turn to individual-level data on birth outcomes.

5.2 Individual-level analysis

We take advantage of our individual-level data and repeat our previous analysis on individual-level outcomes, controlling for individual mothers' characteristics. Thus, instead of constructing cells at the municipality-cohort level, we run the following difference-in-differences model at the individual level:

$$Y_{ick} = \gamma_c + \delta_k + \beta U_{a(c),1975} \times Share_{ick} + \Gamma_1 X_{ick} + \epsilon_{ick}, \quad (4)$$

where now X_{ick} includes an indicator for a first pregnancy, high school completion, any college education, and the mother's age at birth. We perform individual-level analyses exclusively for birth outcomes, for which we observe exact weeks between conception and birth. Standard errors are clustered at the municipality level.

Higher exposure to local administration leads to worse birth outcomes, as shown in Table 4. Panel A presents the results using the specification without any controls, Panel B adds mothers' characteristics as controls, and Panel C adds municipality controls (per capita revenue, per capita spending, and total deficit). We find that infants born in municipalities with 10% higher utilization in 1975—to mothers fully exposed to the reform—have a birth weight that is 35.94 grams lower and a lower gestation age that is 0.209 weeks. While these results are small on average, they are meaningful at the margin. Columns (3)–(5) show that the probability of an infant being born

LBW increases by 1.7 percentage points, or 2.7%, for mothers fully exposed to the reform during pregnancy in municipalities with 10% higher utilization in 1975. The probability of being born VLBW increases by 0.6 percentage points, and the probability of being born before 37 weeks increases by 2.2 percentage points. These changes are large in relative terms, corresponding to increases of 7.5% and 3.8% for mothers fully exposed under a 10% higher utilization rate. Results for the individual-level sample are consistent with the aggregate municipality-cohort results (see Appendix Table A.2).

To understand how much of the effect on neonatal mortality is explained by worse birth outcomes, we use estimates from [Gonzalez et al. \(2006\)](#) on neonatal mortality risk. The authors report that infants born VLBW in Chile in 1990 faced an increased risk of neonatal mortality of 401 per 1,000 births. This implies that the increased likelihood of VLBW of 0.6 percentage points accounts for about 37% of the increase in neonatal mortality in our sample ($0.006 \times 0.401/0.0065$).

Figure 3 presents results for the event-study model at the individual level. As with the difference-in-differences model, we observe a decrease in weight, accompanied by an increase in the share of LBW and pre-term births for cohorts conceived after the first municipality transfer. Although the results are noisy, we find no evidence of pre-trends on the main outcomes. As with mortality rates, the effects on individual birth outcomes decline after quarter 5, suggesting short-term negative effects for cohorts exposed to the reform during pregnancy or in the early months after birth.

5.3 Heterogeneity by mothers' characteristics

In this section, we examine heterogeneity at the individual level by mothers' characteristics, focusing on differences by marital status, education, and age.

We estimate a version of Panel C of Table 4 with interactions between group indicators and the utilization-exposure variable for each characteristic. Table 5 presents the results. Each panel tests for differences across categories of mothers' demographics. Panel A reports results from the stratified regression based on marital status, Panel B by education, and Panels C and D by age, differentiating between teen mothers (Panel C) and older mothers above age 34 (Panel D). For each panel, we test whether

the estimates differ statistically between groups and report the p-values in the bottom rows.

The results show that married women are more negatively affected by the reform than single mothers in terms of average birth weight and gestational age. However, we do not find statistical evidence that infants born to these groups differ. The increased likelihood of an infant being born LBW or VLBW is similar for married and single mothers.

We find similar patterns by mother’s education in Panel B. On average, women with more schooling, defined as completed high school or more, have infants with lower weight as a consequence of the reform. However, on the margin, we do not find differences in the prevalence of LBW or VLBW between educational groups.

Interestingly, we find that age plays an important role in explaining which mothers are more affected by the reform. As shown in the health literature ([Phipps et al., 2002](#); [Salihi et al., 2003](#)),¹² mothers under age 20 or above age 34 face higher risks of adverse birth outcomes in terms of weight and gestational age. Panel C shows that a 10% higher exposure to local administration increases the probability of LBW by 2.3% for non-teen mothers, compared to 6.0% for teen mothers, a coefficient almost three times larger. For all outcomes, we reject the hypothesis of equality of coefficients. Similarly, Panel D shows that for mothers older than 34, higher exposure to the reform raises the probability of LBW by 7.4%, compared to 2.1% for younger mothers—again, about three times the effect.¹³

Overall, our heterogeneity analysis by mothers’ characteristics shows that the reform shifted the distribution of birth weight and gestational age to the left in high-utilization areas. Within these areas, the most affected mothers were those already at higher baseline risk, defined by their age at childbirth.

¹²See more up-to-date discussion on the topic [here](#).

¹³In terms of birth order, Appendix Table A.5 shows the results by birth order, comparing first pregnancies to higher-order pregnancies. First pregnancies appear to be less negatively affected than later ones in terms of birth weight, gestational age, and LBW outcomes. No significant differences are observed for VLBW or pre-term births.

5.4 Threats to identification

The identifying assumption for our empirical strategy is that in the absence of the administrative change of a PCHC, the difference between cohorts with high prenatal exposure to the reform and those with low prenatal exposure would have followed the same trends in high- and low-utilization areas. In other words, no unobserved municipality-specific, cohort-varying factors affect children’s health outcomes in ways that both correlate with a municipality’s utilization rate in 1975 and differentially impact more versus less exposed cohorts.

Recent literature has studied the potential bias of difference-in-differences designs with staggered treatment (Goodman-Bacon, 2021) and continuous treatment (Callaway et al., 2024). In our design, there are two potential sources: selection bias due to treatment effect heterogeneity across different “dose” groups, and time heterogeneity bias when early treated units serve as controls for late-treated units. Thus, to address bias from heterogeneous treatment effects, we use the estimator proposed by de Chaisemartin and D’Haultfoeuille (2024), which allows for a staggered design with group-specific intensities.¹⁴ This estimator compares the outcome evolution of municipality g with that of municipalities not yet treated.

This restriction implies that in our setting, we can analyze results for at most 11 months after the first partially affected cohort. Since pregnancies last 40 weeks (10 months), the timing of treatment definition matters. Specifically, we can (i) test for pre-trends but estimate the effect of higher exposure to local administration only on mothers who are partially affected, where the effects are expected to be smaller, or (ii) estimate the effects of higher exposure on mothers whose entire pregnancy occurred under local administration compared to those whose pregnancies were only partially exposed.

We use two definitions of treated cohorts. The first defines as treated those conceived four months before the first establishment transfer in their municipality or later. The second defines as treated those conceived ten months before the first transfer or later. We use the second sample to test for pre-trends and to allow for seven event-study

¹⁴We use the associated Stata package `did_multiplegt_dyn`.

coefficients (months) to be estimated given restrictions on panel length.

Table A.6 shows results for the first treatment definition. The table presents the average cumulative (total) effects per treatment unit for neonatal and infant mortality. On average, neonatal mortality increases by 0.0055 points and infant mortality by 0.007 points. While noisy, the effects are very similar in magnitude to the average treatment effect on the treated obtained in the difference-in-differences analysis. Importantly, in this analysis, some of the non-treated cohorts are actually partially treated, and the number of available controls is limited by construction. As a result, it is not surprising that the effects on child mortality are less significant.

