Growing Up Insured? Long-Run Effects of a Dependent Health Coverage Expansion

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Abstract

Does health insurance in childhood produce healthier adults, and does the benefit reach their children? I study Ecuador's 2011 extension of social security health coverage to affiliated workers' children under eighteen, linking hospital, employer, and vital-statistics registries for 3.2 million affected cohort members, with treatment defined by parental affiliation fixed before the reform could affect it. Children's use of social security hospitals rose by 6.2 percentage points from a one-percent baseline, beginning in the exact month of administrative implementation. In early adulthood, eligible cohorts were hospitalized for illness six to sixteen percent less frequently, with comparable declines in avoidable admissions and effects that increase with years of potential coverage; all-cause mortality among eligible young women fell by approximately one quarter. Eligible women record four to twenty percent fewer births at ages 17 to 29. In the next generation, health at birth is unchanged, with precisely estimated nulls on birthweight, prematurity, prenatal care, Apgar scores, late stillbirth, and neonatal mortality, while a cesarean decline of five to six percentage points lies on a convergence predating the reform and is reported as suggestive. The results provide the first evidence from linked administrative records on the adult and intergenerational effects of childhood public insurance in a middle-income country.

Keywords: health insurance, dependent coverage, long-run effects, intergenerational transmission, fertility, social security
JEL classification: I13, I12, I18, H55, J13, O15

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1 Introduction

Does giving children access to health insurance make them healthier adults, and does the benefit reach their own children? This paper answers that question using Ecuador's expansion of dependent health coverage. In February 2011, dependent coverage in the Ecuadorian Social Security Institute (IESS) became effective for affiliated workers' children up to age seventeen: from that month, a child was entitled to care in the IESS system by virtue of the parent's affiliation alone, at no additional contribution. It was an entitlement, reaching hundreds of thousands of children in a single administrative stroke.[1] I trace the children this reform covered across two decades of administrative records: from the hospital care they used in the months the policy switched on, to their health and fertility as adults in their twenties, to the births of their own children.

The question matters because we know far more about whether insuring children works in the short run than about whether it lasts. A rich literature from low- and middle-income countries shows that extending health coverage to children raises utilization, protects household finances, and improves infant and child health, in Colombia, Taiwan, Thailand, Vietnam, and Mexico, among others (Camacho and Conover, 2013; Chou et al., 2014; Gruber et al., 2014; Palmer et al., 2015; Limwattananon et al., 2015; Celhay et al., 2019; Conti and Ginja, 2024), and can improve schooling and adolescent outcomes (Chen and Jin, 2012; Alcaraz et al., 2017; Khiem and Kuo, 2022; Huang and Liu, 2023); Mitra et al. (2017) review this evidence. What that literature has rarely been able to do is follow covered children into adulthood: its horizon typically ends where the survey or the childhood registry ends, because the administrative infrastructure to observe the same person as a covered child and again as an adult decades later has been largely confined to high-income countries, and where coverage is universal from birth there is no childhood eligibility margin left to evaluate. The long-run evidence therefore comes almost entirely from United States Medicaid, whose childhood eligibility has been shown to improve adult health, reduce hospitalizations and mortality, and raise educational attainment and earnings (Currie and Gruber, 1996; Wherry and Meyer, 2016; Wherry et al., 2018; Miller et al., 2021; Brown et al., 2020; Goodman-Bacon, 2021; Cohodes et al., 2016). Whether those long-run returns carry over to middle-income countries is an open question: in these settings, contributory coverage is rationed by parental formality, free public alternatives exist alongside it, and baseline disease environments differ. The policy stakes are correspondingly large. Most social-insurance systems in the developing

[1] An earlier, smaller wave (Consejo Directivo Resolution 265, June 2009) had extended dependent coverage to children under six. Because those cohorts are still too young to observe as adults, this paper evaluates the 2011 expansion; I return to the 2009 wave in Section 2 and leave its long-run evaluation to future work.

world still tie coverage to a formal worker and exclude, or only partially cover, that worker's family, and extending coverage to dependents is legislatively simple but fiscally expensive: in Ecuador the children were added at no new contribution, with the financing of the new group left unspecified, and the health fund's resulting losses eventually forced a reallocation of contribution rates away from pensions (Vermeersch and Giovagnoli, 2020). Whether so costly an extension buys long-run health is therefore a first-order input into the design of contributory systems.

I answer the question in three stages, on Ecuadorian administrative data of two kinds. The universe of hospital discharges (2002–2023) provides two decades of microdata on every hospitalization in the country; over this full span the records cannot be linked across sources at the individual level, and I use them in aggregate, at the age-by-month level. For recent years, records are newly linked at the individual level through a 16.5-million-person civil-registry spine: monthly IESS employment registries, hospital discharges carrying personal identifiers (2013–2017), five complete annual birth registries between 2013 and 2019, and the fetal-death (2013–2017) and general mortality registries (2017 and 2019). Stage one establishes that the reform moved actual access: I estimate the contemporaneous effect of the expansion on hospital utilization by newly eligible children, exploiting the timing of the rollout. Stage two follows the covered cohorts into adulthood: I build a 9.6-million person-year panel of the 1988–1998 birth cohorts over 2015–2017 and estimate effects on illness-related hospitalization, ambulatory-care-sensitive admissions, and fertility; a companion panel covering 2017 and 2019 adds mortality. Stage three links 593,912 births to mothers in these cohorts and asks whether the next generation is affected at birth. Throughout, treatment is the interaction between birth-cohort eligibility and parental IESS affiliation measured at a fixed pre-reform date, so that estimates are intention-to-treat effects uncontaminated by the formalization response the reform itself induced (Molina-Vera, 2021).

The ideal experiment would randomize dependent-coverage eligibility among the children of affiliated workers and follow both arms for fifteen years, through their own childbearing. Randomized evaluations of health insurance exist (in Oregon and, recently, at scale in India), but their follow-up horizons span a few years at most, far short of the fifteen that separate childhood coverage from adult outcomes (Finkelstein et al., 2012; Malani et al., 2024). Absent that experiment, the reform supplies two sharp sources of quasi-experimental variation that approximate it. In calendar time, effective access switched on in a single, administratively chosen month. Unusually, statutory eligibility preceded effective access by three months, so the policy carries a built-in placebo: eligibility on paper (November 2010–January 2011) moved utilization not at all, and the jump arrives precisely in the month the

entitlement became usable. In cohort space, the reform drew a line at the February 1993 birth month: children born after it were young enough to gain eligibility, while near-age peers born just before it were not. Comparing eligible to ineligible cohorts, between children of affiliated and non-affiliated parents, yields a cohort-exposure difference-in-differences in the tradition of Duflo (2001) and of the Medicaid cohort-cutoff designs (Card and Shore-Sheppard, 2004; Wherry and Meyer, 2016; Wherry et al., 2018; Miller et al., 2021). I implement the calendar-time design with heterogeneity-robust event-study estimators (Callaway and Sant'Anna, 2021; Borusyak et al., 2024), probe the parallel-trends assumption with honest-trends bounds (Rambachan and Roth, 2023), and reinforce the cohort design with dose-response, sibling fixed-effects, and regression-discontinuity specifications at the eligibility cutoff, together with a battery of placebo cutoffs and population placebos.

Three sets of results emerge. First, the reform moved access immediately and mechanically: children's use of social security hospitals rose by 6.2 percentage points from a baseline of one percent, an approximately sevenfold increase, beginning in the exact month administrative implementation started, after three months of purely statutory eligibility had produced no measurable response. Second, the covered children became healthier adults. Cohorts exposed to dependent eligibility in childhood were hospitalized for illness six to sixteen percent less frequently in early adulthood, with comparable declines in admissions for avoidable, ambulatory-care-sensitive conditions, precisely the admissions that timely primary care prevents, and effects that increase with years of potential childhood coverage. All-cause mortality among eligible young women declined by approximately one quarter. Eligible women also delay family formation, recording between four and twenty percent fewer births at ages 17 to 29.

