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Delivering Privately: Cesarean Practice and Long-Term Maternal Health

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Abstract

We study the causal effect of delivering in a private rather than a public hospital on maternal outcomes in Chile. We exploit a 2003 copayment reduction that expanded access to private hospitals for women insured by the public system as an instrument for private delivery. Combining administrative birth records with hospital discharge data, we estimate an instrumented difference-in-differences model that follows mothers for up to fifteen years after the first birth. Private hospital delivery improves short-term outcomes, reducing prolonged hospitalizations and 30-day readmissions. The offsetting risks come with later pregnancies: women induced to deliver in private hospitals face higher rates of repeat C-sections, cesarean-scar complications, and hysterectomy. Subsequent fertility is unaffected. The results point to a trade-off between short-run improvements and long-run maternal health risks from hospital practices that favor planned cesarean delivery.

JEL classification

I11, I12, I13, I18, J13

Keywords

cesarean section, maternal health, private hospitals, provider incentives

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1 Introduction

Medical practice varies widely across providers treating the same condition, and part of that variation reflects how providers are paid and organized rather than differences in patient need. Childbirth is a leading example. Cesarean rates are systematically higher under private provision: a meta-analysis across countries puts the odds of cesarean delivery in for-profit hospitals at 1.41 times those in not-for-profit hospitals (Hoxha et al., 2017), and in Chile, the setting of this paper, C-section rates among publicly insured first-time mothers in the early 2000s were around 30 percent in public hospitals and above 60 percent in private ones. Worldwide, one in five births is delivered by cesarean (Boerma et al., 2018), and the rise is concentrated in cesareans decided before labor rather than during spontaneous labor (Vogel et al., 2015).

These practice differences matter for women's health because the cesarean has a documented long-run risk profile. The surgery leaves a uterine scar, and the obstetric literature links that scar to complications that compound over subsequent pregnancies: repeat cesareans, abnormal placentation—which can end in emergency hysterectomy at delivery—and uterine rupture, with risks that rise with each additional C-section (Sandall et al., 2018; Keag et al., 2018). These costs fall on the mother, years after the first cesarean. The evidence, however, is observational: women who deliver by cesarean differ from those who deliver vaginally, and causation cannot be inferred (Keag et al., 2018).

This paper asks how delivering in a private hospital—where cesarean rates are twice those of public hospitals—affects maternal health, in the short and the long run. Identification requires variation in where women give birth that is unrelated to their health. We obtain it from a Chilean policy that expanded access to private hospitals for publicly insured women. In April 2003, Chile's public insurer cut the copayment that its mid- and upper-income beneficiaries paid for delivery at a private hospital, reducing the out-of-pocket cost from roughly USD 600 to USD 260. The reform changed only the price patients paid; hospital reimbursement and the financing of public hospitals were unchanged.

Chile's public insurer, Fonasa, classifies its enrollees into four income groups: group A, the poorest, receives free care in the public hospital network, while groups B, C, and D, defined by rising income brackets, may also deliver at a private hospital under contract with Fonasa. For childbirth, that private option is priced through the *Pago Asociado a Diagnóstico* (PAD): Fonasa fixes one bundled price for the full delivery episode—the same whether the birth is vaginal or cesarean—and a patient copayment that is uniform across the three groups. The 2003 reform cut that copayment. Women with private insurance (Isapre) are not eligible for PAD and were unaffected, as were group A women, who remained restricted to public hospitals.

To estimate the effects of private delivery, we link administrative records on births and hospitalizations, enabling us to follow mothers for up to 15 years after their first delivery. We focus on births from 2002 to 2005 and exploit differences in reform eligibility across Fonasa insurance groups to implement an instrumented difference-in-differences strategy. We compare outcomes for women in Fonasa groups B, C, and D (whose copayment for private delivery fell) with those in Fonasa Group A and private insurance (Isapre), using an interaction between eligibility and the post-reform period as an instrument.

Delivering in a private hospital raises the probability of a cesarean at first birth by 44.6 percentage points, consistent with our previous work on this reform (de Elejalde and Giolito, 2021). In the short run, maternal outcomes improve: the hospital stay shortens by 1.4 days, stays of seven days or more fall by about 9 percentage points, and readmissions within 30 days fall by 2.5 percentage points. Part of the decline comes from the mode of delivery itself: a cesarean avoids the wound complications of vaginal birth, with no offsetting rise in cesarean-specific complications within 30 days.

The risks, however, come with later pregnancies: women who delivered privately are 48 percentage points more likely to have a repeat cesarean, 5.7 percentage points more likely to be hospitalized for complications of the cesarean scar within three years, and about 1 percentage point more likely to undergo a hysterectomy by age 35, an effect concentrated in obstetric causes. Unlike recent evidence on unplanned C-sections (Halla et al., 2020), we find no effects on fertility. Expanding access to private maternity care thus improves immediate maternal outcomes while exposing women to a delivery practice that carries persistent health risks.

We test the identifying assumptions. Treatment and control groups show no differential pre-reform trends, in private hospital delivery or in the outcomes. Placebo regressions on predetermined maternal characteristics show no compositional changes around the reform. The reallocation of deliveries could also have congested private hospitals, relieved public ones, or pushed them to compete—spillovers that would violate SUTVA and the exclusion restriction. We drop the control municipalities most exposed to them, restrict the Isapre control group to private hospitals the inflow should not have reached, and compare hospitals by how much of the reallocation they absorbed. Across these checks we find little evidence of a change in the care received at either hospital type or spillover effects: the reform affects a woman's outcomes through her own delivery hospital.

We also test robustness to alternative definitions of the treatment and control groups that address insurance switching, to an alternative instrument built from population shares by age group and municipality, which does not rely on the stability of insurance composition, to a stricter clinical definition of cesarean delivery, to functional form, and to using Fonasa Group D alone as the instrument. The pattern of results is unchanged.

This paper contributes to the quasi-experimental literature on cesarean delivery. That literature identifies the effects of unplanned cesareans, using variation in the hour or day of the birth (Costa-Ramón et al., 2018; Halla et al., 2020; Costa-Ramón et al., 2022), and of cesareans for breech presentation (Jensen and Wüst, 2015; Mühlrad, 2022); its outcomes are the child's health and the mother's fertility and labor supply. The planned cesarean of a low-risk pregnancy lies outside the reach of those instruments: variation in the timing of the birth moves decisions made in labor, and cesareans for breech presentation are medically indicated. We estimate instead the effect of delivering in a private hospital with high cesarean use—the design closest to Card et al. (2023)—and in our setting the additional cesareans are scheduled deliveries, the margin Miller et al. (2025) study through an intervention that reduced cesareans in low-risk first births. Following maternal health for 15 years, we find the sequence that the medical literature documents as associations: repeat cesareans, scar complications, and hysterectomy. Neither the timing-based studies nor the hospital-based ones observe long-run maternal health.

The paper also contributes to the literature on how provider incentives shape medical practice: physician fees (Gruber and Owings, 1996; Gruber et al., 1999), the convenience of scheduled deliveries, and hospital capacity. Fees play no role under PAD's bundled price. In de Elejalde and Giolito (2021) we find evidence of the capacity channel: private hospitals use scheduled cesareans to smooth demand for a fixed capacity. Here we estimate the consequences of that practice for maternal health.

The remainder of this paper is organized as follows. Section 2 discusses the institutional setting in Chile. Section 3 describes the administrative data we use and the process we follow to link datasets from different sources. Section 4 describes our identification strategy. Section 5 reports our results and robustness exercises. Section 6 discusses mechanisms and interpretation, and the last section concludes.

2 Institutional Setting

2.1 The Chilean health insurance system

Health insurance in Chile is mandatory for employees and retirees, who contribute 7% of their taxable income to the system.[1] They can enroll in public insurance, administered by

[1] Self-employed workers were not subject to mandatory contributions during our sample period. Ley 20.255 (2008) introduced the obligation; pension contributions phased in from 2012 and the 7% health contribution only from 2018.

the *Fondo Nacional de Salud* (Fonasa), or in private insurance, supplied by firms called *Isapres* (*Instituciones de Salud Previsional*). Contributors may switch between Fonasa and Isapre but cannot opt out of the system altogether. In 2024, Fonasa covered approximately 16.75 million people (82% of the Chilean population), while Isapres covered 2.76 million (13.7%).[2] During our sample period, Isapre premiums were set by a sex- and age-specific factor table that charged women of reproductive age substantially more than men of the same age, and a subset of Isapre plans offered reduced or partial childbirth coverage. Fonasa classifies its enrollees into four income-based groups. Group A includes those living below the poverty line, those receiving government subsidies, and those with no declared income. The remaining groups are determined by monthly taxable income: Group B for those earning less than approximately USD 360, Group C for those between USD 360 and USD 530, and Group D for those above USD 530.[3] Coinsurance for care in public hospitals depends on the group but not on the diagnosis: zero for groups A and B, 10% for C, and 20% for D.[4] As of 2003, Group A was the largest among Fonasa beneficiaries (36.6%), followed by Group B (32.3%), C (17.7%), and D (13.4%).

Enrollees in groups B, C, and D may use either of two modalities of care. Under the *Modalidad de Atención Institucional* (MAI), they use the public hospital network and pay coinsurance set by their group. Assignment is territorial: beneficiaries enroll in a primary-care center in their municipality of residence, which refers them to a hospital in the same *Servicio de Salud* (Health Service); there are 29 Health Services nationwide. Under the *Modalidad de Libre Elección* (MLE), they choose freely among private providers under contract with Fonasa, and copayments are set centrally, the same for every Fonasa group. Group A may use only MAI.[5]

Public and private providers are paid through different mechanisms, even for the same diagnosis. Public hospitals receive two transfers from Fonasa: the *Programa de Prestaciones Institucionales* (PPI), which funds staff and infrastructure, and the *Programa de Prestaciones Valoradas* (PPV), which pays for services rendered. Physicians at public hospitals are salaried civil servants, so their compensation does not vary with the diagnosis, the procedure, or the

[2] Coverage has shifted toward Fonasa over time. In December 2003, Fonasa covered 10.58 million people (66% of the population) and Isapres 2.73 million (17%).

[3] An enrollee supporting three or more dependents (*cargas familiares*) drops one group, so a worker classified as D on income alone but with three or more dependents is reclassified as C. Reclassification is performed automatically by Fonasa, drawing on payroll and tax records.

[4] Ley 18.469 (1985), art. 30: the State covers the full value of care in the institutional modality for groups A and B, and at least 75 and 50 percent for groups C and D; the 10 and 20 percent copayments for C and D applied throughout our sample period.

[5] The MLE network also formally includes private-style wards inside public hospitals, but these account for only 0.24% of public-hospital deliveries in 2002, the only year for which the financing modality is directly observed in our data.

volume of patients seen.[6]

Within MLE, providers are reimbursed through one of two mechanisms. For most procedures, providers are paid per service according to the MLE fee schedule, and the patient pays the copayment set by that schedule. For a closed list of diagnoses—delivery being one of them—Fonasa instead applies a prospective, DRG-style scheme called *Pago Asociado a Diagnóstico* (PAD), introduced in 1995 (see Barahona-Urbina and Rodríguez, 2011).[7] Under PAD, Fonasa fixes both a hospital fee and a patient copayment per diagnosis, both invariant to the procedures actually performed during the episode and uniform across Fonasa groups B, C, and D. The PAD delivery bundle covers physician and midwife fees, hospitalization, medical examinations, medications, vaccinations, and postnatal care for up to 15 days after discharge, and pays the same amount whether the delivery is vaginal or cesarean. Physician fees are embedded in the bundle, so doctors at PAD private hospitals do not earn more for performing a cesarean than for a vaginal delivery under this scheme. Eligibility for PAD delivery requires (i) enrollment in Fonasa Group B, C, or D; (ii) a singleton birth; (iii) gestational age of at least 37 weeks at delivery; and (iv) a low-risk pregnancy classification.[8]

Isapre enrollees, by contrast, are treated by private providers that bill fee-for-service at their own price lists: the plan covers a share set by the enrollee's contract, subject to coverage ceilings, and the enrollee pays the remainder.

These payment arrangements create different incentives to perform cesareans. On the private side two channels operate. First, deliveries of Isapre patients are paid fee-for-service, a payment form under which a cesarean is typically priced above a vaginal delivery (Gruber and Owings, 1996). Second, for Fonasa groups B, C, and D the fixed-price PAD bundle leaves no such price margin, but cesareans can be scheduled in advance, which allows hospitals to smooth demand and raise capacity utilization (de Elejalde and Giolito, 2021). On the public side both channels are muted: physicians are salaried, and public hospital managers face low-powered performance incentives (Muñoz and Otero, 2025). The difference in incentives is therefore part of what it means to deliver in a private rather than a public hospital, that is, part of the treatment we estimate.

[6] Public-sector physician compensation is governed by Ley 15.076 and Ley 19.664, which set a base salary together with seniority and experience allowances, with no fee-for-service component.

[7] The PAD list comprised 25 diagnoses in 2002 and 26 in 2003. Delivery alone accounted for roughly half of all PAD services and, together with cataract surgery, gallstone surgery, tonsillectomy, and abdominal hernia repair, for over 90 percent. Sources: FONASA, *Arancel de la Modalidad de Libre Elección* (list of PAD diagnoses); and authors' calculations from FONASA, *Boletín Estadístico* (Modalidad Libre Elección, Table 5.1, 1998–2014), for the activity shares.

[8] A physician's statement certifying low risk must be submitted to the chosen hospital by the 37th week. Because the definition of high-risk pregnancy under the PAD requirements is somewhat broad, the hospital may request additional tests and reserves the right to reject the case. If accepted, the woman pays the fixed PAD copayment.

2.2 The 2003 PAD reform

The 2003 reform reduced the patient copayment for PAD delivery from 60% to 25% of the bundle fee. The patient's out-of-pocket cost fell from approximately CLP 415,000 (≈ USD 600) to CLP 178,000 (≈ USD 258) per delivery.[9]The change was implemented on April 2, 2003.[10] Figure 1 shows the share of births in private hospitals by insurance type from 2001 to 2006. Before the reform, the share was already elevated for Group D (around 25%) and low for groups B and C; after April 2003, it rose steadily across all three Fonasa groups, reaching 60% for D, 23% for C, and 8% for B by the third quarter of 2006. The corresponding shares for Isapre-insured women and for Fonasa A women remained essentially flat.

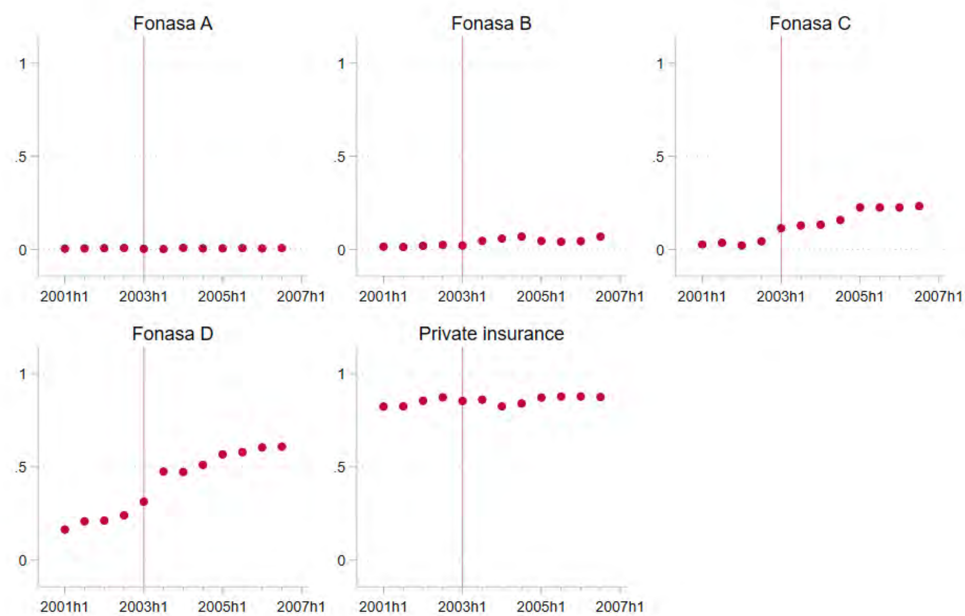


Figure 1: Trends in the share of births in private hospitals by insurance type, 2001–2006

Note: The figure plots the share of first, in-hospital singleton births that occurred in private hospitals, by insurance type at the time of delivery. The data span from the first half of 2001 to the second half of 2006. The sample includes mothers aged 15 to 47 enrolled in public insurance (Fonasa groups A–D) or private insurance (Isapre).

During this period, most private hospitals in Chile performed at least one delivery financed through the PAD system. In 2005, there were 232 hospitals with recorded deliveries: 70 private and 162 public. Of the 70 private hospitals, 11 were not on the PAD list. These 11 hospitals accounted for approximately 3% of total deliveries in 2005.

[9]Converted at the 2002 annual average exchange rate of CLP 691 per USD.

[10]This change was instituted by Decreto 48 of the Ministry of Health, issued on February 11, 2003. The decree is available at http://bcn.cl/2bofl.

Figure 2 plots each hospital's C-section rate against its delivery volume in 2002 (log scale), before the reform. On average, private hospitals (diamonds) have substantially higher C-section rates than public hospitals (circles) of comparable volume. No private hospital in the figure reaches the top of the volume distribution, so the comparison covers only the range in which both sectors are present. This pattern suggests that the reform facilitated access to higher-C-section hospitals, allowing women to switch from low- to high-C-section providers.

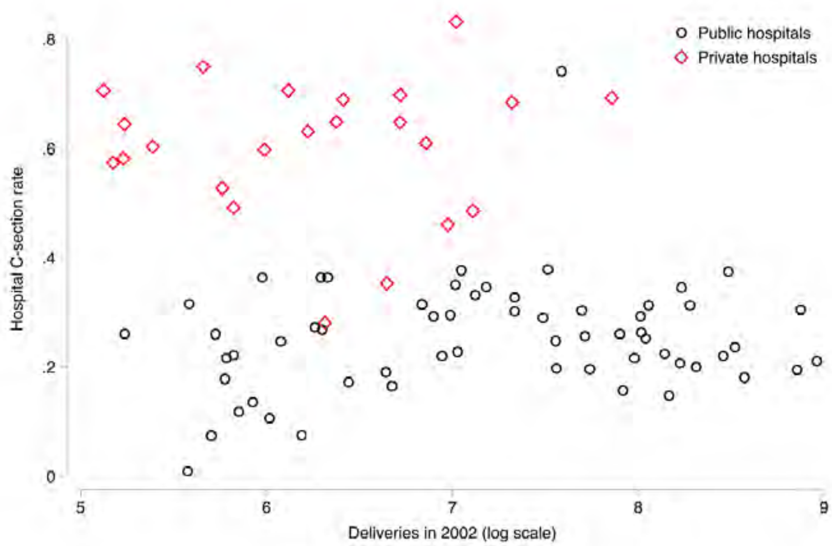


Figure 2: C-section rates and delivery volume across hospitals, 2002

Note: Each point represents a hospital. The y-axis shows the hospital's C-section rate among singleton first births in our estimation sample in 2002. The x-axis shows the log of all deliveries at the hospital that year, from hospital discharge records. Diamonds represent private hospitals; circles represent public hospitals. The figure excludes hospitals with 100 or fewer singleton first births.

Beyond their higher C-section rates, private hospitals in Chile tend to outperform public hospitals on several quality and efficiency measures. Figure D.1 in the Online Appendix compares risk-adjusted length of stay, 30-day readmission rates, in-hospital mortality, and 30-day mortality—both overall and for acute myocardial infarction—using all hospitalizations from 2012 to 2016, controlling for the Charlson comorbidity index and including fixed effects for age, sex, principal diagnosis, and year. Figure D.2 extends these comparisons to additional conditions such as heart failure, stroke, pneumonia, and hip fracture. Although these differences cannot be given a causal interpretation, they suggest that private hospitals are not simply higher-cesarean providers but also tend to score better on standard inpatient quality metrics.[11]

[11]The gap between public and private hospitals may have narrowed in recent years due to policies targeting the management of public establishments. Muñoz and Otero (2025) find that a reform in Chile introducing

The reform changed only one thing: the patient's share of the PAD bundle fee. The bundle fee itself, the embedded physician compensation (the same for vaginal and cesarean deliveries), and the financing of public hospitals through PPI and PPV all remained unchanged apart from the routine annual adjustment of the fee schedule, as did MAI coinsurance and group classification rules. The decree was equally narrow across procedures: beyond delivery, it added only hemodialysis and two emergency care codes to the MAI fee schedule, and modified no other PAD diagnosis.

3 Data

Our analysis combines two administrative datasets from Chile: birth certificates and hospital discharge records, both provided by the Ministry of Health.

The birth certificate data include all registered live births in Chile between 2001 and 2018.[12] Each record contains a unique anonymized ID for both the mother and the child, along with detailed information such as date of birth, birth weight, size at birth, gestational age, indicators for singleton or multiple births, parity, and the delivery hospital. Maternal characteristics—age, municipality of residence, educational attainment, and employment status—are also recorded.

The hospital discharge data include all inpatient admissions in Chile between 2001 and 2020. Each record contains a unique anonymized patient ID, the discharging hospital, discharge date, length of stay, and the principal diagnosis coded under ICD-10. The data also report whether a surgical procedure was performed, the corresponding Fonasa procedure code, and patient characteristics such as age, gender, municipality of residence, and type of insurance.[13]

Crucially, both datasets use a common anonymized identifier, enabling linkage at the individual level.

To construct our final sample, we implement a multi-step record-linking procedure that matches birth records to corresponding hospital admissions for childbirth. We begin by identifying childbirth-related admissions in the hospital discharge data, selecting female patients whose primary diagnosis code begins with "O," which corresponds to pregnancy, childbirth, and the puerperium under ICD-10 classification. We then match a birth to a hospitalization when the patient ID coincides with the mother ID and the hospital discharge

competitive recruitment and improved compensation for public hospital CEOs significantly reduced hospital mortality by 8% in the three years following its adoption.

[12]Although the dataset is available starting in 1992, records prior to 2001 include neither a mother nor a child identifier. The registry records live births only; fetal deaths and other non-live births are not included.

[13]Information on surgical procedure codes is available from 2004 onward.

month is within ±2 months of the recorded birth month; for the small number of records in which the exact discharge date is missing, we use the mother ID and the year of the discharge record, allowing a discrepancy of up to one year.[14] Using this procedure, we match 86 percent of birth records between 2001 and 2018 to hospitalizations.

We then restrict the sample to matched, in-hospital singleton births to nulliparous women between 2001 and 2005. We further limit the sample to women aged 15 to 47 who were covered by either public insurance (Fonasa) or private insurance (Isapre) at the time of delivery.[15]

Using this matched sample, along with birth and hospital discharge data for subsequent years, we construct variables capturing *short-term* (immediately following the first birth hospitalization), *and long-term outcomes*.

Short-term outcomes include: (i) an indicator for *cesarean section*, identified using the principal diagnosis codes and whether a surgical procedure was performed,[16] (ii) *length of stay* in days; (iii) an indicator for *prolonged maternal hospitalization*, defined as a stay of seven days or more; (iv) an indicator for *a readmission within 30 days* of the discharge date, at any public or private hospital, and (v) four indicators for *diagnostic groups within the 30-day readmission window —puerperal infections* (ICD-10 O85, O86, O91, N61), *wound complications* (O90.0–O90.2), with the perineal subset (O90.1, O90.2) reported separately, *complications of procedures* (T81), and *postpartum hemorrhage* (O72) — which together account for approximately 55 percent of all 30-day maternal readmissions in our sample (see Online Appendix B for the operational definitions and Online Appendix Table B.2 for the full ranking of diagnostic codes).

Long-term outcomes include both health and subsequent fertility measures. Health outcomes are obtained from hospital discharge data covering up to 15 years after the first birth, and include hospitalizations related to maternal care, specifically for *cesarean scar complications* (ICD-10 code O342) and *hysterectomy* (ICD-10 code O822 and corresponding surgical procedure codes from Fonasa).[17] The hysterectomy outcome is bounded by maternal age: the main indicator equals one for a hysterectomy before age 35 within 15 years of the first birth, and we also report the same indicator for hysterectomies before ages 30, 40, and 45. For a subsample of mothers with two or more children within 12 years of the first birth,

[14] See Appendix A for more details on the dataset construction.

[15] Due to the substantial number of records lacking IDs in 2001, our estimation sample is restricted to the period 2002–2005. Nevertheless, we include data from 2001 in graphical analyses to assess pre-existing trends. The inclusion of observations from 2001 does not affect our results.

[16] Online Appendix B provides additional details on the construction of the cesarean section variable. We do not classify every diagnosis code into a mode of delivery; as a result, the mode of delivery cannot be identified for 2.0 percent of births in the main sample.

[17] See Online Appendix B for details on the construction of the hysterectomy variable.

we also construct indicators for having *a cesarean section in the second birth* and for having *cesarean sections in both the first and second births* (repeat C-sections). Finally, subsequent fertility outcomes are derived from birth certificate data covering up to 12 years after the first birth. Specifically, we create indicators for *whether the mother had a second or third child* within 1 to 12 years of the first birth, and we count the *cumulative number of children* she has within the same window.

Table 1 presents summary statistics for our main sample, with columns corresponding to different insurance types: Fonasa A (47% of the sample), B (18%), C (10%), D (12%), private insurance (13%), and a final column for the full sample ("All"). The Fonasa groups are income brackets: group A covers women below the poverty line or with no declared income, and groups B, C, and D cover successively higher earnings; only these last three can use private providers under contract with Fonasa.

Panel A reports demographic characteristics at the time of the first birth. In the full sample, mothers are, on average, 23 years old, 29 percent report being employed and the average educational attainment is 11.5 years. As expected, there are differences across insurance groups: mothers in Fonasa A tend to be younger, less likely to be employed, and have fewer years of education, while mothers with private insurance are older, more likely to be employed, and have more years of education.

Panel B summarizes short-term outcomes. In the full sample, 19 percent of deliveries take place in private hospitals, 34 percent are cesarean sections, the average stay is 3.6 days, 6 percent of mothers remain hospitalized for seven days or more, and 2.5 percent are readmitted within 30 days of discharge. Delivery in private hospitals and cesarean rates increase progressively from Fonasa A to private insurance, while stays are longer and prolonged stays and readmissions more frequent among Fonasa A enrollees.

