Changes in Public Administration and Child Mortality: Evidence from a 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 infant mortality during the implementation period. The rise in child mortality is consistent with worse birth outcomes such as low birth weight. Rural municipalities and those with a greater financial deficit 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' preferences for local public 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 the effects of changes in public administration on health outcomes.

In this paper, we focus on the effects of changes in public administration due to a decentralization reform on child mortality and birth outcomes. To do this, we study Chile's 1980s decentralization reform, called the Municipalization of Local Services (Municipalización de Servicios Comunales). Our focus is the administrative decentralization of public primary care health centers (PCHCs), which were transferred from central to municipal control. Because the reform took place under the Pinochet dictatorship, we rule out effects coming from the election of local leaders and people's preferences for local public goods, and instead, we examine the effects associated with the administrative changes in the provision of public health services.

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 staggered 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 to build a dataset at the conception 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 infant mortality. A cohort fully exposed to the municipalization reform during pregnancy and born in a municipality with 10% higher pre-utilization levels has 2.3% higher infant mortality compared to a non-exposed cohort. 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 infant mortality as a short-term pause during our sample period.

We also find that the increase in child mortality is accompanied by increases in the shares of low-birth-weight (LBW) births (below 2,500 grams), as well as in the share of infant deaths due to infectious diseases. 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.Back-of-the-envelope calculations suggest that around 18% of the total effect of the reform on infant mortality is due to the increased risk of LBW births.

We explore heterogeneity by mothers' demographics at birth, and find no systematic differences. For example, 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. We also explore differences by mother's age. We find that older mothers (above the age of 34) have worse birth outcomes, but our results are noisy, and we cannot reject equality of coefficients between older and younger mothers.

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 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,

[1] See 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 includes 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.

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, 2025 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, and Section 6 explores mechanisms. Finally, 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 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

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

[3] A 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.

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 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

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

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 healthcare facilities that provide very basic medical services (called *postas rurales*), 579 are even smaller rural facilities (*estaciones médico rurales*), and 266 are PCHCs.

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

[5]Sometimes 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.

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

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 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.

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

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

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:

$$U_{a,1975} = \frac{PCV_{a,1975}}{Population_{a,1975}} \tag{1}$$

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]

Figure 1 shows the distribution of our primary care utilization rate for the municipalities in our sample. As it can be seen in the figure, there is plenty of variation in this variable, especially above its median value.

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

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. We restrict the sample to municipalities with revenue and spending data in 1985, which we obtain from González et al. (2021). Finally, to make sure we identify the effects of the reform before and after a first transferal, we restrict our main estimation sample to municipalities that we observe with non-missing data for at least 21 months before and 12 months after a first establishment transfer.

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

[10] As 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

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 120 municipalities in our estimation sample, from a universe of 221 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,

pre-term pregnancy lasting 27 weeks.

[11] According to the 1982 Population Census, there were 324 municipalities in Chile in 1982.

and this rate is similar 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 25.5 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% of municipalities in high-utilization areas transferred all their establishments to local administration within two months, compared with 57% 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 2 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 areas 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:

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

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:

$$Y_{cy(k)} = \gamma_c + \delta_{y(k)} + \sum_{\tau} \beta_\tau U_{a(c),1975} \mathbf{1}(\tau = k + 9 - Month_c) + \Gamma_1 X_{cy(k)} + \epsilon_{cy(k)}, \quad (3)$$

where all variables are defined as before, and event time τ is defined as the difference between the conception month plus 9 months for cohort $y(k)$ and the month of the first transfer of a PCHC to municipality c, $Month_c$. Therefore, $\tau = 0$ refers to cohorts conceived 9 months before first transferal The coefficients of interest in this model are β_τs, 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 3.7% higher infant mortality compared to a cohort with no exposure. This result is statistically different from zero at the five-percent level. For neonatal mortality our results are positive but smaller, and we cannot reject the null hypothesis of null effects.

Figure 3 presents the results of the event-study model. Our data are at the monthly level but we pool monthly coefficients into trimesters for ease of exposition. In the figure, we define partially treated cohorts as those affected by the reform during pregnancy but that were conceived before the first transferal (gray shaded area), and fully treated cohorts are those conceived after the first transferal.

Although noisy, the estimates in Figure 3 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) shows that cohorts partially treated by the local administration during pregnancy may be affected by the policy. The estimates are positive but not statistically different from zero for those partially treated during pregnancy, and positive and statistically different from zero for cohorts that were fully treated, consistent with our previous results in Table 2.

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 an 11.7% higher share of deaths related to infections, an 11% higher share of deaths related to the nervous system, and an 18% higher share of deaths related to congenital causes, compared to non-exposed cohorts. Perinatal causes of death have been found to be associated with increased risk of infant mortality (Gonzalez et al., 2006; Kaempffer and Medina, 2000). Although noisy, our estimate suggest that the reform increased the risk of deaths due to perinatal conditions. Finally, we find small and not significant changes in deaths due to respiratory, injuries, or poisoning.