Third, in the next generation, health at birth is unchanged. Across 593,912 linked births, I find precisely estimated null effects on birth weight, prematurity, prenatal care, Apgar scores, late stillbirth, and neonatal mortality: the children of covered mothers are not born measurably healthier or at lower risk. Cesarean delivery is five to six percentage points lower among eligible mothers, consistent with covered children remaining in the public IESS system as adults, but this difference lies on a convergence in delivery mode across cohorts that predates the reform and cannot be separated from it in these data; I report it as suggestive. Transmission at birth, the earliest margin at which it can be observed, therefore appears limited to the timing of fertility and, possibly, the circumstances of delivery.

These findings contribute to four literatures. Most directly, I provide what is, to my knowledge, the first evidence from linked administrative records on the adult health and intergenerational effects of childhood public insurance in a middle-income country. Long-

run evidence of this kind has so far come almost exclusively from U.S. Medicaid (Currie and Gruber, 1996; Wherry and Meyer, 2016; Wherry et al., 2018; Miller et al., 2021; Brown et al., 2020; Goodman-Bacon, 2021); the developing-country literature, though rich, has largely been bounded by its data horizon at outcomes measured in childhood or adolescence (Camacho and Conover, 2013; Chou et al., 2014; Palmer et al., 2015; Celhay et al., 2019; Huang and Liu, 2023; Khiem and Kuo, 2022; Mitra et al., 2017). The Ecuadorian registries extend that horizon into the covered cohorts' own adulthood and parenthood. Second, the three dead statutory months speak to a measurement problem that runs through the policy-evaluation literature: laws and their implementation are not the same event, and dating a reform by its statute can badly attenuate estimated effects (Goodman-Bacon, 2018; Das and Do, 2023). Here the gap between de jure and de facto coverage is not a problem but a mechanism result: access, not entitlement, is what moved utilization, and the design turns it into an internal placebo test.

Third, I add to the small literature on the intergenerational transmission of safety-net programs, which has documented that childhood program exposure improves the next generation's outcomes in the United States (East et al., 2023; Almond and Currie, 2011; Currie and Moretti, 2003), and, more broadly, to the research program that uses linked administrative data to trace adult outcomes back to childhood circumstances (Chetty et al., 2026). The Ecuadorian results extend this evidence to a middle-income setting and discipline it: alongside honest, precise nulls on the birth-endowment margins the literature usually examines, transmission at birth appears confined to fertility timing and, suggestively, the mode of delivery. Finally, the fertility response connects childhood insurance to the economics of family formation, suggesting that coverage in adolescence shifts the timing of first births, and the linked registry infrastructure itself, a population spine joining social-security, hospital, and vital-statistics records, offers a template for long-run program evaluation in other middle-income countries where such linkages are becoming feasible.

The remainder of the paper proceeds as follows. Section 2 describes the Ecuadorian health system, the IESS, and the two reform waves, including the operational rollout that separates statutory from effective coverage. Section 3 presents the administrative registries, the record linkage, and the treatment definition. Section 4 develops the two empirical designs and the falsification battery that disciplines causal claims throughout. Section 5 reports the results: the contemporaneous utilization response, adult health, mortality, and fertility, and the next generation's birth outcomes, including the suggestive delivery-mode pattern. Section 6 concludes, and Appendix A.1 records a candidate design rejected by falsification.

2 Institutional Background

2.1 The Ecuadorian health system

Ecuador's National Health System combines a dominant public sector with a complementary private one, under the stewardship of the Ministry of Public Health (MSP). The MSP plays a dual role. As the National Health Authority it holds the constitutional *rectoría*: it sets national health policy and regulates every provider in the system, public or private. At the same time it is the system's largest provider. MSP facilities are financed from the central government budget and, since the 2008 Constitution, are free at the point of use at all levels of care, including diagnostics, treatment, and medicines. Public providers are organized into the Integrated Public Health Network (RPIS), which comprises MSP facilities, the facilities of the social security institutes, and municipal and other state units; private for-profit and non-profit providers, including SOLCA's cancer network and the charitable hospitals of the Junta de Beneficencia, form the Complementary Private Network. Within this architecture the MSP concentrates the bulk of public infrastructure, operating roughly two thirds of accredited first-level facilities and of second-level hospital units, against about one quarter and one eighth, respectively, for the social security institute (Vermeersch and Giovagnoli, 2020).

Formal insurance coverage is the exception rather than the rule. In 2014, at the opening of the outcome window I use in Stage 2, 58.6 percent of Ecuadorians had no formal health insurance; 26.9 percent were covered by the IESS general regime, 9.2 percent by the rural Seguro Social Campesino or the voluntary regime, 1.5 percent by the armed forces and police institutes, and under one percent exclusively by private insurance. Two features of this landscape matter for everything that follows. First, the relevant counterfactual for a child without IESS eligibility is not the absence of care: it is free MSP care and, especially in the formal-sector families whose children gained eligibility, privately purchased care. The estimates in this paper are therefore effects of access to the contributory system relative to that mixed baseline, not effects of insurance versus no insurance, and part of any utilization response reflects the re-routing of care from the MSP and private sectors into IESS facilities rather than net new care. Second, the system is fragmented: each public subsystem has its own facilities, its own revenue source, and its own payment mechanisms (Vermeersch and Giovagnoli, 2020), so gaining IESS eligibility changes which network a child can enter, not merely a financing label.

2.2 Social security and the IESS

Contributory social security in Ecuador is, by construction, an institution of the formal labor market. It originated in 1928 with the Caja de Pensiones for public and bank employees, on a Bismarckian model in which protection attaches to formal dependent employment and is financed by tripartite contributions from workers, employers, and the state (Porras Velasco, 2015). Coverage was extended to private-sector workers by the 1935 Compulsory Social Insurance Law and the creation of the Caja del Seguro in 1937; the merged funds became the Ecuadorian Social Security Institute (IESS) in 1970 (Carranza Barona and Villavicencio Salazar, 2021). This formal-sector enclave character, protection for salaried workers and exclusion of the informal majority, persisted through the century (Carranza and Cisneros, 2014), and it persists in the mechanics that matter here: affiliation to the IESS general regime follows from registered dependent employment, with wage-proportional employee and employer contributions, and health benefits are administered through the IESS's own Seguro General de Salud Individual y Familiar and delivered in IESS facilities.

Three satellite regimes complete the picture. The Seguro Social Campesino (SSC), piloted in 1968 and extended by law in 1981, covers rural households engaged in smallholder farming or artisanal fishing; it is financed by a solidarity surcharge on all system contributors, a small differentiated contribution from covered household heads, and fiscal transfers, and it protects the household as a unit (Carranza Barona and Villavicencio Salazar, 2021). SSC coverage of children therefore did not depend on the dependent-coverage reforms studied here; this institutional fact underlies the SSC placebo in Section 5.2. The armed forces and police maintain separate institutes (ISSFA, created 1992, and ISSPOL, 1995), which together cover about 1.5 percent of the population and operate their own small facility networks (Vermeersch and Giovagnoli, 2020; Carranza Barona and Villavicencio Salazar, 2021).

The 2008 Constitution declared social security a universal public right and prohibited its privatization, and the years around the reforms saw rapid growth in coverage: affiliation to the three institutes rose from 25 percent of the population in 2006 to 41.4 percent in 2012 (SENPLADES, 2013). This growth reflected the post-2008 push to enforce employer registration, including a 2011 referendum that criminalized the non-affiliation of dependent workers, as well as the formalization response to the dependent-coverage extension itself documented by Molina-Vera (2021). Own-account affiliation, in other words, was trending sharply upward for reasons connected to the reform. This secular growth is why treatment in this paper is defined by *parental* affiliation frozen at a fixed pre-reform date, and why broad adult comparison groups fail pre-trend diagnostics while near-age young-adult controls do not (Section 4.1).