Panel C shows long-term outcomes. In the full sample, 1.4 percent of mothers are hospitalized for cesarean scar complications within 3 years of the first birth, and 0.7 percent undergo a hysterectomy by age 35 within 15 years. Cesarean scar complications are more prevalent among women with private insurance, rising from 0.5 percent in Fonasa A to 4.7 percent; hysterectomy by age 35 is similar across groups.[18] Regarding fertility, 41 percent of mothers have a second child within six years of the first birth, and 14 percent have a third child within nine years. On average they have 2.0 children within twelve years. Fertility outcomes also vary by insurance status: mothers in Fonasa A and those with private insurance are more likely to have additional children.[19]

[18]Table D.1 in the Online Appendix shows how hospitalizations due to cesarean scar complications and hysterectomy increase as the time horizon after delivery lengthens—at 3, 6, 9, 12, and 15 years after the first delivery—across all insurance groups.

[19]Online Appendix Table D.2 provides a detailed summary by insurance group and years since the first

Table 2 presents summary statistics for a restricted sample of women with two or more pregnancies. Across all insurance groups, cesarean section rates are higher at the second birth than at the first, and they still rise steadily from Fonasa A to Fonasa D.[20]

Maternal age at first birth differs across insurance groups and correlates with both delivery mode and maternal health. Every specification below therefore absorbs mother's age fixed effects.

4 Empirical Strategy

We estimate the effects of delivering in a private hospital—defined as any hospital that is not part of the public health system—on short- and long-term outcomes. Women who deliver at private hospitals are not comparable to women who deliver at public hospitals. They are older, better educated and far more likely to be employed (Table 1), and they may differ in unobservables such as their preferences over the mode of delivery or the risk of the pregnancy. These are also determinants of the outcomes we study, so comparing the two groups confounds the effect of the hospital with the characteristics of the women who choose it and of their pregnancies. To address this, we use an instrumented difference-in-differences (DID-IV) strategy that exploits a 2003 policy reform that reduced the copayment for delivery in a private hospital under the PAD scheme. Specifically, we instrument for delivery in a private hospital using the interaction of a post-reform indicator (equal to one for births from April 2003 onward) with indicators for enrollment in Fonasa B, C, or D. These are the three higher-income groups within Fonasa, and the only ones eligible for PAD. Group A, the poorest, could deliver only in public hospitals, and women with private insurance (Isapre) were not eligible for PAD; both serve as comparison groups. We use separate indicators for each Fonasa group to capture the heterogeneous effects of the reform, reflecting group-specific differences in policy take-up.

Our estimating equation is:

$$\begin{aligned} Y_{it} =& \beta_0 + \beta_1 \, Private_{it} + \beta_2 \, Fonasa\ A_{it} + \beta_3 \, Fonasa\ B_{it} \\ &+ \beta_4 \, Fonasa\ C_{it} + \beta_5 \, Fonasa\ D_{it} + \tau_t + \alpha_{a(i)} + \epsilon_{it}, \end{aligned} \tag{1}$$

where Y_{it} is the outcome variable for delivery i in month-year t, $Private_{it}$ is a dummy variable

birth, showing that privately insured mothers are more likely to have a second child within 3, 6, and 9 years—likely reflecting their older age at first birth and a shorter reproductive window.

[20]The sample of mothers with two or more children includes only those who have at least two births within a 12-year period and for whom we are able to identify the mode of delivery for both the first and the second birth.

that equals one if the delivery occurred in a private hospital, $Fonasa\,A_{it}$ through $Fonasa\,D_{it}$ indicate insurance group at delivery, τ_t are month-year fixed effects, $\alpha_{a(i)}$ are mother's age fixed effects, and ϵ_{it} captures unobservables that affect the outcome variable. The coefficient β_1 represents the causal effect of interest. This is our baseline specification: month-year and mother's age fixed effects enter separately, with no interaction between them. Richer fixed-effects structures—an age × year-of-birth interaction, municipality-age and municipality-year fixed effects, and a linear trend for Fonasa A—are introduced in subsequent columns as robustness checks.

The first stage equation is:

$$\begin{aligned} Private_{it} = \; & \pi_0 + \pi_1\,Fonasa\,B_{it} \times Post_t + \pi_2\,Fonasa\,C_{it} \times Post_t \\ & + \pi_3\,Fonasa\,D_{it} \times Post_t + \pi_4\,Fonasa\,A_{it} + \pi_5\,Fonasa\,B_{it} \\ & + \pi_6\,Fonasa\,C_{it} + \pi_7\,Fonasa\,D_{it} + \tau_t + \alpha_{a(i)} + \nu_{it}, \end{aligned} \tag{2}$$

where $Post_t$ is a dummy variable equal to one for deliveries occurring after April 2003.[21] The interaction terms $Fonasa\,Group_{it} \times Post_t$ are used as instruments and capture differential changes in private hospital utilization following the reform across Fonasa groups B, C, and D. The coefficients π_1, π_2, and π_3 thus measure the increase in the probability of delivering in a private hospital after the reform, conditional on insurance type.

Standard errors are clustered at the municipality of residence. Following Abadie et al. (2023), we treat the level of clustering as a design decision: it should follow how the variation we exploit is assigned, not the correlation of residuals within groups.[22]

Following Miyaji (2026), our design identifies the local average treatment effect on the treated (LATET): the effect of delivering at a private hospital for the compliers, the women in Fonasa groups B, C, and D whom the reform moved from a public to a private hospital.[23] The unscaled difference-in-differences and the IV are different estimands. The comparison of eligible and ineligible groups identifies the reform's intention-to-treat effect, whether or not a woman changed hospital; the IV divides that comparison by take-up, and so recovers the effect of the change in hospital for the women who made it. We characterize the compliers at the end of this section.

[21]The norm that decreased the copayment for PAD delivery was approved on February 11th, 2003, but went into effect on April 2nd, 2003.

[22]What the reform assigns is the copayment reduction. It reaches a woman through her insurance group, and how far it moves her hospital choice depends on the private hospitals near her home — their number, their capacity and their distance. That supply varies across municipalities and is fixed before she chooses where to give birth. The hospital of delivery is realized after the assignment and is the margin the reform moves, so it is not the level.

[23]With three instruments, two-stage least squares returns a weighted average of the three group-specific ratios, weighted by the strength of each group's first stage.

We rely on the following identifying assumptions:[24]

No compositional changes:
This assumption rules out changes over time in the distribution of women across insurance groups, which is necessary given our use of repeated cross-sectional data; Miyaji (2026) formally extends the DID-IV framework to this setting. A plausible concern is that the expansion of PAD coverage may have incentivized switching from private insurance (Isapre) to Fonasa, particularly into Group D.[25] In Section 5.3 we assess this empirically. Such switching would mainly affect the composition of group D: if Isapre affiliates moved into D, the group would gain members who are richer, older and already inclined to deliver privately, and both the first stage and the reduced form would pick that up as if it were a response to the reform. We therefore run three checks. The first is a set of placebo regressions using predetermined maternal characteristics — age, education and employment — as outcomes, so that a post-reform shift in the composition of the eligible groups would show up directly. The second varies the treatment and control definitions, adding women with private insurance to the treated group and, separately, dropping group D altogether. The third replaces the woman's own insurance group in the instrument with the share of Fonasa B, C, and D beneficiaries in her municipality-by-age cell, so that the instrument does not use her recorded group at all and is unaffected by switching unless she also moves municipality (Online Appendix Table D.15). None of the three points to compositional change driving our estimates.

Stable unit treatment value (SUTVA):
A woman's outcomes should not depend on where other women deliver. The reform moved a large number of deliveries from public to private hospitals, and that reallocation could reach a woman whether or not her own hospital changed. Three channels operate on the women who stayed where they were. The inflow of patients in Fonasa groups B, C, and D could congest the private hospitals participating in PAD and worsen outcomes for the Isapre women already delivering there. The matching outflow could decongest public hospitals and improve outcomes for the Fonasa A women who remain. Public hospitals facing new competitive pressure could also raise quality, which would again improve Fonasa A outcomes. The first channel contaminates our Isapre comparison group and the other two contaminate our Fonasa A comparison group, so in each case the difference between insurance groups would no longer isolate the change in hospital.

The same congestion also reaches the women the reform moved. A woman who switched delivered at a hospital the reform itself had made more congested, and Chilean

[24] These assumptions follow the theoretical framework outlined in Miyaji (2026).

[25] Although moving into Fonasa is relatively easy, returning to an Isapre is costly and uncertain—insurers can reject applicants or deny coverage for pre-existing conditions—so temporary or strategic switching is unlikely.

private hospitals use scheduled cesareans to manage capacity (de Elejalde and Giolito, 2021), so the added volume can change her care. Our estimate would then combine the effect of delivering at a private hospital with the effect of congestion there.

Several features of the reallocation lead us to expect these effects to be second order. The same flow of deliveries is a large proportional shock to private maternity wards and a small one to public ones, so congestion is primarily a private-sector question. On the public side the scope for a strategic response is limited: physicians are salaried and public hospital managers face low-powered performance incentives (Muñoz and Otero, 2025). On the private side the shock fell only on obstetric wards, and the same scheduling gives them room to spread the additional volume across the calendar rather than queue it. Section 5.3 and Online Appendix C find evidence consistent with these arguments.

Exclusion restriction:
The instruments—interactions between insurance type and a post-reform indicator—should affect outcomes only through their effect on the choice of delivery hospital. Several changes around the reform could break this: other policies moving at the same time, other parts of the same decree, and the reallocation of deliveries the reform produced. The reallocation is the SUTVA question treated above; we take up the other two here.

Two other reforms fell near our window: (i) the 2005 regulation limiting Isapres' ability to adjust premiums (implemented in 2006), and (ii) the introduction of the AUGE/GES program in July 2005, which expanded coverage for a set of prioritized health conditions, including preterm birth. Although both policies could affect maternal health in general, there is little reason to expect differential effects across insurance groups. To be conservative, we nonetheless restrict the sample to births between 2002 and 2005, which ends before the premium regulation took effect and leaves only the final months exposed to AUGE/GES.

Another concern is that PAD copayment reductions may have applied to other procedures affecting maternal health. In practice, the reform was narrowly targeted: beyond deliveries, it only extended MAI subsidies to hemodialysis and certain emergency care services,[26] which are unlikely to matter for first-time mothers.

Monotonicity:
The expansion of PAD coverage should weakly increase the probability of delivering in a private hospital for the affected groups. The reform cut the patient's copayment from 60 to 25 percent of the PAD bundle, so the price of private delivery moved the same way for every woman in Fonasa B, C, and D. A defier requires a channel pushing against that price,

[26]Decreto Supremo N ° 48 (MINSAL, 11 February 2003) raised the State subsidy for PAD delivery from 40% to 75%, added hemodialysis to the MAI fee schedule, and added two emergency care codes. It modified no other PAD diagnosis.

and the reallocation supplies one: the same flows added congestion in private wards and relieved it in public ones, so relative quality moved opposite to relative price. Our first stages are net flows and cannot by themselves rule this out. Two checks make a violation unlikely. First, the first stage is positive within every subgroup defined by predetermined characteristics, including the women with the least access to private hospitals — the testable implication of the no-defiers condition (Imbens and Angrist, 1994) — as reported in Online Appendix Figure D.7. Second, the congestion and competition evidence in Section 5.3 finds little evidence of a change in the quality of care on either side of the market.

No anticipation:
Individuals should not adjust their choice of delivery hospital before the reform. Because PAD copayment reductions applied only after implementation and women could not benefit in advance, there was no incentive to change behavior beforehand.

Parallel trends:
In the absence of the reform, trends in the probability of delivering at a private hospital would have been similar across insurance groups. To address this, we estimate differences in pre-reform trends in private hospital deliveries among mothers with different insurance types.

Likewise, in the absence of the reform, C-section rates and other maternal health outcomes would have followed similar trends across insurance groups. We estimate the same pre-reform trend differences for these outcomes.

Relevance:
Finally, the reform must increase the probability of delivering at a private hospital for women enrolled in Fonasa B, C, and D relative to women in Fonasa A and Isapre. We test this condition using the first-stage regression to verify that the instrument has a statistically significant effect on treatment status.

Before we estimate equation (1), we assess whether the data support our identification strategy.

First, we test the relevance condition using the first stage estimates from equation (2). Table 3 presents these results. In our baseline specification (column 1), we find that women in Fonasa Group D—the highest income group within Fonasa—are 28 percentage points more likely to deliver in private hospitals after the reform, compared to increases of 12 percentage points for Group C and 3.1 percentage points for Group B.

These estimates are robust to sequentially adding age-year fixed effects (column 2), municipality-age and municipality-year fixed effects (column 3), and a linear trend for Fonasa Group A (column 4). To formally test for relevance, we compute the effective F-statistic

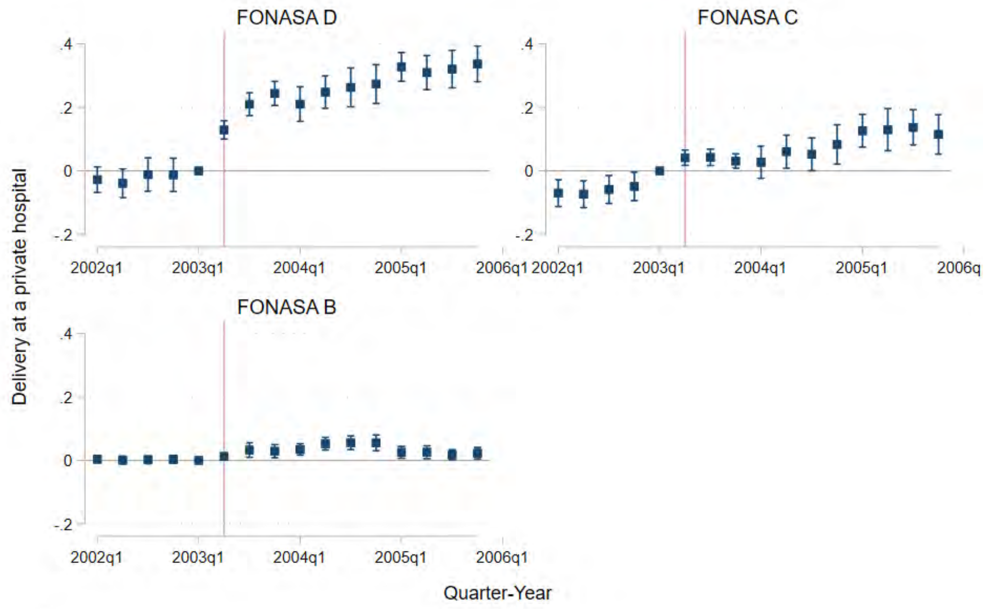


Figure 3: Dynamic effects of the policy on the probability of delivery in a private hospital

Note: This figure plots estimated coefficients from equation (3), where each point represents the interaction between quarter dummies and insurance group status (Fonasa B, C, or D), relative to the reference period (the quarter preceding the PAD reform). The dependent variable is an indicator for delivery in a private hospital. Vertical lines represent 95% confidence intervals. Fonasa A and Isapre beneficiaries serve as the comparison group. Standard errors are clustered at the municipality of residence.

proposed by Montiel Olea and Pflueger (2013) and reject the null of weak instruments at the 5% significance level.[27]

Second, we test the parallel trends assumption by checking for pre-treatment differences in trends for both treatment and outcomes. To this end, we estimate the following equation:

$$\begin{aligned} Y_{it} \quad = \quad & \gamma_0 + \textstyle\sum_{\tau=1}^{T} \gamma_{1,\tau} Fonasa\ D_{it} \times \mathbb{1}(t=\tau) + \sum_{\tau=1}^{T} \gamma_{2,\tau}\ Fonasa\ C_{it} \times \mathbb{1}(t=\tau) \\ & + \textstyle\sum_{\tau=1}^{T} \gamma_{3,\tau} Fonasa\ B_{it} \times \mathbb{1}(t=\tau) + \gamma_4\ Fonasa\ A_{it} + \gamma_5\ Fonasa\ B_{it} \\ & + \gamma_6\ Fonasa\ C_{it} + \gamma_7\ Fonasa\ D_{it} + \tau_t + \alpha_{a(i)} + \eta_{it}, \end{aligned} \tag{3}$$

where each $\gamma_{j,\tau}$ is the coefficient of the interaction between a quarter dummy and Fonasa group status, with the quarter prior to the reform as the reference period. Figure 3 plots the time-varying coefficients from equation (3) for the probability of delivering at a private hospital (our treatment variable). The results show no evidence of differential pre-trends

[27]The effective F-statistic, proposed by Montiel Olea and Pflueger (2013), is a statistic used to test for weak instruments in instrumental variable models under heteroskedasticity or clustered errors. For our application, with three instruments and standard errors clustered at the municipality of residence, a critical value of 30.4 corresponds to a 5% maximal asymptotic relative bias of the 2SLS estimator in the worst-case direction.

between women in Fonasa groups B, C or D and those in Fonasa A or Isapre.

The IV identifies an effect for the compliers, the women the reform moved from public to private hospitals, and they need not be representative of Fonasa groups B, C, and D. With heterogeneous treatment effects this is not a source of bias; it defines the population whose effect the IV identifies. It does bear on how widely the estimates generalize, so we characterize the compliers directly.

Following Abadie (2003), for a predetermined characteristic X we compare the mean of X interacted with the private-hospital indicator within Fonasa groups B, C, and D before and after the reform, and divide the change by the change in the private-delivery share. The comparison is taken entirely within the eligible groups and uses no control group. Women who never deliver privately contribute nothing to the interaction; women who would deliver privately either way contribute their own X before and after the reform, so they difference out; the ratio is therefore the mean of X among the compliers. The only contamination it admits is a change in X across cohorts of the women already delivering privately; the table notes bound it.

Table 4 reports the results alongside three reference groups within Fonasa groups B, C, and D: women who delivered privately before the reform, the full pool, and women who delivered publicly after it. Most characteristics come from the birth certificate, which records the mother's employment status along with gestational age and birth weight; we count a birth as preterm below 37 weeks and as low weight below 2,500 grams. Obstetric hospitalization during pregnancy comes instead from the discharge records, and counts any obstetric admission in the 280 days before delivery.

On socioeconomic characteristics the compliers resemble the pre-reform private group more than the pool, with 13.2 years of education against 13.5 and 11.8 respectively, and 66 percent in the labor force against 68 and 46. They are younger than the private group (25.4 against 26.2 years) and older than the pool (23.4). On clinical risk, preterm birth (2.8 percent) and low birth weight (3.0 percent) are at the private group's level (3.1 and 2.7), half the pool's (6.3 and 5.3). Pre-delivery obstetric hospitalization is 10.4 percent, above the private group's 5.4 but below the pool's 14.2. PAD eligibility requires a certified low-risk, term pregnancy, so the reform drew women at lower risk than those who stayed. Selection on risk therefore runs in the conservative direction for the adverse effects we estimate. Our estimates apply most directly to the lower-risk end of Fonasa groups B, C, and D, the population that a further extension of private-provider access would be most likely to reach.

5 Results

5.1 Short-term outcomes

Table 5 presents estimates of the effect of delivering at a private hospital on short-term outcomes, including cesarean delivery (Panel A), length of hospital stay (Panel B), hospital stays of seven days or more (Panel C), and 30-day readmissions (Panel D). Column (1) reports OLS estimates, while columns (2) to (5) report IV estimates with increasingly rich sets of controls. The baseline IV specification in Column (2) includes month-year of birth and maternal age fixed effects. Column (3) adds interactions between maternal age and year of birth. Column (4) further adds municipality-by-year and municipality-by-age interactions. Column (5) further includes a linear time trend interacted with a dummy for enrollment in Fonasa Group A, the lowest-income group.

Delivering in a private hospital raises the probability of a cesarean section at first birth by 44.6 percentage points (Panel A, Column 2), from a sample mean of 34 percent—consistent with our earlier work (de Elejalde and Giolito, 2021).

The IV estimates for cesarean delivery are larger than the OLS estimates. This is consistent with the interpretation that our estimates capture a local average treatment effect for women in Fonasa groups B, C, and D—the middle- and higher-income brackets of public insurance—who switched from public to private hospitals in response to the copayment reduction. These women, who tend to have lower income and education than typical private hospital users, may be more susceptible to physician-induced demand. An alternative explanation is that women who switch from public to private hospitals may have had stronger underlying preferences for elective C-sections that public providers were less willing to accommodate. However, existing survey evidence suggests that such differences in preferences are unlikely to explain the gap. A third reading follows from the complier characterization in Section 4. The women the reform moved had low-risk, term pregnancies, so few of them would have met a medical indication for cesarean in a public hospital.[28]

The magnitude of our estimates aligns with differences in C-section rates across hospital types. In our sample, the cesarean rate is 28% in public hospitals and 61% in private hospitals—a gap of 33 percentage points. For comparison, Card et al. (2023) report a 7 percentage point difference between high- and low-C-section hospitals in the U.S. and find that switching to a higher-rate hospital increases the probability of cesarean delivery by

[28] Angeja et al. (2006), using a survey of pregnant women in Santiago, show that both public and private patients overwhelmingly preferred vaginal over cesarean delivery—77% of public and 79% of private patients favored vaginal birth, while only 11% and 8%, respectively, preferred a cesarean—indicating no significant difference in preferences across hospital types. The survey cannot tell us who switched, but its authors read it as evidence that preferences are unlikely to be the main factor behind Chile's cesarean rates.

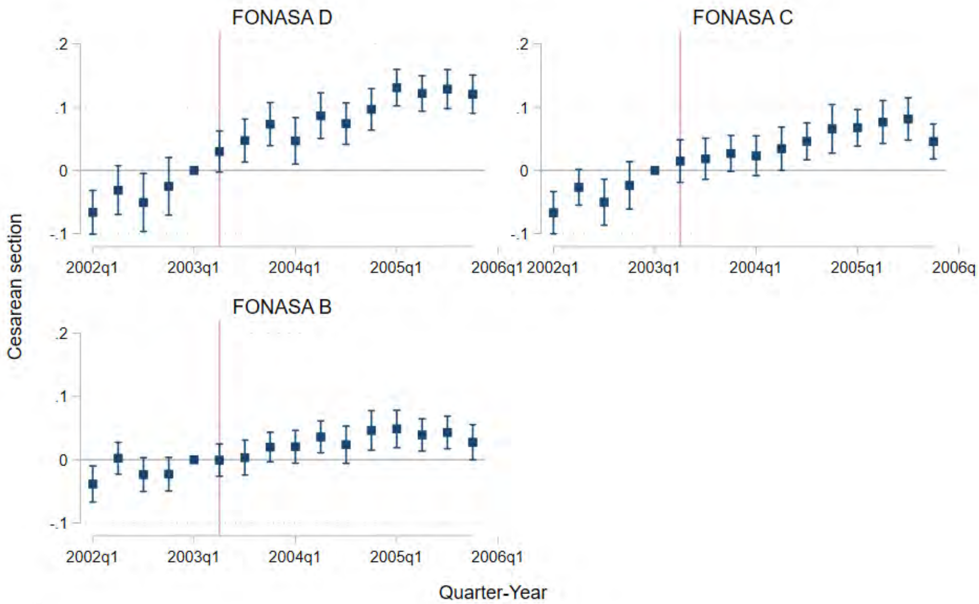


Figure 4: Dynamic effects of the policy on the probability of cesarean section at first birth

Note: This figure plots estimated coefficients from equation (3), where each point represents the interaction between quarter dummies and insurance group status (Fonasa B, C, or D), relative to the reference period (the quarter preceding the PAD reform). The dependent variable is an indicator for cesarean delivery at first birth. Vertical lines represent 95% confidence intervals. Fonasa A and Isapre beneficiaries serve as the comparison group. Standard errors are clustered at the municipality of residence.

11 percentage points. The larger baseline gap in our setting aligns with the larger estimated effect.

Private delivery also shortens the hospital stay. The average stay falls by 1.4 days (Panel B), roughly 40 percent of the sample mean of 3.6 days, and the share of stays lasting seven days or more falls by 9.4 percentage points (Panel C), from a mean of 5.9 percent. Two distinct margins drive this. One is the shorter interval between admission and delivery for a cesarean than for a vaginal birth: a scheduled cesarean does not wait for labor to begin (Card et al., 2023), and the stay piles up at three-day length—the probability of exactly a three-day stay rises by 21 percentage points, while longer stays become less common (see Appendix Table D.4 for length of stay at the five-, seven- and ten-day cutoffs). Splitting the stay at the delivery confirms this: the admission-to-birth segment falls by 0.93 days and the birth-to-discharge segment by 0.63 (Online Appendix Table D.5), so both shorten but the admission side falls by more. The remainder is the shrinking upper tail: stays of seven days or more are the margin most informative about health, since prolonged stays reflect perioperative complications such as ileus (delayed return of bowel function), endometritis, and wound infection (Blumenfeld et al., 2015). Were a longer stay instead a sign of closer monitoring of

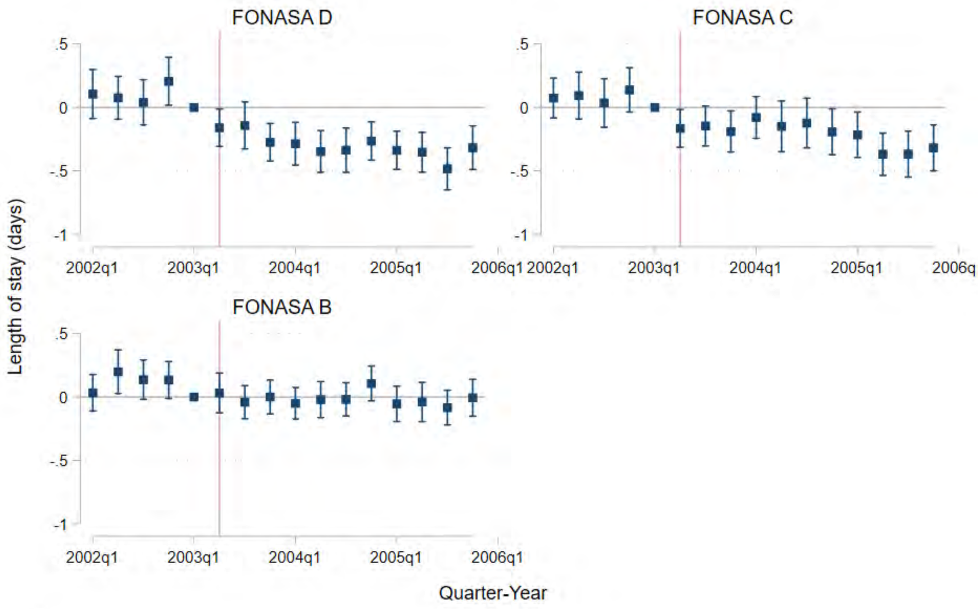


Figure 5: Dynamic effects of the policy on length of hospital stay

Note: This figure plots estimated coefficients from equation (3), where each point represents the interaction between quarter dummies and insurance group status (Fonasa B, C, or D), relative to the reference period (the quarter preceding the PAD reform). The dependent variable is the length of the hospital stay, in days. Vertical lines represent 95% confidence intervals. Fonasa A and Isapre beneficiaries serve as the comparison group. Standard errors are clustered at the municipality of residence.

complex patients, the women discharged sooner would fare worse; the readmission results in the next paragraphs do not show that.