Table A.7 presents results for the second analysis sample, which defines treated units as cohorts conceived at most ten months before the first transfers. We cannot reject the null hypothesis of joint nullity for the pre-periods (p-values of 0.53 and 0.55 for neonatal and infant mortality, respectively). The magnitude for neonatal mortality is similar to the main result, though not significant. However, the estimated effects for infant mortality are smaller, as the treated units are only partially treated.

5.5 Robustness checks

An additional concern in our estimation is potential simultaneous policies and economic changes that may also affect child mortality outcomes. To assess this, we use municipalities that completed the municipalization process before 1985 to perform a placebo exercise, checking for changes in conception-cohort trends at the start of the second wave in municipalities with high and low 1975 utilization levels. Appendix B presents the detailed analysis. We find no evidence of child mortality outcomes changing in these placebo municipalities after February 1987.

We also rule out the possibility of PCHCs and schools being transferred at the same time. Although both the transfer of schools and primary care establishments started in 1980, the process for schools was fairly quick, and by 1985, only 841 out of around 6,500 schools had not been transferred (Montt Leiva, 1995). This implies that the second wave of municipalization of primary care establishments does not overlap with the municipalization of schools.

Third, to check that there are no unobserved factors that change over time at the regional level, such as hospital-system-level variables that affect children’s outcomes and correlate with PCHC utilization in 1975, we add region-specific time trends to our main individual-level regression. Table A.4 presents the results, which remain statistically significant and similar in magnitude to the main results.

Finally, to rule out the possibility that specific HSAs are driving the results, we run separate regressions leaving one HSA out at a time. Figure A.3 shows that no single HSA drives the findings.

6 Mechanisms

Qualitative evidence from *Cuadernos Médicos Sociales* suggests that physicians viewed the municipalization process as fraught with issues.¹⁵ Physicians from different municipalities reported varying levels of involvement from Servicio de Salud (Regional Health Service) during the municipalization process. For example, Dr. Oscar Carmona recounted in 1992 that although the municipality where he worked had the necessary financial resources, the way those resources were allocated was the main obstacle to implementing the reform: “It was squandered in an impressive way. Because the “Servicio de Salud” did not advise them, health teams felt abandoned, and found themselves at the mercy of people who had no idea about healthcare” (Carmona, 1992).

In municipalities where there was less involvement from the mayor’s office, fewer problems were documented. For example, Dr. Hans Oppermann says, “Fortunately, both under the mayor from the previous administration and the current one, there was no major interference from the mayors office in the health department [...]. This spared us a series of problems, because practically everything kept functioning as it was—the distribution of medications, health policies, what the department had to do from a technical standpoint. We followed what the (Health) Service stipulated, because the position of municipal department director was always held by a physician” (Oppermann,

¹⁵*Cuadernos Médicos Sociales* is a journal established by Chilean physicians in 1959 to discuss the interaction between health and its social determinants. More information about the journal and its publications can be found [here](#).

1992).

The inexperience of the local administration in healthcare was a common complaint shared by multiple physicians. We present several quotes supporting this idea:

“Some purchases have to go through the municipality, to the municipal procurement department, where it takes 3 to 6 months to acquire them” (Carmona, 1992).

“The discretionary way in which mayors and/or municipal health corporations assigned functions and responsibilities, hired personnel, and set salaries—without competitive processes or objective criteria [...] led to a lack of trust in the system on the part of the employees and to a sense of job insecurity, which resulted in an attitude of servility rather than in properly performing their duties” (González, 1992).

“Where a clinic used to use 50 vials of vitamins B1 and B12 per quarter, 15,000 of each were purchased for the same period—and nobody checked and nobody controlled [...]. Yet, to make up for the lack of budget, the salaries of the professionals who joined were gradually reduced” (Carmona, 1992).

Given the previous evidence, we hypothesize that one of the main mechanisms explaining our results was the lack of administrative health expertise among municipal authorities and employees. Both anecdotal evidence from the period and theoretical work on inefficiencies in decentralization reforms support the idea that a lack of local expertise could explain our results (Mookherjee, 2015).

Unfortunately, we lack data on health workers at the establishment level or systematic information on how mayors made decisions beyond the qualitative evidence presented earlier. Hence, we proxy for the administrative burden faced by municipalities using their observable characteristics and focus on four sets of variables: municipal resources, rurality, number of establishments, and speed of the transfer process.

6.1 Municipal resources

We start our analysis by studying the differential effects of the reform by a municipality’s financial resources at baseline. We define a municipality’s per capita deficit as the difference between per capita spending and per capita revenue. We use data from [González et al. \(2021\)](#), who digitize historical municipal budgets, and choose 1985 as our baseline year. While we do not observe a municipality’s specific health deficit, we use the total per capita deficit to proxy for its total financial burden.

We divide our sample between municipalities with high per capita deficit (above median in 1985) and low per capita deficit (below median in 1985). Panel A of Table 6 shows the results, with mortality outcomes in the first two columns and individual birth outcomes in the next four. The results show no differences in mortality outcomes but larger, more negative effects of the reform on birth weight and gestational age for women giving birth in municipalities with high per capita deficit. For example, the effect on the prevalence of infants born VLBW is three times larger in municipalities with a higher municipal deficit. In Appendix Table A.8 we explore whether these effects come from municipalities with low revenue or high spending. We do not find differential effects along those margins on our main outcomes, suggesting that the effects of the reform are not due to the socioeconomic status of a municipality (proxied by revenue) but instead to the financial burden municipalities faced at the time of the transfers.

6.2 Share of rural establishments transferred

In Panel B of Table 6, we explore differences by rurality. We consider a municipality as rural if more than two-thirds of its PCHCs are exclusive to very rural areas (*Estaciones Rurales*). The idea behind this exercise is that the more rural a municipality is, the more isolated it tends to be and the less likely it is to have the resources to adjust quickly to the reform in the short term.

The results show that rural municipalities experienced worse outcomes due to the reform. We find that higher exposure to local administration has a greater impact on infant mortality. A 10% increase in exposure increases the infant mortality rate by 5.2% (compared to the average 4.2% increase). With a p-value of 0.051, we can

reject the hypothesis that the effect is the same in rural and urban municipalities. However, we cannot reject heterogeneous effects by rurality for the other outcomes due to large standard errors for the coefficients in urban municipalities, though the difference between coefficients is substantial.

6.3 Administrative burden

The previous analysis shows that rural municipalities and those with greater deficits experienced more negative effects in the short term. In this section, we explore if the effects go beyond financial constraints and are instead related to the administration of the reform itself.

To explore the hypothesis that municipalities facing a greater administrative burden experienced worse effects from decentralization, we examine two proxies for administrative burden: the number of transferred establishments and the speed at which they were transferred to local authorities. We first split our sample between municipalities with a large number of transfers relative to the sample median (Panel C). We then consider the municipalization process as “fast” or “all at once” when all transfers of a municipality occur in a two-month window, and “gradual” otherwise (Panel D).

Panel C shows that municipalities with a large number of transfers behave similarly to those with fewer transfers as a consequence of the reform. While some of the effects are larger in the “few transfers” category, we find no statistical evidence that the effects differ. This suggests that the administrative burden was not about the number of transfers but rather the speed of the process, as shown in Panel D.

Panel D reveals that the effects are considerably larger for municipalities that had all their establishments transferred simultaneously, compared to those with staggered transfers. In all the outcomes presented in Table 6, the effects of the reform are larger in absolute value for municipalities that had all their establishments transferred at once. When the process ends within two months, we can reject at the 10% level the hypothesis that the coefficients are equal for the share of deaths due to perinatal causes and birth-weight-related outcomes. Moreover, the magnitudes of the coefficients for municipalities with a fast municipalization process are around ten times larger for the

probability of LBW and three times larger for the probability of VLBW.