Infant mortality was already in a downward trend in Chile in this period of time; therefore, because these outcomes are infrequent in the population, in the next subsection we study more deeply the effects of the reform during pregnancy using 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:

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

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 35.36 grams lower. While this result is small on average, it is meaningful at the margin. Columns (3)–(5) show that the probability of an infant being born LBW increases by 1.4 percentage points, or 2.2%, for mothers fully exposed to the reform during pregnancy in municipalities with 10% higher utilization in 1975. We find smaller and not significant results for gestational age, pre-term birth, and VLBW. 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 mortality is explained by worse birth outcomes, we use estimates from Kaempffer and Medina (2000) on infant mortality risk. The authors report that infants born LBW in Chile at the end of the 1990s faced an increased risk of infant mortality of 117 per 1,000 births. This implies that the increased likelihood of LBW of 1.4 percentage points accounts for about 17.8% of the increase in infant mortality in our sample ($0.014 \times 0.117/0.0092$). We believe that this is a lower bound, as the prevalence of LBW births was higher in 1990 compared to the end of the decade.[12]

Figure 4 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.

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

[12] We use estimates from Kaempffer and Medina (2000), who present infant mortality risk statistics in Chile in 1998. We could compute their numbers in our sample; however, in our data we cannot separate causes of deaths for births below 1 year old between pre-term births and LBW.

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 substantially on the margin. 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 does not play a crucial role in explaining which mothers are more affected by the reform, in contrast to what has been found in the health literature (Phipps et al., 2002; Salihu et al., 2003),[13] that 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 there are no systematic differences between mothers under and above age 20. For all outcomes, except for birth weight, we cannot reject the hypothesis of equality of coefficients. Similarly, Panel D shows that the effects are similar for mothers under and above age 34. The only exception is gestational age, where older mothers have shorter pregnancies than younger mothers on average.[14]

Overall, our results suggest the reform shifted the distribution of birth weight and gestational age to the left in high-utilization areas, compared to low-utilization areas, but our heterogeneity analysis does not allow us to conclude that the reform affected more vulnerable groups of mothers, compared to less vulnerable mothers.

[13] See more up-to-date discussion on the topic here.

[14] In terms of birth order, Appendix Table A.4 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

5.4.1 Bias in difference-in-difference

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'Haultfœuille (2024), which allows for a staggered design with group-specific intensities.[15] 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

[15] We use the associated Stata package did_multiplegt_dyn.

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

Table A.5 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.6 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.

5.4.2 Selection into the private sector

An important concern in estimating the effects of the reform is that, concurrent with the municipalization of public services, a new private health system emerged. It is estimated that by 1990, 17.8% of the country's population was insured in the private sector, and this group was wealthier on average.[16]

The concern is that, as a consequence of the reform, mothers with better birth outcomes may have migrated from the public to the private sector, particularly in the most exposed municipalities. In our data, we do not observe mothers' insurance type; however, we can bound our estimates by assuming that the most selected mothers moved to the private sector. Following the spirit of Lee bounds (Lee, 2009), we consider two extreme cases. First, we compute an upper bound by excluding from our sample the

[16] Statistics obtained from Superintendencia de Salud y Fonasa.

top 17.8% of women with the highest birth outcomes in highly exposed municipalities. Second, we compute a lower bound by assuming that the bottom 17.8% of women with the lowest birth outcomes in highly exposed municipalities moved to the private sector. If the primary concern is that positively selected women were more likely to receive care in the private sector, our main interest lies in the upper bound. Our results, corresponding to Table 4, panel C, are reported in Appendix Table A.7. The table shows that the bound estimates include our main estimates. This suggests that potential attrition due to migration from the public to the private sector is unlikely to fully explain the observed effects of the reform on birth outcomes.

5.4.3 Additional 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.

6 Mechanisms

Qualitative evidence from *Cuadernos Médicos Sociales* suggests that physicians viewed the municipalization process as fraught with issues.[17] Physicians from different mu-

[17] *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

nicipalities 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, 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

publications can be found here.

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 larger effects in mortality outcomes in high-deficit municipalities, but the differences are not statistically significant; however, we find larger and 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 six 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 exlusive 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 birth weight. A 10% increase in exposure decreases birth weight by 32.3 grams. With a p-value of 0.082, 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 the coefficients is substantial

6.3 Administrative burden

The previous analysis shows that rural municipalities and those with greater deficits experienced more negative effects during our sample period. 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, in most cases, 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 six times larger for the probability of LBW.

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. 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 infant mortality. The higher mortality is linked to worse health outcomes at birth. We also examine heterogeneous effects by mothers' demographics and do not find evidence of large differences between mothers.

When exploring mechanisms, we find results consistent with the idea that an increased 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 a lack of experience in local administration played a role.

Our mortality results contrast with previous analyses of health decentralization in other settings, which 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," shape the potential negative effects of decentralization efforts on children's outcomes.