Readmissions within 30 days fall by 2.5 percentage points (Panel D of Table 5), from a mean of 2.5 percent. A readmission also measures the use of health services, not health alone: it records a contact with the hospital system, and two sectors may differ in how readily they admit a woman who presents with the same condition. Reading the decline as an improvement in health therefore requires knowing what the readmissions were for. To interpret this decline, Table 6 decomposes 30-day readmissions by diagnosis. Readmission diagnoses are more informative than the record of the delivery itself, which carries a delivery code whatever complication accompanied it: about 70 percent of first-birth stays of seven days or more still record O80–O84 as the principal diagnosis. A readmission, by contrast, is coded by the condition that prompted it. We group them into the four categories that account for most readmissions: puerperal infections, obstetric wound complications, complications of procedures, and postpartum hemorrhage. The decline is concentrated in wound complications (Panel B), and the perineal-wound subset (Panel C) accounts for almost all of it. These can be explained by the change in the mode of delivery: private delivery substitutes cesarean

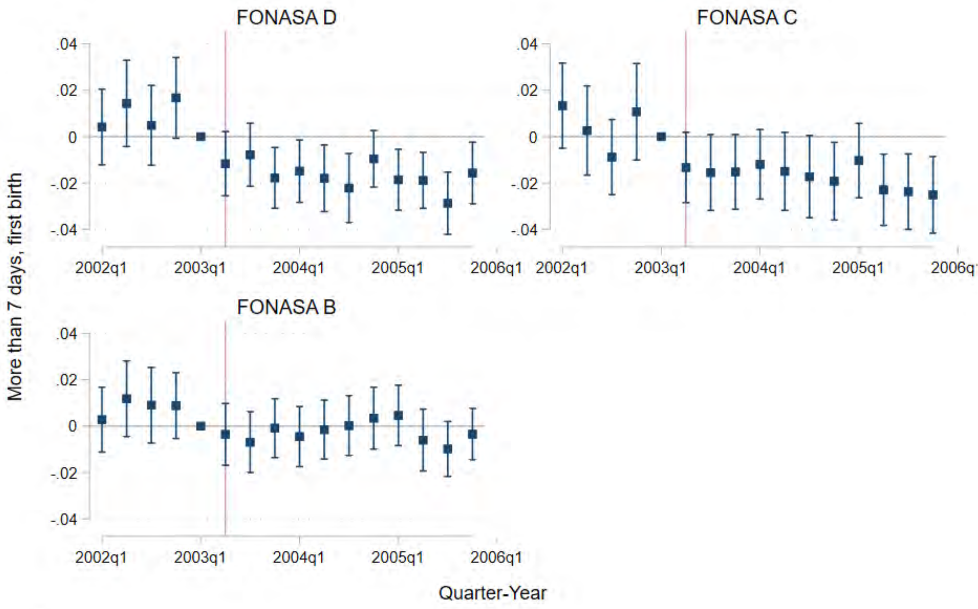


Figure 6: Dynamic effects of the policy on the probability of prolonged hospitalization

Note: This figure plots estimated coefficients from equation (3), where each point represents the interaction between quarter dummies and insurance group status (Fonasa B, C, or D), relative to the reference period (the quarter preceding the PAD reform). The dependent variable is an indicator for maternal hospitalization lasting seven days or more. Vertical lines represent 95% confidence intervals. Fonasa A and Isapre beneficiaries serve as the comparison group. Standard errors are clustered at the municipality of residence.

for vaginal birth, and fewer vaginal births mean fewer perineal lacerations and the infections that follow them. The perineal channel alone accounts for about 22 percent of the total readmission decline.

At the same time, nothing rises on the cesarean side: puerperal infections, procedure-related complications and postpartum hemorrhage all show no significant increase, even though cesarean section is a documented risk factor for postpartum infection (Sandall et al., 2018) and hemorrhage (Yunas et al., 2025). Within the first 30 days, then, the shift to private delivery lowers readmissions, in part through the fall in the diagnoses tied to the change in the mode of delivery, with no offsetting rise on the cesarean side. The complications specific to cesarean delivery do not appear at this horizon; they are deferred, surfacing over the longer horizon we study below.

The effect on stays of seven days or more raises a concern about survivor composition: if the reform moved women toward hospitals where more of them died, the survivors would look healthier without any improvement in care. Maternal mortality, measured from linked death records, shows no response to the reform (Online Appendix D.11).

Figures 4 through 7 plot dynamic reduced-form estimates of the policy's effects on

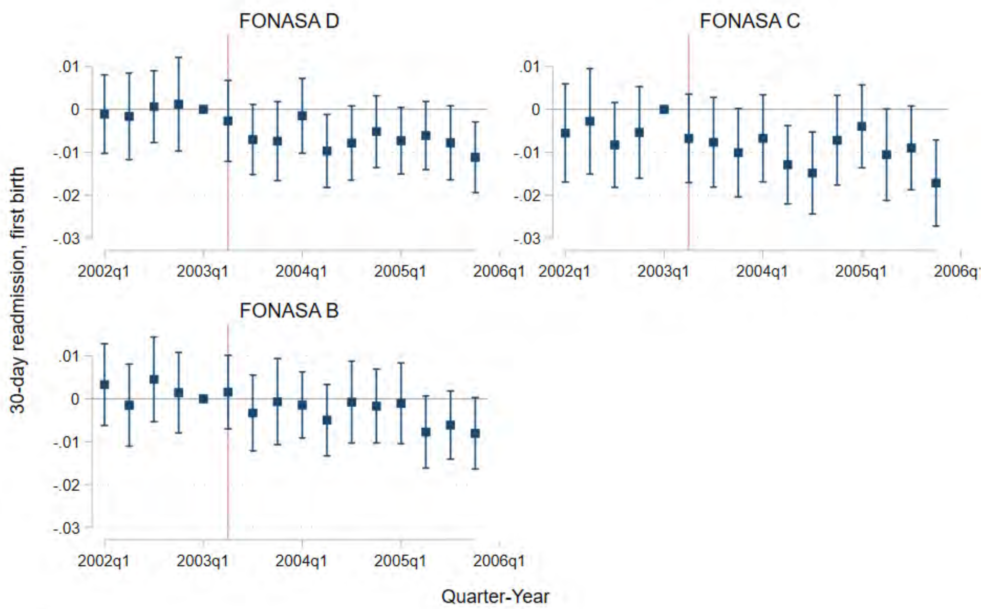


Figure 7: Dynamic effects of the policy on the probability of 30-day readmission

Note: This figure plots estimated coefficients from equation (3), where each point represents the interaction between quarter dummies and insurance group status (Fonasa B, C, or D), relative to the reference period (the quarter preceding the PAD reform). The dependent variable is an indicator for maternal readmission within 30 days of delivery. Vertical lines represent 95% confidence intervals. Fonasa A and Isapre beneficiaries serve as the comparison group. Standard errors are clustered at the municipality of residence.

short-term outcomes using quarter-level interactions with insurance group. Across all four outcomes—cesarean delivery (Figure 4), length of stay (Figure 5), prolonged hospitalization (Figure 6), and 30-day readmission (Figure 7)—we find no evidence of differential pre-trends between Fonasa groups B, C, and D and the comparison groups (Fonasa A and Isapre). Estimated coefficients in the pre-reform period are small and statistically insignificant, supporting the plausibility of the parallel trends assumption.

Following the reform, the timing and magnitude of effects track the increase in private hospital deliveries shown in Figure 3. Among Fonasa D beneficiaries, deliveries in private hospitals rise beginning in Q2 of 2003, followed by sharp increases in Q3 and again in 2004. C-section rates for this group exhibit a nearly identical pattern, suggesting a tight behavioral response. For groups C and B, take-up of private hospital delivery intensifies in 2004, coinciding with increases in C-section rates. This temporal alignment strengthens the case for a causal interpretation of our estimates.

For length of stay, prolonged hospitalization, and 30-day readmissions, the dynamic estimates show negative effects concentrated after the reform, particularly for groups D and C—the groups with the largest exposure to the reform.

If the fall in readmissions reflected different clinical practice in the two sectors, we would expect the difference to persist. To see whether it does, we follow the same women over a longer horizon.

Figure 8 reports the same IV specification estimated on the probability of an admission in each of six consecutive windows after discharge from the first birth: 0–30 days, 31–60, 61–90, 91–180, 181 days to one year, and one to three years. The windows do not overlap, so each estimate refers only to admissions beginning in that interval, and the exercise is repeated separately for admissions to public and to private hospitals. Admissions for an uncomplicated delivery are excluded, so a later birth is not itself counted as a hospitalization.

In the first thirty days the fall sits entirely on the public side: admissions there drop by 2.8 percentage points, while admissions to private hospitals are unchanged. Between one and three years, however, admissions rise by 4.9 percentage points overall, entirely at private hospitals (+8.5 percentage points), while public admissions keep falling. The next subsection takes up what brings these women back: repeat cesarean delivery and complications of the cesarean scar.

5.2 Long-term outcomes

Figure 8 showed that the women who switched to private-hospital delivery return to hospital one to three years later, almost entirely at private hospitals—the horizon at which subsequent pregnancies arrive. The reason is obstetric: a cesarean leaves a permanent uterine scar,

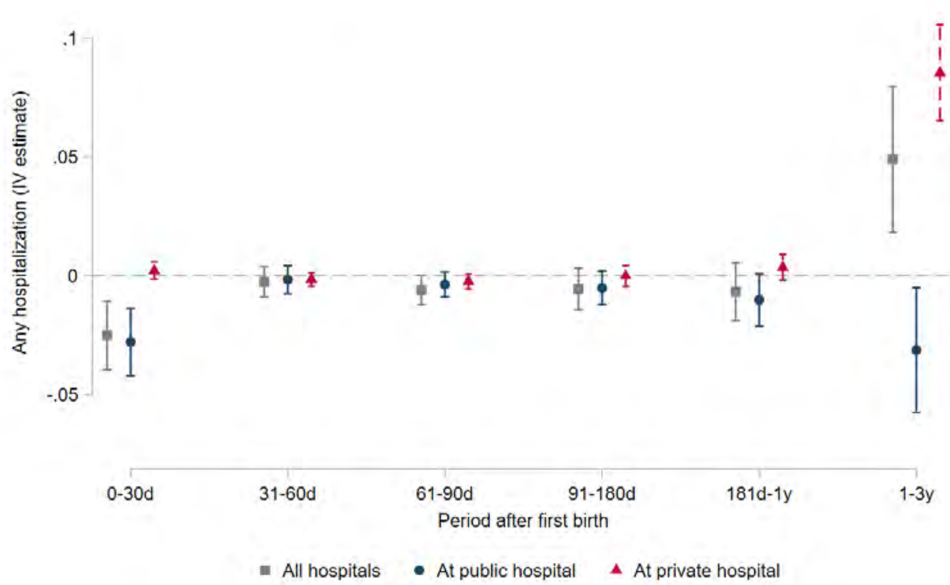


Figure 8: Effect of private-hospital delivery on subsequent hospitalization, by horizon and hospital type

Note: This figure plots IV estimates of the effect of delivering at a private hospital on the probability of any hospitalization within each window after the first birth, estimated separately for admissions to all hospitals, to public hospitals, and to private hospitals. Uncomplicated delivery codes (ICD-10 O80–O84) are excluded. The instrument is the interaction of Fonasa group with the post-reform indicator, and the specification matches the baseline IV (month-year of birth and maternal age fixed effects, standard errors clustered at the municipality of residence). Vertical lines represent 95% confidence intervals.

and a scarred uterus carries added risk in every subsequent pregnancy and delivery. Each subsequent cesarean is performed over the same scar, and the obstetric literature traces the complications that this accumulation produces in later pregnancies. We follow four outcomes over the years after the first birth: repeat cesarean sections, hospitalization for cesarean-scar complications, hysterectomy, and whether any of this changes subsequent fertility. We read the first three as successive steps along that sequence, and each subsection below asks whether the estimates appear where the sequence implies they should.

Repeat C-sections

Table 7 presents OLS and IV estimates of the effect of delivering at a private hospital on the probability of a cesarean section at the first birth (Panel A), the second birth (Panel B), and repeat cesarean sections (Panel C). This sample is restricted to women with at least two births within twelve years of the first.

The results show substantial and persistent effects of private hospital delivery on cesarean use. In Column (2), delivering at a private hospital increases the probability of a cesarean at first birth by 46 percentage points, at second birth by 35 percentage points, and the probability of a repeat C-section (i.e., cesareans in both births) by 48 percentage points. These findings imply strong path dependence: private hospital exposure at first birth substantially increases the likelihood of subsequent cesarean delivery.

Figure 9 plots dynamic reduced-form estimates of the policy's effect on repeat C-sections, using quarter-level interactions by insurance group. The timing of effects aligns closely with the rise in first-birth cesarean rates shown in Figure 4, with initial increases for Fonasa Group D in early 2003, followed by groups C and B by 2004. This pattern is consistent with the interpretation that increases in repeat C-sections are driven by earlier shifts in first-birth cesarean delivery associated with expanded access to private hospitals.

The IV effect on repeat cesarean has a plausible clinical explanation. Among women whose first birth was a cesarean and who have a second observed birth, the rate of vaginal delivery after cesarean (VBAC) is 0.28 at public hospitals versus 0.08 at private hospitals. This gap reflects prevailing clinical practice.[29] Online Appendix Table D.3 reports the tabulation.

Hospitalization due to cesarean scar complications

Cesarean scar complications are recorded when a woman is admitted, after her first birth, with dehiscence, niche, adhesions, or related disruption of a prior uterine scar. As reported

[29] At a university hospital in Santiago, the repeat-cesarean rate among multiparas with a prior CS rose from 70% to 81% between 1997–2002 and 2003–2005 (Salinas et al., 2007).

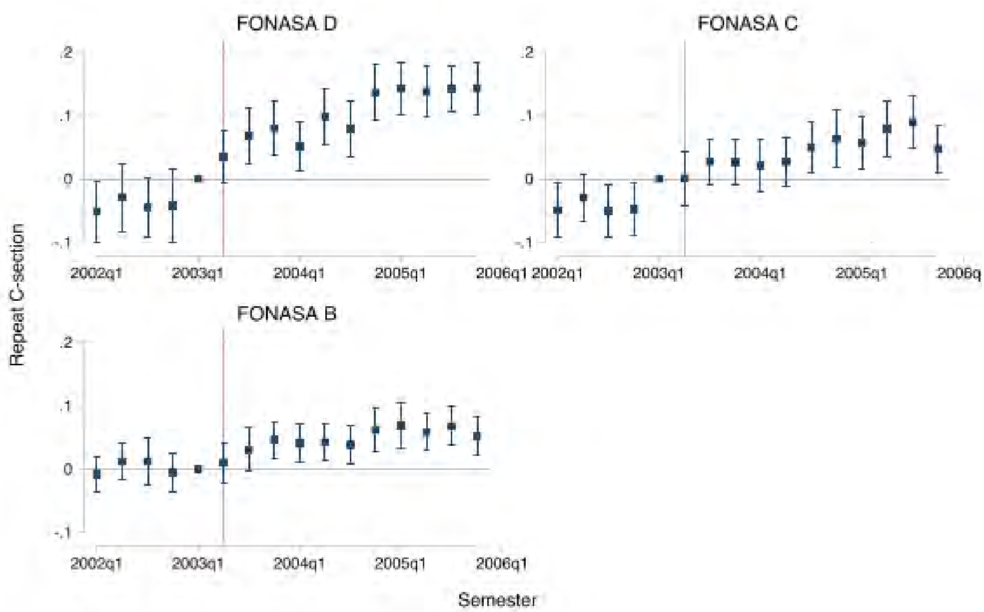


Figure 9: Dynamic effects of the policy on the probability of repeat cesarean sections

Note: This figure plots estimated coefficients from equation (3), where each point represents the interaction between quarter dummies and insurance group status (Fonasa B, C, or D), relative to the reference period (the quarter preceding the PAD reform). The dependent variable is an indicator for C-sections in both the first and second births. Vertical lines represent 95% confidence intervals. Fonasa A and Isapre beneficiaries serve as the comparison group. Standard errors are clustered at the municipality of residence.

in Table 1, 1.4 percent of women in our sample experience such an admission within three years of the first birth.

Table 8 reports OLS and IV estimates of cesarean scar hospitalization within three years of the first birth. Delivery at a private hospital increases the probability of such an admission by 5.7 to 6.8 percentage points across columns (2) to (5).

Figure 10 plots IV estimates of the effect of delivering at a private hospital on the probability of hospitalization due to cesarean scar complications at horizons from 3 to 15 years after the first birth. Each bar is a point estimate with its confidence interval from a separate IV regression. The estimated effect rises from 5.7 percentage points at year 3 to 10.9 percentage points by year 12, after which it stabilizes. The horizon profile is consistent with cumulative scar exposure as women under the private-sector pathway face higher rates of repeat cesarean at subsequent births (previous subsection).

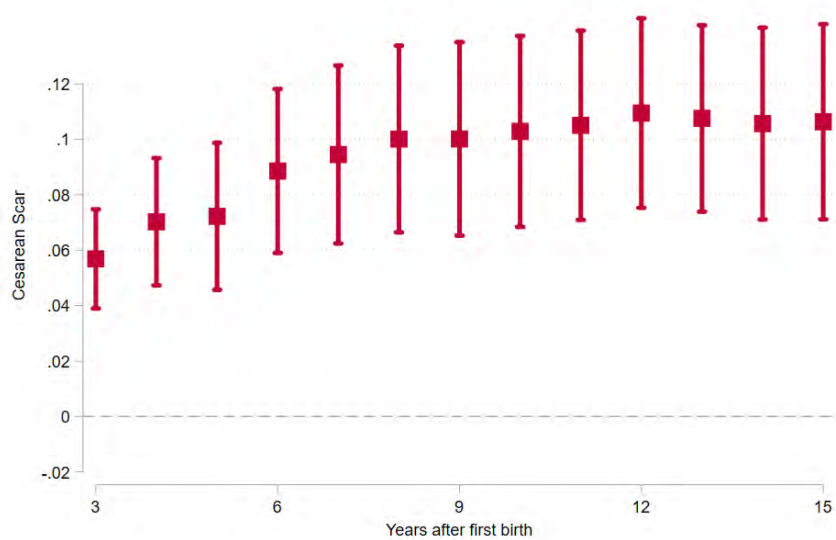


Figure 10: IV estimates of hospitalization due to cesarean scar complications at different horizons (3–15 years) after first birth

Note: This figure plots the effect of delivering at a private hospital on the probability of hospitalization due to cesarean scar complications for different horizons after first birth. Each estimate is from a separate IV regression using insurance group as an instrument. All regressions include month-year of birth fixed effects and maternal age fixed effects. Standard errors are clustered at the municipality of residence.

Because a scar complication requires a prior uterine scar, the effect has two parts. Delivering privately raises the probability of a cesarean at first birth by 44.6 percentage points (Table 5), which enlarges the population in which a scar complication can occur at all. We decompose the total effect into the part that comes from the higher probability of a cesarean, holding the probability of a scar hospitalization within each mode of delivery constant (the extensive margin), and the part that comes from changes in that probability, holding the cesarean rate constant (the intensive margin). At private hospitals the scar rate is 7.0 percent after a cesarean and 1.0 percent after a vaginal delivery, so the mechanical channel (extensive margin) accounts for about 47 percent of the three-year effect. Online Appendix D.3 derives the decomposition.

Figure 11 plots the dynamic reduced form estimates of the probability of hospitalization due to cesarean scar complications 3 years after the first birth. The estimates show no evidence of differential pre-trends across insurance groups prior to the reform. Following the reform, however, there is a clear divergence between treated and comparison groups, consistent with the timing of increased private hospital use.

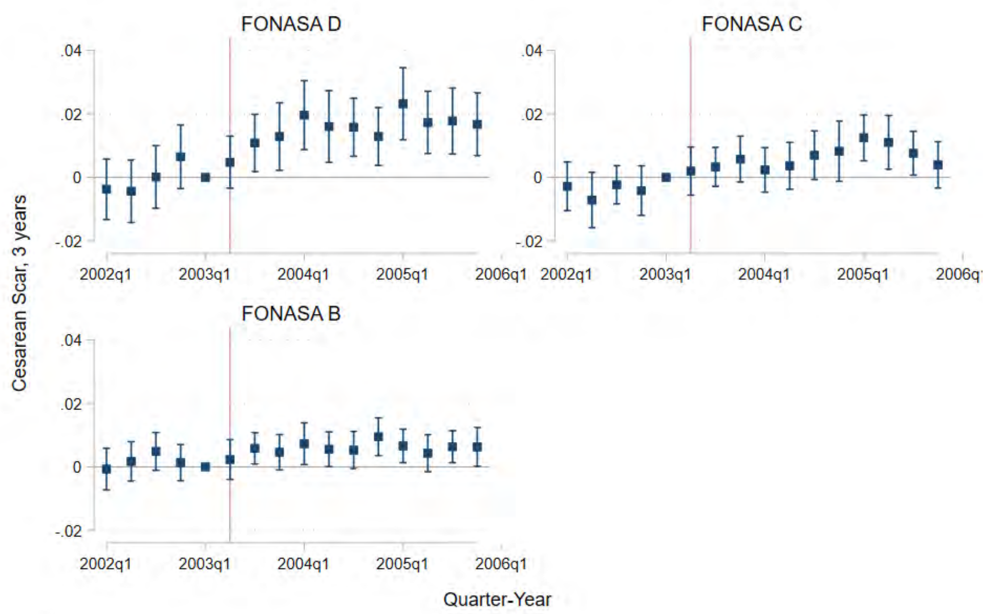


Figure 11: Dynamic effects of the policy on the probability of hospitalization due to cesarean scar complications 3 years after first birth

Note: This figure plots estimated coefficients from equation (3), where each point represents the interaction between quarter dummies and insurance group status (Fonasa B, C, or D), relative to the reference period (the quarter preceding the PAD reform). The dependent variable is an indicator for hospitalization due to cesarean scar complications 3 years after first birth. Vertical lines represent 95% confidence intervals. Fonasa A and Isapre beneficiaries serve as the comparison group. Standard errors are clustered at the municipality of residence.

Hysterectomy

Hysterectomy is the surgical removal of the uterus, a major intervention that ends fertility permanently. It is among the most severe non-fatal maternal outcomes the discharge records contain: the WHO near-miss criteria treat it as a marker of severe maternal morbidity (Say et al., 2009). Two broad categories cover virtually all cases. *Obstetric* hysterectomies are those whose principal diagnosis falls under pregnancy, childbirth, or the puerperium (ICD-10 chapter O); they are typically performed at delivery or during a subsequent pregnancy. In women who have had a prior cesarean, their leading cause is placenta accreta—a condition in which the placenta invades the uterine wall, typically at the cesarean scar, requiring removal of the uterus at a subsequent delivery—and their incidence rises sharply with each additional cesarean (Silver et al., 2006; Marshall et al., 2011; Clark and Silver, 2011). *Non-obstetric* hysterectomies are elective or semi-elective procedures performed outside pregnancy for gynecological conditions such as fibroids, uterine prolapse, abnormal bleeding, or oncological indications. The two categories differ sharply in who they affect and when. Non-obstetric procedures dominate at older ages and are largely unrelated to delivery mode; obstetric procedures concentrate at reproductive ages and, in women with a prior cesarean, are driven mainly by complications of subsequent pregnancies. Any causal effect of a first-birth cesarean on hysterectomy must therefore run through the obstetric channel. The mechanism carries three testable implications: the effect should fall on obstetric rather than non-obstetric hysterectomy, it should appear at young ages, and it should arise in subsequent pregnancies rather than at the first birth. We examine each in turn.

Figure 12 shows where hysterectomies sit in the age distribution, split by whether the principal diagnosis is obstetric. Obstetric cases are 5.6 percent of all hysterectomies in the discharge records but 46.7 percent of those performed at age 35 or younger; the procedures at older ages treat chiefly fibroids, along with prolapse, abnormal bleeding and cancer. A pooled outcome would therefore mix the obstetric channel, the only one through which delivery mode can operate, with cases unrelated to it. We bound the outcome at age ≤ 35 accordingly. Within the obstetric category the dominant code is O82.2, cesarean ending in hysterectomy, and its rate per 1,000 deliveries is negligible at first births and rises steeply with parity (Online Appendix Figure D.3), so any effect of a first cesarean must accumulate through later pregnancies rather than appear at the first birth.

Take the age implication first. The prediction is an IV effect that emerges at the ages where obstetric hysterectomies are common—the late 20s and early 30s—and adds little thereafter. Figure 13 reports IV estimates of the effect of private hospital delivery on hysterectomy at four age bounds—≤ 30, ≤ 35, ≤ 40, ≤ 45—within 15 years of the first birth. The coefficient is small and not significant at ≤ 30, rises sharply and becomes significant at ≤ 35, and stabilizes at higher caps. The pattern matches the prediction: events concentrate in the late 20s and early 30s, when these cohorts have their subsequent pregnancies, and additional follow-up at older ages adds little to the effect. We take ≤ 35 as the cap for the estimates we report below.