2.3 Dependent health coverage

For most of the IESS's history, health coverage reached the affiliate alone. Spouses could be covered only through an additional contribution, and affiliates' children had no claim on IESS care. Statute moved first, and idly: the 2001 Social Security Law provided coverage for affiliates' children under six (Carranza Barona and Villavicencio Salazar, 2021), but the provision went unimplemented for nearly a decade while the law itself sat in partial constitutional limbo after 2004. Operational coverage for young children arrived only with Consejo Directivo Resolution 265 of June 2009, which made coverage effective for affiliates' children ages one to five (wave 1). The second wave extended eligibility to all remaining minors: statutory eligibility for children up to age seventeen was adopted in 2010 and held de jure from November 2010, but the entitlement became usable only in February 2011 under Resolution CD 357, when the IESS's administrative systems began recognizing dependents in practice and children could actually be seen in IESS facilities. The children were incorporated without any additional contribution from their parents, and the financing of the new beneficiary group was left unspecified (Vermeersch and Giovagnoli, 2020), an ambiguity that helps explain both the lag between statute and administrative readiness and the financial strains that surfaced later.[2]

The gap between the statute book and the enrollment window is thus not incidental to this reform; it is a recurring feature of the institution. For wave 1 the gap lasted eight years; for wave 2, three months. The paper exploits the second, short gap directly: because eligibility existed on paper from November 2010 while effective access began in February 2011, the three intervening months provide a built-in placebo that separates the effect of legal entitlement from the effect of administrative access (Section 5.1). Wave 1 plays no identifying role in this paper: the cohorts it covered are still too young to observe as adults, and its long-run evaluation is left to future work.

2.4 Implications for the empirical design

Four institutional features map directly into the design. First, because general-regime affiliation follows from formal employment, the cross-sectional treatment margin, having an IESS-affiliated parent, is a formality margin, and it is measured at a fixed pre-reform date to insulate it from the affiliation responses the reform itself induced (Molina-Vera, 2021).

[2]The health fund's losses eventually prompted Resolution CD 501 of 2015, which temporarily reallocated contribution shares toward health (from 5.71 to 9.94 percent of the wage base) (Vermeersch and Giovagnoli, 2020). This financing-side change applies uniformly across the cohorts I compare and postdates treatment assignment.

Second, because statutory and operational coverage came apart, the event is dated to the operational month, February 2011, and the dead statutory months serve as a falsification window rather than a problem. Third, because SSC coverage of rural households was family-based and untouched by the dependent-coverage resolutions, SSC-covered populations, identified by parish of residence, since SSC care generates no hospital-discharge records of its own, furnish a population whose children's coverage should not respond to the 2011 rollout. Fourth, because the uninsured majority retains free access to MSP care, and because the MSP network itself was expanding under the 2008 constitutional mandate, all estimates are interpreted as effects of contributory-system access relative to the care families would otherwise have obtained in the MSP and private sectors, with common supply-side expansion absorbed by time and cohort fixed effects and differenced out against children of non-affiliated parents.

3 Data

I combine five administrative sources. First, the universe of hospital discharges (INEC Egresos Hospitalarios, 2002–2023), which records age, diagnosis, and the treating institution, permitting the IESS share of discharges to be tracked by age and month over two decades. Second, monthly IESS employment registries (from 2006), from which I construct each parent's affiliation status in a fixed pre-reform window. Third, a linked-generations spine built from the civil registry, connecting 16.5 million individuals to their parents via pseudonymized identifiers; restricting to birth cohorts 1988–1998 with an observed parental identifier yields 3,193,261 individuals. Parental identifiers are essentially universal in these cohorts (95 percent of fathers, 99 percent of mothers observed). The window spans roughly five cohorts on each side of the February-1993 eligibility cutoff. Older cohorts are observed in 2015–2017 only at ages never reached by any eligible cohort, contributing no identifying overlap; younger cohorts remain covered dependents during the observation window, confounding the contemporaneous access effect with the longer-run health effect. Fourth, individual-level linked hospital discharges (2013–2017); identifier coverage improves from 48 percent in 2013 to 97 percent by 2017 as the national record-linkage system matured, so the analysis window is 2015–2017, where coverage exceeds 92 percent and is nearly uniform across facility sectors. Fifth, the vital-statistics registries: the complete annual birth registries (2013, 2015, 2016, 2017, and 2019, yielding 593,912 births to study-cohort mothers after restricting to registry-year births), the fetal-death registries (2013–2017), and the general mortality registries with usable personal identifiers for 2017 and 2019.

Treatment is defined at the family level: a child is treated if either parent recorded three consecutive monthly contributions in December 2010–February 2011, the quarter ending at operational implementation. The three-consecutive-month window mirrors the IESS's own benefit rule, under which health coverage becomes usable only after three consecutive monthly contributions, so the definition captures precisely the families whose children could obtain care when the entitlement became operational. Freezing affiliation at this date precedes any behavioral response of formalization to the reform itself; 1.99 million parents qualify, and 93 percent of them also satisfy a stricter six-month definition. Children whose parents appear only in the SSC regime (217,469 stable SSC-only parents) form the placebo group: their families held coverage before 2011, so the reform changed nothing for them.

4 Empirical Strategy

4.1 Stage 1: utilization

The first stage compares the IESS share of hospital discharges across age groups and months. For the 2011 wave, treated ages 6–17 are compared to near-age controls 18–24 (broader adult controls fail parallel trends due to secular growth in own-affiliation among prime-age adults):

$$s_{at} = \alpha_a + \lambda_t + \sum_{k \neq -1} \beta_k \, \mathbf{1}\{t - t^* = k\} \times \text{Treat}_a + \varepsilon_{at}, \tag{1}$$

where s_{at} is the IESS share for age a in month t and t^* is February 2011. Estimates are reported from the Callaway–Sant'Anna estimator with January 2011 as base period; in this single-cohort design they coincide numerically with two-way fixed effects, and the Borusyak–Jaravel–Spiess imputation estimator yields similar estimates. Sensitivity to differential trends is assessed with age-specific linear trends and with Rambachan–Roth relative-magnitude bounds at quarterly event time. One institutional detail sharpens the interpretation of the statutory window. The three-consecutive-contribution rule that governs benefit use (Sections 2 and 3) binds on the affiliate's own contribution history, not on the dependent: parents in the treatment definition had contributed continuously through the pre-reform quarter, so their children's coverage required no additional waiting once the system recognized them. An absence of response during the statutory months therefore reflects administrative implementation rather than the mechanical expiry of a waiting period. The complementary channel, parents who first affiliated after the November statute and whose three-contribution window matured exactly in February, cannot account for the pattern either: the response it can generate is bounded by the flow of new affiliates over three months,

which is small relative to the stock of long-affiliated families, and the children of that stock faced no waiting yet showed no response before February. At most this channel contributes a minor component of the February step.

4.2 Stage 2: long-run outcomes

The second stage is a cross-sectional cohort design. Outcomes of individual i of birth cohort c observed in year t follow

$$y_{ict} = \delta_c + \lambda_t + \delta_{p(i)} + \beta_1 \text{Affil}_i + \beta_2 \, \text{Affil}_i \times \text{Elig}_c + X_i'\gamma + \varepsilon_{ict}, \tag{2}$$

where Elig_c indicates birth after February 9, 1993 (under eighteen at implementation) and δ_c are birth year–month fixed effects absorbing common cohort shocks. Affil_i is the family-level treatment of Section 3: an indicator that a parent met the IESS's own benefit criterion, three consecutive monthly contributions, in the quarter ending at implementation, so the interaction compares children whose household's coverage was usable the moment eligibility opened with children whose household's was not. The identifying assumption is not that children of affiliated and non-affiliated parents are comparable in levels; they are not, and the estimates report the difference directly. The coefficient β_1 reflects the socioeconomic gradient between formal- and informal-sector families and carries no causal interpretation. The assumption concerns slopes: absent the reform, the affiliation gap would evolve smoothly across birth cohorts, whereas CD 357 creates a step at February 1993, because only children born after that date were still under eighteen when coverage became operational. The interaction β_2 measures that step and is an intention-to-treat estimate throughout: actual coverage comes and goes with parental employment while assignment does not, so misclassification attenuates the estimates toward zero and they bound the effect of continuous coverage from below. Appendix Table 8 reports the schedule linking each cohort to its age at implementation, eligibility status, and scheduled years of dependent coverage.[3] All outcomes are flow rates observed at overlapping ages, so cohorts are compared at the same age in different calendar years; differences in age at measurement cannot generate spurious cohort patterns. Standard errors are clustered by birth year–month.