These results are very much consistent with the anecdotal evidence presented earlier in this section, where healthcare workers reported high levels of frustration that mayors unfamiliar with the healthcare system did not prioritize healthcare needs. They also align with the idea that municipalities likely learned to manage better over the medium term, which helps explain why we observe effects only in the short term. For local administrators whose establishments were all transferred at once, the shock was larger because they had no time to adjust, and our results suggest that this negatively affected children’s health outcomes.

7 Conclusion

Decentralization efforts are common around the world, but their effects are theoretically ambiguous. We examine the effects of a decentralization reform on children’s mortality and birth outcomes in the context of a reform implemented during a dictatorship, which rules out the “voting with your feet” channel and allows us to focus on the administrative channel at the time of implementation.

We compile archival records of the date of administrative transfer of public PCHCs to local governments during the 1980s in Chile. We find negative effects of higher exposure to local administration on neonatal and infant mortality in the short run. The higher mortality is associated with a greater share of deaths due to perinatal conditions, which are also linked to worse health outcomes at birth. Our results suggest that the increased likelihood of LBW births explains at least one-third of the total effect on neonatal mortality, and the increase in deaths due to perinatal conditions explains 50% of the total effect on infant mortality in our sample.

We also examine heterogeneous effects by mothers’ demographics and find evidence that the reform disproportionately affected mothers with high-risk pregnancies, measured by age. This indicates that on average, the reform increased the likelihood of worse birth outcomes for children born to mothers already at higher risk of complications at baseline.

When exploring mechanisms, we find results consistent with the idea that an in-

creased administrative burden on local administrators may explain the findings. More vulnerable municipalities, measured by their financial deficits, or rural municipalities, which were likely to be less connected with the rest of the healthcare system, experienced larger negative effects. Importantly, we also find suggestive evidence that lack of experience in local administration played a role.

Our mortality results contrast with previous analyses of health decentralization in other settings, that found positive effects on outcomes. In contrast, this study highlights how the implementation process itself and local administrative expertise, beyond individuals' ability to "vote with their feet," shapes the potential negative effects of decentralization efforts on children's outcomes.

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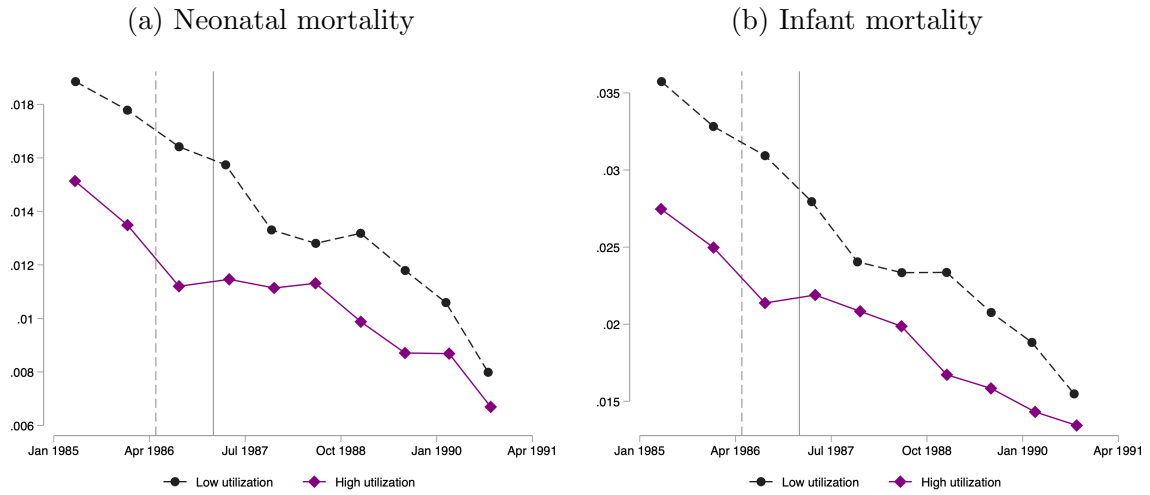
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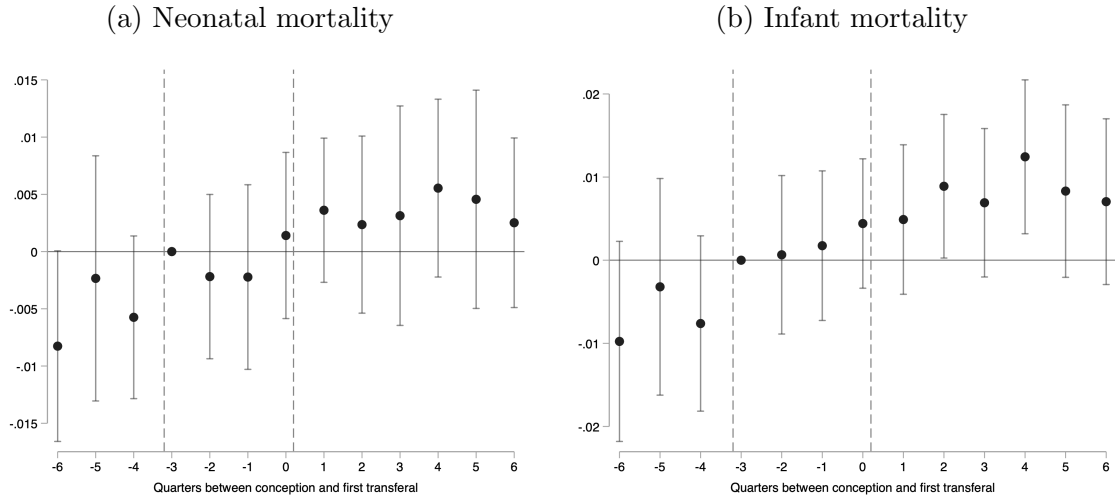
Figures and tables