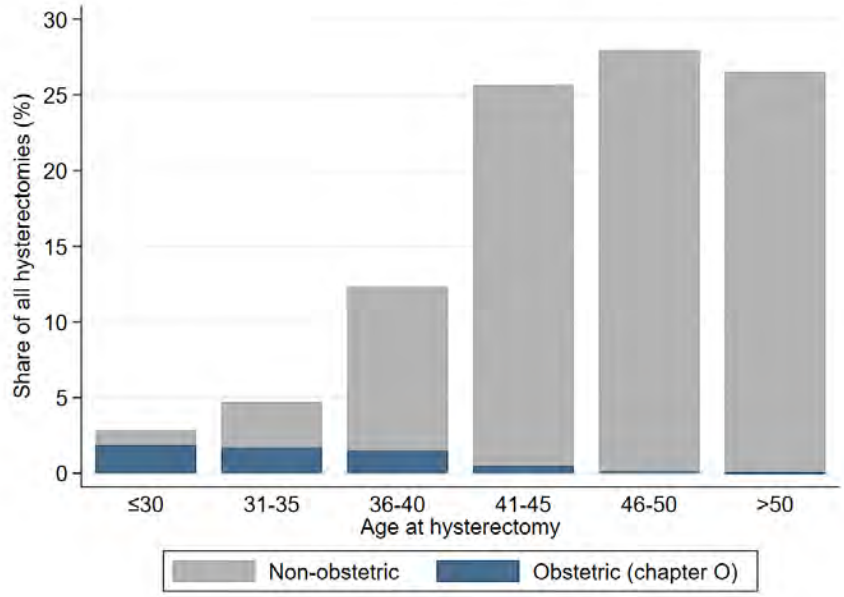


Figure 12: Age distribution of hysterectomies in Chilean hospital discharge records

Note: Distribution by five-year age bin of all hysterectomy episodes recorded in Egresos Hospitalarios (DEIS), 2004–2018, women aged 15–70 ($N = 224{,}171$). Episodes are identified using the codes listed in Online Appendix B. Obstetric episodes ($N = 12{,}532$): principal diagnosis in ICD-10 chapter O. Non-obstetric ($N = 211{,}639$): outside chapter O.

The second implication is that the effect should fall on the obstetric category. The age-bound evidence aggregates the obstetric and non-obstetric components. Table 9 reports OLS and IV estimates of the effect of private hospital delivery on hysterectomy before age 35 within 15 years of the first birth. Panel A reports the total effect; Panels B and C decompose it into the obstetric (ICD-10 chapter O) and non-obstetric components defined above. The total IV estimate is positive and significant; the decomposition allocates the bulk of the effect to the obstetric category, with no detectable effect on the non-obstetric one. The third implication also holds: the effect within the obstetric category is concentrated in subsequent pregnancies rather than at the first birth itself (Online Appendix Table D.7). The three predictions of the obstetric-channel mechanism—young ages, obstetric component, subsequent pregnancies—all hold, though hysterectomy is a rare outcome and each component is individually imprecise. We return to the mechanism in the Discussion.

A final check is whether the pooled IV effect reflects a discrete response to the reform rather than a pre-existing trend. Figure 14 shows the reduced-form event study for the total outcome. Pre-reform coefficients are close to zero across Fonasa groups B, C, and D, with no detectable trend. Post-reform, point estimates for Fonasa D are positive in most quarters, with two individually significant coefficients in 2004; for Fonasa B and C the quarterly pattern

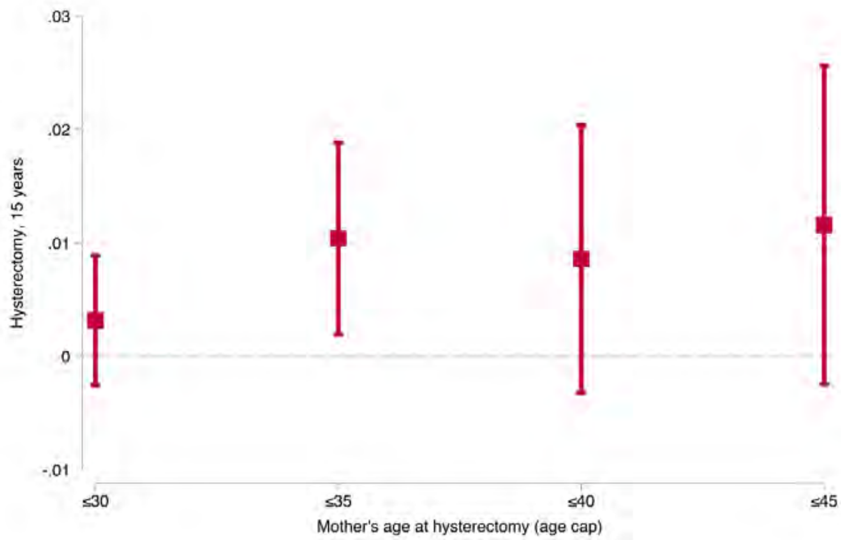


Figure 13: IV estimates of hysterectomy by age bound, 15-year window

Note: IV estimates by age bound $A \in \{30, 35, 40, 45\}$ within 15 years of first birth. Main estimation sample. Instruments: Fonasa B, C, and D dummies interacted with the post-reform indicator. Specification: month-year of birth and mother's age fixed effects. 95% confidence intervals from standard errors clustered at the municipality of residence.

is imprecisely estimated. At a sample mean of 0.7%, quarter-by-quarter precision is limited, and the pooled IV in Table 9 is the more informative summary.

Subsequent fertility

Table 10 presents OLS and IV estimates of the effect of delivering at a private hospital on the probability of having two or more children 6 years after the first birth (Panel A), the probability of having three or more children 9 years after the first birth (Panel B), and the cumulative number of children 12 years after the first birth (Panel C). Because our sample consists of first births, this last outcome equals one plus the number of subsequent births. We do not find a significant effect of delivery at a private hospital on any of the three outcomes in the baseline specification.

Figures 15a and 15b plot the IV estimates of the effect of delivering at a private hospital on subsequent fertility over a 1- to 12-year horizon following the first birth. Figure 15a shows that the estimates for having two children or more are statistically insignificant at every horizon except the longest, where the estimate is 0.052, significant at the 5 percent level. Figure 15b shows that the estimates for having three children or more are small and statistically insignificant across all time-horizons.[30] Figure 15c repeats this exercise for the

[30]Online Appendix Figures D.6a and D.6b show no evidence of differential pre-treatment trends in fertility

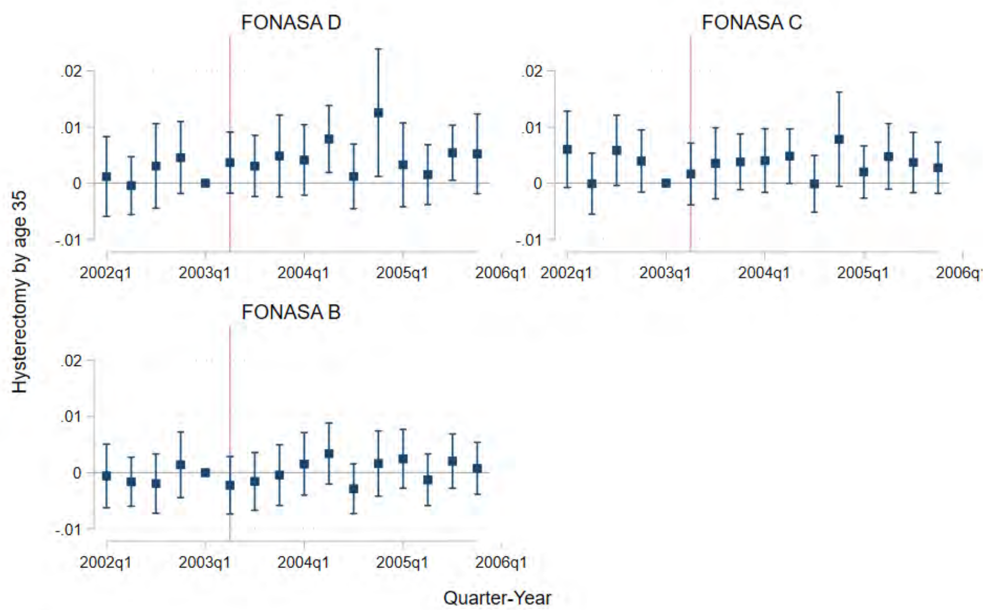


Figure 14: Dynamic effects on hysterectomy by age 35, by Fonasa group

Note: Reduced-form event-study coefficients on Fonasa $_j\times$ quarter for $j \in \{B, C, D\}$, with Fonasa A omitted and 2003q1 as the reference period. The red vertical line marks 2003q2, the first post-reform quarter. Specification: month-year of birth and mother's age fixed effects. Vertical bars are 95% confidence intervals from standard errors clustered at the municipality of residence.

number of children. The estimates remain small and statistically insignificant across the full horizon, confirming that the single-horizon results reported in Table 10 are not driven by the chosen window.

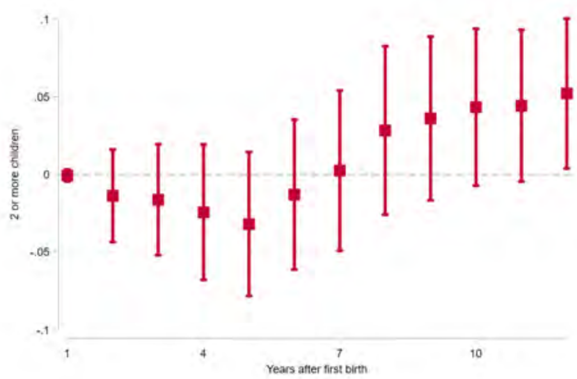
Despite the higher rates of repeat cesarean, scar complications, and hysterectomy documented above, we find no corresponding reduction in fertility. We return to this in the Discussion.

5.3 Robustness checks

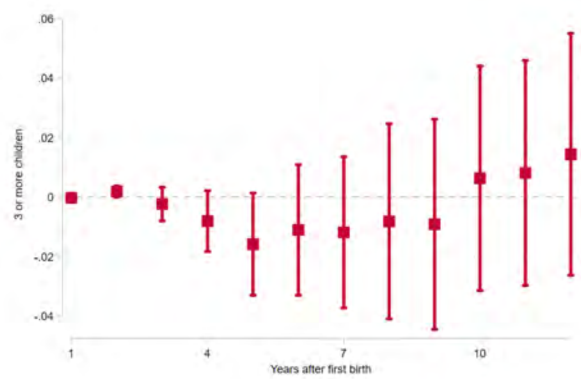
We run several exercises to validate our identification strategy and to check the robustness of our results.

First, the reform may have changed the care hospitals provided, not only which hospital a woman chose. The inflow of women in Fonasa groups B, C, and D could have congested private hospitals; the matching outflow could have relieved public ones and left them facing new competition. The reform would then affect outcomes beyond a woman's own choice of hospital, breaking the exclusion restriction. Three features of the setting lead us to expect these effects to be second order. Public maternity wards were much larger, so the same reallocation was a large proportional shock to private hospitals and a small one to public ones: after the reform deliveries at private hospitals stand about 16 percent above their pre-reform mean and deliveries at public hospitals about 6 percent below (Online Appendix C). The scope for a strategic response on the public side is in any case limited: physicians are salaried and public hospital managers face low-powered performance incentives (Muñoz and Otero, 2025). The private side absorbed the larger shock, but it fell only on obstetric wards, where Chilean private hospitals are known to use cesarean sections to schedule births and manage capacity (de Elejalde and Giolito, 2021), which leaves room to spread additional volume across the calendar rather than queue it. We use Fonasa A alone as the control group and drop the municipalities closest to a private hospital, where decongestion and any competitive response should be strongest (Appendix C.1). We use Isapre alone and restrict it to private hospitals with no recorded PAD deliveries, which the reform's inflow should not have reached (C.2). We then compare hospitals that absorbed more of the reallocation with those that absorbed less (C.3). We also relate outcomes to how busy a hospital was relative to its own busiest pre-reform months, and to how many other women delivered there in the same week (C.4).

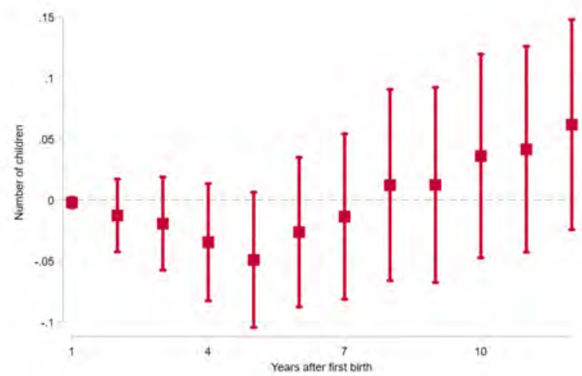
across insurance groups, supporting the validity of the identification strategy. In the post-reform period, the estimated effects on both outcomes—having two or more or three or more children— are statistically insignificant, reinforcing the conclusion that private hospital delivery does not significantly influence long-run fertility decisions.



(a) Two or more children



(b) Three or more children



(c) Number of children

Figure 15: IV estimates of subsequent fertility outcomes at different horizons (1–12 years) after first birth

Note: Each panel plots the effect of delivering at a private hospital on a subsequent fertility outcome for different horizons after the first birth: the probability of having two or more children (panel a), the probability of having three or more children (panel b), and the cumulative number of children (panel c). Each estimate is from a separate IV regression using insurance group as an instrument. All regressions include month-year of birth fixed effects and maternal age fixed effects. Standard errors are clustered at the municipality of residence.

Online Appendix C reports the estimates. Narrowing the comparison groups moves the point estimates but leaves the pattern of results unchanged. Across these designs we find little evidence of a change in care for the patients already being treated at either hospital type. The few significant coefficients do not even point the same way: an inflow of Fonasa B, C, and D patients is associated with shorter stays for Isapre women, while a higher within-hospital utilization ratio is associated with longer ones. The two exercises exploit different variation, so they need not agree, but the pattern is hard to square with a systematic congestion effect. Congestion effects are unlikely to be exactly zero in a market of shared, capacity-constrained hospitals, and we do not claim they are absent; we claim they are second order here.

Second, to assess whether our results could be driven by compositional changes in the sample, we estimate a series of placebo regressions using predetermined maternal characteristics as outcomes. These include maternal age, education, and employment status at the time of first birth.

Online Appendix Table D.8 reports OLS and IV estimates of the effect of delivering at a private hospital on maternal age. The OLS estimates show a positive correlation: women delivering in private hospitals tend to be older. However, this relationship disappears once we instrument for private hospital delivery using insurance type, regardless of the set of controls included.

Online Appendix Table D.9 presents similar placebo tests for maternal education and employment. In OLS, both characteristics are positively and significantly correlated with private hospital delivery. The IV estimates for education remain statistically significant in some specifications, but the effect loses significance once a linear trend in Fonasa A is included, indicating that the relationship is not robust. This attenuation may reflect underlying trends in education—particularly the rapid expansion in university enrollment during the study period, which could confound the association.[31] For maternal employment, the IV estimates show a non-significant effect.

Overall, these placebo results provide reassurance that the main findings are not driven by compositional changes in maternal characteristics, and support the validity of the identification strategy.

Third, we examine robustness to alternative definitions of the treatment and control groups. In one specification, we expand the treated group to include women with private insurance in addition to those in Fonasa B, C, or D. This addresses the concern that some women—particularly in group D—may have switched from private to public coverage

[31] According to official statistics from the Chilean Ministry of Education, average years of schooling among the labor force increased from 10.2 years in 2000 to 11.1 years in 2006 (Ministerio de Educación de Chile, Indicadores de la Educación 2006, p. 93, available at https://centroestudios.mineduc.cl/wp-content/uploads/sites/100/2017/06/IndicadoresdelaEducacion2006.pdf)

following the policy reform. If our results remain robust under this broader treatment definition, such compositional shifts are less likely to bias our baseline estimates. In a complementary approach, we restrict the sample to women enrolled in Fonasa A, B, or C, excluding group D altogether. This strategy offers an additional check against insurance switching into group D, at the cost of reduced statistical power due to lower private hospital take-up among groups B and C.

Online Appendix Tables D.10 to D.13 present IV estimates from two robustness exercises that use insurance type as an instrument, based on alternative definitions of the treatment and control groups. In each table, column (1) reports estimates from our benchmark treatment and control definition. Column (2) presents estimates from a specification that also includes the interaction between private insurance and the post-treatment indicator as an additional instrument, thereby redefining both the treatment group (Fonasa B, C, and D, plus private insurance) and the control group (Fonasa A). Columns (3) and (4) report results from a second exercise that restricts the sample to women enrolled in public insurance, using those in Fonasa A as the control group. Column (3) retains our benchmark instrument set, while column (4) excludes group D from the sample, using enrollment in groups B and C as instruments for delivery at a private hospital.

Overall, leaving aside fertility outcomes—which, as stated above, are generally insignificant—the significance of the results is robust across both exercises, although the magnitudes are less stable when private insurance is included as part of the treatment group. The exception is in Online Appendix Table D.12: the hysterectomy effect loses significance once private insurance is added to the treated group and when group D is excluded (panel B, columns 2 and 4).

Fourth, we use an alternative instrument that is more robust to compositional changes. Specifically, we construct the 2018 share of the population enrolled in Fonasa groups B, C, or D, calculated by age group and municipality of residence. We then estimate a difference-in-differences IV (DID-IV) model, using this group-level share as an instrument for private hospital delivery. This strategy rests on the assumption that individuals' age group and municipality of residence are stable over time—a credible condition, as these characteristics are unlikely to be directly affected by the reform.

Online Appendix Tables D.15 through D.19 report results from the alternative specification using the group-level Fonasa B, C, or D share as an instrument. Table D.15 presents first stage estimates and confirms that the instrument satisfies the relevance assumption, with strong and statistically significant effects on the probability of delivering at a private hospital. Online Appendix Tables D.16 through D.19 show the IV estimates with the Fonasa B, C, or D share as an instrument for our outcomes. The point estimates are generally larger in magnitude than those from our baseline specification, but are also less precise, reflecting the more limited variation available in the aggregated (age–municipality) instrument.[32]

Fifth, most of the identifying variation comes from Fonasa D. Online Appendix Table D.14 re-estimates the IV using Fonasa D $\times$ Post as the sole instrument. Across outcomes the estimates keep their sign and their significance and stay close to the baseline.

Sixth, our definition of mode of delivery is deliberately broad. For births that can be matched to a hospital discharge record, we classify the delivery using both the principal diagnosis code and whether a surgical procedure was performed (see Appendix Section B.1 for details). While several ICD-10 codes unambiguously identify the delivery mode, others are more ambiguous.

To verify that our results are not driven by these borderline categories, we construct a narrower definition of delivery mode. Following Jackson et al. (2012), we restrict classification to a subset of codes that clearly indicate cesarean or vaginal delivery, and we set the mode of delivery to missing for all remaining codes (Appendix Table D.20 describes this alternative definition). Table D.21 compares results obtained under both definitions. The estimates are very similar, indicating that our findings are not sensitive to the way we classify delivery mode.

Finally, to assess the sensitivity of our findings to functional form assumptions, we estimate an IV Probit model using a control function approach. Online Appendix Tables D.22 to D.25 report estimates from an IV Probit. In particular, column (3) of each table presents Probit estimates of equation (1), using as a control variable the residuals from the first stage equations.

The results are close to those obtained with the linear IV approach (column (2)), although the coefficients are generally smaller—except in the cases of prolonged hospitalization and 30-day readmission (Table D.22).

6 Discussion

The reform induces some women to deliver in private hospitals, where a cesarean is far more likely, and we interpret the results that follow as consequences of that change in the mode of delivery. Immediately after the birth, outcomes improve: prolonged stays and thirty-day readmissions are less frequent. The average stay also falls, in part because scheduled delivery shortens the time from admission to birth. The improvement is confined to complications of

[32] For example, column (3) of Panel B in Online Appendix Table D.19 shows a sizable negative effect on the probability of having a third child within 9 years. However, the effect becomes statistically insignificant once age-year interactions are included, as shown in column (4).

vaginal birth, mainly perineal wounds; the complications specific to cesarean do not rise to offset them. The cesarean's own risks are deferred to later pregnancies. The early decline in hospital admissions is not a substitution across providers: admissions to public hospitals fall and are not compensated by increases in admissions to private hospitals. One to three years later the same women are more likely to return to hospital (Figure 8).

Repeat cesarean links the short-run and long-run findings. A first cesarean creates a uterine scar, and Chilean obstetric practice during this period treated a prior scar as an indication for a second cesarean rather than as a case for attempting vaginal birth. The practice was strongest at private hospitals: the VBAC rate there was 8 percent, against 28 percent at public hospitals, so a first cesarean at a private hospital was even more likely to be followed by another. What the instrument identifies is therefore entry into a recurring procedure rather than a single operation, and the long-run risks operate through the repetitions. These are effects for the low-risk women the reform moved into private hospitals (Section 4).

The cesarean-scar effect has two parts. A first cesarean creates the scar, so part of the effect is mechanical: private delivery increases the number of women who carry a scar into subsequent pregnancies. The decomposition in Section 5.2 and Online Appendix D.3 shows that this mechanical part accounts for about half of the effect, with most of the remainder coming from a higher complication rate within each mode of delivery. We read that residual as repeated exposure: with VBAC uncommon at private hospitals, a first cesarean there leads to systematic repeat cesareans, and each cesarean over the same scar adds risk. The effect grows over the horizon as later pregnancies arrive.

Hysterectomy extends the same process. Accumulated scarring raises the risk of placenta accreta and uterine rupture, and a fraction of these end in hysterectomy (Marshall et al., 2011; Clark and Silver, 2011; Silver et al., 2006). Our estimates place the effect where that pathway predicts: in the obstetric component, not in the non-obstetric one, and at the pregnancies that follow the first birth.

Alternative explanations do not fit the pattern of the estimates. More intensive surgical practice at private hospitals, or better access producing more diagnoses, would show up in the non-obstetric category. A general move toward private elective care after the reform would spread procedures over the whole follow-up rather than concentrate them at later pregnancies. Nor can the effect be a copayment-driven move of existing procedures into the private sector: the reform left the cost of a hysterectomy at a private provider unchanged for women in Fonasa groups B, C, and D. A provider-driven reading—a private hospital adding a hysterectomy to a later cesarean—fits poorly as well: peripartum hysterectomy permanently ends fertility and carries substantial surgical risk, and it is the standard response to severe bleeding or placental complications at delivery, circumstances in which the surgeon has little discretion. The dose-response in the medical literature—risk rising with each additional cesarean—also appears in health systems with very different reimbursement rules, which is hard to reconcile with an effect driven by the incentives of one type of provider. The estimate is imprecise and large relative to the base rate of hysterectomy before age 35, so we treat it as evidence on where the effect falls rather than on its magnitude.

We find no effect of private delivery on subsequent fertility. Halla et al. (2020) find a sizable reduction, but their estimate is for non-planned cesarean: they restrict the sample to births at 39 weeks or later to abstract from planned procedures. Our variation does not reach that margin, since the reform shifts women to private hospitals, where planned cesarean is the prevailing practice.[33]

The hysterectomy effect also comes too late in the birth sequence to reduce fertility. The pathway from cesarean to hysterectomy operates through accumulated scarring across subsequent pregnancies, and the decomposition locates the hysterectomy effect at later pregnancies rather than at the first birth. By the time the procedure occurs, most women have already completed the bulk of their childbearing, so hysterectomy ends childbearing that has already occurred rather than preventing later births. The long-run risks we document are therefore not offset by lower fertility; they accumulate over the same number of pregnancies.

The closest study to ours is Card et al. (2023), who use distance to high- versus low-cesarean hospitals in California to estimate the health effects of delivery practices. They find that mothers and infants delivered where cesarean rates are higher fare better at first: infants are born in better condition and are readmitted less often in the neonatal period, and mothers have fewer perineal lacerations. The offsetting cost in their data falls on infants, who make more emergency visits for respiratory problems over the following year. Our short-run maternal results match the benefit side of their finding, including the reduction in perineal injury. The difference is on the cost side. Their analysis centers on infant health and looks at mothers only around the time of delivery, so it does not capture the maternal cost we find, which is obstetric: a cesarean scar raises the risk of every later pregnancy, and the harm shows up as repeat cesareans, scar complications, and hysterectomy. We see that cost because we follow the mother, not the infant, and for fifteen years rather than one.

[33] Wilbert and Homme (2026), in a working paper that instruments the two margins separately, report a sizable reduction after unplanned cesarean and no significant effect after planned cesarean.

7 Conclusion

In this paper we exploit a 2003 copayment reduction that increased access to private hospitals for women in Fonasa groups B, C, and D as an instrument for private delivery. The reform generated a sizable shift from public to private maternity care, providing plausibly exogenous variation in hospital choice. Using linked administrative data and an IV difference-in-differences design, we estimate the causal effect of private hospital delivery on maternal outcomes up to fifteen years after the first birth.

Women induced to deliver in private hospitals have better short-term outcomes: fewer prolonged hospitalizations and lower rates of readmission. Over the long run they face higher risks of repeat C-sections, cesarean-scar complications, and hysterectomy. We find no evidence that private delivery affects subsequent fertility. The trade-off runs through the scheduling of planned C-sections, which improves the outcomes observed at the delivery itself and raises the risks of later pregnancies. The estimates apply to low-risk first-time mothers drawn into private hospitals, in a system where vaginal birth after cesarean was rare.

Reforms to maternity care are typically evaluated on outcomes observed at or shortly after delivery. Our results show that the evaluation window matters: the costs of the practices such reforms encourage arrive years later, with the next pregnancies.

Declaration of generative AI and AI-assisted technologies in the manuscript preparation process

During the preparation of this work the authors used Claude (Anthropic) in order to improve the readability and consistency of the manuscript and to check the text against the reported estimates. After using this tool, the authors reviewed and edited the content as needed and take full responsibility for the content of the published article.

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8 Tables

Table 1: Descriptive statistics by insurance type

	Fonasa A	Fonasa B	Fonasa C	Fonasa D	Priv. Ins.	All
Panel A. Demographics						
Age	20.853	22.414	23.223	25.134	27.372	22.725
	(4.486)	(5.594)	(5.489)	(5.525)	(5.488)	(5.554)
Mother employed	0.050	0.324	0.509	0.605	0.682	0.293
	(0.217)	(0.468)	(0.500)	(0.489)	(0.466)	(0.455)
Mother's education	10.515	11.297	11.691	12.678	14.474	11.545
	(2.425)	(2.410)	(2.192)	(2.332)	(2.482)	(2.745)
Panel B. Short-term outcomes						
Private hospital	0.007	0.043	0.142	0.457	0.858	0.191
	(0.082)	(0.202)	(0.349)	(0.498)	(0.349)	(0.393)
Cesarean section	0.247	0.311	0.341	0.522	0.557	0.340
	(0.432)	(0.463)	(0.474)	(0.500)	(0.497)	(0.474)
Length of stay (days)	3.813	3.660	3.477	3.157	3.191	3.593
	(2.579)	(2.704)	(2.253)	(2.034)	(1.636)	(2.421)
7+ days hospitalization	0.076	0.068	0.053	0.033	0.015	0.059
	(0.265)	(0.252)	(0.223)	(0.178)	(0.121)	(0.236)
30-day readmission	0.030	0.027	0.021	0.015	0.012	0.025
	(0.171)	(0.162)	(0.144)	(0.123)	(0.107)	(0.155)
Panel C. Long-term outcomes						
Cesarean scar complic. in 3 years	0.005	0.008	0.013	0.025	0.047	0.014
	(0.068)	(0.091)	(0.112)	(0.155)	(0.211)	(0.117)
Hysterectomy by age 35, 15 years	0.007	0.007	0.007	0.009	0.008	0.007
	(0.084)	(0.084)	(0.083)	(0.094)	(0.087)	(0.085)
Second birth in 6 years	0.403	0.375	0.378	0.393	0.509	0.408
	(0.490)	(0.484)	(0.485)	(0.488)	(0.500)	(0.491)
Third birth in 9 years	0.151	0.117	0.113	0.102	0.151	0.135
	(0.358)	(0.321)	(0.316)	(0.303)	(0.358)	(0.342)
Number of children, 12 years	2.016	1.909	1.888	1.838	1.936	1.953
	(0.865)	(0.802)	(0.784)	(0.753)	(0.810)	(0.829)
Observations	129,577	48,196	28,033	31,992	35,853	273,651

Note: This table reports descriptive statistics by insurance type at the time of first birth: Fonasa A, B, C, D, and private insurance (Isapre). Panels report demographic characteristics (A), short-term delivery outcomes (B), and long-term outcomes (C). Unless otherwise specified, all statistics refer to the mother's first birth. The sample includes in-hospital, singleton first births to women aged 15–47 with public or private insurance between 2002 and 2005.