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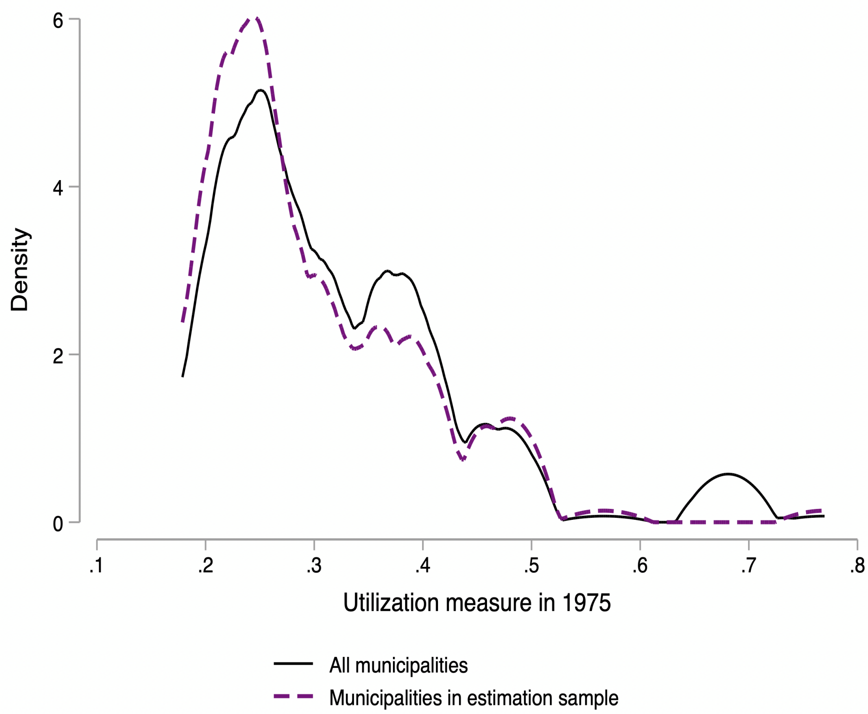
Figures and tables

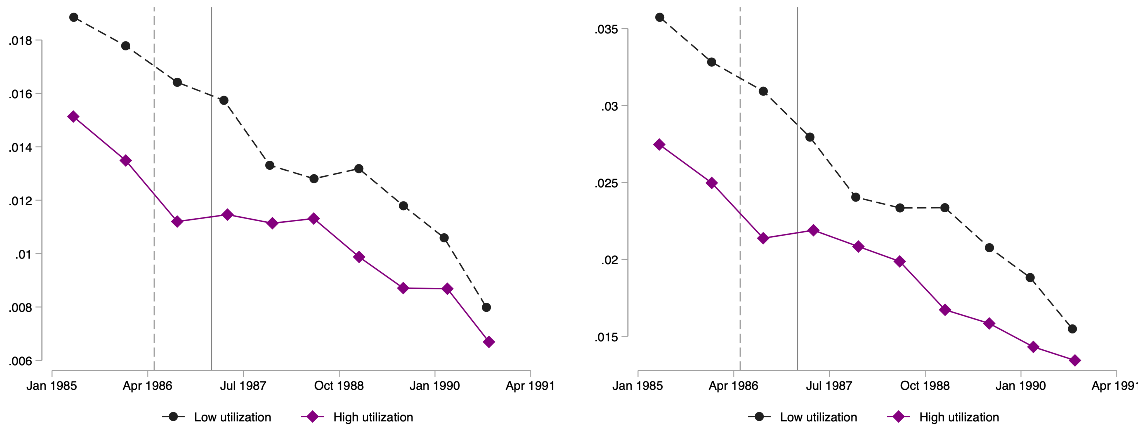
Figure 1: Density of utilization rate in 1975



Notes: This figure shows the distribution of utilization rates in 1975. Density in black corresponds to all municipalities with non-missing information (equivalent to column (1) in Table 1). The density in purple is for the municipalities in the estimation sample (column (2) in Table 1).

Figure 2: Mortality rate by quarter of conception

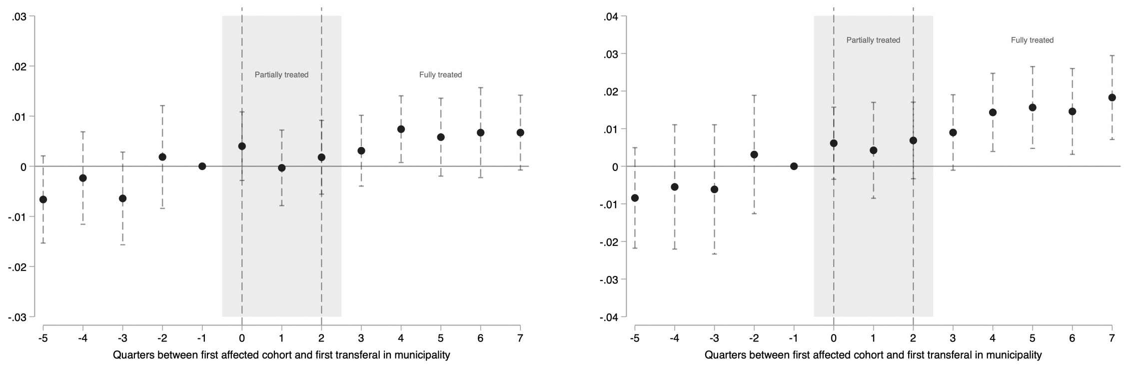
(a) Neonatal mortality (b) Infant mortality



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, and the solid vertical line represents the date of the last transfer. For this figure, we use a pre-estimation sample which is not restricted to the relative event time window.