Figure 1: Mortality rate by quarter of conception



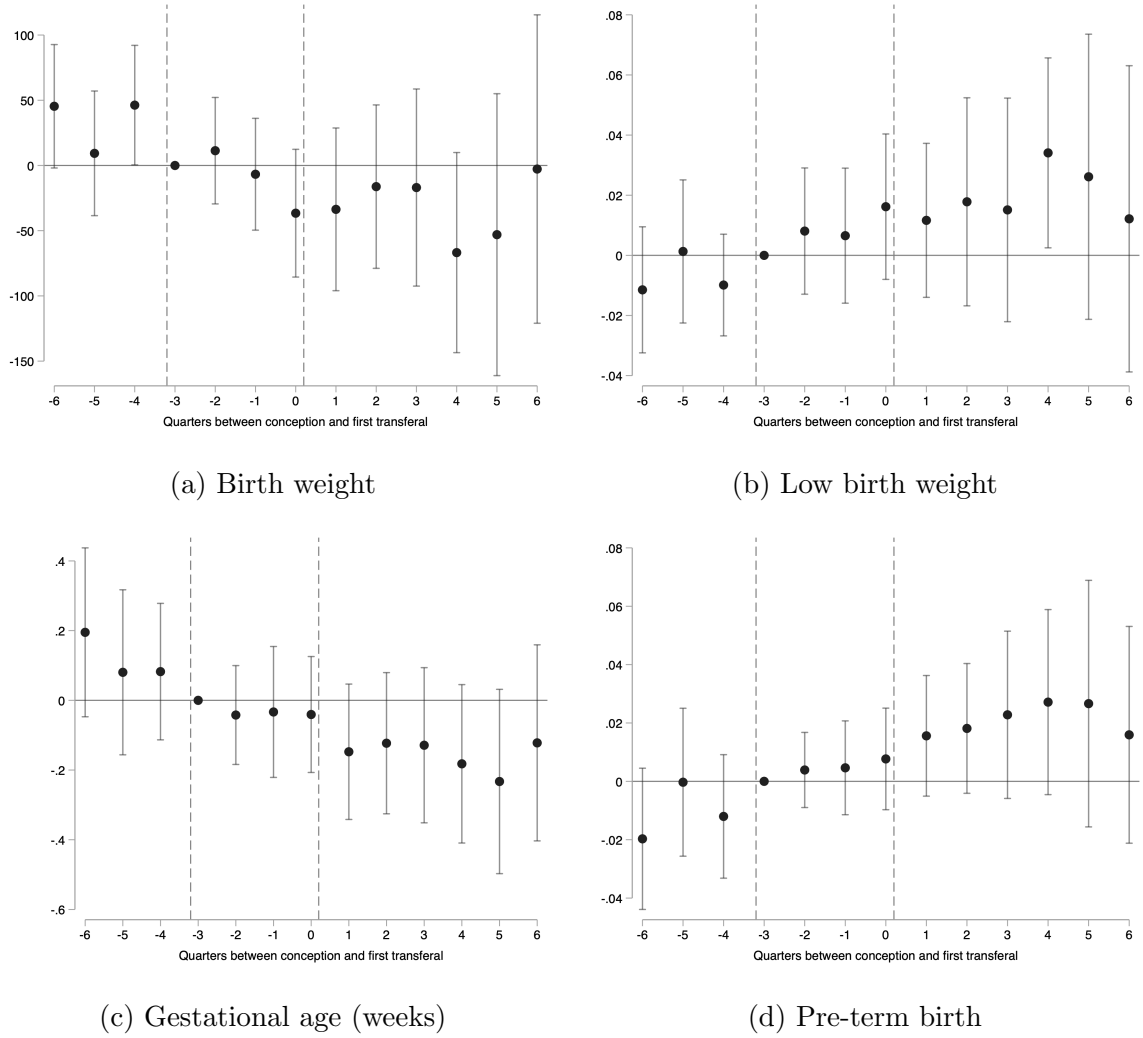
Notes: These figures show average mortality rates by quarter of conception, weighted by the number of births in each municipality-month. The sample is split into low- and high-utilization areas, defined by the median of the variable in 1975. The vertical dashed line represents the date of the first transfer in the estimation sample, and the solid vertical line represents the date of the last transfer.

Figure 2: Effect of the reform on short-term mortality outcomes



Notes: Panel (a) shows estimates of event-study coefficients from equation 3 for neonatal mortality, and panel (b) shows estimates for infant mortality. Controls include average mothers' characteristics by municipality of residence and municipality characteristics. The vertical lines represent the relative quarter of conception for the first cohort whose pregnancy was partially affected by the reform (relative time -3), and for the first cohort whose entire pregnancy was affected by the reform (relative time 0). The magnitudes shown are relative to event time -3, and we consider event times from -4 to -6 to be the pre-reform period.

Figure 3: Effect of the reform on individual birth outcomes



Notes: These figures show estimates of event-study coefficients from equation 3 on individual birth outcomes. Controls include individual mothers' characteristics and municipality characteristics. The vertical lines represent the relative quarter of conception for the first cohort whose pregnancy was partially affected by the reform (relative time -3), and for the first cohort whose entire pregnancy was affected by the reform (relative time 0). The magnitudes shown are relative to event time -3, and we consider event times from -4 to -6 to be the pre-reform period.

Table 1: Summary statistics before the second wave of municipalization

	All municipalities (1)	Estimation sample (2)	Estimation sample by utilization	
			Below median (3)	Above median (4)
Panel A: Counts				
Population	39,883.273	41,894.838	28,902.988	55,097.083
Births	69.425	72.663	47.228	98.510
Panel B: Birth outcomes				
Birth weight (grams)	3,238.499	3,236.582	3,236.975	3,236.182
Gestational age (weeks)	39.025	39.032	39.055	39.008
Low birth weight (LBW)	0.062	0.062	0.063	0.061
Very low birth weight (VLBW)	0.004	0.004	0.004	0.004
Pre-term birth	0.056	0.054	0.055	0.053
Panel C: Mother characteristics				
Mother's age	25.430	25.494	25.588	25.399
Single mothers	0.326	0.329	0.335	0.323
Mothers with high-school	0.212	0.206	0.166	0.245
Mothers with higher education	0.018	0.018	0.015	0.021
Panel D: Municipality characteristics				
Utilization in 1975	0.319	0.298	0.222	0.374
Total number of transfers	7.477	7.902	9.219	6.563
% rural establishments	0.098	0.091	0.082	0.103
% transfers within 2 months	0.585	0.638	0.574	0.703
Per-capita municipal revenue	11.304	10.965	11.681	10.238
Per-capita municipal spending	11.162	10.861	11.637	10.073
Municipal deficit	1.114	1.094	1.207	0.979
Observations (municipality x month)	2,706	1,494	753	741
Municipalities	227	126	63	63

Notes: This table shows summary statistics for the municipalities in 1985, before the beginning of the second wave of municipalization. Column (1) shows summary statistics for the 227 Chilean municipalities that existed in 1975 with utilization data. Column (2) reports summary statistics for the 126 municipalities in the estimation sample that experienced a transfer during the second wave. Column (3) covers municipalities in the estimation sample with utilization levels below the sample median, and column (4) covers those with utilization levels above the median.

Table 2: Municipalization effects on child mortality outcomes

	(1)	(2)	(3)
Panel A: Neonatal Mortality (< 28 days)			
Interaction	0.0073*** (0.0017)	0.0073*** (0.0017)	0.0065*** (0.0018)
Sample mean	0.012	0.012	0.012
R^2	0.089	0.090	0.090
Panel B: Infant mortality (<1 year)			
Interaction	0.0110*** (0.0035)	0.0110*** (0.0035)	0.0092*** (0.0032)
Sample mean	0.022	0.022	0.022
R^2	0.149	0.149	0.150
Observations	8,945	8,945	8,945
Municipalities	126	126	126
Avg. mother charact.	No	Yes	Yes
Municipality controls	No	No	Yes

Notes: This table reports difference-in-differences estimates of the effect of the reform on child mortality, measured at the municipality-month level, from equation 2 and weighted by the number of births in each municipality and month. The variable “Interaction” is defined as past utilization multiplied by the share of expected months a pregnancy is exposed to the municipalization process. Average mother characteristics are collapsed at the birth level and include average age, share single, share with high school completed, and share with at least one year of college. Municipality controls include municipal per capita income, per capita spending, and total deficit. Standard errors are clustered at the municipality level. Significance levels: 10%*, 5%**, 1%***.

Table 3: Municipalization effects on child mortality by causes of death

	Respiratory	Congenital	Infections	Injury/ poison	Nervous system	Perinatal
	(1)	(2)	(3)	(4)	(5)	(6)
Interaction	0.0010 (0.0014)	0.0009 (0.0013)	0.0012** (0.0005)	-0.0009 (0.0010)	0.0005 (0.0003)	0.0054*** (0.0015)
Sample mean	0.003	0.005	0.001	0.003	0.001	0.008
R^2	0.071	0.041	0.041	0.053	0.033	0.073
Observations	8,945	8,945	8,945	8,945	8,945	8,945
Municipalities	126	126	126	126	126	126
Avg. mother charact.	Yes	Yes	Yes	Yes	Yes	Yes
Municipality controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table shows difference-in-differences estimates of the effect of the reform on infant mortality by causes of death, measured at the municipality-month level, from equation 2 and weighted by the number of births in each municipality and month. The variable “Interaction” is defined as past utilization multiplied by the share of expected months a pregnancy is exposed to the municipalization process. Average mother characteristics are collapsed at the birth level and include average age, share single, share with high school completed, and share with at least one year of college. Municipality controls include municipal per capita income, per capita spending, and total deficit. Standard errors are clustered at the municipality level. Significance levels: 10%*, 5%**, 1%***.