Appendix Figure 8 illustrates why the parental link is essential: year-adjusted aggregate cohort means decline monotonically across the 1988–1998 cohorts, because cohort, calendar year, and age at measurement are collinear in a single cross-cohort comparison and any eligi-

[3]The 1993 cohort is partially treated: only births from February onward were eligible, with scheduled coverage measured in months. I keep the cohort in all figures and treat its coefficient as a lower bound.

bility effect is diluted by the non-affiliated population share. The link breaks this collinearity: within a birth cohort, affiliated and non-affiliated children are observed at the same ages in the same years, and the design reduces to an event study in cohort space, presented alongside the results below.

The smoothness assumption is testable, and the tests have power against the relevant confound. I apply the same falsification battery to every outcome: placebo eligibility cutoffs at birth dates where no reform occurred, estimated among never-eligible cohorts (February 1990) and among always-eligible cohorts (February 1996). The battery is null for the outcomes on which I place causal interpretations. For late stillbirth the placebo interactions are 0.57 (s.e. 0.57) and −0.78 (s.e. 0.51) per 1,000, and for adult mortality 0.05 (s.e. 0.07) and −0.07 (s.e. 0.07) (Appendix Tables 9 and 10). The same battery rejects two candidate designs, which I currently discard: own adult formality, where placebo cutoffs reproduce the estimate at the true cutoff almost exactly (−1.68 and −1.35 against −1.70); and cesarean delivery, where placebo cutoffs return −2.1 and −1.6 percentage points ($p < 0.01$), a pattern consistent with a smooth convergence of the gap across cohorts, discussed in Section 5.5. Causal language in this paper is reserved for outcomes for which the battery is null.

5 Results

5.1 Stage 1: dependent coverage and children's hospital care

Figure 1 presents the raw policy variation. The IESS share of hospital discharges among children ages 6–17 remains close to one percent for nearly a decade, while the corresponding share for young adults, who held coverage in their own right, drifts slowly upward. The February 2011 expansion appears directly in the raw series: the children's share breaks sharply in a single month and rises steadily thereafter.

The timing of that break identifies the margin that mattered. Statutory eligibility began in November 2010, when the reform was published; the IESS operational resolution took effect in February 2011. Figure 2 shows that three months of purely statutory eligibility produced no detectable change in utilization: the event-study coefficients for November and December 2010 are 0.002 and 0.046 percentage points. Utilization doubled in the month the administrative apparatus became operational, rising by 1.3 percentage points in February 2011 alone. Access was created by implementation rather than by legislation, and the three months of statutory-only eligibility additionally serve as a test against anticipatory responses.

Figure 3 presents the event study for the second wave (CD 357, ages 6–17, February

2011), which is the focus of the analysis: it affects twelve birth cohorts, generates the age-eighteen eligibility cutoff used in Stage 2, and is the only wave for which statutory and operational dates are separated.[4] Pre-treatment coefficients are jointly indistinguishable from zero (average pre-period coefficient 0.36 percentage points, $p = 0.14$) and decline toward zero as treatment approaches. The effect appears immediately, at 2.6 percentage points in the first quarter, rises as enrollment frictions resolve, and stabilizes 6–7 percentage points above the counterfactual within eighteen months. The pooled average treatment effect is 6.16 percentage points (s.e. 0.42), approximately a sevenfold increase over the pre-reform baseline of one percent. Estimates are numerically identical across the Callaway–Sant'Anna and two-way fixed effects estimators in this single-cohort design, and estimates from the Borusyak–Jaravel–Spiess imputation estimator are similar (appendix).

Table 1 reports the sensitivity of the estimate to differential trends. Absorbing nineteen age-specific linear trends moves the two-way fixed effects estimate from 5.25 to 4.64 percentage points, consistent with a level shift at the operational date rather than a differential trend. Following Rambachan and Roth (2023), I ask how large a violation of parallel trends the estimates can withstand. Post-treatment violations are indexed by $\bar{M}$, the permitted quarter-to-quarter movement in the counterfactual gap expressed as a multiple of the largest such movement observed in the pre-period; the breakdown value is the largest $\bar{M}$ at which the confidence interval still excludes zero. Formal sensitivity analysis (Figure 4) shows that the first-quarter impact survives violations more than three times the largest observed pre-period deviation, that the year-one average survives violations twice that size, and that the three-year average survives violations of equal size. Because relative-magnitude bounds permit violations to accumulate, bounds on multi-year averages are mechanically wider; at those horizons, identification rests on the discontinuity at the administratively determined date.

[4] The first wave (CD 265, June 2009, ages one to five) affects cohorts born after 2003, who are too young for the adult outcomes studied in Stage 2. A companion event study of its utilization response will be incorporated in a subsequent version.

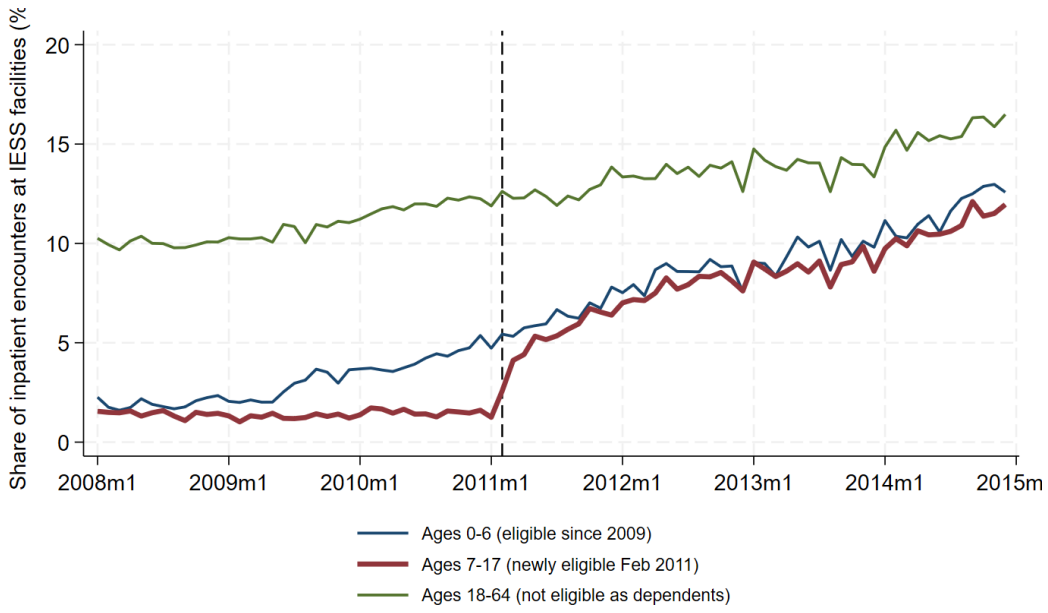


Figure 1: IESS share of hospital discharges by age group. Vertical lines mark the June 2009 (ages 1–5, CD 265) and February 2011 (ages 6–17, CD 357) expansions.

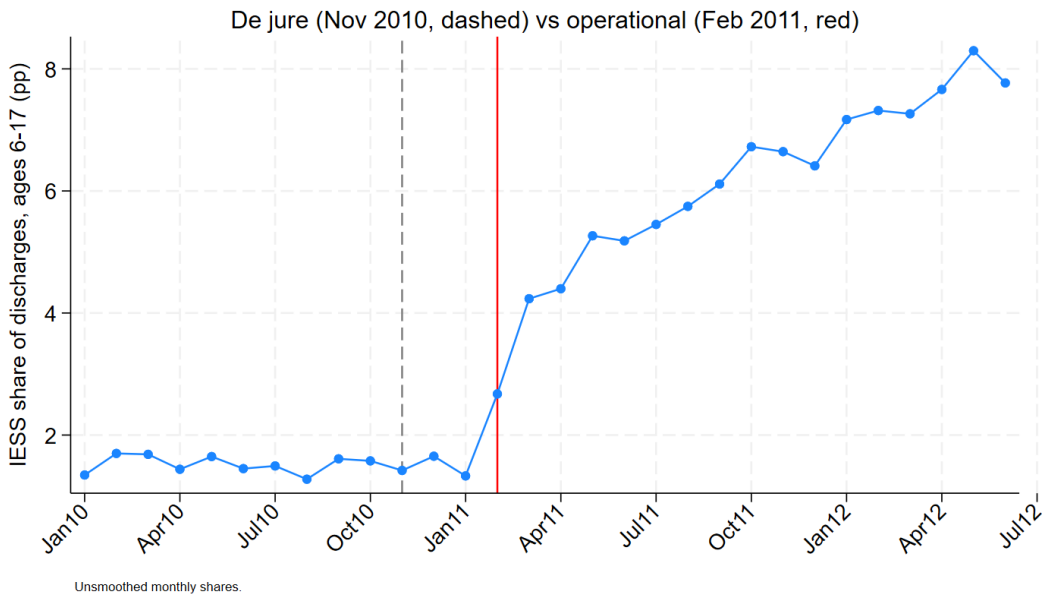


Figure 2: Monthly IESS share of discharges, ages 6–17, around the second reform. Statutory eligibility begins November 2010; the operational resolution takes effect February 2011.