Table 2: Descriptive statistics by insurance type: Sample of mothers with two or more births

	Fonasa A	Fonasa B	Fonasa C	Fonasa D	Priv. Ins.	All
C-section (first pregnancy)	0.233	0.291	0.324	0.502	0.536	0.317
	(0.423)	(0.454)	(0.468)	(0.500)	(0.499)	(0.465)
C-section (second pregnancy)	0.331	0.421	0.448	0.597	0.605	0.419
	(0.470)	(0.494)	(0.497)	(0.491)	(0.489)	(0.493)
Repeated C-section	0.165	0.232	0.262	0.441	0.474	0.252
	(0.371)	(0.422)	(0.440)	(0.496)	(0.499)	(0.434)
Observations	82,028	28,967	16,484	17,710	18,799	163,988

Note: This table reports the probability of C-section in the first and second births for mothers with at least two births within 12 years of the first. Columns correspond to insurance type at the time of the first birth: Fonasa A, B, C, D, and private insurance (Isapre). The sample includes in-hospital, singleton first births to women aged 15–47 with public or private insurance between 2002 and 2005.

Table 3: First stage estimates

	Private Hospital			
	(1)	(2)	(3)	(4)
Fonasa B * post	0.031***	0.031***	0.036***	0.015
	[0.007]	[0.007]	[0.007]	[0.011]
Fonasa C * post	0.122***	0.121***	0.107***	0.087***
	[0.021]	[0.020]	[0.018]	[0.018]
Fonasa D * post	0.283***	0.278***	0.258***	0.239***
	[0.021]	[0.021]	[0.020]	[0.019]
Age-Year FE		X	X	X
Municipality-Age FE			X	X
Municipality-Year FE			X	X
Linear trend				X
Effective F-stat	88.6	86.8	87.7	67.4
Mean DV	0.191	0.191	0.191	0.191
Observations	273,651	273,649	273,468	273,468

Note: All columns include month-year of birth and mother's age fixed effects; the rows below indicate additional fixed effects. Linear trend is a linear trend for Fonasa A. Effective F-stat is the effective F-statistic proposed by Montiel Olea and Pflueger (2013) to test the presence of weak instruments. Mean DV denotes the average of the dependent variable in the estimation sample. Standard errors clustered at the municipality of residence are reported in brackets.
*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table 4: Mean characteristics of compliers and reference groups, Fonasa groups B, C, and D

	Pre-reform private	Compliers	B/C/D pool	Post-reform public
Panel A. Socioeconomic characteristics				
Age	26.193	25.437	23.428	22.835
	(0.189)	(0.185)	(0.094)	(0.091)
Years of education	13.460	13.242	11.808	11.478
	(0.102)	(0.072)	(0.046)	(0.047)
Mother employed	0.679	0.662	0.455	0.402
	(0.011)	(0.016)	(0.009)	(0.009)
Urban residence	0.976	0.965	0.916	0.903
	(0.006)	(0.005)	(0.009)	(0.009)
Panel B. Risk indicators				
Advanced maternal age (35 or over)	0.077	0.056	0.048	0.043
	(0.009)	(0.006)	(0.002)	(0.002)
Pre-delivery obstetric hospit.	0.054	0.104	0.142	0.163
	(0.006)	(0.007)	(0.004)	(0.005)
Preterm birth (< 37 weeks)	0.031	0.028	0.063	0.074
	(0.004)	(0.003)	(0.001)	(0.002)
Low birth weight (< 2500 g)	0.027	0.030	0.053	0.063
	(0.004)	(0.003)	(0.001)	(0.001)
Observations	2,150	—	108,221	63,567

Notes. Sample: first births, singleton, mother aged 15–47, 2002–2005, Fonasa groups B, C, and D (108,221 observations). The compliers column is a before-and-after Wald ratio: the change at April 2003 in the mean of the characteristic interacted with private-hospital delivery, divided by the change in the private-delivery share (8.2 to 22.6 percent). Standard errors are from a bootstrap with 200 replications, clustered at the municipality of residence. Drift in a characteristic across cohorts of the women delivering privately before the reform would enter the ratio with weight 0.57; the observed change among private deliveries bounds it below 4 percent of the estimate in six of the eight rows, at 0.018 for pre-delivery obstetric hospitalization and 0.008 for advanced maternal age, in each case moving the compliers toward the pre-reform private column. The remaining columns are sample means: women delivering at private hospitals before April 2003, all women, and women delivering at public hospitals after April 2003. Preterm birth and low birth weight are partly post-treatment.

Table 5: Effect of delivery at private hospital on short-term maternal outcomes (at first birth): OLS and IV estimates

	OLS	IV Fonasa B, C, D			
	(1)	(2)	(3)	(4)	(5)
Panel A. Cesarean section, first birth					
Private Hospital	0.211***	0.446***	0.443***	0.452***	0.417***
	[0.016]	[0.037]	[0.036]	[0.041]	[0.042]
Mean DV	0.340	0.340	0.340	0.340	0.340
Observations	268,252	268,252	268,250	268,060	268,060
Panel B. Length of stay (days)					
Private Hospital	-0.585***	-1.437***	-1.413***	-1.437***	-1.329***
	[0.096]	[0.148]	[0.147]	[0.150]	[0.159]
Mean DV	3.593	3.593	3.593	3.593	3.593
Observations	273,586	273,586	273,584	273,403	273,403
Panel C. 7+ days hospitalization					
Private Hospital	-0.050***	-0.094***	-0.091***	-0.090***	-0.082***
	[0.004]	[0.013]	[0.013]	[0.013]	[0.014]
Mean DV	0.059	0.059	0.059	0.059	0.059
Observations	273,586	273,586	273,584	273,403	273,403
Panel D. 30-day readmission					
Private Hospital	-0.011***	-0.025***	-0.025***	-0.025***	-0.015*
	[0.001]	[0.007]	[0.007]	[0.008]	[0.009]
Mean DV	0.025	0.025	0.025	0.025	0.025
Observations	273,651	273,651	273,649	273,468	273,468
Age-Year FE			X	X	X
Municipality-Age FE				X	X
Municipality-Year FE				X	X
Linear trend					X

Note: All columns include month-year of birth and mother's age fixed effects; the rows below indicate additional fixed effects. Linear trend is a linear trend for Fonasa A. Mean DV denotes the average of the dependent variable in the estimation sample. Standard errors clustered at the municipality of residence are reported in brackets.
*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table 6: Effect of delivery at private hospital on 30-day postpartum morbidity, by diagnosis (at first birth): OLS and IV estimates

	OLS	IV Fonasa B, C, D			
	(1)	(2)	(3)	(4)	(5)
Panel A. Puerperal infections (O85+O86+O91+N61), 30 days					
Private Hospital	-0.0036***	-0.0044	-0.0050	-0.0056	-0.0015
	[0.0005]	[0.0037]	[0.0038]	[0.0041]	[0.0049]
Mean DV	0.0072	0.0072	0.0072	0.0072	0.0072
Panel B. Wound complications (O900+O901+O902), 30 days					
Private Hospital	-0.0015***	-0.0067***	-0.0068***	-0.0074**	-0.0081**
	[0.0003]	[0.0025]	[0.0026]	[0.0029]	[0.0034]
Mean DV	0.0024	0.0024	0.0024	0.0024	0.0024
Panel C. Perineal wound complications (O901+O902), 30 days					
Private Hospital	-0.0013***	-0.0056**	-0.0054**	-0.0062**	-0.0076**
	[0.0003]	[0.0023]	[0.0024]	[0.0026]	[0.0032]
Mean DV	0.0020	0.0020	0.0020	0.0020	0.0020
Panel D. Complications of procedures (T81), 30 days					
Private Hospital	-0.0003	-0.0014	-0.0013	-0.0017	-0.0017
	[0.0003]	[0.0012]	[0.0012]	[0.0013]	[0.0014]
Mean DV	0.00068	0.00068	0.00068	0.00068	0.00068
Panel E. Postpartum hemorrhage (O72), 30 days					
Private Hospital	-0.0001	0.0001	0.0002	0.0002	0.0002
	[0.0001]	[0.0011]	[0.0011]	[0.0012]	[0.0013]
Mean DV	0.0006	0.0006	0.0006	0.0006	0.0006
Age-Year FE			X	X	X
Municipality-Age FE				X	X
Municipality-Year FE				X	X
Linear trend					X
Observations	273,651	273,651	273,649	273,468	273,468

Note: All columns include month-year of birth and mother's age fixed effects; the rows below indicate additional fixed effects. Linear trend is a linear trend for Fonasa A. Each panel is a separate regression for a diagnosis group identified within 30 days of the first birth. Mean DV denotes the average of the dependent variable in the estimation sample. Standard errors clustered at the municipality of residence are reported in brackets. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table 7: Effect of delivery at private hospital on C-section in the first and second births: OLS and IV estimates

	OLS	IV Fonasa B, C, D			
	(1)	(2)	(3)	(4)	(5)
Panel A. C-section in first birth					
Private Hospital	0.215***	0.456***	0.452***	0.453***	0.412***
	[0.017]	[0.047]	[0.048]	[0.057]	[0.059]
Mean DV	0.317	0.317	0.317	0.317	0.317
Observations	163,988	163,988	163,986	163,797	163,797
Panel B. C-section in second birth					
Private Hospital	0.176***	0.353***	0.338***	0.353***	0.305***
	[0.017]	[0.034]	[0.035]	[0.038]	[0.043]
Mean DV	0.419	0.419	0.419	0.419	0.419
Observations	163,988	163,988	163,986	163,797	163,797
Panel C. Repeat C-section					
Private Hospital	0.220***	0.485***	0.478***	0.495***	0.451***
	[0.017]	[0.042]	[0.042]	[0.046]	[0.049]
Mean DV	0.252	0.252	0.252	0.252	0.252
Observations	163,988	163,988	163,986	163,797	163,797
Age-Year FE			X	X	X
Municipality-Age FE				X	X
Municipality-Year FE				X	X
Linear trend					X

Note: All columns include month-year of birth and mother's age fixed effects; the rows below indicate additional fixed effects. The sample is restricted to women with at least two births within 12 years of the first, for whom the mode of delivery can be identified in both the first and second birth. Linear trend is a linear trend for Fonasa A. Mean DV denotes the average of the dependent variable in the estimation sample. Standard errors clustered at the municipality of residence are reported in brackets.
*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table 8: Effect of delivery at private hospital on hospitalization due to cesarean scar complications: OLS and IV estimates

	OLS	IV Fonasa B, C, D			
	(1)	(2)	(3)	(4)	(5)
Cesarean scar hospitalization within three years					
Private Hospital	0.030***	0.057***	0.061***	0.066***	0.068***
	[0.003]	[0.009]	[0.009]	[0.009]	[0.010]
Mean DV	0.014	0.014	0.014	0.014	0.014
Observations	273,651	273,651	273,649	273,468	273,468
Age-Year FE			X	X	X
Municipality-Age FE				X	X
Municipality-Year FE				X	X
Linear trend					X

Note: All columns include month-year of birth and mother's age fixed effects; the rows below indicate additional fixed effects. Linear trend is a linear trend for Fonasa A. Mean DV denotes the average of the dependent variable in the estimation sample. Standard errors clustered at the municipality of residence are reported in brackets.
*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table 9: Effect of delivery at private hospital on hysterectomy by age 35, 15-year window: OLS and IV estimates

	OLS	IV Fonasa B, C, D			
	(1)	(2)	(3)	(4)	(5)
Panel A. Total					
Private Hospital	0.003	0.010**	0.010**	0.012**	0.013**
	[0.002]	[0.004]	[0.005]	[0.005]	[0.006]
Mean DV	0.007	0.007	0.007	0.007	0.007
Observations	273,651	273,651	273,649	273,468	273,468
Panel B. Obstetric (chapter O)					
Private Hospital	0.002	0.008***	0.008**	0.009**	0.011**
	[0.002]	[0.003]	[0.003]	[0.004]	[0.005]
Mean DV	0.004	0.004	0.004	0.004	0.004
Observations	273,651	273,651	273,649	273,468	273,468
Panel C. Non-obstetric					
Private Hospital	0.001*	0.002	0.002	0.003	0.002
	[0.000]	[0.003]	[0.003]	[0.003]	[0.004]
Mean DV	0.003	0.003	0.003	0.003	0.003
Observations	273,651	273,651	273,649	273,468	273,468
Age-Year FE			X	X	X
Municipality-Age FE				X	X
Municipality-Year FE				X	X
Linear trend					X

Note: OLS and IV estimates of the effect of delivery at a private hospital on the probability of hysterectomy before age 35 within 15 years of the first birth. Panel A reports the total effect; Panel B restricts to obstetric hysterectomies (principal diagnosis in ICD-10 chapter O); Panel C restricts to non-obstetric ones. Instruments are Fonasa B, C, and D dummies interacted with the post-reform indicator. All specifications include month-year of birth and mother's age fixed effects; columns (3)–(5) add age $\times$ year-of-birth interactions, columns (4)–(5) add municipality $\times$ age and municipality $\times$ year-of-birth interactions, and column (5) adds a Fonasa A–specific linear time trend. Mean DV is the sample mean of the dependent variable. Standard errors clustered at the municipality of residence in brackets. ***, **, * indicate significance at the 1%, 5%, and 10% level.

Table 10: Effect of delivery at private hospital on subsequent fertility: OLS and IV estimates

	OLS	IV Fonasa B, C, D			
	(1)	(2)	(3)	(4)	(5)
Panel A. 2 or more children, 6 years					
Private Hospital	0.045***	-0.013	0.005	0.002	0.017
	[0.005]	[0.025]	[0.025]	[0.027]	[0.031]
Mean DV	0.408	0.408	0.408	0.408	0.408
Observations	273,651	273,651	273,649	273,468	273,468
Panel B. 3 or more children, 9 years					
Private Hospital	0.017***	-0.009	-0.005	-0.004	0.008
	[0.005]	[0.018]	[0.019]	[0.019]	[0.022]
Mean DV	0.135	0.135	0.135	0.135	0.135
Observations	273,651	273,651	273,649	273,468	273,468
Panel C. Number of children, 12 years					
Private Hospital	0.058***	0.062	0.058	0.071	0.113**
	[0.012]	[0.044]	[0.046]	[0.046]	[0.054]
Mean DV	1.953	1.953	1.953	1.953	1.953
Observations	273,651	273,651	273,649	273,468	273,468
Age-Year FE			X	X	X
Municipality-Age FE				X	X
Municipality-Year FE				X	X
Linear trend					X

Note: All columns include month-year of birth and mother's age fixed effects; the rows below indicate additional fixed effects. Linear trend is a linear trend for Fonasa A. Mean DV denotes the average of the dependent variable in the estimation sample. Standard errors clustered at the municipality of residence are reported in brackets.
*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Appendix

A Dataset construction

A.1 Data sources

Our analysis combines two administrative datasets from Chile: birth certificates and hospital discharge records, both provided by the Ministry of Health.

The birth certificate data include all registered live births between 2001 and 2018; fetal deaths and other non-live births are not recorded. Each record contains a unique anonymized ID for both the mother and the child, along with detailed information such as date of birth, birth weight, size at birth, gestational age, indicators for singleton or multiple births, parity, and the delivery hospital. Maternal characteristics—age, municipality of residence, educational attainment, and employment status—are also reported.

The hospital discharge data cover all inpatient admissions between 2001 and 2020. For each hospitalization, the data report a unique anonymized patient identifier, the discharging hospital, discharge date, length of stay, principal diagnosis coded under ICD-10, whether a surgical procedure was performed, Fonasa procedure codes (since 2004), and patient characteristics such as age, gender, municipality of residence, and insurance type (Fonasa group or Isapre). Because both datasets rely on the same anonymized identifier system, we can link births and hospitalizations at the individual level.

A.2 Identifying obstetric hospitalizations to match with birth data

We identify childbirth-related hospitalizations by selecting female patients whose principal diagnosis begins with the letter "O," corresponding to pregnancy, childbirth, and the puerperium under ICD-10. For these admissions, we keep the patient identifier, discharge date, length of stay, hospital identifier, municipality of residence, and patient age.

When multiple obstetric records occur for the same patient within a month, we retain the record that most clearly corresponds to delivery: first those with diagnosis codes O80–O84, and, if more than one remains (or none exists), the one with the latest discharge date. The resulting dataset is then used to match births to hospitalizations.

A.3 Linking births to hospital records

We link birth certificates to childbirth hospitalizations using the anonymized mother identifier.

A birth is linked to a hospitalization when the mother identifier coincides with the patient identifier and the hospitalization discharge month falls within two months (before or after) of the birth month.

For the small number of cases in which the exact discharge date is missing but the year is reported, we match using the mother identifier and the calendar year, allowing the recorded year to differ from the birth year by up to one year.

The rules are applied sequentially, each one only to the births left unmatched by the previous step. Two further rules complete the linked file. When no identifier match is found, a demographic rule matches a birth to a discharge agreeing on date of birth, municipality of residence, maternal age and delivery hospital, allowing the dates to differ by up to three days and maternal age by one year; it recovers 3.7 percent of births. A final step applies the same identifier rules to multiple births. Neither enters the estimation sample, which keeps only singleton births linked through the mother identifier.

Overall, 86 percent of birth records between 2001 and 2018 are linked through the mother identifier; of these, over 99 percent use the discharge month and the rest the calendar year. Within the years of the estimation sample the rate is 76 percent. The shortfall reflects obstetric discharge records that lack a patient identifier, which are far more common in 2002 than in later years.

A.4 Sample selection

From the matched data, we define the sample used in the empirical analysis. We restrict to in-hospital singleton births to nulliparous women and focus on deliveries occurring between 2001 and 2005. We further limit the sample to mothers aged 15 to 47 who were enrolled in either public insurance (Fonasa) or private insurance (Isapre), and we require complete information on delivery hospital and insurance type.

For mothers enrolled in Fonasa whose specific insurance group was not reported, we impute the missing group using information from hospitalizations in adjacent years. This correction applies to 2.8 percent of observations among Fonasa enrollees.

Because a relatively large share of records lack identifiers in 2001, the estimation sample is restricted to 2002–2005. However, data from 2001 are retained for descriptive figures used to assess pre-existing trends.

A.5 Outcome construction

Using this matched sample, together with subsequent birth and hospitalization records, we construct the outcomes analyzed in the paper.

Short-term outcomes are measured at the delivery hospitalization or in the 30 days after its discharge. They comprise an indicator for cesarean delivery, length of stay in days, long-stay indicators (a stay of five, seven or ten days or more), and an indicator for readmission within thirty days of discharge.

Long-term outcomes are derived from hospital discharge data observed for up to fifteen years after the first delivery. These include hospitalizations related to maternal obstetric care, particularly cesarean-scar complications and hysterectomy, identified using ICD-10 diagnosis codes and corresponding Fonasa procedure codes. The hysterectomy outcome used in the analysis is bounded by maternal age: the main indicator equals one for a hysterectomy before age 35 occurring within 15 years of the first birth, with bounds at ages 30, 40 and 45 reported as well (Online Appendix B). For mothers who have additional births within twelve years, we also construct indicators for cesarean delivery in the second birth and for having cesarean deliveries in both the first and second births. Finally, we use subsequent birth certificates to construct indicators for whether the mother has a second or third child within one to twelve years after the initial birth.

B Variables definition

Cesarean section:

Table B.1 summarizes the classification of the binary indicator Cesarean Section, constructed from ICD-10 diagnosis codes recorded at delivery. Because we match hospital discharge data to birth records, all observations correspond to delivery events. However, in Chile's hospital data, childbirth hospitalizations are often coded under pregnancy or labor complications rather than delivery-specific ICD-10 codes (O80-O84). We therefore use an expanded list of ICD-10 codes covering pregnancy, labor, and delivery-related diagnoses.

Table B.1: Definition of Cesarean Section

ICD-10 diagnosis code	C-section = 1	C-section = 0
O140, O141, O149, O160, O240, O249, O266, O300, O321, O322, O324, O325, O329, O330, O331, O334, O335, O339, O342, O348, O349, O363, O365, O366, O368, O410, O411, O441, O459, O603, O610, O619, O620, O621, O622, O639, O640, O641, O648, O649, O651, O654, O655, O662, O664, O669, O680, O682, O688, O689, O691, O820, O821, O822, O828, O829, O842	Yes	No
O100, O130, O244, O269, O360, O400, O420, O421, O429, O470, O471, O479, O480, O600, O601, O602, O623, O629, O668, O681, O698, O700, O701, O702, O703, O709, O714, O757, O758, O800, O801, O808, O809, O810, O811, O812, O813, O814, O815, O830, O831, O832, O833, O834, O838, O839, O840, O841, O860, O909, O990, O992, O993, O996, O998	No	Yes
O848, O849	has_surgery = 1	has_surgery = 0

Note: This table summarizes the classification of the binary indicator C-section, constructed from ICD-10 diagnosis codes recorded at delivery. Codes listed in the first row are always classified as cesarean section (C-section = 1), while codes in the second row are always classified as noncesarean (C-section = 0). For diagnoses O848 and O849, the classification depends on whether a surgical procedure is recorded in the hospital discharge data: the delivery is classified as a C-section when $has\_surgery = 1$, and as noncesarean when $has\_surgery = 0$. All remaining obstetric diagnoses are left unclassified and the mode of delivery is coded as missing.

Length of stay:

Length of stay is the number of days between admission and discharge recorded in the discharge record of the delivery hospitalization. It is recorded in whole days. Values that are negative or above 60 days are treated as recording errors and set to missing. The long-stay indicators equal one for stays of five, seven, or ten days or more. The seven-day indicator is the one we refer to as prolonged hospitalization; it and length of stay in days are reported in the main text, while the five- and ten-day indicators are reported in Online Appendix D.

30-day readmission:

The 30-day readmission indicator equals one when the mother has a new hospital admission within 30 days of the discharge from the delivery hospitalization, at any public or private hospital. We date each subsequent admission by its admission date, computed as the discharge date minus the recorded length of stay, and count it when that date falls after the delivery discharge and no more than 30 days later. Records carrying a delivery code (ICD-10 O80–O84, covering spontaneous, assisted and cesarean deliveries) are excluded, so that a re-coded or transferred record of the same birth does not count as a readmission. One exception is retained: cesarean section with hysterectomy (O82.2) falls inside that range but is kept, because it records a hysterectomy rather than a re-coded delivery. The measure is all-cause: it counts admissions under any other principal diagnosis, obstetric or not.

Short-term morbidity diagnostic groups:

We complement the binary 30-day readmission indicator with four diagnostic-group indicators that capture the dominant categories of maternal readmission. Each indicator equals one if the mother had any readmission in the (1, 30]-day window after the first-birth discharge with a principal ICD-10 diagnosis in the listed codes: *puerperal infections* (O85, O86, O91, N61); *wound complications* (O90.0–O90.2), with the perineal subset (O90.1, O90.2) reported separately; *complications of procedures* (T81); and *postpartum hemorrhage* (O72). Table B.2 ranks all diagnostic codes accounting for at least 1.3% of 30-day readmissions in the estimation sample. The codes assigned to the four diagnostic groups together cover approximately 55% of all 30-day readmissions. Two residual subcodes within O90 — O90.8 (other complications of the puerperium, NEC) and O90.9 (complication of the puerperium, unspecified) — are not assigned to a specific group.

Table B.2: Most common diagnostic codes in 30-day maternal readmissions

Code	Description	Percent
Puerperal infections		36.7
O85	Puerperal sepsis	14.9
O86	Other puerperal infections	11.8
O91	Infections of breast associated with childbirth	7.3
N61	Inflammatory disorders of breast	2.7
Wound complications (O90.0–O90.2)		12.1
O901	Disruption of perineum wound following delivery	8.8
O900	Disruption of cesarean delivery wound	1.9
O902	Hematoma of obstetric wound	1.4
Complications of procedures (T81)		3.4
Postpartum hemorrhage (O72)		2.8
Subtotal		55.0
Other obstetric (Chapter O)		21.7
Non-obstetric (e.g., cholelithiasis, appendicitis)		23.2
All readmissions		100.0

Note: The table ranks all ICD-10 codes that account for at least 1.3% of all 30-day maternal readmissions among first-birth mothers in the estimation sample (2002–2005, singletons, ages 15–47, with known insurance and municipality of residence). Rows in italics are the morbidity categories; the indented rows below each list the individual codes it contains. Codes are reported at the three-character category level by default; O90 is split at the subcode level because its subcodes enter different panels (O90.0–O90.2 form the wound-complications group, and O90.1 and O90.2 also form the perineal subset). Delivery codes (O80–O84) and administrative/encounter codes (chapter Z) are excluded, as are codes recorded under the mother's identifier that correspond to neonatal diagnoses (chapter P). Codes below the 1.3% cutoff are collapsed into the *Other* row.

Hysterectomy:

Defined as a hospital discharge with ICD-10 code O82.2 (cesarean section with hysterectomy) or with any of the following Fonasa surgical procedure codes: 2003009–2003010 (abdominal hysterectomy with unilateral/bilateral anexectomy), 2003014 (vaginal hysterectomy), 2003015 (radical hysterectomy with pelvic and para-aortic lymph node dissection), 2003016 (total hysterectomy with concurrent urinary incontinence surgery), 2501034 (unspecified hysterectomy), and 2004005 (cesarean section with hysterectomy). Since surgical procedure codes are available only from 2004 onward, hysterectomies identified through those codes are counted only from three years after the first birth, uniformly across cohorts. O82.2 is a diagnosis and is available in every year, so it is counted from the birth itself.