Table 4: Municipalization effects on individual birth outcomes

	Birth outcomes				
	Birth weight (1)	Gestational age (2)	LBW (3)	VLBW (4)	Pre-term birth (5)
Panel A: No controls					
Interaction	-19.349 (19.299)	-0.149*** (0.041)	0.012** (0.005)	0.003** (0.001)	0.015*** (0.004)
Panel B: Mother characteristics controls					
Interaction	-20.556 (18.864)	-0.142*** (0.041)	0.012** (0.005)	0.003** (0.001)	0.014*** (0.004)
Panel C: Municipality controls					
Interaction	-35.941* (20.342)	-0.209*** (0.051)	0.017** (0.007)	0.006*** (0.002)	0.022*** (0.006)
Sample mean	3,255.653	38.928	0.063	0.008	0.058
Observations	740,804	740,804	740,804	740,804	740,804

Notes: This table shows difference-in-differences estimates of the effect of the reform on individual outcomes at birth, from equation 2. The variable “Interaction” is defined as past utilization multiplied by the share of expected months a pregnancy is exposed to the municipalization process. Birth weight is measured in grams, gestational age is measured in weeks, and “LBW” stands for low birth weight and corresponds to a birth weight below 2,500 grams. “VLBW” stands for very low birth weight and corresponds to a birth weight below 1,500 grams, and a pre-term birth is defined as an infant born before 37 weeks. Panel A shows results with no controls. Panel B adds mothers’ characteristics at birth, including, age, a dummy for single, a dummy for first pregnancy, a dummy for high school completion, and a dummy for college education. Panel C includes mothers’ characteristics at birth, as in Panel B, along with municipality-level controls: municipality per capita income and per capita spending, and municipality deficit. Standard errors are clustered at the mother’s municipality of residence at birth. Significance levels: 10%*, 5%** , 1%***.

Table 5: Heterogeneous effects of municipalization by mothers' demographics at birth

	Birth outcomes				
	Birth weight (1)	Gestational age (2)	LBW (3)	VLBW (4)	Pre-term (5)
Panel A. Mother's marital status					
Interaction* married	-53.571** (22.198)	-0.256*** (0.050)	0.020*** (0.007)	0.006*** (0.002)	0.026*** (0.005)
Interaction* single	-2.599 (18.455)	-0.114* (0.060)	0.013* (0.007)	0.004* (0.002)	0.014** (0.006)
Mean Y married	3,283.895	38.937	0.057	0.007	0.055
Mean Y single	3,197.636	38.911	0.074	0.009	0.064
p-value	0.000	0.000	0.124	0.121	0.003
Panel B. Mother's education					
Interaction * less than HS	-10.063 (16.274)	-0.190*** (0.047)	0.014** (0.006)	0.005*** (0.002)	0.019*** (0.004)
Interaction * HS or more	-62.049** (25.034)	-0.225*** (0.061)	0.022** (0.008)	0.006*** (0.002)	0.025*** (0.007)
Mean Y less than HS	3242.46	38.956	0.067	0.008	0.059
Mean Y HS or more	3277.81	38.882	0.056	0.007	0.056
p-value	0.000	0.279	0.056	0.637	0.259
Panel C. Teen mother (age at birth < 20)					
Interaction* teen mom = 0	-25.600 (21.026)	-0.153*** (0.052)	0.014* (0.007)	0.005** (0.002)	0.018*** (0.006)
Interaction * teen mom = 1	-120.397*** (24.100)	-0.646*** (0.067)	0.046*** (0.008)	0.009*** (0.002)	0.054*** (0.007)
Mean Y older mom	3,272.396	38.932	0.060	0.008	0.057
Mean Y teen mom	3,148.689	38.905	0.077	0.009	0.066
p-value	0.000	0.000	0.000	0.008	0.000
Panel D. Older mother (age at birth > 34)					
Interaction* older mom = 0	-20.536 (20.640)	-0.147*** (0.052)	0.013* (0.007)	0.005** (0.002)	0.016*** (0.005)
Interaction * older mom = 1	-180.208*** (23.247)	-0.768*** (0.069)	0.055*** (0.008)	0.010*** (0.002)	0.073*** (0.008)
Mean Y younger mom	3,253.225	38.956	0.061	0.008	0.056
Mean Y older mom	3,278.394	38.672	0.074	0.010	0.077
p-value	0.000	0.000	0.000	0.000	0.000
Observations	740,804	740,804	740,804	740,804	740,804

Notes: This table shows difference-in-differences estimates, corresponding to Panel C of Table 4, stratified by mothers' characteristics at birth. The row labeled "P-value" at the bottom of each panel reports the two-sided p-value of the hypothesis of equality of coefficients. Standard errors are clustered at the mother's municipality of residence at birth. Significance levels: 10%*, 5%**, 1%***.

Table 6: Heterogeneous effects by resources and administrative burden

	Mortality outcomes		Birth outcomes			
	Infant	Perinatal	Birth weight	Gestational age	LBW	VLBW
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Municipal per-capita deficit						
Interaction* low deficit	0.010*** (0.003)	0.005*** (0.002)	-20.974 (26.252)	-0.126* (0.068)	0.014 (0.009)	0.003* (0.002)
Interaction* high deficit	0.008** (0.004)	0.006*** (0.002)	-51.995** (21.187)	-0.295*** (0.057)	0.021*** (0.008)	0.009*** (0.002)
Mean Y low	0.021	0.007	3,254.335	38.932	0.063	0.008
Mean Y high	0.022	0.008	3,256.777	38.925	0.063	0.008
P-value	0.591	0.815	0.018	0.009	0.254	0.001
Panel B. Rural versus urban areas						
Interaction* urban	0.004 (0.004)	0.004 (0.003)	7.871 (34.521)	-0.042 (0.148)	0.007 (0.010)	0.003 (0.004)
Interaction* rural	0.011*** (0.003)	0.006*** (0.002)	-31.466 (20.794)	-0.190*** (0.054)	0.016** (0.007)	0.005*** (0.002)
Mean Y urban	0.023	0.008	3,242.758	38.898	0.066	0.008
Mean Y rural	0.021	0.008	3,250.052	38.936	0.062	0.008
P-value	0.051	0.260	0.119	0.249	0.160	0.397
Panel C. Number of transfers						
Interaction* few transfers	0.010*** (0.004)	0.006*** (0.002)	-47.456*** (15.827)	-0.223*** (0.044)	0.021*** (0.006)	0.006*** (0.002)
Interaction* many transfers	0.009** (0.004)	0.005*** (0.002)	-17.094 (22.596)	-0.182* (0.095)	0.011 (0.008)	0.005* (0.003)
Mean Y few	0.020	0.007	3,258.469	38.954	0.062	0.008
Mean Y many	0.023	0.008	3,251.995	38.895	0.064	0.008
P-value	0.719	0.675	0.144	0.662	0.216	0.949
Panel D. Speed of transferal process						
Interaction* gradual	0.008 (0.005)	0.002 (0.003)	-2.897 (25.233)	-0.112** (0.056)	0.002 (0.006)	0.002 (0.002)
Interaction* all at once	0.010*** (0.003)	0.006*** (0.002)	-42.988*** (15.277)	-0.227*** (0.056)	0.021*** (0.005)	0.006*** (0.002)
Mean Y gradual	0.021	0.008	3,253.333	38.897	0.063	0.008
Mean Y all at once	0.022	0.008	3,257.807	38.957	0.062	0.008
P-value	0.644	0.069	0.064	0.144	0.003	0.046
Observations	8,945	8,945	740,804	740,804	740,804	740,804