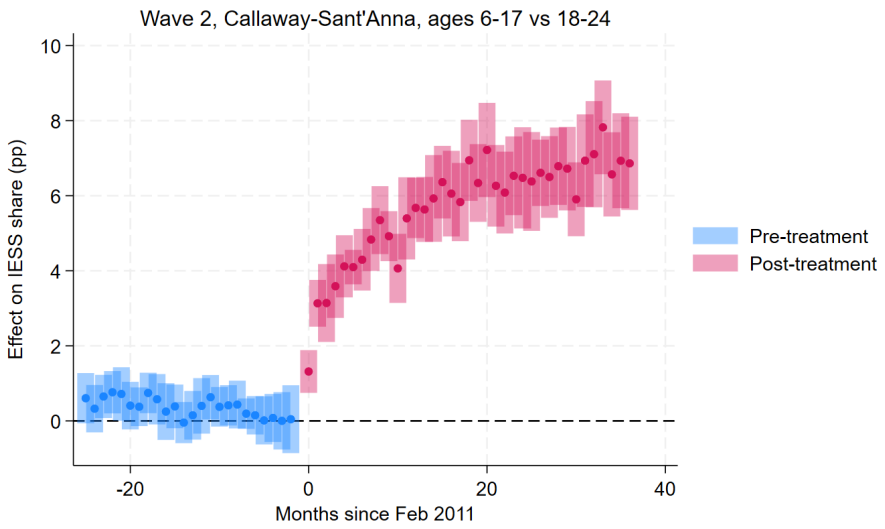


Figure 3: Event study, ages 6–17 vs. 18–24 (Callaway–Sant'Anna, base period January 2011). Outcome: IESS share of discharges (pp).

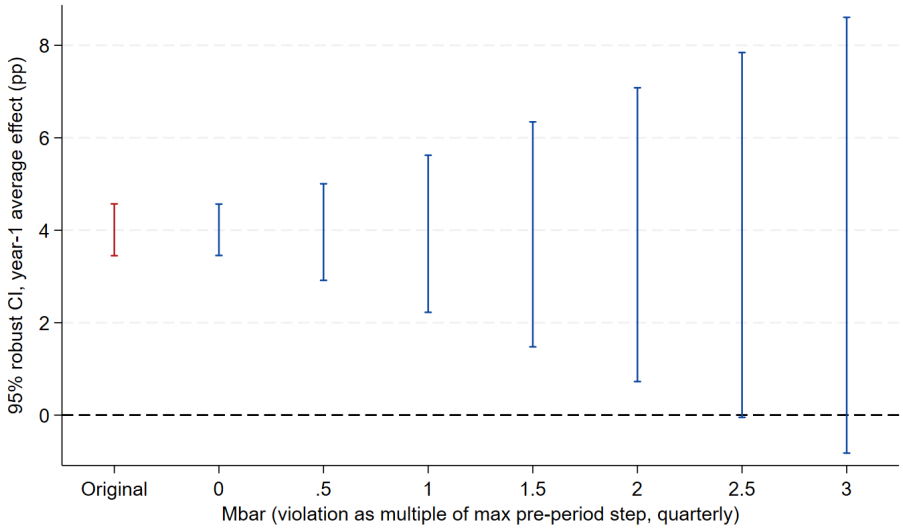


Figure 4: Rambachan–Roth relative-magnitude bounds on the year-one average effect, quarterly event time.

Table 1: Wave-2 ATT: sensitivity to differential trends

	(1) TWFE	(2) TWFE + age trends	(3) CS ATT
D	5.254***	4.642***	
	(0.490)	(0.311)	
ATT			6.163***
			(0.424)
Constant	3.283***	3.452***	
	(0.135)	(0.085)	
Observations	2052	2052	

5.2 Stage 2: health in early adulthood

I follow the same children into early adulthood using individual-level linked discharge records: a person-year panel of 3.19 million cohort members over 2015–2017, at ages 17–29. Admissions are classified by ICD-10 chapter into illness, ambulatory-care-sensitive (ACS), obstetric, and injury categories; obstetric admissions are analyzed separately because they track fertility rather than avoidable morbidity.

Figure 5 presents the design as an event study in cohort space: coefficients on Affil× birth cohort, with 1992 as base, for illness hospitalization. The base 1992 affiliation gap is +0.69 percentage points (s.e. 0.04) on a non-affiliated rate of 1.8 percent; this is the socioeconomic gradient, absorbed by the main effect. Around it, the pre-cutoff profile declines from +0.18 percentage points at the 1988 cohort to +0.13 at 1989 and then flattens: the 1990 and 1991 deviations are +0.03 and −0.01, individually and jointly indistinguishable from zero ($p = 0.55$). A joint test over all four pre-cutoff cohorts rejects ($p < 0.001$), driven entirely by the 1988–1989 convergence, which I document rather than absorb: the affiliated advantage was eroding at a fitted 0.05 percentage points per cohort (s.e. 0.014) at the far cohorts. Against that decelerating profile, the 1993 cohort drops discretely to −0.14, and the gap remains between −0.15 and −0.20 through 1997, averaging −0.16 percentage points. Two checks separate the step from the drift. First, the step between the adjacent 1992 and 1993 cohorts is roughly three times the fitted drift per cohort. Second, extrapolating the pre-period linear trend fails out of sample: the trend predicts a 1998 deviation of −0.30, whereas the estimated deviation from the extrapolated trend at 1998 is +0.29 (95% CI 0.07 to 0.51; Appendix Figure 9). The convergence visible at the far cohorts had flattened before the

cutoff and did not continue after it.[5]

Table 2 reports the pooled estimates. For eligible cohorts, illness admissions fall by 0.20 percentage points per person-year ($t = -7.9$), approximately eleven percent of the baseline rate, and ACS admissions fall by approximately ten percent. The effect is dose-responsive: each additional year of potential childhood coverage reduces illness admissions by 0.032 percentage points, so that a child covered for five years exhibits essentially the full effect. Estimates are unchanged when the boundary birth month is excluded and under the stricter six-month affiliation definition.

Table 3 reports two robustness checks. First, allowing affiliated families a group-specific linear cohort trend, which is nearly collinear with a step at 1993 given six post-cutoff cohorts, attenuates the interaction to -0.14 percentage points while leaving it statistically significant ($t = -2.4$); the trend term itself is small and insignificant, consistent with a break at the eligibility cutoff rather than a smooth trend. The ACS point estimate retains 87 percent of its magnitude under this control but loses precision, as expected for a rare outcome. Second, restricting the comparison to SSC children, whose families also held insurance and who share any access trends specific to covered populations, doubles the effect to -0.39 percentage points ($t = -8.7$). The estimates therefore bracket a reduction in illness hospitalization of six to sixteen percent, with remaining biases signed toward zero. Figure 6 is consistent with this interpretation: SSC children, whose coverage predates the reform, show no downward break at 1993; their gap drifts mildly upward for the youngest cohorts, consistent with expanding rural access, a force that would attenuate rather than generate my estimate if present among IESS families.