We split hysterectomies into two categories. *Obstetric* hysterectomies are episodes whose principal diagnosis falls in ICD-10 chapter O (O00–O99); they include O82.2 and other peripartum complications. *Non-obstetric* hysterectomies are episodes whose principal diagnosis lies outside chapter O.

For the IV analysis we restrict the outcome to events at age ≤ 35 within 15 years of the first birth—the window in which any obstetric channel running through accumulated cesarean scars should appear. Within that window we further separate *peripartum* events (O82.2 recorded within one year of the first birth) from *subsequent-pregnancy* events (O82.2 recorded between one and fifteen years after the first birth).

C Congestion and competition

The reform moved deliveries from public to private hospitals, and that reallocation could have changed the care hospitals provide to women whose own hospital did not change. Three channels are possible. The inflow of patients in Fonasa groups B, C, and D could congest private hospitals, worsening outcomes for the Isapre women already delivering there. The matching outflow could decongest public hospitals, improving outcomes for the Fonasa A women who remain. Public hospitals facing new competitive pressure could also raise quality, which would again improve Fonasa A outcomes. The first channel contaminates our Isapre comparison group and the other two our Fonasa A comparison group. Congestion also bears on the compliers, who deliver at hospitals the reform itself made busier. Each channel would violate the exclusion restriction.

We first measure the volume shock each hospital type absorbed. For each hospital and month we divide deliveries by that hospital's mean monthly deliveries over 2002q1–2003q1, and regress the ratio on quarter indicators and hospital fixed effects, with 2003q1 as the reference quarter and the pre-reform mean as weight. Across the post-reform quarters, deliveries at private hospitals stand about 16 percent above their pre-reform mean, reaching 22 percent in the last quarter of 2003; deliveries at public hospitals stand about 6 percent below. Public hospitals deliver four fifths of the births in our sample, so the same reallocation is a large proportional shock to private hospitals and a small one to public hospitals. Congestion is primarily a private-hospital question.

C.1 Spillovers to the Fonasa A comparison group

Our first exercise uses Fonasa A alone as the control group and allows for spatial spillovers to those women, whether from decongestion or from a competitive response, following Butts (2023). Using the population-weighted mean distance from a woman's municipality to the nearest private hospital, computed from 2017 census block centroids, we define Ring 1 (≤ 10 km), Ring 2 (10–50 km), and Ring 3 (> 50 km), and progressively drop the most exposed Fonasa A municipalities from the control group. Table C.1 reports the full sample (column 1), the Fonasa-only sample (column 2), Fonasa groups B, C, and D plus Rings 2 and 3 (column 3), and Fonasa groups B, C, and D plus Ring 3 alone (column 4). Columns 2 to 4 exclude Isapre, so congestion of private hospitals should not reach the control group, and dropping the Fonasa A municipalities nearest to a private hospital removes those where exposure to decongestion and to a competitive response should be greatest. Cesarean section ranges from 0.363 to 0.395 across columns 2 to 4 and readmission from -0.026 to -0.028. Prolonged hospitalization drifts from -0.092 to -0.113, still inside the column 2 confidence interval but in the direction a competition spillover into the nearest municipalities would imply.

Table C.1: Spatial spillovers: IV estimates excluding Isapre and the Fonasa A municipalities nearest to a private hospital

	(1) Full sample	(2) Fonasa only	(3) Rings 2–3	(4) Ring 3 only
Panel A. Cesarean section				
Private hospital	0.446***	0.395***	0.363***	0.370***
	[0.037]	[0.039]	[0.040]	[0.042]
Mean DV	0.340	0.308	0.325	0.354
Obs.	268,168	233,432	172,461	130,564
Panel B. 7+ days hospitalization				
Private hospital	-0.094***	-0.092***	-0.109***	-0.113***
	[0.013]	[0.014]	[0.015]	[0.018]
Mean DV	0.059	0.066	0.066	0.062
Obs.	273,501	237,658	175,697	133,086
Panel C. 30-day readmission				
Private hospital	-0.025***	-0.027***	-0.028***	-0.026***
	[0.007]	[0.008]	[0.009]	[0.010]
Mean DV	0.025	0.026	0.028	0.026
Obs.	273,566	237,717	175,739	133,119

Note: IV estimates of the effect of private hospital delivery. The sample excludes Fonasa A women in municipalities for which the distance to the nearest private hospital is unavailable. Instruments: Fonasa B, C, and D interacted with Post. Absorbs month-year of birth and mother's age fixed effects, as in the paper's baseline specification. Mean DV denotes the average of the dependent variable in the estimation sample. Standard errors clustered at the municipality of residence in brackets. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

C.2 Congestion of private hospitals

The congestion channel bears on the Isapre comparison group rather than on Fonasa A, so we narrow that group toward women the reform's inflow should not have reached. Column 1 of Table C.2 is the full sample, in which Fonasa A and Isapre both serve as control groups.

Columns 2 and 3 drop Fonasa A and leave Isapre alone: column 2 uses all Isapre women, and column 3 keeps only those at private hospitals with no recorded PAD deliveries, about 40 percent of Isapre births in the sample. Those hospitals took none of the reform's PAD volume, so the inflow should not have reached them. Prolonged hospitalization barely moves, from −0.080 to −0.073, and readmission is small and insignificant in both columns, −0.011 and −0.009. Cesarean section is smaller in the restricted group, 0.402 against 0.477, with confidence intervals that overlap substantially, and both are within 10 percent of our baseline estimate of 0.446.

Table C.2: Congestion of private hospitals: IV estimates with Isapre as the control group, at all private hospitals and at hospitals without PAD deliveries

	(1) Full sample	(2) B-D + Isapre	(3) B-D + Isapre (non-PAD)
Panel A. Cesarean section			
Private hospital	0.446***	0.477***	0.402***
	[0.037]	[0.038]	[0.046]
Mean DV	0.340	0.424	0.394
Obs.	268,252	140,910	120,253
Panel B. 7+ days hospitalization			
Private hospital	-0.094***	-0.080***	-0.073***
	[0.013]	[0.012]	[0.014]
Mean DV	0.059	0.044	0.049
Obs.	273,586	144,043	122,741
Panel C. 30-day readmission			
Private hospital	-0.025***	-0.011	-0.009
	[0.007]	[0.008]	[0.009]
Mean DV	0.025	0.019	0.021
Obs.	273,651	144,074	122,771

Note: IV estimates of the effect of private hospital delivery. Instruments: Fonasa B, C, and D interacted with Post. Absorbs month-year of birth and mother's age fixed effects, as in the paper's baseline specification. Mean DV denotes the average of the dependent variable in the estimation sample. Standard errors clustered at the municipality of residence in brackets. Col. (1): full sample, with Fonasa A and Isapre both untreated. Cols. (2)–(3): Fonasa A is dropped and Isapre is the control group. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

C.3 Hospitals more and less exposed to the reallocation

The tests above narrow the comparison groups. We can also use variation across hospitals in how much of the reallocation each one absorbed: if congestion or a competitive response moved outcomes, patients at more exposed hospitals should show larger changes. We compare Isapre patients across private hospitals that received a large versus a small inflow of women in Fonasa groups B, C, and D, and Fonasa A patients across public hospitals that lost a large versus a small share of them.

For each private hospital h we define $\text{Inflow}_h = (BCD_h^{post} - BCD_h^{pre})/n_h^{pre}$, and for each public hospital $\text{Outflow}_h = (BCD_h^{pre} - BCD_h^{post})/n_h^{pre}$, where BCD^{pre} and BCD^{post} are mean monthly deliveries by women in Fonasa groups B, C, and D over 2002m1–2003m3 and 2003m4–2005m12, and n_h^{pre} is mean monthly total deliveries in the pre-reform window.[34]

We estimate $y_{ihm} = \alpha_h + \alpha_m + \beta\,(\text{Exposure}_h \times \text{Post}_m) + \varepsilon_{ihm}$, where Exposure_h is Inflow_h in the Isapre sample at private hospitals and Outflow_h in the Fonasa A sample at public hospitals, with hospital, month-year, and maternal age fixed effects and standard errors clustered at the hospital level.

Table C.3 reports the estimates. For Isapre women at private hospitals, cesarean section shows no detectable relation to inflow, while prolonged hospitalization and readmission carry negative coefficients significant at the 10 percent level. Moving a hospital from the 25th to the 75th percentile of inflow changes prolonged hospitalization by 0.41 percentage points and readmission by 0.36, about 4 percent and 14 percent of our baseline estimates for those outcomes. For Fonasa A women at public hospitals, none of the estimates is significant.

[34] Both measures are winsorized at the 99th percentile, since hospitals with few pre-reform births produce unstable ratios, and we keep hospitals averaging at least five deliveries a month before the reform.

Table C.3: Hospital exposure to the volume shift and patient outcomes

	(1) C-section	(2) Long stay	(3) Readmit. 30d
Panel A. Isapre patients at private hospitals			
Inflow of Fonasa B, C, and D × Post	-0.0074	-0.0061*	-0.0054*
	[0.0267]	[0.0034]	[0.0029]
Effect of p25–p75 increase	-0.0050	-0.0041*	-0.0036*
Mean DV	0.555	0.013	0.011
Observations	28,127	29,092	29,093
Panel B. Fonasa A patients at public hospitals			
Outflow of Fonasa B, C, and D × Post	0.0055	-0.0196	-0.0092
	[0.0417]	[0.0391]	[0.0135]
Effect of p25–p75 increase	0.0006	-0.0020	-0.0010
Mean DV	0.247	0.076	0.030
Observations	126,011	128,174	128,208

Note: "Effect of p25–p75 increase" scales the coefficient by the interquartile range of the exposure measure across hospitals. Absorbs hospital, month-year of birth, and mother's age fixed effects. Mean DV denotes the average of the dependent variable in the estimation sample. Standard errors clustered at the hospital level in brackets. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

C.4 Congestion within hospitals

Congestion also varied within a hospital over time, which lets us ask whether a hospital practiced differently when it was busy.

For the first exercise we define a capacity benchmark $\bar{q}_h$ as the 90th percentile of hospital h's monthly count of obstetric episodes over 2001–2002, and a monthly utilization ratio $u_{hm} = q_{hm}/\bar{q}_h$.[35] At private hospitals the ratio stands at 0.77 in the reference quarter and 0.97 after the reform; at public hospitals it falls from 0.78 to 0.73.[36] We regress outcomes on u_{hm} with hospital, month-year, and maternal age fixed effects, separately for Isapre enrollees at private hospitals and Fonasa A enrollees at public hospitals, clustering at the hospital.

[35] Obstetric episodes are hospital discharges with a principal diagnosis in ICD-10 O60–O92: labor complications, deliveries, and puerperal complications.

[36] Reference-quarter (2003q1) level and average post-reform level implied by a regression of the monthly utilization ratio on quarter indicators with hospital fixed effects, estimated separately for private and public hospitals.

The variation is within hospital across months.

Table C.4 reports both samples. Higher utilization shows no detectable relation to cesarean section or readmission in either sample. Prolonged hospitalization among Isapre women at private hospitals is the exception, positive and significant at the 10 percent level; applied to the observed rise in utilization at private hospitals it implies about 0.12 percentage points, a little over 1 percent of our baseline estimate for that outcome.

Table C.4: Hospital utilization and patient outcomes

	(1) C-section	(2) Long stay	(3) Readmit. 30d
Panel A. Isapre enrollees at private hospitals			
Util. ratio	-0.0142	0.0061*	-0.0030
	[0.0245]	[0.0036]	[0.0026]
Mean DV	0.561	0.013	0.011
Observations	26,908	27,803	27,804
Panel B. Fonasa A enrollees at public hospitals			
Util. ratio	-0.0173	-0.0159	0.0011
	[0.0196]	[0.0105]	[0.0054]
Mean DV	0.247	0.076	0.030
Observations	124,893	127,003	127,037

Note: The coefficient is on the continuous utilization ratio $u_{hm} = q_{hm}/\bar{q}_h^{90}$, where q_{hm} is the hospital's monthly count of obstetric episodes (ICD-10 O60–O92) and $\bar{q}_h^{90}$ is the 90th percentile of that count in 2001–2002. Absorbs hospital, month-year, and maternal age fixed effects. Mean DV denotes the average of the dependent variable in the estimation sample. Standard errors clustered at the hospital level in brackets. Sample: 2002q1–2005q4. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

The second exercise uses variation that is exogenous to the reform. Following Bensnes (2026), we instrument actual delivery crowding with the clustering of inferred due dates. For woman i delivering at hospital h we set the due date to the date of birth plus the weeks of gestation remaining,[37] and take as instrument Z_{iw}, the count of other women at the same hospital whose due date falls in the same week. Due dates are set at conception, roughly

[37] Gestational age is recorded in whole weeks, so the implied due date is pinned only to within a few days, and the instrument is correspondingly noisier than the day-level measure available in the Norwegian registry.

forty weeks before delivery, so the timing that generates Z_{iw} is plausibly predetermined with respect to the reform. The measure is also invariant to when the delivery actually takes place: a birth brought forward by a week lowers recorded gestational age by a week and leaves the implied due date unchanged. The endogenous variable X_{iw} is the count of other women who actually delivered at hospital h in the same week as woman i.

We instrument X_{iw} with Z_{iw}, with hospital × month-year and maternal age fixed effects, separately for Isapre enrollees at private hospitals and Fonasa A enrollees at public hospitals, clustering at the hospital, over 2003–2005.[38] The interaction absorbs hospital-month differences in crowding and in composition, leaving variation across weeks within a hospital-month. Forecasted deliveries predict actual concurrent deliveries, with effective F-statistics of 116.5 in the Isapre sample and 111.4 in the Fonasa A sample (Table C.5). The IV estimates in Table C.6 are indistinguishable from zero for all three outcomes in both samples.

Table C.5: Maternity crowding: first stage

	(1) Isapre (private)	(2) Fonasa A (public)
Forecasted deliveries	0.0955***	0.0688***
	[0.0093]	[0.0066]
Effective F-stat	116.5	111.4
Observations	23,544	102,425

Note: The dependent variable is the number of other women delivering at the same hospital in the same ISO birth week. The instrument is the number of other women at the same hospital whose inferred due date (birth date $+ (40 - g) \times 7$ days, where g is gestational age in weeks) falls in the same ISO week. The effective F-statistic is the Montiel Olea and Pflueger (2013) weak-instrument robust statistic. Absorbs hospital × month-year and maternal age fixed effects. Standard errors clustered at the hospital level in brackets. Sample: 2003q1–2005q4 (2002 excluded because undercoverage biases within-sample instrument counts). *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

[38] We drop 2002, where undercoverage of the discharge records would bias the within-hospital counts that build the instrument.

Table C.6: Maternity crowding and patient outcomes

	(1) C-section	(2) Long stay	(3) Readmit. 30d
Panel A. Isapre enrollees at private hospitals			
Concurrent deliveries (IV)	-0.0123	-0.0047	-0.0039
	[0.0082]	[0.0033]	[0.0038]
Mean DV	0.551	0.014	0.011
Observations	22,760	23,543	23,544
Panel B. Fonasa A enrollees at public hospitals			
Concurrent deliveries (IV)	-0.0071	-0.0034	-0.0028
	[0.0055]	[0.0024]	[0.0017]
Mean DV	0.250	0.078	0.031
Observations	100,707	102,392	102,425

Note: The dependent variable is listed at the head of each column. The endogenous variable is the number of other women delivering at the same hospital in the same ISO birth week (X_{iw}), instrumented by the forecasted deliveries Z_{iw} described in the text. Absorbs hospital × month-year and maternal age fixed effects. Mean DV denotes the average of the dependent variable in the estimation sample. Standard errors clustered at the hospital level in brackets. Sample: 2003q1–2005q4 (2002 excluded because undercoverage biases within-sample instrument counts). *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Congestion effects are unlikely to be exactly zero in a market of shared, capacity-constrained hospitals. Of the eighteen spillover coefficients in Tables C.3, C.4 and C.6, three reach significance at the 10 percent level: the largest is about 14 percent of the corresponding effect of private-hospital delivery, and the other two are under 5 percent. A channel of that size does not account for our results.

D Tables and Figures

D.1 Summary Statistics

Table D.1: Descriptive statistics: Long-term outcomes by insurance type

	Fonasa A	Fonasa B	Fonasa C	Fonasa D	Priv. Ins.	All
Panel A. Cesarean scar hospitalization						
3 years	0.005	0.008	0.013	0.025	0.047	0.014
	(0.068)	(0.091)	(0.112)	(0.155)	(0.211)	(0.117)
6 years	0.013	0.022	0.026	0.054	0.092	0.031
	(0.111)	(0.145)	(0.160)	(0.225)	(0.289)	(0.173)
9 years	0.022	0.034	0.038	0.074	0.117	0.044
	(0.146)	(0.182)	(0.191)	(0.262)	(0.322)	(0.206)
12 years	0.029	0.044	0.045	0.084	0.129	0.053
	(0.168)	(0.206)	(0.207)	(0.278)	(0.336)	(0.224)
15 years	0.037	0.052	0.051	0.093	0.136	0.061
	(0.189)	(0.223)	(0.221)	(0.290)	(0.343)	(0.239)
Panel B. Hysterectomy						
3 years	0.000	0.000	0.000	0.000	0.001	0.000
	(0.016)	(0.017)	(0.017)	(0.021)	(0.024)	(0.018)
6 years	0.001	0.001	0.001	0.002	0.003	0.001
	(0.028)	(0.032)	(0.035)	(0.049)	(0.055)	(0.037)
9 years	0.003	0.004	0.005	0.008	0.011	0.005
	(0.052)	(0.062)	(0.071)	(0.087)	(0.104)	(0.069)
12 years	0.006	0.009	0.010	0.016	0.023	0.010
	(0.078)	(0.096)	(0.101)	(0.124)	(0.151)	(0.102)
15 years	0.011	0.016	0.018	0.027	0.040	0.018
	(0.106)	(0.126)	(0.133)	(0.162)	(0.197)	(0.135)
Observations	129,577	48,196	28,033	31,992	35,853	273,651

Note: This table reports the share of mothers hospitalized for cesarean scar complications (Panel A) and for hysterectomy (Panel B) at 3, 6, 9, 12, and 15 years after the first birth, by insurance type at the time of the first birth. The sample includes in-hospital, singleton first births to women aged 15–47 with public or private insurance between 2002 and 2005.

Table D.2: Descriptive statistics: Subsequent fertility by insurance type

	Fonasa A	Fonasa B	Fonasa C	Fonasa D	Priv. Ins.	All
Panel A. 2 or more children						
3 years	0.152	0.138	0.141	0.153	0.260	0.163
	(0.359)	(0.345)	(0.348)	(0.360)	(0.439)	(0.369)
6 years	0.403	0.375	0.378	0.393	0.509	0.408
	(0.490)	(0.484)	(0.485)	(0.488)	(0.500)	(0.491)
9 years	0.594	0.560	0.559	0.556	0.630	0.585
	(0.491)	(0.496)	(0.497)	(0.497)	(0.483)	(0.493)
12 years	0.707	0.673	0.668	0.648	0.690	0.688
	(0.455)	(0.469)	(0.471)	(0.478)	(0.463)	(0.463)
Panel B. 3 or more children						
3 years	0.004	0.003	0.003	0.004	0.006	0.004
	(0.064)	(0.056)	(0.058)	(0.059)	(0.079)	(0.064)
6 years	0.058	0.044	0.041	0.040	0.078	0.054
	(0.233)	(0.204)	(0.197)	(0.197)	(0.269)	(0.226)
9 years	0.151	0.117	0.113	0.102	0.151	0.135
	(0.358)	(0.321)	(0.316)	(0.303)	(0.358)	(0.342)
12 years	0.248	0.198	0.188	0.168	0.204	0.218
	(0.432)	(0.399)	(0.391)	(0.374)	(0.403)	(0.413)
Observations	129,577	48,196	28,033	31,992	35,853	273,651

Note: This table reports the probability of having a second child and a third child within 3, 6, 9 and 12 years of the first birth, by insurance type at the time of the first birth. The sample includes in-hospital, singleton first births to women aged 15–47 with public or private insurance between 2002 and 2005.

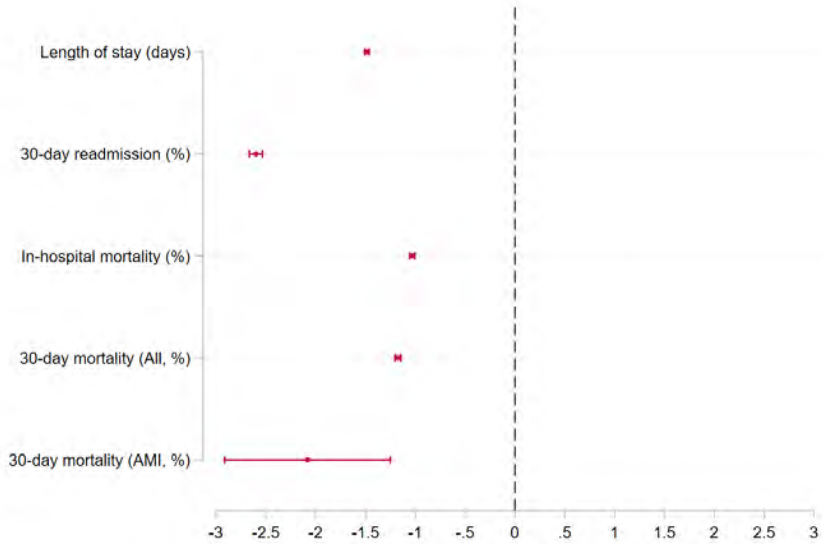
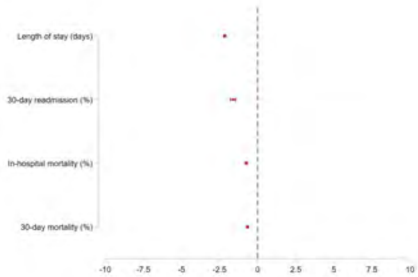
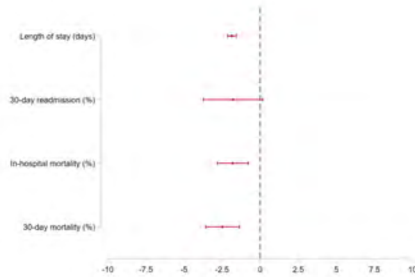


Figure D.1: Risk-adjusted hospital outcomes: Private vs. public hospitals, 2012–2016

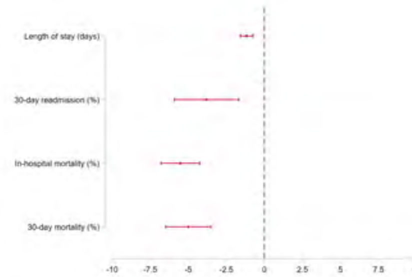
Note: This figure plots 95% confidence intervals for the coefficient on the private hospital indicator from regressions where the outcome (listed on the x-axis) is regressed on a private hospital dummy and controls for the Charlson comorbidity index, age, sex, principal diagnosis, and year fixed effects.



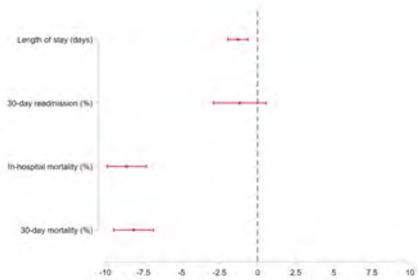
(a) Emergency surgery



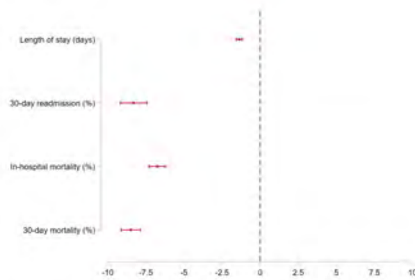
(b) Acute myocardial infarction



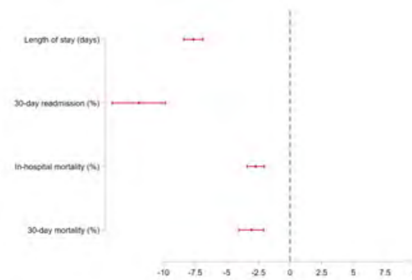
(c) Heart failure



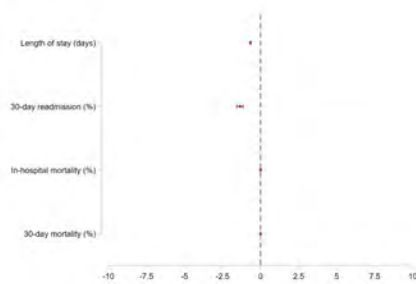
(d) Stroke



(e) Pneumonia



(f) Hip fracture



(g) Delivery

Figure D.2: Risk-adjusted hospital outcomes: Private vs. public hospitals by condition, 2012–2016

Note: These figures show 95% confidence intervals for the coefficient on the private hospital indicator from regressions where the outcome (listed on the x-axis) is regressed on a private hospital dummy and controls for the Charlson comorbidity index, age, sex, principal diagnosis, and year fixed effects.

Table D.3: Share of vaginal delivery after cesarean (VBAC), by hospital of second birth

	Public	Private	All
Share VBAC at 2nd birth	0.281	0.081	0.204
	(31,924)	(19,981)	(51,905)

Note: Cell entries are the share of second births that were vaginal; the number of second births is reported in parentheses. Sample restricted to women whose first birth was a cesarean and who have an observed second birth. VBAC is defined as a vaginal delivery at the second birth. Hospital classification refers to the hospital of the second birth.

D.2 Length of stay

Table D.4 reports IV estimates for length of stay in days (Panel A) and for indicators at several cutoffs (Panels B–E). Private-hospital delivery shortens the stay by 1.44 days, about 40 percent of the sample mean. The probability of a three-day stay rises, while stays of four days or more fall at every cutoff.

Part of the reduction comes from when women are admitted, not only from earlier discharge. We split the stay into an admission-to-birth segment and a birth-to-discharge segment (Table D.5). Both shorten, but the admission side falls by more.[39]

Stays of seven days or more are rare (5.9 percent) and lie in the upper tail. In a large U.S. multicenter study, Blumenfeld et al. (2015) attribute such stays to perioperative complications such as ileus (delayed return of bowel function), endometritis, and wound complications.