Notes: This table shows difference-in-differences estimates. Columns (1) and (2) are equivalent to column (3) of Table 2, and columns (3)–(6) are equivalent to Panel C of Table 4, stratified by characteristics of the municipalization process at the municipality level the year before the start of the second wave of municipalization. In Panel A the sample is split at the median of a municipality's per capita deficit, defined as the difference between per capita spending and per capita revenue at baseline. Panel B splits the sample between municipalities with high and low shares of rural PCHCs, where "high" is defined as being in the top 25% of the distribution of rural establishments at baseline. Panel C defines the municipalization process in a municipality based on the speed at which the establishments were transferred to local administration, where "all at once" is defined as all establishments in a municipality being transferred within a two-month window. Panel D divides the sample into municipalities above and below the median number of establishments at baseline. The row labeled "p-value" at the bottom of each panel reports the two-sided p-value of the hypothesis of equality of coefficients. Standard errors are clustered at the mother's municipality of residence at birth. Significance levels: 10%*, 5%** , 1%***.

A Supplemental figures and tables

Figure A.1: Examples of excerpts in *Diario Oficial*

MINISTERIO DE SALUD				
Nómina de decretos que aprueban Convenios entre los Servicios de Salud que se indican y las II. Municipalidades que se señalan sobre traspasos de Establecimientos Asistenciales, sus Bases y Extracto.				
Servicio de Salud	Ilustre Municipalidad	Decreto N°	Fecha	Establecimiento Asistencial
Coquimbo	La Higuera	336	28.10.81	Posta Rural La Higuera.
Coquimbo	Vicuña	337	28.10.81	Postas Rurales de Taleuna, El Tambo, El Molle, Peralillo, Diaguitas, Rivadía y Huanta.
Coquimbo	Ovalle	338	28.10.81	Postas Rurales de Sotagui, Limari, Las Sosas y Cerrillo de Tamaya.
Coquimbo	Los Vilos	339	28.10.81	Postas Rurales de Calmanes, Quanguil, Quilimari y Tilama.
Coquimbo	La Serena	340	28.10.81	Postas Rurales de Algarrobito y Las Rojas y Consultorios Generales Urbanos de Pedro Aguirre Cerda y Las Compañías.
Coquimbo	Palmuano	341	28.10.81	Postas Rurales de Pisco Elqui, Monte Grande y Horcón y Consultorio General Rural de Palmuano.
Bío-Bío	Santa Bárbara	342	28.10.81	Postas Rurales de Ralco y Pitril.
Bío-Bío	Nacimiento	343	28.10.81	Postas Rurales de San Roque, Carrilán y Millapoa.
Bío-Bío	Tucapel	344	28.10.81	Postas Rurales de Tucapel, Trupán y Polcura.
Bío-Bío	Yumbel	345	28.10.81	Postas Rurales de Rere, de la Aguada, Romeo y Río Claro.
Bío-Bío	Quillico	346	28.10.81	Postas Rurales de Quilleco, Canteras y Villa Mercedes.
Bío-Bío	Negrete	347	28.10.81	Postas Rurales de Negrete.

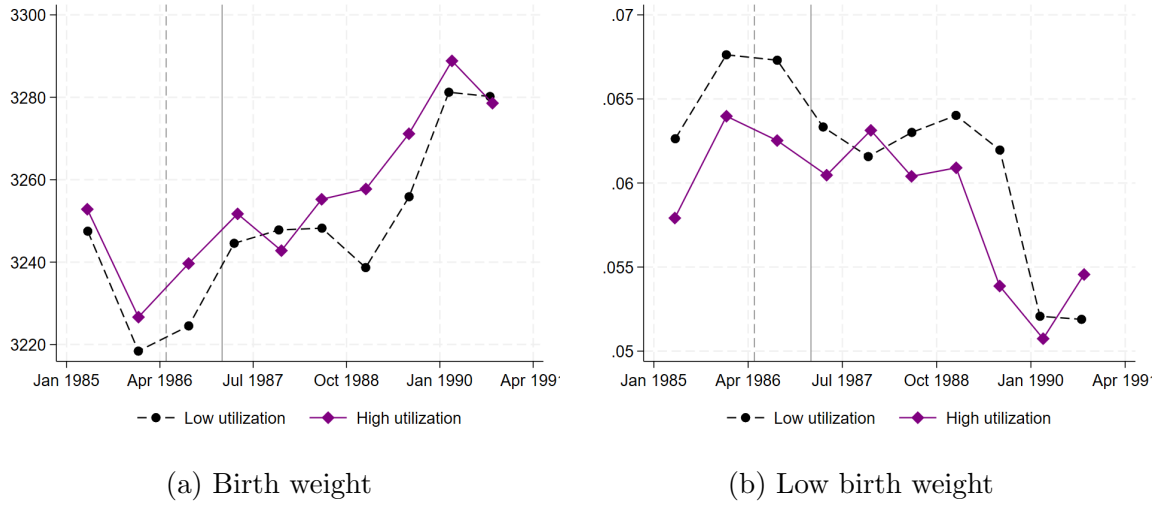
(a) Initial decrees

Ministerio de Salud					
NOMINA DE DECRETOS QUE APROBEN MODIFICACIONES DE LOS CONVENIOS SUSCRITOS ENTRE LOS SERVICIOS DE SALUD QUE SE INDICAN Y LAS ILUSTRES MUNICIPALIDADES QUE SE SEÑALAN, RELATIVOS A TRASPASO DE ESTABLECIMIENTOS ASISTENCIALES Y SU EXTRACTO					
Servicio de Salud	I. Municipalidad	Decreto N°	Fecha	N° y Fecha Decreto Modificado	Establecimiento Asistencial
Coquimbo	Monte Patria	351	24-03-87	141/87	Consultorio General Rural: Monte Patria. Estaciones Médicas Rurales: Campanario, Los Molles, Cerrillos de Rapel, El Colpo, Huasilla, Tulahuén Oriente y La Vatiola.
Coquimbo	Punitaqui	352	24-03-87	140/87	Consultorio General Rural: Punitaqui. Estación Médica Rurales: El Peral, el Llanito, El Maqui de Quile, Los Corrales, La Higuera de Quile, Las Nipas, El Peral de Quile, Portezuelo Blanco, Alto de Pichón, El Alto Bajo, El Alto Alto, La Rinconada, El Ajial de Quile, El Quile, Nueva Aurora, Algarrobo de Hornillos, Litpanpa y el Alameda de Quile.
Coquimbo	Río Hurtado	353	24-03-87	146/87	Consultorio Rural: Pichasca. Estaciones Médicas Rurales: Huampulla y Tabaqueros.
Coquimbo	Mincha	354	24-03-87	145/87	Consultorio General Rural: Canda Baja. Estaciones Médicas Rurales: Jabonería, Quebrada de Linares, El Talhué, Los Pinos, El Durazno, Alhuemilla, La Cortadera, Las Barrancas, La Parrilla, Los Canelos, El Colligüe, Pozo Honda, Puerto Oscuro, El Totoral, Atelaura, Las Tazas, El Potrero, Matancilla, Agua Fria Alta y Mincha Sur.
Libertador General Bdo. O'Higgins	Montazal	355	24-03-87	192/87	Consultorio General Rural: San Francisco de Montazal.
Libertador General Bdo. O'Higgins	Coltauco	356	24-03-87	193/87	Consultorio General Rural: Coltauco. Estación Médica Rural: Costa de Idahué.
Libertador General Bdo. O'Higgins	Placilla	357	24-03-87	204/87	Consultorio General Rural: Placilla.
Libertador General Bdo. O'Higgins	Las Cabras	358	24-03-87	199/87	Consultorio General Rural: Las Cabras.
Libertador General Bdo. O'Higgins	Peralillo	359	24-03-87	209/87	Consultorio General Rural: Peralillo. Estaciones Médicas Rurales: Nillhue Cornejo y Nillhue Barahona.
Libertador General Bdo. O'Higgins	Requinoa	360	24-03-87	190/87	Consultorio General Rural: Requinoa.