Finally, obstetric admissions among women indicate a fertility margin. Eligible affiliated women record 1.8 percentage points fewer obstetric admissions per person-year on a base of 10.5. Allowing affiliated families a group-specific cohort trend attenuates the interaction to 0.39 percentage points, still statistically significant; in contrast to the illness results, the trend term itself is sizable here, indicating that part of the raw difference reflects a gradual widening of fertility differences across cohorts rather than the eligibility step alone. Restricting the comparison to SSC families yields 2.1 percentage points. The estimates therefore bracket a reduction in births at ages 17–29 of between approximately four and

[5]The attenuated 1998 point estimate (-0.05) reflects offsetting contemporaneous access effects rather than a weaker health effect: the 1998 cohort is the only one still covered during the panel window, aging out during 2016. Within that cohort, the affiliation gap in hospitalization falls from $+0.88$ percentage points in 2015, while covered, to $+0.66$ in 2016 and $+0.42$ in 2017 after aging out (year interactions -0.23, s.e. 0.08, and -0.47, s.e. 0.16). Coverage raises utilization while it lasts, consistent with Section 5.1, and the longer-run decline emerges as coverage ends.

twenty percent (Appendix Table 12). Childhood coverage thus shifted not only health but the timing of family formation.

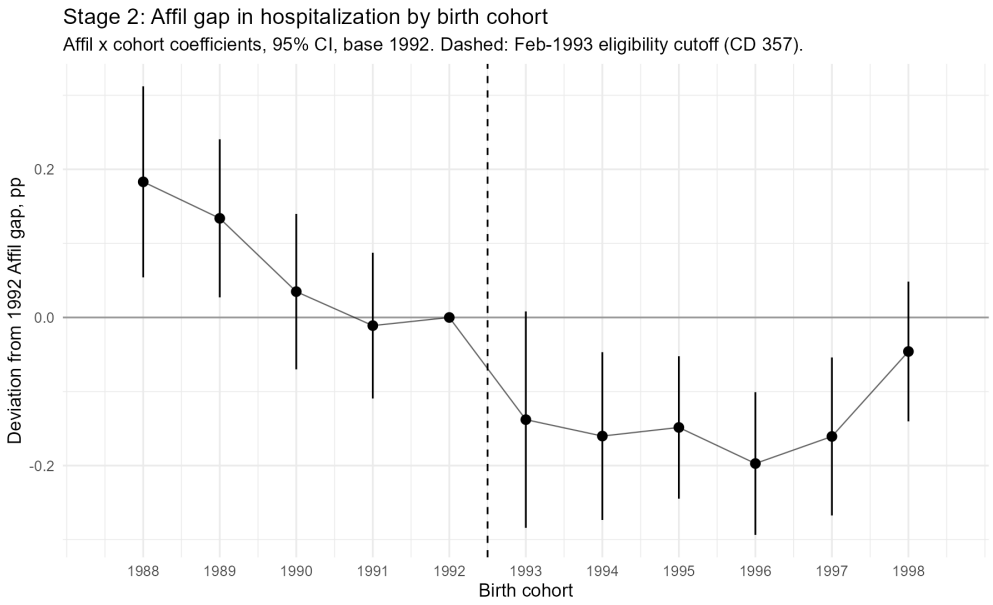


Figure 5: Event study in cohort space: deviations of the affiliated–non-affiliated gap in illness hospitalization from its 1992 level, by birth cohort, with 95% confidence intervals. Fixed effects: birth year–month, calendar year, sex, and province of birth; standard errors clustered by birth year–month. The base 1992 gap is +0.69 percentage points (s.e. 0.04). The dashed line marks the February-1993 eligibility cutoff (CD 357). The 1990 and 1991 cohorts are jointly indistinguishable from the base gap ($p = 0.55$); the text discusses the 1988–1989 convergence and the 1998 cohort.

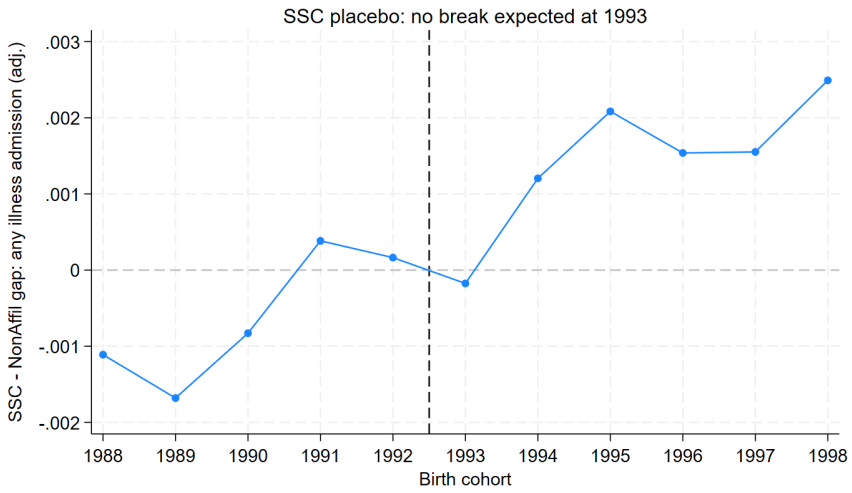


Figure 6: Placebo: gap in illness admissions between SSC and non-affiliated families by birth cohort (SSC dependents held family coverage before 2011).

Table 2: Childhood coverage eligibility and hospitalization in early adulthood

	(1) Any illness	(2) Any ACS	(3) Ill. donut	(4) Ill. affil6	(5) Ill. dose	(6) Obst. (women)
Affil	0.0075***	0.0007***	0.0075***		0.0068***	-0.0082***
	(0.0002)	(0.0001)	(0.0002)		(0.0002)	(0.0009)
Elig	0.0006**	0.0001*	0.0007**	0.0006**	0.0000	0.0069***
	(0.0003)	(0.0001)	(0.0003)	(0.0003)	(0.0003)	(0.0008)
AxE	-0.0020***	-0.0002***	-0.0020***			-0.0180***
	(0.0003)	(0.0001)	(0.0003)			(0.0011)
Affil6				0.0075***		
				(0.0002)		
A6xE				-0.0019***		
				(0.0003)		
AxExp					-0.0003***	
					(0.0001)	
Constant	0.0173***	0.0020***	0.0172***	0.0174***	0.0176***	0.1055***
	(0.0002)	(0.0000)	(0.0002)	(0.0002)	(0.0002)	(0.0005)
Observations	9564051	9564051	9499974	9564051	9564051	4723041

Table 3: Robustness: group-specific cohort trends and the SSC comparison

	(1) Illness, +Affil trend	(2) ACS, +Affil trend	(3) Illness, vs SSC
Affil	0.0071***	0.0007***	0.0083***
	(0.0003)	(0.0001)	(0.0004)
Elig	0.0004	0.0001	0.0028***
	(0.0003)	(0.0001)	(0.0006)
AxE	-0.0014**	-0.0002	-0.0039***
	(0.0006)	(0.0001)	(0.0005)
Affil_tr	-0.0001	-0.0000	
	(0.0001)	(0.0000)	
Constant	0.0174***	0.0020***	0.0163***
	(0.0002)	(0.0000)	(0.0004)
Observations	9564051	9564051	3429393

5.3 Mortality

The mortality registries permit three further margins: adult mortality of the cohort members themselves and, for their children, late stillbirth and neonatal mortality.

For adult mortality I construct a person-year panel of the 3.19 million cohort members over the two registry years with usable identifiers (2017 and 2019): 6.38 million person-years and 5,827 deaths, a rate of 0.91 per 1,000 consistent with national rates at these ages. Table 4 reports estimates by sex, the relevant split because male mortality at these ages is dominated by external causes (1.38 versus 0.44 deaths per 1,000), for which insurance has no plausible channel. Among women, eligibility reduces all-cause mortality by 0.105 per 1,000 person-years (s.e. 0.039, $p < 0.01$), approximately one quarter of the female base rate. Among men the estimate is a statistically insignificant +0.10. Cause-of-death decompositions are underpowered at these death counts: the female point estimates for amenable and external causes are both negative but individually insignificant. Placebo eligibility cutoffs at 1990 and 1996 are null (Appendix Table 10). The pattern across sexes, with an effect where insurance-sensitive causes dominate and none where external causes dominate, is the pattern a health effect of coverage would produce.