[39] Card et al. (2023) carry out a similar admission/discharge decomposition for U.S. hospitals.

Table D.4: IV estimates: length of stay and prolonged-hospitalization cutoffs

	OLS	IV Fonasa B, C, D			
	(1)	(2)	(3)	(4)	(5)
Panel A. Length of stay (days, continuous)					
Private Hospital	-0.585***	-1.437***	-1.413***	-1.437***	-1.329***
	[0.096]	[0.148]	[0.147]	[0.150]	[0.159]
Mean DV	3.593	3.593	3.593	3.593	3.593
Panel B. Length of stay = 3 days					
Private Hospital	0.175***	0.206***	0.209***	0.195***	0.191***
	[0.029]	[0.046]	[0.046]	[0.041]	[0.040]
Mean DV	0.400	0.400	0.400	0.400	0.400
Panel C. Prolonged hospitalization (≥5 days)					
Private Hospital	-0.122***	-0.257***	-0.254***	-0.252***	-0.241***
	[0.009]	[0.026]	[0.026]	[0.026]	[0.027]
Mean DV	0.159	0.159	0.159	0.159	0.159
Panel D. Prolonged hospitalization (≥7 days, as in the main tables)					
Private Hospital	-0.050***	-0.094***	-0.091***	-0.090***	-0.082***
	[0.004]	[0.013]	[0.013]	[0.013]	[0.014]
Mean DV	0.059	0.059	0.059	0.059	0.059
Panel E. Prolonged hospitalization (≥10 days)					
Private Hospital	-0.020***	-0.032***	-0.031***	-0.032***	-0.033***
	[0.002]	[0.007]	[0.007]	[0.008]	[0.009]
Mean DV	0.023	0.023	0.023	0.023	0.023
Age-Year FE			X	X	X
Municipality-Age FE				X	X
Municipality-Year FE				X	X
Linear trend					X
Observations	273,586	273,586	273,584	273,403	273,403

Note: IV estimates of the effect of private-hospital delivery on length of stay at the first birth. Panel A is length of stay in days; Panel B is an indicator for a stay of exactly three days; Panels C–E are indicators for stays of at least five, seven, and ten days. Column (1) reports OLS; columns (2)–(5) report IV estimates with increasingly rich fixed effects. All regressions include month-year of birth and maternal age fixed effects. Endogenous variable: private hospital. Instruments: Fonasa groups B, C, and D times Post (April 2003). Standard errors, clustered at the municipality of residence, in brackets. *** p<0.01, ** p<0.05, * p<0.10.

Table D.5: IV estimates: length of stay split into admission-to-birth and birth-to-discharge segments

	OLS	IV Fonasa B, C, D			
	(1)	(2)	(3)	(4)	(5)
Panel A. Length of stay: admission-to-birth segment (days)					
Private Hospital	-0.528***	-0.929***	-0.933***	-0.994***	-0.876***
	[0.047]	[0.136]	[0.139]	[0.151]	[0.159]
Mean DV	0.823	0.823	0.823	0.823	0.823
Observations	262,191	262,191	262,188	262,000	262,000
Panel B. Length of stay: birth-to-discharge segment (days)					
Private Hospital	-0.074	-0.635***	-0.610***	-0.572***	-0.548***
	[0.070]	[0.095]	[0.092]	[0.098]	[0.110]
Mean DV	2.804	2.804	2.804	2.804	2.804
Observations	262,191	262,191	262,188	262,000	262,000
Age-Year FE			X	X	X
Municipality-Age FE				X	X
Municipality-Year FE				X	X
Linear trend					X

Note: IV estimates of the effect of private-hospital delivery on the two components of length of stay, in days. Panel A is the admission-to-birth segment; Panel B is the birth-to-discharge segment. Both segments are estimated on the subsample for which the two intervals are non-negative. Column (1) reports OLS; columns (2)–(5) report IV estimates with increasingly rich fixed effects. All regressions include month-year of birth and maternal age fixed effects. Endogenous variable: private hospital. Instruments: Fonasa groups B, C, and D times Post (April 2003). Standard errors, clustered at the municipality of residence, in brackets. *** p<0.01, ** p<0.05, * p<0.10.

D.3 Decomposition of the cesarean-scar effect

A scar complication requires a prior uterine scar. Decomposing the unconditional outcome S by first-birth delivery mode $C \in \{0, 1\}$ gives

$$\Pr(S = 1) = \Pr(C = 1)\ \Pr(S = 1 \mid C = 1) + \Pr(C = 0)\ \Pr(S = 1 \mid C = 0).$$

Differentiating with respect to private-hospital delivery and identifying each piece by 2SLS yields

$$\beta_S = \underbrace{\beta_C \cdot [\Pr(S{=}1 \mid C{=}1) - \Pr(S{=}1 \mid C{=}0)]}_{\text{extensive (more cesareans)}} + \underbrace{\Pr(C{=}1) \cdot \beta_{S|C=1}}_{\text{intensive (per-CS)}} + \underbrace{\Pr(C{=}0) \cdot \beta_{S|C=0}}_{\text{per-vaginal}}.$$

Table D.6 reports the inputs. Columns (2) and (3) condition on a post-treatment outcome shifted by the instrument, so the coefficients are not interpretable as LATE estimates of the per-CS or per-vaginal effect; they are reported only to supply the inputs to the decomposition above.

Table D.6: Decomposition inputs: outcome means and 2SLS estimates by first-birth delivery mode

	(1) All deliveries	(2) CS=1 only	(3) CS=0 only
Private Hospital	0.057***	0.084***	0.010***
	[0.009]	[0.018]	[0.003]
Mean DV	0.014	0.037	0.002
Observations	273,651	91,302	176,949

Note: IV estimates of the effect of private-hospital delivery on the cesarean-scar outcome (any O34.2 diagnosis within three years of the first birth). Column (1) uses the full sample; column (2) restricts to women whose first birth was by cesarean; column (3) restricts to women whose first birth was vaginal. Endogenous variable: private hospital. Instruments: Fonasa groups B, C, and D times Post (April 2003). Fixed effects: month-year of birth and age at first birth, the paper's baseline specification. Standard errors clustered at the municipality of residence. Columns (2) and (3) condition on a post-treatment outcome shifted by the instrument and are reported only as inputs to the decomposition; they do not identify LATE. The decomposition below evaluates the scar rates by delivery mode at private hospitals. *** p<0.01, ** p<0.05, * p<0.10.

Plugging in the estimates from Table D.6 together with the first-stage cesarean coefficient $\beta_C = 0.45$ from Table 7 and the sample share $\Pr(C{=}1) = 0.34$: the extensive margin contributes $0.45 \cdot (0.037 - 0.002) \approx 0.016$ $0.45 \cdot (0.070 - 0.010) \approx 0.027$, the per-vaginal channel contributes $0.66 \cdot 0.010 \approx 0.007$, and the residual intensive (per-CS) channel accounts for the remaining ≈ 0.043 ≈ 0.023 of the 0.066 0.057 IV effect. one quarter About half of the unconditional scar effect comes from additional cesareans at private hospitals, and most of the remainder from a higher scar rate within each mode of delivery.

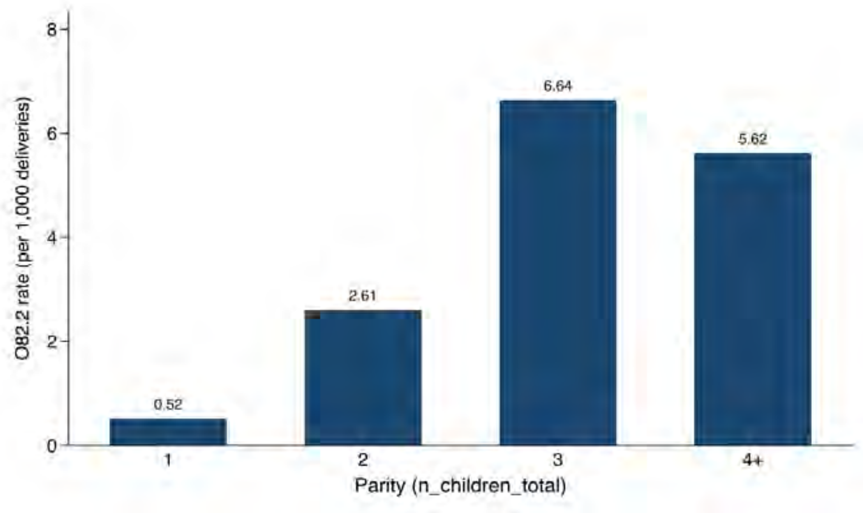


Figure D.3: Cesarean hysterectomy (ICD-10 O82.2) per 1,000 deliveries, by parity

Note: Rate of cesarean hysterectomy (ICD-10 O82.2) per 1,000 deliveries, by parity of the delivery. Sample: all hospital deliveries with principal diagnosis O80–O84 in Egresos Hospitalarios (DEIS), 2002–2018. Parity 4 groups all higher orders.

D.4 IV and reduced form estimates

D.4.1 Postpartum wound complications

This subsection presents the dynamic counterparts to the wound-complication estimates reported in Table 6. Figures D.4 and D.5 plot the reduced-form effect of the reform on the probability of a wound complication within 30 days of the first birth, and on the perineal-wound subset, for each Fonasa group relative to the Fonasa A and Isapre comparison group. The decline emerges after the reform and is concentrated in Groups D and C, the groups most exposed to the reform, consistent with the substitution of cesarean for vaginal delivery discussed in the text.

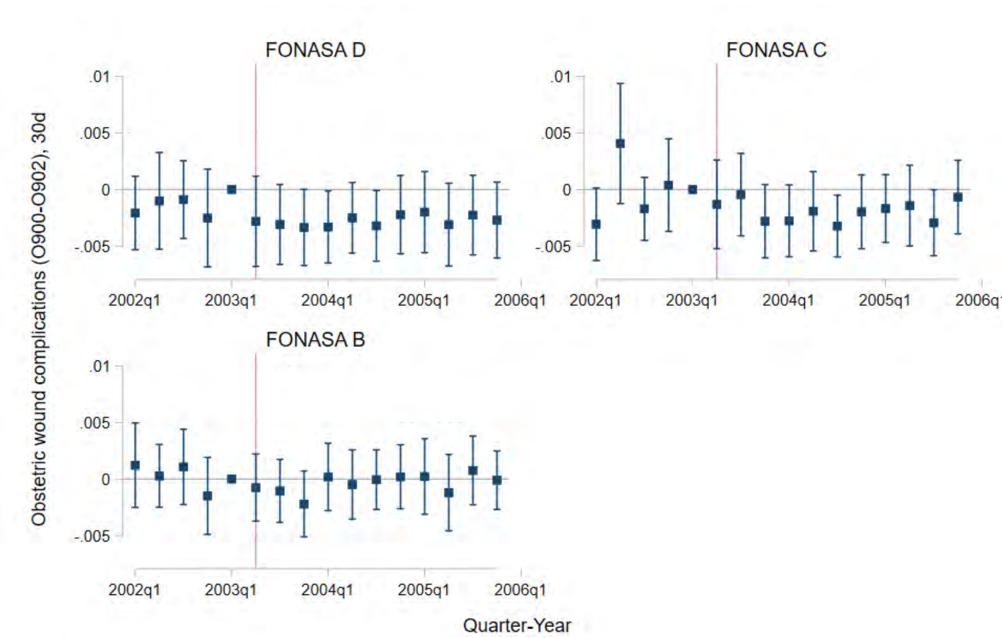


Figure D.4: Dynamic effects of the policy on 30-day wound complications

Note: Each panel plots reduced-form coefficients on the interaction of a Fonasa group indicator with quarter dummies, relative to the reference quarter 2003q1, from a regression of an indicator for any wound complication (ICD-10 O90.0, O90.1, O90.2) within 30 days of the first birth. The omitted comparison group is Fonasa A and Isapre. The specification includes month-year of birth and maternal age fixed effects. Vertical lines represent 95% confidence intervals; the vertical line marks the reform quarter.

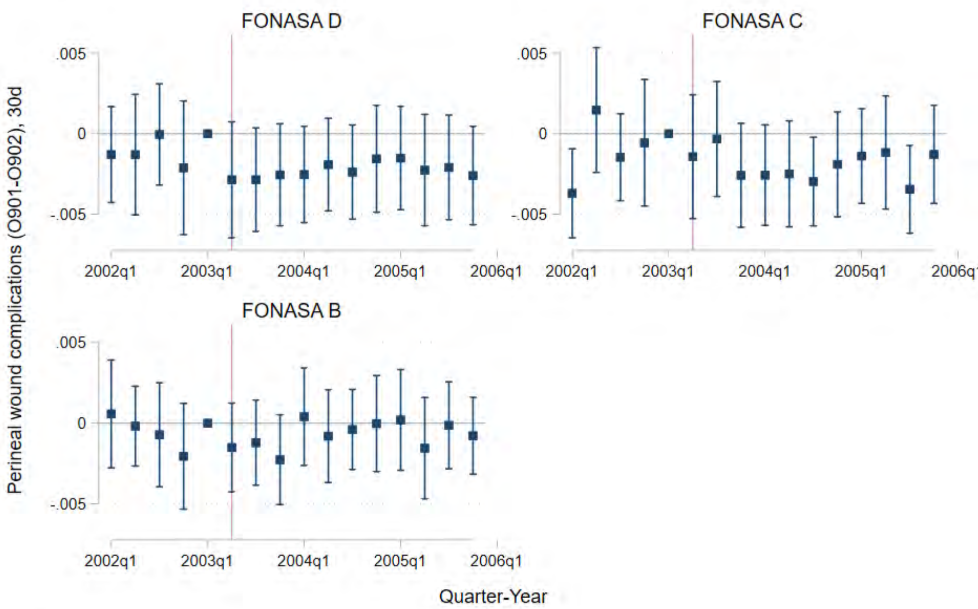


Figure D.5: Dynamic effects of the policy on 30-day perineal-wound complications

Note: Each panel plots reduced-form coefficients on the interaction of a Fonasa group indicator with quarter dummies, relative to the reference quarter 2003q1, from a regression of an indicator for a perineal-wound complication (ICD-10 O90.1, O90.2) within 30 days of the first birth. The omitted comparison group is Fonasa A and Isapre. The specification includes month-year of birth and maternal age fixed effects. Vertical lines represent 95% confidence intervals; the vertical line marks the reform quarter.

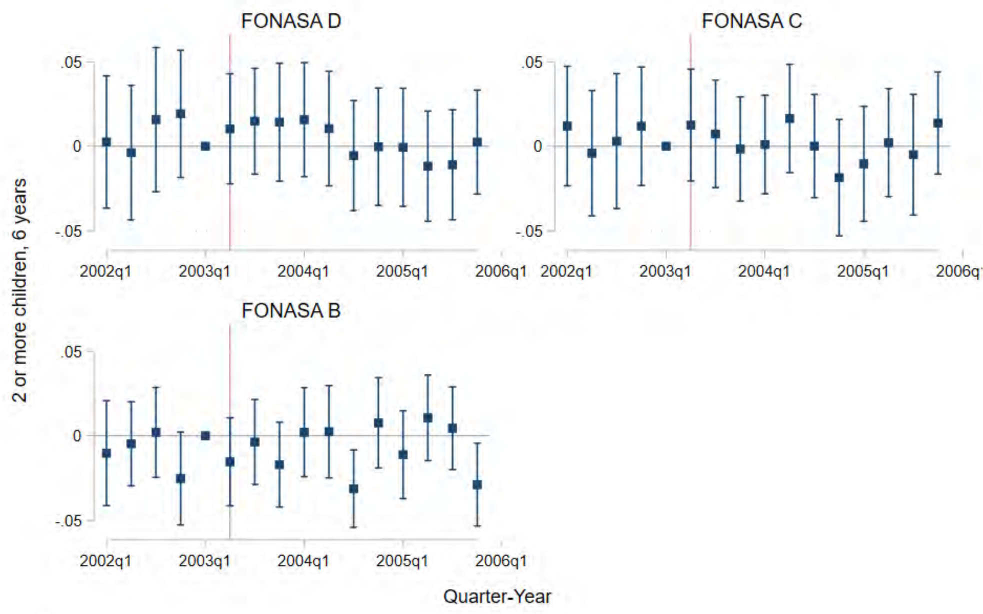
D.4.2 Hysterectomy

Table D.7: Effect of delivery at private hospital on cesarean hysterectomy (ICD-10 O82.2): peripartum vs. subsequent pregnancy

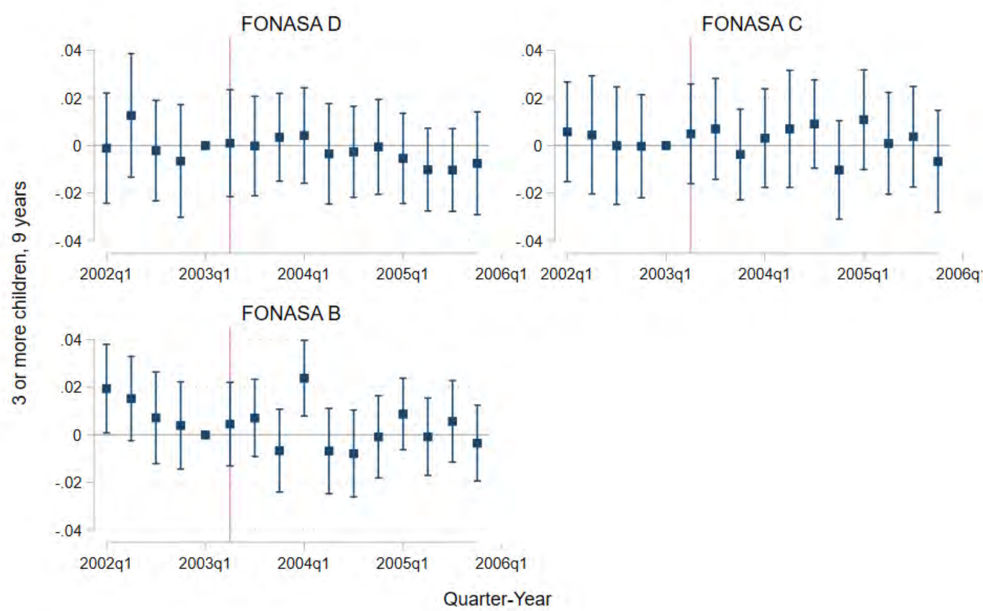
	OLS	IV Fonasa B, C, D			
	(1)	(2)	(3)	(4)	(5)
Panel A. O82.2 peripartum at first birth ($\leq$ 1 year)					
Private Hospital	-0.000	0.002	0.002	0.002	0.002
	[0.000]	[0.001]	[0.002]	[0.002]	[0.001]
Mean DV	0.0003	0.0003	0.0003	0.0003	0.0003
Observations	273,651	273,651	273,649	273,468	273,468
Panel B. O82.2 subsequent pregnancy ($>$ 1 year, $<$ 15 years)					
Private Hospital	0.003	0.006**	0.005**	0.006*	0.007*
	[0.002]	[0.002]	[0.002]	[0.003]	[0.004]
Mean DV	0.003	0.003	0.003	0.003	0.003
Observations	273,651	273,651	273,649	273,468	273,468
Age-Year FE			X	X	X
Municipality-Age FE				X	X
Municipality-Year FE				X	X
Linear trend					X

Note: OLS and IV estimates of the effect of delivery at a private hospital on cesarean hysterectomy (ICD-10 O82.2), main sample, restricted to events before age 35 within 15 years of the first birth. Panel A restricts to O82.2 recorded within 1 year of the first birth (peripartum). Panel B restricts to O82.2 recorded more than 1 year and within 15 years after the first birth (subsequent pregnancy). Instruments are Fonasa B, C, and D dummies interacted with the post-reform indicator. All specifications include month-year of birth and mother's age fixed effects; columns (3)–(5) add age $\times$ year-of-birth interactions, columns (4)–(5) add municipality $\times$ age and municipality $\times$ year-of-birth interactions, and column (5) adds a Fonasa A–specific linear time trend. Mean DV is the sample mean of the dependent variable. Standard errors clustered at the municipality of residence in brackets. ***, **, * indicate significance at the 1%, 5%, and 10% level.

D.4.3 Fertility



(a) Probability of having two children or more, 6 years after first birth



(b) Probability of having three children or more, 9 years after first birth

Figure D.6: Dynamic effects of the policy on subsequent fertility after first birth

Note: These figures plot estimated coefficients from equation (3), where each point represents the interaction between quarter dummies and insurance group status (Fonasa B, C, or D), relative to the reference period (the quarter preceding the PAD reform). The dependent variables are an indicator for having two children or more 6 years after first birth and an indicator for having three children or more 9 years after first birth. Vertical lines represent 95% confidence intervals. Fonasa A and Isapre beneficiaries serve as the comparison group. Standard errors are clustered at the municipality of residence.

D.5 Placebos

Table D.8: Placebo test: Maternal age

	OLS	IV Fonasa B, C, D		
	(1)	(2)	(3)	(4)
Private Hospital	2.093***	-0.293	-0.376	-0.567
	[0.159]	[0.431]	[0.440]	[0.477]
Mean DV	22.725	22.725	22.725	22.725
Observations	273,651	273,651	273,621	273,621
Month-Year FE	X	X	X	X
Municipality-Year FE			X	X
Linear Trend				X

Note: This table reports OLS and IV estimates of the effect of delivering at a private hospital on maternal age at first birth–a predetermined characteristic. Columns (2) to (4) report IV estimates using individual insurance type as the instrument. All columns include month-year of birth fixed effects; mother's age is the dependent variable here, so it is not absorbed. Linear trend is a linear trend for Fonasa A. Mean DV denotes the average of the dependent variable in the estimation sample. Standard errors clustered at the municipality of residence are reported in brackets.
*** Significant at the 1 percent level. ** Significant at the 5 percent level.
* Significant at the 10 percent level.

Table D.9: Placebo test: Maternal education and employment

	OLS	IV Fonasa B, C, D			
	(1)	(2)	(3)	(4)	(5)
Panel A. Mother's years of education					
Private Hospital	1.093***	0.538***	0.413***	0.359**	0.050
	[0.092]	[0.150]	[0.149]	[0.143]	[0.165]
Mean DV	11.545	11.545	11.545	11.545	11.545
Observations	273,639	273,639	273,637	273,456	273,456
Panel B. Mother employed					
Private Hospital	0.124***	0.027	0.011	0.008	0.030
	[0.008]	[0.025]	[0.026]	[0.028]	[0.032]
Mean DV	0.293	0.293	0.293	0.293	0.293
Observations	273,651	273,651	273,649	273,468	273,468
Age-Year FE			X	X	X
Municipality-Age FE				X	X
Municipality-Year FE				X	X
Linear trend					X

Note: All columns include month-year of birth and mother's age fixed effects; the rows below indicate additional fixed effects. Linear trend is a linear trend for Fonasa A. Mean DV denotes the average of the dependent variable in the estimation sample. Standard errors clustered at the municipality of residence are reported in brackets.
*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

D.6 Monotonicity: testing the no-defiers condition

Monotonicity requires that the reform moved no woman away from private delivery. This condition has a testable implication (Imbens and Angrist, 1994): the first stage must be weakly positive within every subgroup defined by predetermined covariates, since a significantly negative first stage in any such subgroup would signal defiers. Figure D.7 reports this check. We re-estimate the first stage—a regression of an indicator for private-hospital delivery on the three subtier interactions (Fonasa B, C, and D, each interacted with the post-reform indicator), with month-year of birth and maternal-age fixed effects and standard errors clustered at the municipality of residence—separately within subgroups defined by maternal age (under 25, 25–34, 35 and over), urban versus rural residence, and terciles of residential access to private hospitals. The access measure is the population-weighted count of PAD private maternity hospitals within 75 km of the mother's municipality of residence, computed from pre-reform data and therefore predetermined; its lowest tercile identifies the women with the least private-sector access, for whom public-sector decongestion would be most likely to induce retention. Because the first stage is a difference-in-differences, each subgroup retains both treated (Fonasa B, C, and D) and comparison (Fonasa A and Isapre) mothers, so the subgroups partition the full sample rather than the treated group alone. Each cell yields three coefficients, one per subtier.

The point estimate is positive in every cell. As in the pooled first stage, the effect is largest for Fonasa D (about 0.28), intermediate for C (about 0.12), and smallest for B (about 0.03), and this ordering holds across all subgroups. In the stress case of lowest private access, the estimates are 0.02 (B), 0.12 (C), and 0.27 (D), all positive. That cell holds the only interval that includes zero, for Fonasa B—the weakest instrument in the cell with the least private capacity to switch into—and its point estimate stays positive, so this is not a sign reversal. This pattern is consistent with the no-defiers condition. The implication is necessary but not sufficient—averaging within cells could in principle conceal a small number of defiers—but, read together with the economic argument in Section 4, it provides direct support for monotonicity.

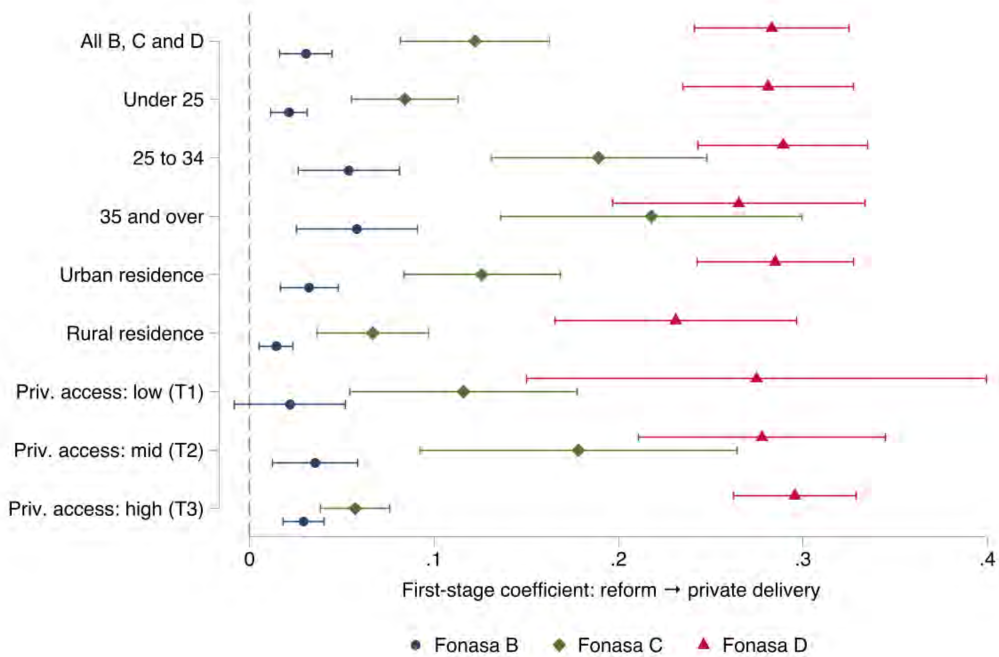


Figure D.7: Monotonicity check: first-stage estimates within predetermined subgroups
Note: Dots are first-stage coefficients and whiskers are 95% confidence intervals for the effect of the reform on the probability of private-hospital delivery, estimated separately within the subgroups listed on the vertical axis. Each subgroup is estimated with the three subtier instruments (Fonasa B, C, and D, each interacted with the post-reform indicator), with month-year of birth and maternal-age fixed effects and standard errors clustered at the municipality of residence. The no-defiers (monotonicity) condition implies a weakly positive first stage in every subgroup.