(b) Subsequent decrees

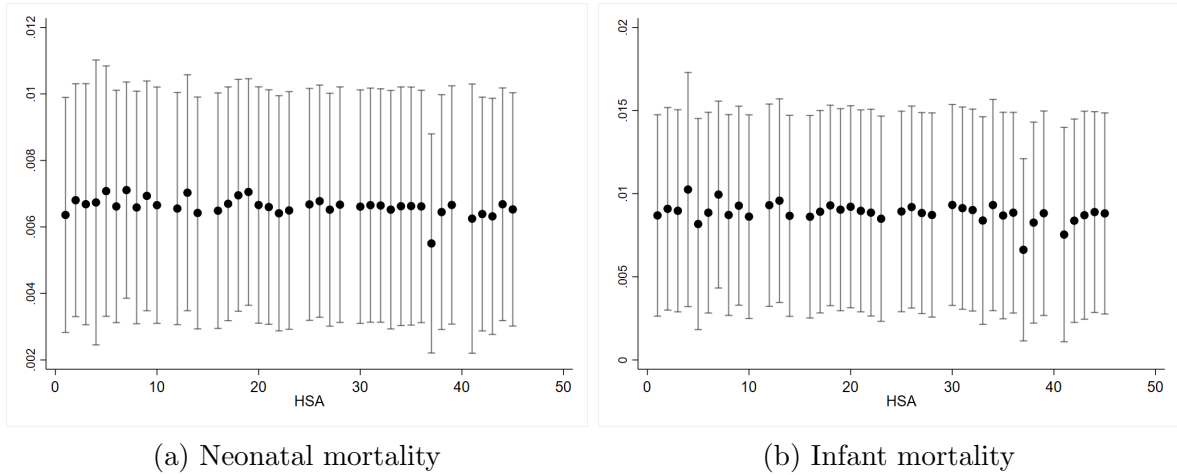
Notes: These figures present examples of excerpts from the *Diario Oficial* used to construct the transfer dataset. Panel (a) shows initial decrees transferring establishments to municipalities, while panel (b) shows subsequent decrees transferring additional establishments.

Figure A.2: Birth outcomes by quarter of conception



Notes: These figures show average birth outcomes by quarter of conception, weighted by the number of births in each municipality-month. Panel (a) plots birth weight, and panel (b) plots the fraction of births defined as low birth weight (less than 2,500 grams). The sample is split in low- and high-utilization areas, defined by the median of the variable in 1975. The vertical dashed line represents the date of the first transfer in the estimation sample, and the solid vertical line represents the date of the last transfer.

Figure A.3: Leave-one-out by HSA



Notes: These figures show difference-in-differences estimates, equivalent to column (3) of Table 2, together with their corresponding 95% confidence intervals for neonatal and infant mortality, obtained by dropping each health service area (HSA) one at a time.

Table A.1: Transfers summary

Category	Years	Own data	Miranda, 1990	Heyermann, 1995
Consultorios	1981-1985	68	94	103
Consultorios	1987-1988	196	191	192
Consultorios	1989	2	-	25
Consultorios	Total	266	-	320
Postas Rurales	1981-1985	310	291	290
Postas Rurales	1987-1988	722	703	703
Postas Rurales	1989	3	-	43
Postas Rurales	Total	1035	-	1036
Estaciones Rurales	1981-1985	130	165	156
Estaciones Rurales	1987-1988	280	335	335
Estaciones Rurales	1989	99	-	52
Estaciones Rurales	Total	509	-	543

Notes: This table presents details on the total number of establishments transferred by type in different periods in our collected data and compares it with the data from [Miranda et al. \(1990\)](#) and [Heyermann \(1995\)](#).

Table A.2: Municipalization effects on aggregated birth outcomes

	(1) Birth weight	(2) Gestational age	(3) LBW	(4) VLBW	(5) Pre-term
Interaction	-17.5129 (16.2252)	-0.1192*** (0.0386)	0.0108** (0.0048)	0.0017 (0.0012)	0.0144*** (0.0044)
Sample mean	3253.710	38.969	0.060	0.004	0.055
R^2	0.387	0.413	0.107	0.027	0.134
Observations	8,945	8,945	8,945	8,945	8,945
Municipalities	126	126	126	126	126
Avg mother characteristics	Yes	Yes	Yes	Yes	Yes
Municipality Controls	Yes	Yes	Yes	Yes	Yes

Notes: This table shows difference-in-differences estimates of birth outcomes, measured at the municipality-month level, from equation 2 and weighted by the number of births in each municipality and month. The variable “Interaction” is defined as past utilization multiplied by the share of expected months a pregnancy is exposed to the municipalization process. Average mother characteristics are collapsed at the birth level and include average age, share single, share with high school completed, and share with at least one year of college. Municipality controls include municipal per capita income, per capita spending, and total deficit. Standard errors are clustered at the municipality level. Significance levels: 10%*, 5%** , 1%***.

Table A.3: Gestational age bounds

	Bound : Full term		Bound: 27 weeks	
	Neonatal mortality	Infant mortality	Neonatal mortality	Infant mortality
Interaction	0.0062*** (0.0018)	0.0083*** (0.0030)	0.0069*** (0.0019)	0.0094*** (0.0033)
Sample mean	0.012	0.022	0.012	0.022
R^2	0.089	0.158	0.094	0.161
Observations	8,945	8,945	8,945	8,945
Municipalities	126	126	126	126

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table presents difference-in-differences estimates of the effect of the reform on child mortality from equation 2, in which we make two extreme assumptions regarding missing gestational age information. For the upper bound, we assume a full-term pregnancy lasting 40 weeks; for the lower bound, we assume a very pre-term pregnancy lasting 27 weeks.

Table A.4: Municipalization effects on individual birth outcomes, controlling for region-specific time trends

	Birth weight (1)	Gestational age (2)	LBW (3)	VLBW (4)	Pre-term birth (5)
Interaction	-27.911* (15.193)	-0.136** (0.067)	0.016** (0.006)	0.006*** (0.002)	0.014** (0.006)
Sample mean	3255.653	38.928	0.063	0.008	0.058
Observations	740,804	740,804	740,804	740,804	740,804

Notes: This table shows difference-in-differences estimates of the effect of the reform on individual outcomes, equivalent to Panel C of Table 4. The specifications control for mothers' characteristics at birth, municipality characteristics, and region-specific time trends. Standard errors are clustered at the mother's municipality of residence at birth. Significance levels: 10%*, 5%** , 1%***.