For the next generation's survival margins, I estimate the same specification on 547,767 pregnancies of cohort mothers (2013–2017, combining the fetal-death and birth registries under a common capture process) and on 203,965 births (2017 and 2019) linked to the mortality registries for neonatal death. Late stillbirth is unaffected: the interaction is −0.04 per 1,000 among teenage mothers and +0.13 among adult mothers, both insignificant (Table 5), with null placebos (Appendix Table 9) and no differential late-gestation observability by treatment status (−0.0008, s.e. 0.0015). Neonatal mortality is likewise unaffected (+0.57 per 1,000, s.e. 0.80, on a base of approximately six).

Table 4: Adult mortality by sex

Dep. Vars.:	died1000	amen1000	ext1000	died1000	amen1000	ext1000
	W all	W amen	W ext	M all	M amen	M ext
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Affil	-0.0041	-0.0039	-0.0104	-0.3973***	-0.0256*	-0.2398***
	(0.0296)	(0.0128)	(0.0208)	(0.0599)	(0.0137)	(0.0499)
Affil × Elig	-0.1047***	-0.0276	-0.0317	0.1021	0.0036	0.0529
	(0.0387)	(0.0171)	(0.0290)	(0.0828)	(0.0189)	(0.0679)
Fixed-effects						
bym	Yes	Yes	Yes	Yes	Yes	Yes
prov_birth	Yes	Yes	Yes	Yes	Yes	Yes
yrisk	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	3,148,002	3,148,002	3,148,002	3,225,152	3,225,152	3,225,152
R^2	6.64×10^{-5}	5.26×10^{-5}	6.04×10^{-5}	0.00011	7.9×10^{-5}	8.45×10^{-5}
Within R^2	3.13×10^{-6}	1.76×10^{-6}	1.37×10^{-6}	1.7×10^{-5}	1.37×10^{-6}	1×10^{-5}

Clustered birth year–month (bym) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 5: Late stillbirth by mother's age at delivery

Dependent Variable:	fetal1000	
	Teen (¡20)	Adult (20+)
Model:	(1)	(2)
Variables		
Affil	-0.2049	-0.5675**
	(0.1777)	(0.2760)
Affil × Elig	-0.0408	0.1335
	(0.4267)	(0.4726)
Fixed-effects		
bym	Yes	Yes
yr	Yes	Yes
mage	Yes	Yes
prov_ev	Yes	Yes
Fit statistics		
Observations	127,192	383,546
R^2	0.00087	0.00081
Within R^2	3.65×10^{-6}	1.5×10^{-5}

Clustered (bym) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

5.4 The next generation: birth outcomes

If childhood coverage improved health and shifted fertility, the children of treated mothers may be born into measurably different circumstances. Birth is the earliest margin at which intergenerational transmission can be observed, and I measure it in the complete annual birth registries: 593,912 births to study-cohort mothers across five registry years, with mother-cohort (year–month), birth-year, mother-age, and province fixed effects. The anchor estimates use the 2017 and 2019 registries (218,556 births), whose identifier and outcome completeness exceed 97 percent; pooling all five years, and excluding 2013, where identifier coverage is weakest, leaves every estimate essentially unchanged (Table 7). Two caveats apply: outcomes are conditional on a birth occurring, so the fertility response documented above implies compositional selection, and mothers enter through the time-invariant spine,

absorbed by cohort fixed effects.

Table 6 shows that health at birth is unchanged. The effects of mothers' eligibility on birthweight (+5.1 g, s.e. 5.1), low birthweight (−0.23 percentage points, s.e. 0.28), prematurity (−0.29, s.e. 0.28), any prenatal care (+0.03, s.e. 0.09), and low five-minute Apgar (+0.06, s.e. 0.09) are precisely estimated null effects; the birthweight confidence interval excludes effects larger than fifteen grams in either direction. Together with the stillbirth and neonatal results of Section 5.3, every health margin measurable at birth is unaffected.[6] These outcomes capture the margins measurable at delivery; whether maternal eligibility shapes the next generation's health beyond birth lies outside the window of the present registries.

5.5 Delivery mode

The one birth outcome that moves is the mode of delivery, and it requires careful treatment. Affiliated mothers deliver by cesarean 10.5 to 11.5 percentage points more often than non-affiliated mothers, on a national base near fifty percent. Among eligible cohorts this gradient is 4.8 to 6.0 percentage points smaller ($t \approx 11$ across samples), a large difference that is stable across registry years.

The cohort profile, however, does not support a causal reading (Figure 7). The cesarean gap converges continuously across the full cohort range: from +3.7 percentage points (relative to 1992) at the 1988 cohort, through the cutoff without a distinguishable jump, to −5.9 at 1998. The fitted pre-cutoff drift is 0.90 percentage points per cohort (s.e. 0.16), statistically indistinguishable from the rate at which the profile continues after the cutoff. Measured against the extrapolated pre-period trend, every post-cutoff deviation is insignificant (Appendix Figure 10). Placebo eligibility cutoffs return significant effects of −2.1 and −1.6 percentage points (Appendix Table 11), consistent with the smooth convergence. A regression-discontinuity comparison of mothers born within 12, 18, or 24 months of the February-1993 boundary finds no jump (interactions of 0.40, −0.56, and 0.10 percentage points, all insignificant), noting that scheduled exposure approaches zero at the boundary, so an exposure-based effect would itself be attenuated there.

Two interpretations are observationally equivalent in these data. A secular convergence in delivery mode between formal- and informal-sector families, predating the reform, generates exactly this profile; so does a coverage effect that accumulates with years of childhood exposure, since scheduled exposure rises approximately linearly from zero at the 1993 cohort

[6] Prematurity point estimates are uniformly negative and reach marginal significance in the pooled sample excluding 2013 (−0.42, s.e. 0.17); I note the sign pattern without placing weight on it.

to five years at 1998, and the gap narrows by roughly one percentage point per additional scheduled year. The data cannot separate these interpretations, and I therefore report the delivery-mode result as suggestive. One reading consistent with the exposure interpretation is that mothers who used IESS services in childhood continue to deliver in the public and social security systems as adults, where cesarean rates are lower than in private facilities. The two interpretations diverge for cohorts born after 1998, whose deliveries are only beginning to enter the registries: a mechanical convergence implies that the gradient continues through zero and reverses sign, while an exposure effect implies that it stabilizes once cohorts reach full childhood coverage. The continuation of this profile therefore provides a direct test between the two interpretations.

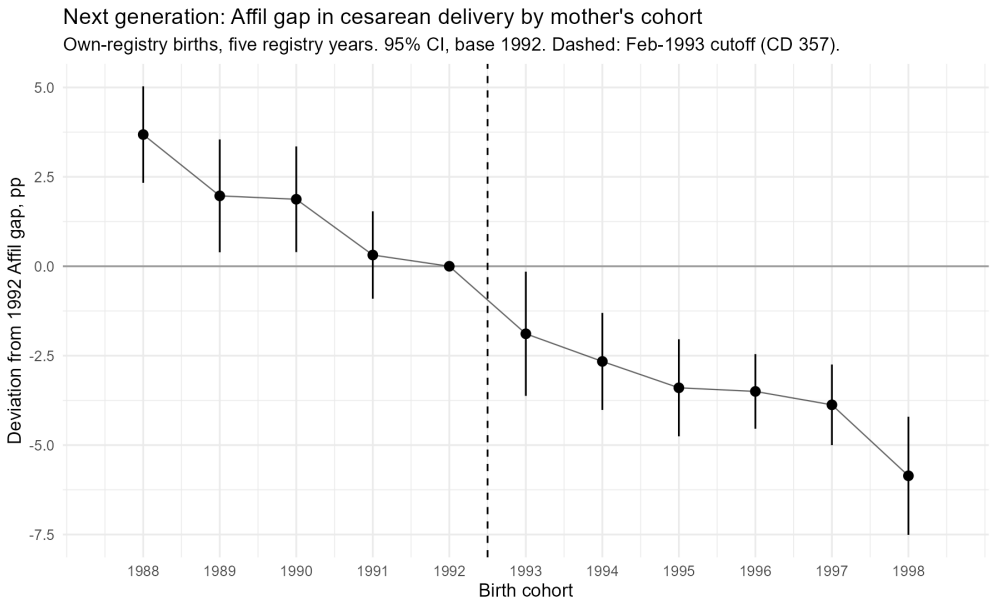


Figure 7: Event study in cohort space: deviations of the affiliated–non-affiliated gap in cesarean delivery from its 1992 level, by mother's birth cohort, with 95% confidence intervals. Complete birth registries, five registry years. The base 1992 gap is +9.1 percentage points (s.e. 0.4). The dashed line marks the February-1993 eligibility cutoff. The profile declines at approximately 0.9 percentage points per cohort on both sides of the cutoff; see Section 5.5.