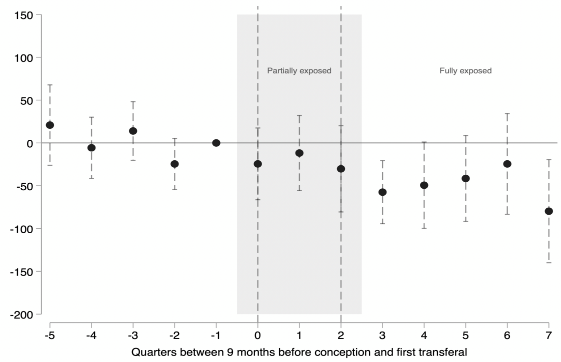
Figure 3: Effect of the reform on short-term mortality outcomes

(a) Neonatal mortality (b) Infant mortality

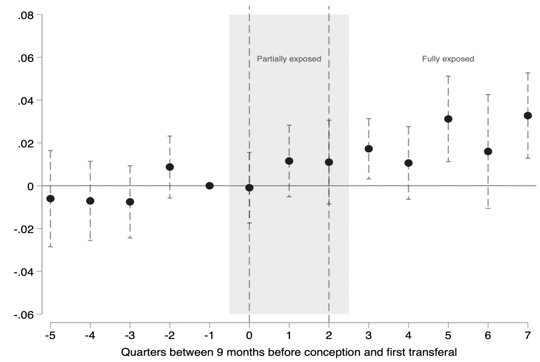


Notes: Panel (a) shows estimates of event-study coefficients from equation 3 for neonatal mortality, and panel (b) shows estimates for infant mortality. Controls include average mothers' characteristics by municipality of residence and municipality characteristics. Partially treated cohorts as those affected by the reform during pregnancy but that were conceived before the first transferal (gray shaded area), and fully treated cohorts are those conceived after the first transferal. The vertical lines represent the relative quarter of conception for the first cohort whose pregnancy was partially affected by the reform (relative time 0), and for the first cohort whose entire pregnancy was affected by the reform (relative time 3).

Figure 4: Effect of the reform on individual birth outcomes



(a) Birth weight



(b) Low birth weight

Notes: These figures show estimates of event-study coefficients from equation 3 on individual birth outcomes. Controls include individual mothers' characteristics and municipality characteristics. Partially treated cohorts as those affected by the reform during pregnancy but that were conceived before the first transferal (gray shaded area), and fully treated cohorts are those conceived after the first transferal. The vertical lines represent the relative quarter of conception for the first cohort whose pregnancy was partially affected by the reform (relative time 0), and for the first cohort whose entire pregnancy was affected by the reform (relative time 3). Additional birth outcomes can be found in Appendix Figure A.3.

Table 1: Summary statistics before the second wave of municipalization

	All Municipalities	Estimation sample	Estimation sample by utilization	
			Below median	Above median
	(1)	(2)	(3)	(4)
Panel A: Counts				
Population	39,800.379	41,817.610	29,144.723	54,490.498
Births	69.387	72.715	48.000	97.431
Panel B: Birth outcomes				
Birth weight (grams)	3,238.567	3,236.634	3,236.947	3,236.321
Gestational age (weeks)	39.024	39.029	39.057	39.000
Low birth weight (LBW)	0.062	0.062	0.064	0.061
Very low birth weight (VLBW)	0.004	0.004	0.004	0.004
Pre-term birth	0.056	0.054	0.056	0.053
Panel D: Mother characteristics				
Mother's age	25.431	25.500	25.602	25.398
Single mothers	0.326	0.330	0.337	0.322
Mothers with high-school	0.213	0.207	0.167	0.247
Mothers with higher education	0.018	0.018	0.015	0.021
Panel E: Municipality characteristics				
Utilization in 1975	0.319	0.296	0.221	0.371
Total number of transfers	7.471	7.908	9.350	6.467
% rural establishments	0.097	0.088	0.082	0.098
% transfers within 2 months	0.581	0.633	0.567	0.700
Per-capita municipal revenue	11.311	10.965	11.916	10.014
Per-capita municipal spending	11.170	10.865	11.890	9.840
Municipal deficit	1.058	0.990	1.003	0.978
Municipalities	221	120	60	60

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	0.0042*	0.0042*	0.0038
	(0.0025)	(0.0025)	(0.0026)
Sample mean	0.013	0.013	0.013
R^2	0.090	0.091	0.091
Observations	4,680	4,680	4,680
Panel B: Infant mortality (<1 year)			
Interaction	0.0098**	0.0100**	0.0092**
	(0.0039)	(0.0040)	(0.0043)
Sample mean	0.025	0.025	0.025
R^2	0.151	0.151	0.152
Observations	4,680	4,680	4,680
Municipalities	120	120	120
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

	(1) Respiratory	(2) Congenital	(3) Infections	(4) Injury/poison	(5) Nervous system	(6) Perinatal
Interaction	0.0007	0.0027*	0.0017**	-0.0017	0.0011**	0.0031
	(0.0016)	(0.0015)	(0.0008)	(0.0012)	(0.0005)	(0.0021)
Sample mean	0.004	0.005	0.001	0.003	0.001	0.009
R^2	0.079	0.042	0.057	0.071	0.044	0.082
Observations	4,680	4,680	4,680	4,680	4,680	4,680
Municipalities	120	120	120	120	120	120
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

	(1) Birth weight	(2) Gestational age	(3) LBW	(4) VLBW	(5) Pre-term birth
Panel A: No controls					
Interaction	-23.376	0.017	0.012**	0.001	0.002
	(16.586)	(0.040)	(0.005)	(0.002)	(0.005)
Panel B: Mother characteristics controls					
Interaction	-24.559	0.022	0.012**	0.001	0.002
	(15.904)	(0.041)	(0.005)	(0.002)	(0.005)
Panel C: Municipal characteristics controls					
Interaction	-35.363*	-0.028	0.014*	0.003	0.006
	(18.509)	(0.063)	(0.007)	(0.002)	(0.007)
Sample mean	3,243.421	38.922	0.065	0.008	0.059
Observations	395,637	395,637	395,637	395,637	395,637