Table A.5: Municipalization effects on individual birth outcomes by birth order

	Birth outcomes				
	Birth weight (1)	Gestational age (2)	LBW (3)	VLBW (4)	Pre-term (5)
Interaction* high order birth	-44.001** (20.751)	-0.253*** (0.055)	0.020*** (0.007)	0.005*** (0.002)	0.024*** (0.005)
Interaction* first birth	-25.268 (20.711)	-0.146*** (0.053)	0.015* (0.008)	0.006** (0.002)	0.019*** (0.006)
Mean Y high order birth	3,299.178	38.895	0.059	0.009	0.058
Mean Y first birth	3,193.723	38.975	0.068	0.007	0.057
P-value	0.031	0.004	0.092	0.838	0.156
Observations	740,804	740,804	740,804	740,804	740,804

Notes: This table shows difference-in-differences estimates of the effect of the reform on individual outcomes, equivalent to Panel C of Table 4, stratified by birth order. Standard errors are clustered at the mother's municipality of residence at birth. Significance levels: 10%*, 5%** , 1%***.

Table A.6: Municipalization effects on fully treated units

	Neonatal mortality (1)	Infant mortality (2)
Average total effect	0.0048 (0.0077)	0.0064 (0.0106)
Observations	79,074	79,074
Switchers x periods	46,364	46,364

Notes: This table presents results for the estimator proposed by [de Chaisemartin and D'Haultfoeulle \(2024\)](#). The event time is relative to the month of conception and to four months before the first transfer. Standard errors are clustered at the municipality level. Significance levels: 10%*, 5%** , 1%***.

Table A.7: Municipalization effects on partially treated units

	Neonatal mortality (1)	Infant mortality (2)
Average Total Effect	0.0050 (0.0089)	0.0005 (0.0114)
Observations	74,748	74,748
Switchers x periods	42,414	42,414
p-value joint nullity	0.531	0.552

Notes: This table presents results for the estimator proposed by [de Chaisemartin and D'Haultfœuille \(2024\)](#), using a sample of cohorts conceived before the first establishment transfer in a municipality. The event time is relative to the month of conception and to ten months before the first transfer. Standard errors are clustered at the municipality level. Significance levels: 10%*, 5%** , 1%***.

Table A.8: Heterogeneous effects by resources and administrative burden

	Mortality outcomes		Birth outcomes				
	Infant	Perinatal	Birth weight	Gestational age	LBW	VLBW	Pre-term
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Municipal per-capita deficit							
Interaction* low deficit	0.010*** (0.003)	0.005*** (0.002)	-20.974 (26.252)	-0.126* (0.068)	0.014 (0.009)	0.003* (0.002)	0.017*** (0.006)
Interaction* high deficit	0.008** (0.004)	0.006*** (0.002)	-51.995** (21.187)	-0.295*** (0.057)	0.021*** (0.008)	0.009*** (0.002)	0.027*** (0.006)
Mean Y low	0.021	0.007	3,254.335	38.932	0.063	0.008	0.057
Mean Y high	0.022	0.008	3,256.777	38.925	0.063	0.008	0.058
p-value	0.591	0.815	0.018	0.009	0.254	0.001	0.147
Panel B. Municipal per-capita revenue							
Interaction* low revenue	0.012*** (0.003)	0.006*** (0.001)	-53.699*** (15.238)	-0.238*** (0.068)	0.024*** (0.007)	0.006** (0.002)	0.026*** (0.007)
Interaction* high revenue	0.006 (0.004)	0.005** (0.002)	-29.537 (26.347)	-0.194*** (0.058)	0.015* (0.009)	0.006*** (0.002)	0.020*** (0.006)
Mean Y low	0.023	0.008	3,242.626	38.931	0.065	0.008	0.058
Mean Y high	0.021	0.007	3,260.188	38.927	0.062	0.008	0.058
p-value	0.068	0.409	0.219	0.546	0.217	0.779	0.409
Panel C. Municipal per-capita spending							
Interaction* low spending	0.012*** (0.003)	0.007*** (0.001)	-55.335*** (16.563)	-0.249*** (0.073)	0.024*** (0.007)	0.005** (0.002)	0.027*** (0.008)
Interaction* high spending	0.006* (0.004)	0.004** (0.002)	-30.790 (24.763)	-0.196*** (0.053)	0.016** (0.008)	0.006*** (0.002)	0.020*** (0.006)
Mean Y low	0.023	0.008	3,241.529	38.923	0.065	0.008	0.059
Mean Y high	0.021	0.008	3,260.653	38.930	0.062	0.008	0.057
p-value	0.077	0.211	0.162	0.434	0.195	0.639	0.340
Observations	8,945	8,945	740,804	740,804	740,804	740,804	740,804

Notes: This table shows difference-in-differences estimates. Columns (1) and (2) are equivalent to column (3) of Table 2, and columns (3)–(7) are equivalent to Panel C of Table 4, stratified by characteristics of the municipalization process at the municipality level. In Panel A the sample is split at the median of a municipality's per capita deficit, defined as the difference between per capita spending and per capita revenue at baseline. Panels B and C repeat the exercise by per capita revenue and per capita spending. The row labeled "p-value" at the bottom of each panel reports the two-sided p-value of the hypothesis of equality of coefficients. Standard errors are clustered at the mother's municipality of residence at birth. Significance levels: 10%*, 5%** , 1%***.

B Start of municipalization: Placebo analysis

We compare results between our main sample of municipalities, where the first transfer occurred between 1987 and 1988, and a placebo sample of municipalities where the last transfer happened before 1985. We impose the same restrictions on the placebo sample.

Importantly, the placebo sample consists of always-treated cohorts for the 1985–1990 period. Then, if there is a differential effect across municipalities with high and low utilization in 1985—as our results indicate—their trends over time should differ in the analysis period. Therefore, the placebo sample is not useful for showing that municipalities were trending similarly, but it is useful to show that nothing else occurred in 1987–1988 that differentially affected child mortality rates.

To assess if there is a break in trends in 1985, we run the following model for both high- and low-utilization placebo subsamples:

$$Y_{ck} = \alpha_c + \gamma(k - Feb\ 1987) + \beta 1\{k \geq Feb\ 1987\} + \Gamma X_{ck} + \epsilon_{ck}, \quad (5)$$

where Y_{ck} is the outcome for a cohort conceived in month k in municipality m . We control for linear trends and include municipality-cohort-level covariates related to mothers' average characteristics. The coefficient β captures the change in mortality for cohorts conceived at the start of the second wave. We report robust standard errors and weight observations by the number of children born in each municipality-conception-month cell.

Table B.1 presents the results, which show no statistically significant jump at the start of the second wave for municipalities whose municipalization process ended in 1981–1982. We interpret these findings as evidence that aside from municipalization, no other unobservable factors affected cohort trends in either high- or low-utilization areas in February 1987.

Table B.1: Placebo analysis

	Below Mean		Above Mean	
	Neonatal Mortality	Infant Mortality	Neonatal Mortality	Infant Mortality
Post	-0.0008 (0.0012)	-0.0023 (0.0016)	0.0003 (0.0015)	0.0011 (0.0021)
Sample mean	0.011	0.019	0.011	0.021
Observations	2,356	2,356	1,224	1,224
R^2	0.044	0.089	0.036	0.059

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table shows estimates for the change in mortality for cohorts conceived at the start of the second wave, restricted to being born in municipalities who had finished their municipalization process by 1982. The results are presented for a subsample of municipalities with below mean utilization in 1975 as well as a subset of municipalities with above mean municipalization.