D.7 Alternative treatment/control definitions

Table D.10: Robustness: Short-run outcomes using alternative treatment/control definitions

	Main sample		Fonasa only	
	(1) Main	(2) B, C, D + Private	(3) Main	(4) B + C only
Panel A. Cesarean section, first birth				
Private Hospital	0.443***	0.438***	0.378***	0.517***
	[0.036]	[0.070]	[0.038]	[0.081]
Mean DV	0.340	0.340	0.340	0.340
Observations	268,250	268,250	233,511	202,238
Panel B. 7+ days hospitalization				
Private Hospital	-0.091***	-0.155***	-0.084***	-0.173***
	[0.013]	[0.031]	[0.014]	[0.033]
Mean DV	0.059	0.059	0.059	0.059
Observations	273,584	273,584	237,738	205,751
Panel C. 30-day readmission				
Private Hospital	-0.025***	-0.061***	-0.027***	-0.060***
	[0.007]	[0.020]	[0.009]	[0.022]
Mean DV	0.025	0.025	0.025	0.025
Observations	273,649	273,649	237,797	205,804

Note: This table reports IV estimates using alternative treatment and control definitions. Columns (1) and (2) use the full sample of women with public or private insurance. Column (1) follows our benchmark treatment and control definition, instrumenting private hospital delivery with enrollment in Fonasa groups B, C, or D. Column (2) extends the treatment group to include privately insured women. Columns (3) and (4) restrict the sample to publicly insured women. Column (3) excludes private insurance, while Column (4) further excludes Fonasa Group D, retaining only Groups A, B, and C.
All specifications include month-year fixed effects and age of mother-year of birth interactions. Standard errors clustered at the municipality of residence are reported in brackets.
*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table D.11: Robustness: C-section in the first and second births using alternative treatment/control definitions

	Main sample		Fonasa only	
	(1) Main	(2) B, C, D + Private	(3) Main	(4) B + C only
Panel A. C-section in first birth				
Private Hospital	0.452***	0.404***	0.383***	0.622***
	[0.048]	[0.094]	[0.049]	[0.112]
Mean DV	0.317	0.317	0.317	0.317
Observations	163,986	163,986	145,186	127,475
Panel B. C-section in second birth				
Private Hospital	0.338***	0.318***	0.289***	0.424***
	[0.035]	[0.089]	[0.036]	[0.111]
Mean DV	0.419	0.419	0.419	0.419
Observations	163,986	163,986	145,186	127,475
Panel C. Repeat C-section				
Private Hospital	0.478***	0.460***	0.412***	0.637***
	[0.042]	[0.087]	[0.042]	[0.113]
Mean DV	0.252	0.252	0.252	0.252
Observations	163,986	163,986	145,186	127,475

Note: This table reports IV estimates using alternative treatment and control definitions. Columns (1) and (2) use the full sample of women with public or private insurance. Column (1) follows our benchmark treatment and control definition, instrumenting private hospital delivery with enrollment in Fonasa groups B, C, or D. Column (2) extends the treatment group to include privately insured women. Columns (3) and (4) restrict the sample to publicly insured women. Column (3) excludes private insurance, while Column (4) further excludes Fonasa Group D, retaining only Groups A, B, and C.
All specifications include month-year fixed effects and age of mother-year of birth interactions. Standard errors clustered at the municipality of residence are reported in brackets.
*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table D.12: Robustness: Hospitalizations due to obstetric complications using alternative treatment/control definitions

	Main sample		Fonasa only	
	(1)	(2)	(3)	(4)
	Main	B, C, D + Private	Main	B + C only
Panel A. Cesarean scar, 3 years				
Private Hospital	0.061***	0.042***	0.048***	0.056***
	[0.009]	[0.015]	[0.009]	[0.016]
Mean DV	0.014	0.014	0.014	0.014
Observations	273,649	273,649	237,797	205,804
Panel B. Hysterectomy, age $\leq$ 35, 15 years				
Private Hospital	0.010**	0.002	0.010**	0.002
	[0.005]	[0.009]	[0.005]	[0.010]
Mean DV	0.007	0.007	0.007	0.007
Observations	273,649	273,649	237,797	205,804

Note: This table reports IV estimates using alternative treatment and control definitions. Columns (1) and (2) use the full sample of women with public or private insurance. Column (1) follows our benchmark treatment and control definition, instrumenting private hospital delivery with enrollment in Fonasa groups B, C, or D. Column (2) extends the treatment group to include privately insured women. Columns (3) and (4) restrict the sample to publicly insured women. Column (3) excludes private insurance, while Column (4) further excludes Fonasa Group D, retaining only Groups A, B, and C.
All specifications include month-year fixed effects and age of mother-year of birth interactions. Standard errors clustered at the municipality of residence are reported in brackets.
*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table D.13: Robustness: Subsequent fertility using alternative treatment/control definitions

	Main sample		Fonasa only	
	(1) Main	(2) B, C, D + Private	(3) Main	(4) B + C only
Panel A. 2 or more children, 6 years				
Private Hospital	0.005	-0.073	-0.004	-0.016
	[0.025]	[0.060]	[0.027]	[0.065]
Mean DV	0.408	0.408	0.408	0.408
Observations	273,649	273,649	237,797	205,804
Panel B. 3 or more children, 9 years				
Private Hospital	-0.005	0.017	0.009	0.033
	[0.019]	[0.040]	[0.020]	[0.043]
Mean DV	0.135	0.135	0.135	0.135
Observations	273,649	273,649	237,797	205,804

Note: This table reports IV estimates using alternative treatment and control definitions. Columns (1) and (2) use the full sample of women with public or private insurance. Column (1) follows our benchmark treatment and control definition, instrumenting private hospital delivery with enrollment in Fonasa groups B, C, or D. Column (2) extends the treatment group to include privately insured women. Columns (3) and (4) restrict the sample to publicly insured women. Column (3) excludes private insurance, while Column (4) further excludes Fonasa Group D, retaining only Groups A, B, and C.

All specifications include month-year fixed effects and age of mother-year of birth interactions. Standard errors clustered at the municipality of residence are reported in brackets.

*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table D.14: Robustness: IV estimates using Fonasa D $\times$ Post as the sole instrument

	Baseline	Fonasa A + D only	Fonasa A + D + Isapre
	(1)	(2)	(3)
Panel A. Cesarean section			
Private Hospital	0.446***	0.394***	0.433***
	[0.037]	[0.039]	[0.038]
Mean DV	0.340	0.302	0.347
Observations	268,252	158,613	193,354
Panel B. 7+ days hospitalization			
Private Hospital	-0.094***	-0.087***	-0.086***
	[0.013]	[0.013]	[0.012]
Mean DV	0.059	0.068	0.058
Observations	273,586	161,528	197,376
Panel C. 30-day readmission			
Private Hospital	-0.025***	-0.027***	-0.024***
	[0.007]	[0.008]	[0.007]
Mean DV	0.025	0.027	0.024
Observations	273,651	161,568	197,422
Panel D. Cesarean scar, 3 years			
Private Hospital	0.057***	0.047***	0.056***
	[0.009]	[0.010]	[0.010]
Mean DV	0.014	0.009	0.016
Observations	273,651	161,568	197,422
Panel E. Hysterectomy, age $\leq$ 35			
Private Hospital	0.010**	0.010**	0.011**
	[0.004]	[0.004]	[0.005]
Mean DV	0.007	0.007	0.007
Observations	273,651	161,568	197,422

Note: Column (1) reproduces the paper's baseline specification on the full estimation sample. Columns (2) and (3) use only Fonasa D $\times$ Post as the instrument; column (2) restricts the sample to Fonasa A and Fonasa D mothers, and column (3) adds Isapre mothers. All regressions include month-year of birth and mother's age fixed effects and dummies for the included insurance groups; standard errors clustered at the municipality of residence are reported in brackets.

*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

D.8 IV and reduced form estimates with share Fonasa B, C or D

Table D.15: First stage estimates

	Private Hospital	
	(1)	(2)
Share Fonasa B, C, or D * post	0.211***	0.215***
	[0.041]	[0.045]
Month-Year FE	X	X
Age mother FE	X	X
Municipality-Age FE	X	X
Age-Year FE		X
Effective F-stat	26.9	22.7
Mean DV	0.191	0.191
Observations	273,498	273,493

Note: Share Fonasa B, C, or D is the 2018 share of the population enrolled in Fonasa groups B, C, or D, measured by age group and municipality of residence. Effective F-stat is the effective F-statistic proposed by Montiel Olea and Pflueger (2013) to test the presence of weak instruments. Mean DV is the average of the dependent variable in the estimation sample. All columns include month-year of birth, mother's age and municipality-age fixed effects. Standard errors clustered at the municipality of residence level.
*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table D.16: Effect of delivery at private hospital on short-term maternal outcomes: OLS and IV estimates with share Fonasa B, C, or D

	OLS	IV Fonasa B, C, D		IV Share B, C, D	
	(1)	(2)	(3)	(4)	(5)
Panel A. Cesarean section, first birth					
Private Hospital	0.234***	0.450***	0.452***	0.760***	0.747***
	[0.021]	[0.043]	[0.043]	[0.133]	[0.155]
Mean DV	0.340	0.340	0.340	0.340	0.340
Observations	268,088	268,093	268,088	268,093	268,088
Panel B. 7+ days hospitalization					
Private Hospital	-0.053***	-0.095***	-0.094***	-0.150***	-0.167***
	[0.005]	[0.013]	[0.013]	[0.048]	[0.053]
Mean DV	0.059	0.059	0.059	0.059	0.059
Observations	273,428	273,433	273,428	273,433	273,428
Panel C. 30-day readmission					
Private Hospital	-0.010***	-0.025***	-0.025***	-0.082**	-0.093***
	[0.001]	[0.008]	[0.008]	[0.032]	[0.035]
Mean DV	0.025	0.025	0.025	0.025	0.025
Observations	273,493	273,498	273,493	273,498	273,493
Month-Year FE	X	X	X	X	X
Municipality-Age FE	X	X	X	X	X
Age-Year FE	X		X		X

Note: Columns (2) and (3) report IV estimates using individual insurance type as the instrument. Columns (4) and (5) use the 2018 share of the population enrolled in Fonasa groups B, C, or D, measured by age group and municipality of residence, as the instrument. All columns include month-year of birth and municipality-age fixed effects; columns (2) and (4) add age-by-year-of-birth interactions. Mean DV denotes the average of the dependent variable in the estimation sample. Standard errors clustered at the municipality of residence are reported in brackets.
*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table D.17: Effect of delivery at private hospital on C-section in the first and second births: OLS and IV estimates with share Fonasa B, C, or D

	OLS	IV Fonasa B, C, D		IV Share B, C, D	
	(1)	(2)	(3)	(4)	(5)
Panel A. C-section in first birth					
Private Hospital	0.232***	0.450***	0.450***	1.001***	1.069***
	[0.025]	[0.057]	[0.058]	[0.235]	[0.280]
Mean DV	0.317	0.317	0.317	0.317	0.317
Observations	163,838	163,842	163,838	163,842	163,838
Panel B. C-section in second birth					
Private Hospital	0.201***	0.352***	0.343***	0.664***	0.635***
	[0.013]	[0.036]	[0.037]	[0.198]	[0.220]
Mean DV	0.419	0.419	0.419	0.419	0.419
Observations	163,838	163,842	163,838	163,842	163,838
Panel C. Repeat C-section					
Private Hospital	0.247***	0.486***	0.486***	0.874***	0.903***
	[0.017]	[0.045]	[0.046]	[0.192]	[0.219]
Mean DV	0.252	0.252	0.252	0.252	0.252
Observations	163,838	163,842	163,838	163,842	163,838
Month-Year FE	X	X	X	X	X
Municipality-Age FE	X	X	X	X	X
Age-Year FE	X		X		X

Note: The sample is restricted to women with at least two births within 12 years of the first, for whom the mode of delivery can be identified in both the first and second birth. Columns (2) and (3) report IV estimates using individual insurance type as the instrument. Columns (4) and (5) use the 2018 share of the population enrolled in Fonasa groups B, C, or D, measured by age group and municipality of residence, as the instrument. All columns include month-year of birth and municipality-age fixed effects; columns (2) and (4) add age-by-year-of-birth interactions. Standard errors clustered at the municipality of residence are reported in brackets.
*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table D.18: Effect of delivery at private hospital on hospitalizations due to obstetric complications: OLS and IV estimates with share Fonasa B, C, or D

	OLS	IV Fonasa B, C, D		IV Share B, C, D	
	(1)	(2)	(3)	(4)	(5)
Panel A. Cesarean scar, 3 years					
Private Hospital	0.030***	0.059***	0.064***	0.198***	0.234***
	[0.003]	[0.009]	[0.010]	[0.067]	[0.064]
Mean DV	0.014	0.014	0.014	0.014	0.014
Observations	273,493	273,498	273,493	273,498	273,493
Panel B. Hysterectomy, age $\leq$ 35, 15 years					
Private Hospital	0.002***	0.010**	0.011**	0.017	0.010
	[0.001]	[0.004]	[0.004]	[0.015]	[0.015]
Mean DV	0.007	0.007	0.007	0.007	0.007
Observations	273,493	273,498	273,493	273,498	273,493
Month-Year FE	X	X	X	X	X
Municipality-Age FE	X	X	X	X	X
Age-Year FE	X		X		X

Note: Columns (2) and (3) report IV estimates using individual insurance type as the instrument. Columns (4) and (5) use the 2018 share of the population enrolled in Fonasa groups B, C, or D, measured by age group and municipality of residence, as the instrument. All columns include month-year of birth and municipality-age fixed effects; columns (2) and (4) add age-by-year-of-birth interactions. Mean DV denotes the average of the dependent variable in the estimation sample. Standard errors clustered at the municipality of residence are reported in brackets.
*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table D.19: Effect of delivery at private hospital on subsequent fertility: OLS and IV estimates with share Fonasa B, C, or D

	OLS	IV Fonasa B, C, D		IV Share B, C, D	
	(1)	(2)	(3)	(4)	(5)
Panel A. 2 or more children, 6 years					
Private Hospital	0.038***	-0.013	0.006	-0.096	0.023
	[0.005]	[0.025]	[0.025]	[0.141]	[0.158]
Mean DV	0.408	0.408	0.408	0.408	0.408
Observations	273,493	273,498	273,493	273,498	273,493
Panel B. 3 or more children, 9 years					
Private Hospital	0.011***	-0.011	-0.006	-0.200*	-0.139
	[0.003]	[0.018]	[0.019]	[0.108]	[0.114]
Mean DV	0.135	0.135	0.135	0.135	0.135
Observations	273,493	273,498	273,493	273,498	273,493
Month-Year FE	X	X	X	X	X
Municipality-Age FE	X	X	X	X	X
Age-Year FE	X		X		X

Note: Columns (2) and (3) report IV estimates using individual insurance type as the instrument. Columns (4) and (5) use the 2018 share of the population enrolled in Fonasa groups B, C, or D, measured by age group and municipality of residence, as the instrument. All columns include month-year of birth and municipality-age fixed effects; columns (2) and (4) add age-by-year-of-birth interactions. Mean DV denotes the average of the dependent variable in the estimation sample. Standard errors clustered at the municipality of residence are reported in brackets.
*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

D.9 Alternative definition of Cesarean Section

Table D.20: Alternative definition of Cesarean Section

ICD-10 diagnosis code	C-section = 1	C-section = 0
O820, O821, O822, O828, O829, O842	Yes	No
O700, O701, O702, O703, O709, O714, O757, O800, O801, O808, O809, O810, O811, O812, O813, O814, O815, O830, O831, O832, O833, O834, O838, O839, O840, O841	No	Yes
O848, O849	has_surgery = 1	has_surgery = 0

Note: This table summarizes an alternative, more restrictive, classification of the binary indicator C-section following Jackson et al. (2012), constructed from ICD-10 diagnosis codes recorded at delivery. Codes listed in the first row are always classified as cesarean section (C-section = 1), while codes in the second row are always classified as noncesarean (C-section = 0). For diagnoses O848 and O849, the classification depends on whether a surgical procedure is recorded in the hospital discharge data: the delivery is classified as a C-section when $has\_surgery = 1$, and as noncesarean when $has\_surgery = 0$. All remaining obstetric diagnoses are left unclassified and the mode of delivery is coded as missing.

Table D.21: Robustness: C-section in the first and second births using different definitions of C-sections

	Main definition		Alternative definition	
	(1)	(2)	(3)	(4)
Panel A. Cesarean section, first birth				
Private Hospital	0.452***	0.412***	0.433***	0.382***
	[0.048]	[0.059]	[0.060]	[0.070]
Mean DV	0.317	0.317	0.195	0.195
Observations	163,986	163,797	116,728	116,504
Panel B. C-section in second birth				
Private Hospital	0.338***	0.305***	0.321***	0.270***
	[0.035]	[0.043]	[0.055]	[0.065]
Mean DV	0.419	0.419	0.303	0.303
Observations	163,986	163,797	116,728	116,504
Panel C. Repeat C-section				
Private Hospital	0.478***	0.451***	0.418***	0.367***
	[0.042]	[0.049]	[0.058]	[0.066]
Mean DV	0.252	0.252	0.157	0.157
Observations	163,986	163,797	116,728	116,504
Month-Year FE	X	X	X	X
Age-Year FE	X	X	X	X
Municipality-Year FE		X		X
Municipality-Age FE		X		X
Linear trend		X		X

Note: This table reports IV estimates using alternative definitions for Cesarean section. Columns (1) and (2) use the definition in the main paper (see Table B.1 for details). Columns (3) and (4) use an alternative definition (see Table D.20 for details). The sample is restricted to women with at least two births, for whom the mode of delivery can be identified in both the first and second birth. All columns include month-year of birth and age-by-year-of-birth fixed effects. Standard errors clustered at the municipality of residence are reported in brackets.
*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

D.10 IV Probit estimates

Table D.22: Robustness: IV Probit (control function) estimates of delivery at private hospital on short-term maternal outcomes (at first birth)

	OLS	Linear IV	IV Probit
	(1)	(2)	(3)
Panel A. Cesarean section, first birth			
Private Hospital (marginal effect)	0.211***	0.446***	0.388***
	[0.016]	[0.037]	[0.028]
Mean DV	0.340	0.340	0.340
Observations	268,252	268,252	268,252
	(1)	(2)	(3)
Panel B. 7+ days hospitalization			
Private Hospital (marginal effect)	-0.050***	-0.094***	-0.152***
	[0.004]	[0.013]	[0.016]
Mean DV	0.059	0.059	0.059
Observations	273,586	273,586	273,580
	(1)	(2)	(3)
Panel C. 30-day readmission			
Private Hospital (marginal effect)	-0.011***	-0.025***	-0.031***
	[0.001]	[0.007]	[0.009]
Mean DV	0.025	0.025	0.025
Observations	273,651	273,651	273,645
Month-Year FE	X	X	X
Age FE	X	X	X

Note: This table reports estimated marginal effects of delivering at a private hospital on short-term maternal outcomes. Columns (1) reports OLS estimates, column (2) reports linear IV estimates, and column (3) reports IV Probit estimates. IV estimates use individual insurance type as the instrument. All columns include month-year of birth and mother's age fixed effects. Mean DV denotes the average of the dependent variable in the estimation sample. Standard errors clustered at the municipality of residence are reported in brackets.
*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table D.23: Robustness: IV Probit (control function) estimates of delivery at private hospital on C-section in the first and second births

	OLS	Linear IV	IV Probit
	(1)	(2)	(3)
Panel A. C-section in first birth			
Private Hospital (marginal effect)	0.215***	0.456***	0.390***
	[0.017]	[0.047]	[0.037]
Mean DV	0.317	0.317	0.317
Observations	163,988	163,988	163,986
	(1)	(2)	(3)
Panel B. C-section in second birth			
Private Hospital (marginal effect)	0.176***	0.353***	0.340***
	[0.017]	[0.034]	[0.031]
Mean DV	0.419	0.419	0.419
Observations	163,988	163,988	163,988
	(1)	(2)	(3)
Panel C. Repeat C-section			
Private Hospital (marginal effect)	0.220***	0.485***	0.374***
	[0.017]	[0.042]	[0.033]
Mean DV	0.252	0.252	0.252
Observations	163,988	163,988	163,988
Month-Year FE	X	X	X
Age FE	X	X	X

Note: This table reports estimated marginal effects of delivering at a private hospital on repeat C-sections for a sample of women with two or more births. Columns (1) reports OLS estimates, column (2) reports linear IV estimates, and column (3) reports IV Probit estimates. IV estimates use individual insurance type as the instrument. All columns include month-year of birth and mother's age fixed effects. Mean DV denotes the average of the dependent variable in the estimation sample. Standard errors clustered at the municipality of residence are reported in brackets.
*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table D.24: Robustness: IV Probit (control function) estimates of delivery at private hospital on hospitalizations due to obstetric complication

	OLS	Linear IV	IV Probit
	(1)	(2)	(3)
Panel A. Cesarean scar hospitalization, 3 years			
Private Hospital (marginal effect)	0.030***	0.057***	0.041***
	[0.003]	[0.009]	[0.006]
Mean DV	0.014	0.014	0.014
Observations	273,651	273,651	273,596
	(1)	(2)	(3)
Panel B. Hysterectomy by age 35, 15 years			
Private Hospital (marginal effect)	0.003	0.010**	0.010**
	[0.002]	[0.004]	[0.004]
Mean DV	0.007	0.007	0.007
Observations	273,651	273,651	265,275
Month-Year FE	X	X	X
Age FE	X	X	X

Note: This table reports estimated marginal effects of delivering at a private hospital on hospitalizations due to obstetric complication. Columns (1) reports OLS estimates, column (2) reports linear IV estimates, and column (3) reports IV Probit estimates. IV estimates use individual insurance type as the instrument. All columns include month-year of birth and mother's age fixed effects. Mean DV denotes the average of the dependent variable in the estimation sample. Standard errors clustered at the municipality of residence are reported in brackets.
*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

Table D.25: Robustness: IV Probit (control function) estimates of delivery at private hospital on subsequent fertility

	OLS	Linear IV	IV Probit
	(1)	(2)	(3)
Panel A. 2 or more children, 6 years			
Private Hospital (marginal effect)	0.045***	-0.013	-0.012
	[0.005]	[0.025]	[0.025]
Mean DV	0.408	0.408	0.408
Observations	273,651	273,651	273,568
	(1)	(2)	(3)
Panel B. 3 or more children, 9 years			
Private Hospital (marginal effect)	0.017***	-0.009	-0.002
	[0.005]	[0.018]	[0.021]
Mean DV	0.135	0.135	0.135
Observations	273,651	273,651	273,366
Month-Year FE	X	X	X
Age FE	X	X	X

Note: This table reports estimated marginal effects of delivering at a private hospital on subsequent fertility. Columns (1) reports OLS estimates, column (2) reports linear IV estimates, and column (3) and (4) report IV Probit estimates. IV estimates use individual insurance type as the instrument. All columns include month-year of birth and mother's age fixed effects. Mean DV denotes the average of the dependent variable in the estimation sample. Standard errors clustered at the municipality of residence are reported in brackets.
*** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.

D.11 Maternal mortality

A concern with the short-run results is survivor composition: if delivering at a private hospital raised the risk of maternal death, the women observed after discharge would be a healthier selection, and the improvements in length of stay and readmission could reflect that selection rather than better care. We link the mothers in our sample to the national death register through the same anonymized identifier that links births to hospital discharges, and measure mortality as death of the mother from any cause within one year of the delivery discharge. The register linkage counts deaths wherever they occur; this matters because discharge timing is itself one of our outcomes, so a measure restricted to deaths during the delivery admission could move mechanically with it.

Table D.26 reports OLS and IV estimates of the effect of private-hospital delivery on maternal mortality within one year, all-cause (Panel A) and from obstetric causes (ICD-10 chapter O, Panel B). None of the IV estimates is statistically significant. With a mean of 0.3 deaths per 1,000 births the test has little power, so we do not read the point estimates; but no estimate shows a statistically significant increase in maternal mortality at private hospitals.

Table D.26: Effect of delivery at private hospital on maternal mortality: OLS and IV estimates

	OLS	IV Fonasa B, C, D		
	(1)	(2)	(3)	(4)
Panel A. Maternal mortality, 1 year (all-cause)				
Private Hospital	-0.0003**	-0.0008	-0.0008	-0.0013
	[0.0001]	[0.0010]	[0.0011]	[0.0012]
Mean DV	0.0003	0.0003	0.0003	0.0003
Observations	273,651	273,651	273,468	273,468
Panel B. Maternal mortality, 1 year (obstetric causes, ICD-10 O-codes)				
Private Hospital	-0.0001**	-0.0006	-0.0005	-0.0005
	[0.0001]	[0.0006]	[0.0006]	[0.0007]
Mean DV	0.0001	0.0001	0.0001	0.0001
Observations	273,651	273,651	273,468	273,468
Age-Year FE			X	X
Municipality-Age FE			X	X
Municipality-Year FE			X	X
Linear trend				X

Note: IV estimates of the effect of private-hospital delivery on maternal mortality within one year of the first birth. Column (1) reports OLS; columns (2)–(4) are IV, instrumenting private-hospital delivery with Fonasa groups B, C, and D interacted with Post. All columns include month-year and maternal age fixed effects; additional fixed effects are indicated in the lower panel. Panel A covers all-cause maternal mortality; Panel B restricts to obstetric causes (ICD-10 O-codes). Mean DV denotes the average of the dependent variable in the estimation sample. Standard errors clustered at the municipality of residence in brackets. *** Significant at the 1 percent level. ** Significant at the 5 percent level. * Significant at the 10 percent level.