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	Gestational age	LBW	VLBW	Pre-term
	(1)	(2)	(3)	(4)	(5)
Panel A. Mother's marital status					
Interaction* married	-47.963**	-0.060	0.015**	0.003	0.009
	(20.268)	(0.061)	(0.007)	(0.002)	(0.007)
Interaction* single	-11.642	0.030	0.012	0.002	-0.000
	(17.289)	(0.076)	(0.008)	(0.003)	(0.009)
Mean Y married	3272.383	38.934	0.059	0.007	0.056
Mean Y single	3,181.128	38.897	0.078	0.009	0.067
p-value	0.001	0.055	0.409	0.425	0.095
Panel B. Mother's education					
Interaction * less than HS	-21.468	-0.025	0.014**	0.004	0.004
	(16.085)	(0.063)	(0.007)	(0.003)	(0.006)
Interaction * HS or more	-53.232**	-0.034	0.014	0.002	0.009
	(23.298)	(0.068)	(0.009)	(0.003)	(0.008)
Mean Y less than HS	3228.500	38.949	0.069	0.008	0.061
Mean Y HS or more	3269.001	38.874	0.058	0.008	0.057
p-value	0.021	0.789	0.976	0.128	0.377
Panel C. Teen mother (age at birth $<$ 20)					
Interaction* teen mom = 0	-39.568*	-0.038	0.014*	0.003	0.008
	(20.126)	(0.067)	(0.008)	(0.003)	(0.007)
Interaction * teen mom = 1	-1.807	0.051	0.014**	0.002	-0.006
	(13.546)	(0.059)	(0.007)	(0.002)	(0.008)
Mean Y older mom	3,259.454	38.926	0.063	0.008	0.058
Mean Y teen mom	3,137.362	38.901	0.080	0.009	0.067
p-value	0.058	0.147	0.985	0.876	0.076
Panel D. Older mother (age at birth $>$ 34)					
Interaction* older mom = 0	-33.117*	-0.014	0.014*	0.003	0.005
	(19.287)	(0.063)	(0.008)	(0.002)	(0.007)
Interaction * older mom = 1	-52.219**	-0.152*	0.015	0.002	0.015
	(21.182)	(0.088)	(0.011)	(0.004)	(0.011)
Mean Y younger mom	3,240.690	38.947	0.064	0.008	0.058
Mean Y older mom	3,264.686	38.682	0.077	0.010	0.078
p-value	0.341	0.028	0.949	0.898	0.306
Observations	395,637	395,637	395,637	395,637	395,637

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%***.

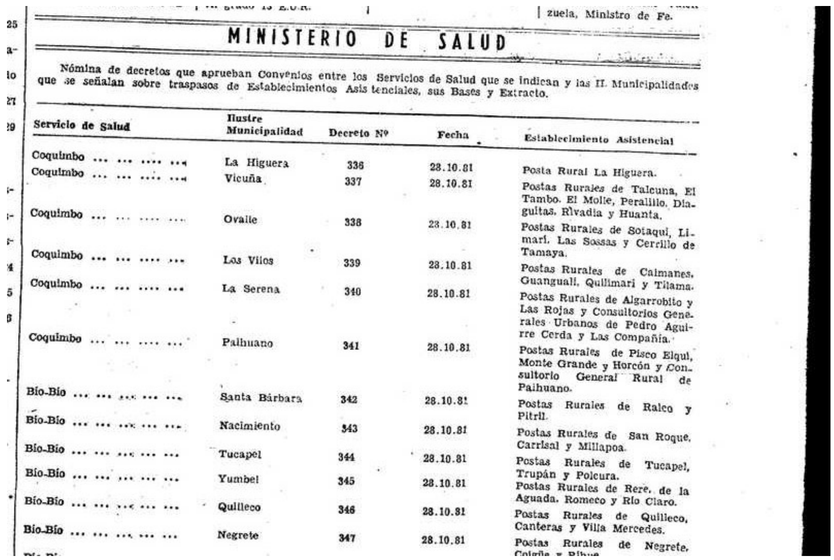
Table 6: Heterogeneous effects by resources and administrative burden

	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	0.007**	0.002	9.068	0.288	-0.004	-0.004
	(0.004)	(0.002)	(40.155)	(0.209)	(0.016)	(0.004)
Interaction* high deficit	0.011**	0.004	-60.404**	-0.231**	0.024**	0.007**
	(0.005)	(0.002)	(28.111)	(0.116)	(0.011)	(0.003)
Mean Y low	0.025	0.009	3,238.622	38.938	0.067	0.008
Mean Y high	0.025	0.009	3,245.935	38.911	0.064	0.008
P-value	0.441	0.526	0.015	0.001	0.012	0.001
Panel B. Rural versus urban areas						
Interaction* urban	0.006	0.002	11.867	0.184	0.004	-0.000
	(0.004)	(0.003)	(31.680)	(0.152)	(0.011)	(0.005)
Interaction* rural	0.011**	0.004*	-32.329*	-0.015	0.013*	0.003
	(0.005)	(0.002)	(18.726)	(0.064)	(0.007)	(0.003)
Mean Y urban	0.028	0.01	3,227.091	38.884	0.069	0.008
Mean Y rural	0.023	0.008	3,247.065	38.932	0.064	0.008
P-value	0.280	0.386	0.082	0.150	0.299	0.421
Panel C. Number of transfers						
Interaction* few transfers	0.009*	0.004	-47.816***	-0.032	0.018***	0.003
	(0.005)	(0.003)	(13.522)	(0.047)	(0.006)	(0.002)
Interaction* many transfers	0.010**	0.003	-12.935	-0.023	0.008	0.003
	(0.004)	(0.002)	(21.526)	(0.133)	(0.010)	(0.004)
Mean Y few	0.022	0.008	3,246.041	38.950	0.064	0.008
Mean Y many	0.026	0.010	3,238.985	38.886	0.067	0.008
P-value	0.797	0.753	0.088	0.946	0.270	0.868
Panel D. Speed of transferal process						
Interaction* gradual	0.007	-0.001	-8.189	0.037	0.003	0.000
	(0.007)	(0.004)	(18.738)	(0.064)	(0.007)	(0.003)
Interaction* all at once	0.010**	0.005**	-46.494***	-0.055	0.019***	0.004
	(0.004)	(0.002)	(14.859)	(0.075)	(0.006)	(0.003)
Mean Y gradual	0.025	0.001	3,238.286	38.887	0.066	0.008
Mean Y all at once	0.024	0.008	3,246.986	38.953	0.065	0.008
P-value	0.514	0.084	0.043	0.302	0.043	0.172
Observations	4,680	4,680	395,637	395,637	395,637	395,637

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*



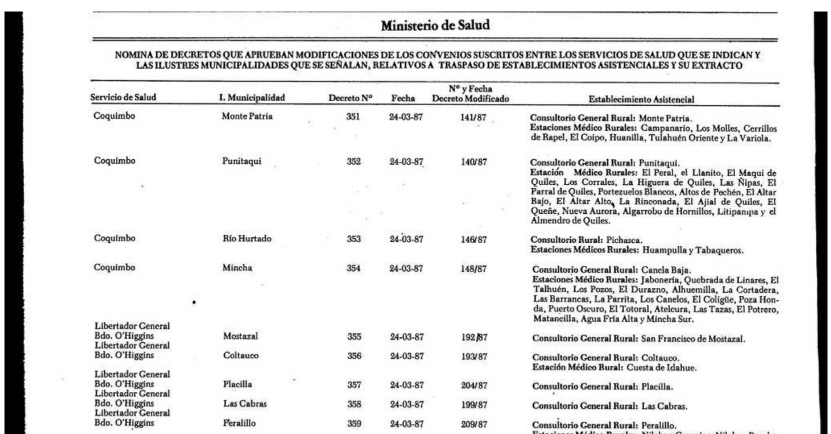
zuela, Ministro de Fe.

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 Nº	Fecha	Establecimiento Asistencial
Coquimbo	La Higuera	336	28.10.81	Posta Rural La Higuera.
Coquimbo	Vicuña	337	28.10.81	Postas Rurales de Talcuna, El Tambo. El Molle, Peralillo. Diaguitas. Rivadia y Huanta.
Coquimbo	Ovalle	338	23.10.81	Postas Rurales de Sotaqui, Limari. Las Sossas y Cerrillo de Tamaya.
Coquimbo	Los Vilos	339	28.10.81	Postas Rurales de Caimanes, Guanguali, 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 Compañia.
Coquimbo	Paihuano	341	28.10.81	Postas Rurales de Pisco Elqui, Monte Grande y Horcón y Consultorio General Rural de Paihuano.
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, Carrisal 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. Romeco y Rio Claro.
Bío-Bío	Quilleco	346	28.10.81	Postas Rurales de Quilleco, Canteras y Villa Mercedes.
Bío-Bío	Negrete	347	28.10.81	Postas Rurales de Negrete, Coigüe y Rihue

(a) Initial decrees



Ministerio de Salud

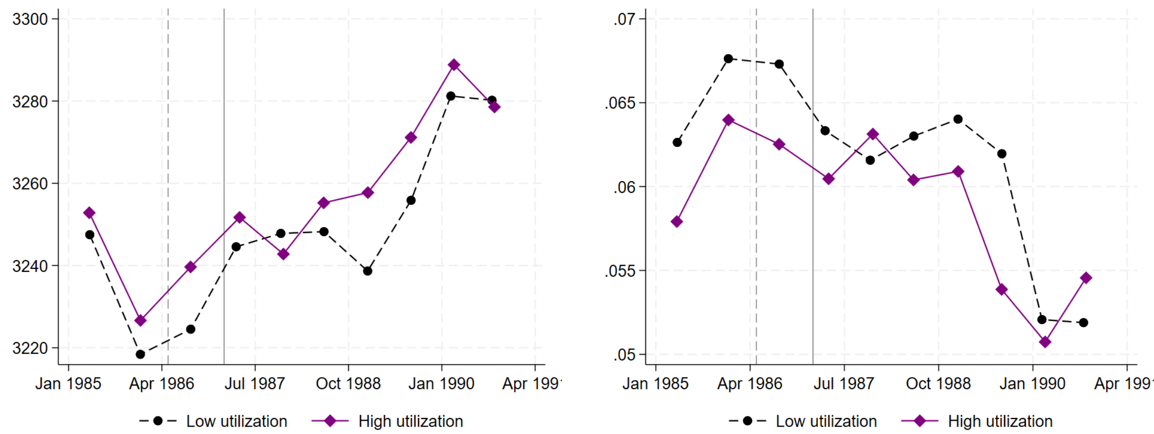
NOMINA DE DECRETOS QUE APRUEBAN MODIFICACIONES DE LOS CONVENIOS SUSCRITOS ENTRE LOS SERVICIOS DE SALUD QUE SE INDICAN Y LAS ILUSTRES MUNICIPALIDADES QUE SE SEÑALAN, RELATIVOS A TRASPASO DE ESTABLECIMIENTOS ASISTENCIALES Y SU EXTRACTO

Servicio de Salud	I. Municipalidad	Decreto Nº	Fecha	Nº y Fecha Decreto Modificado	Establecimiento Asistencial
Coquimbo	Monte Patria	351	24-03-87	141/87	**Consultorio General Rural:** Monte Patria. **Estaciones Médico Rurales:** Campanario, Los Molles, Cerrillos de Rapel, El Coipo, Huanilla, Tulahuén Oriente y La Variola.
Coquimbo	Punitaqui	352	24-03-87	140/87	**Consultorio General Rural:** Punitaqui. **Estación Médico Rurales:** El Peral, el Llanito, El Maqui de Quiles, Los Corrales, La Higuera de Quiles, Las Ñipas, El Parral de Quiles, Portezuelos Blancos, Altos de Pechén, El Altar Bajo, El Altar Alto, La Rinconada, El Ajial de Quiles, El Queñe, Nueva Aurora, Algarrobo de Hornillos, Litipampa y el Almendro de Quiles.
Coquimbo	Río Hurtado	353	24-03-87	146/87	**Consultorio Rural:** Pichasca. **Estaciones Médicos Rurales:** Huampulla y Tabaqueros.
Coquimbo	Mincha	354	24-03-87	148/87	**Consultorio General Rural:** Canela Baja. **Estaciones Médico Rurales:** Jabonería, Quebrada de Linares, El Talhuén, Los Pozos, El Durazno, Alhuemilla, La Cortadera, Las Barrancas, La Parrita, Los Canelos, El Coligüe, Poza Honda, Puerto Oscuro, El Totoral, Atelcura, Las Tazas, El Potrero, Matancilla, Agua Fría Alta y Mincha Sur.
Libertador General Bdo. O'Higgins	Mostazal	355	24-03-87	192/87	**Consultorio General Rural:** San Francisco de Mostazal.
Libertador General Bdo. O'Higgins	Coltauco	356	24-03-87	193/87	**Consultorio General Rural:** Coltauco. **Estación Médico Rural:** Cuesta de Idahue.
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 Médico Rurales:** Nilahue Cornejo y Nilahue Barahona.
Libertador General Bdo. O'Higgins	Requínoa	360	24-03-87	190/87	**Consultorio General Rural:** Requínoa.

(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

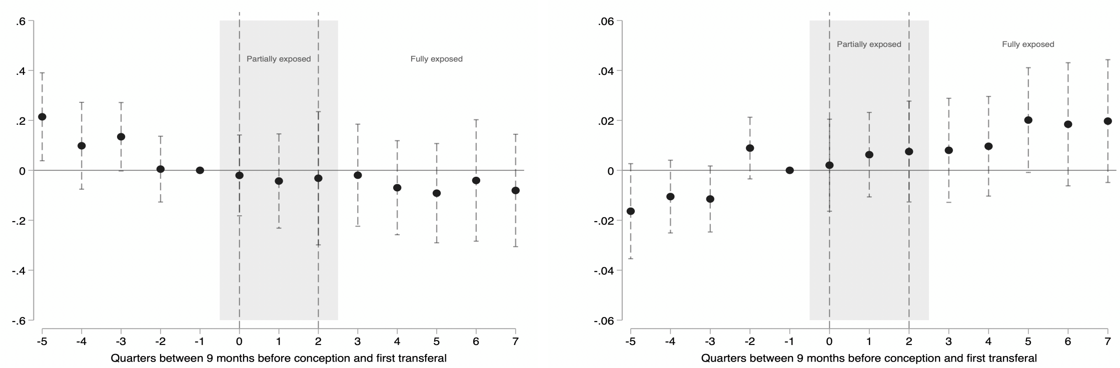


(a) Birth weight

(b) Low birth weight

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: Effect of the reform on individual birth outcomes



(a) Gestational age (weeks)

(b) Pre-term birth

Notes: These figures show estimates of event-study coefficients from equation 3 on individual birth outcomes. Controls include individual mothers' characteristics and municipality characteristics. Partially treated cohorts as those affected by the reform during pregnancy but that were conceived before the first transferal (gray shaded area), and fully treated cohorts are those conceived after the first transferal. The vertical lines represent the relative quarter of conception for the first cohort whose pregnancy was partially affected by the reform (relative time 0), and for the first cohort whose entire pregnancy was affected by the reform (relative time 3).

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 (grs)	(2) Gestational age (weeks)	(3) LBW	(4) VLBW	(5) Pre-term
Interaction	-27.9536*	0.0222	0.0125**	-0.0002	0.0045
	(15.7478)	(0.0407)	(0.0055)	(0.0014)	(0.0052)
Sample mean	3242.202	38.971	0.062	0.004	0.057
R^2	0.369	0.427	0.106	0.046	0.177
Observations	4,680	4,680	4,680	4,680	4,680
Municipalities	120	120	120	120	120
Avg. mother charact.	Yes	Yes	Yes	Yes	Yes
Municipality Controls	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses

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

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 moartality	Infant mortality	Neonatal mortality	Infant mortality
Interaction	0.0036	0.0083**	0.0041	0.0083*
	(0.0025)	(0.0040)	(0.0028)	(0.0046)
Sample mean	0.013	0.025	0.013	0.025
R^2	0.090	0.160	0.095	0.163
Observations	4,680	4,680	4,680	4,680
Municipalities	120	120	120	120

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 by birth order

	Birth outcomes				
	Birth weight	Gestational age	LBW	VLBW	Pre-term
	(1)	(2)	(3)	(4)	(5)
Interaction* high order birth	-42.427**	-0.072	0.016**	0.002	0.009
	(19.529)	(0.069)	(0.007)	(0.003)	(0.006)
Interaction* first birth	-25.423	0.034	0.011	0.003	0.002
	(18.070)	(0.066)	(0.007)	(0.002)	(0.008)
Mean Y high order birth	3286.227	38.891	0.061	0.009	0.061
Mean Y first birth	3181.263	38.967	0.071	0.007	0.058
P-value	0.071	0.051	0.225	0.410	0.142
Observations	395,637	395,637	395,637	395,637	395,637

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.5: Municipalization effects on fully treated units

	(1) Share Deaths: 28 days	(2) Share Deaths: 1 year
Average Total Effect	0.0047	0.0061
	(0.0078)	(0.0105)
Observations	78957	78957
Switchers x Periods	46247	46247

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: This table presents results for the estimator proposed by de Chaisemartin and D'Haultfœuille (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.6: Municipalization effects on partially treated units

	(1) Share Deaths: 28 days	(2) Share Deaths: 1 year
Average Total Effect	0.0013	−0.0002
	(0.0099)	(0.0122)
Observations	74486	74486
Switchers x Periods	42196	42196
p-value Joint Nullity	0.299	0.293

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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.7: Bounds on municipalization effects

	Birth outcomes				
	Birth weight	Gestational age	LBW	VLBW	Pre-term
	(1)	(2)	(3)	(4)	(5)
Upper bound	-30.615**	0.042	0.007*	0.002*	-0.003
	(14.609)	(0.050)	(0.004)	(0.001)	(0.004)
Lower bound	-39.405**	-0.028	0.016**	0.003	0.007
	(17.045)	(0.067)	(0.008)	(0.003)	(0.008)
Observations	358,041	358,041	358,041	358,041	358,041

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 upper bound is estimated in the sample that trims the 17.8% highest outcomes of women in highly exposed municipalities, and the lower bound is estimated in the sample that trims 17.8% of the lowest outcomes of women in highly exposed municipalities. Standard errors are clustered at the mother's municipality of residence at birth. 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	0.007**	0.002	9.068	0.288	-0.004	-0.004	-0.015
	(0.004)	(0.002)	(40.155)	(0.209)	(0.016)	(0.004)	(0.019)
Interaction* high deficit	0.011**	0.004	-60.404**	-0.231**	0.024**	0.007**	0.020*
	(0.005)	(0.002)	(28.111)	(0.116)	(0.011)	(0.003)	(0.011)
Mean Y low	0.025	0.009	3238.622	38.938	0.067	0.008	0.061
Mean Y high	0.025	0.009	3245.935	38.911	0.064	0.008	0.059
p-value	0.441	0.526	0.015	0.001	0.012	0.001	0.020
Panel B. Municipal per-capita revenue							
Interaction* low revenue	0.010**	0.004*	-36.165*	-0.090	0.018**	0.003	0.013
	(0.004)	(0.002)	19.033)	(0.083)	(0.009)	(0.003)	(0.009)
Interaction* high revenue	0.002	-0.003	-33.658	0.038	0.010	0.002	-0.002
	(0.006)	(0.003)	(21.522)	(0.096)	(0.009)	(0.003)	(0.010)
Mean Y low	0.024	0.008	3234.767	38.917	0.066	0.008	0.060
Mean Y high	0.025	0.009	3247.835	38.926	0.065	0.008	0.060
p-value	0.097	0.008	0.898	0.245	0.390	0.644	0.099
Panel C. Municipal per-capita spending							
Interaction* low spending	0.011**	0.004**	-37.486*	-0.113	0.019*	0.003	0.015
	(0.004)	(0.002)	(21.890)	(0.095)	(0.010)	(0.003)	(0.011)
Interaction* high spending	0.004	-0.002	-33.508	0.052	0.009	0.002	-0.003
	(0.006)	(0.003)	(21.347)	(0.092)	(0.010)	(0.003)	(0.010)
(0.006)							
Mean Y low	0.024	0.008	3234.196	38.910	0.067	0.009	0.061
Mean Y high	0.025	0.009	3248.233	38.930	0.064	0.008	0.059
p-value	0.164	0.008	0.822	0.102	0.324	0.653	0.040
Observations	4,680	4,680	395637	395637	395637	395637	395637

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:

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

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	-0.0023	0.0003	0.0011
	(0.0012)	(0.0016)	(0.0015)	(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.