Table 6: Mother's childhood coverage eligibility and birth outcomes (complete registries, 2017 and 2019)

Dep. Vars.:	I(100*lbw)	peso	I(100*preterm)	I(100*cpren_any)	I(100*cesarea)	I(100*apgar5_lo)
	LBW	Peso(g)	Preterm	AnyPrenatal	Cesarea	Apgar¡7
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Affil	0.3640**	-8.306**	0.6126***	0.5961***	11.48***	-0.0523
	(0.1770)	(3.820)	(0.2177)	(0.0550)	(0.3806)	(0.0615)
Affil × Elig	-0.2480	5.239	-0.3354	0.0199	-5.932***	0.0528
	(0.2795)	(5.039)	(0.2760)	(0.0917)	(0.5377)	(0.0890)
Fixed-effects						
bym	Yes	Yes	Yes	Yes	Yes	Yes
yr	Yes	Yes	Yes	Yes	Yes	Yes
mage	Yes	Yes	Yes	Yes	Yes	Yes
prov_ev	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	214,289	214,289	214,140	218,144	218,556	213,529
R^2	0.00781	0.03081	0.00856	0.01588	0.08178	0.00255
Within R^2	1.59×10^{-5}	2.85×10^{-5}	6.28×10^{-5}	0.00054	0.00612	3.13×10^{-6}

Clustered (bym) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 7: Birth outcomes across sample windows (five registry years)

Dependent Variables:	I(100*lbw)	peso	I(100*preterm)	I(100*cpren_any)	I(100*cesarea)	I(100*apgar5_lo)
	LBW	Peso(g)	Preterm	AnyPrenatal	Cesarea	Apgar¡7
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Affil	-0.2387*	0.9644	0.3272**	0.9134***	10.51***	-0.0463
	(0.1272)	(2.368)	(0.1288)	(0.0437)	(0.2985)	(0.0456)
Affil × Elig	0.2708	1.201	-0.3003*	-0.1445**	-4.792***	-0.0105
	(0.1789)	(3.158)	(0.1592)	(0.0647)	(0.4130)	(0.0607)
Fixed-effects						
bym	Yes	Yes	Yes	Yes	Yes	Yes
yr	Yes	Yes	Yes	Yes	Yes	Yes
mage	Yes	Yes	Yes	Yes	Yes	Yes
prov_ev	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	576,254	576,254	564,710	582,979	589,668	479,219
R^2	0.00782	0.02860	0.00677	0.02090	0.08300	0.00212
Within R^2	6.31×10^{-6}	2.28×10^{-6}	1.56×10^{-5}	0.00069	0.00553	6.06×10^{-6}

Clustered (bym) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

6 Conclusion

This paper follows the children affected by Ecuador's dependent coverage expansion from the month access opened, through early adulthood, to the births of their own children. The analysis combines the universe of hospital discharges over two decades, a civil-registry spine linking 3.2 million members of the affected cohorts to their parents, monthly employment registries fixing parental affiliation before the reform could affect it, and the complete individual-level hospital, mortality, and birth registries. Identification rests on two sources of variation: the timing of the reform across months and age groups, and the February-1993 birth cohort cutoff created by the age-eighteen eligibility rule. The associated smoothness assumption is tested throughout with placebo cutoffs, which are null where causal claims are made and which rejected other candidate designs, reported here as negative results.

The results form a sequence. Children's use of social security hospitals was unchanged through three months of statutory eligibility and rose approximately sevenfold beginning in the exact month of administrative implementation. In early adulthood, the eligible children of affiliated parents were hospitalized for illness six to sixteen percent less frequently, with

comparable declines in admissions avoidable through primary care and effects that increase with years of potential childhood coverage, and all-cause mortality among eligible young women was approximately one quarter lower. Eligible women record between four and twenty percent fewer births at ages 17 to 29. In the next generation, every health margin measurable at birth (birthweight, prematurity, prenatal care, Apgar scores, late stillbirth, and neonatal mortality) is unchanged, with precisely estimated null effects, while the mode of delivery shifts in a pattern consistent with, but not attributable to, mothers' childhood coverage.

The evidence suggests three conclusions. First, in a contributory system, access is created by administrative implementation rather than by legislation; evaluations dated to statutory changes may miss the policy entirely. Second, childhood health insurance yields measurable health returns in adulthood in a middle-income setting, extending to mortality, and the intention-to-treat design implies that these returns are lower bounds on the effect of continuous coverage. Third, transmission to the following generation at birth, the earliest margin at which it can be observed, is limited: it operates through the timing of fertility and possibly the circumstances of delivery, not through health at birth.

The children observed in these birth records are themselves only beginning to accumulate health histories, and the cohorts whose deliveries will distinguish the competing readings of the delivery-mode pattern are only now entering the registries. Following this second generation, and the maturing first one, through childhood and beyond is the natural continuation of this research. Nearer extensions include the companion event study of the first reform wave, mortality registries for the remaining years, which would permit infant mortality, and the national disability registry with acquisition dates.

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A Appendix

Table 8: Eligibility and exposure schedule by birth cohort

Birth cohort	Age, Feb 2011	Dependent-eligible (CD 357)	Years of coverage to 18
1988	22	No	0
1989	21	No	0
1990	20	No	0
1991	19	No	0
1992	18	No	0
1993	17	Partial (post-Feb births)	0
1994	16	Yes	1
1995	15	Yes	2
1996	14	Yes	3
1997	13	Yes	4
1998	12	Yes	5

Notes: CD 357 (Feb 2011) extended IESS dependent coverage to affiliates' children under 18. Cohorts born after Feb 1993 were under 18 at implementation; coverage runs from Feb 2011 to the 18th birthday. CD 265 (2009, ages 1–5) is irrelevant for these cohorts, aged 11+ in 2009. The Nov 2010–Jan 2011 de jure window produced zero utilization change (Table 3).

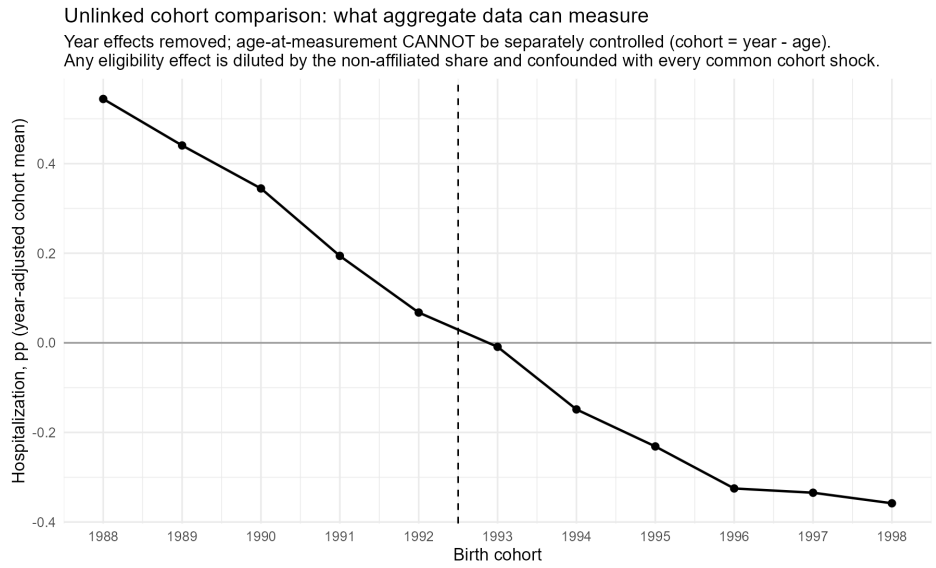


Figure 8: The unlinked comparison: year-adjusted cohort means of hospitalization. Without the parental link, age at measurement cannot be separately controlled (cohort equals year minus age), and any eligibility effect is diluted by the non-affiliated share and confounded with common cohort shocks.

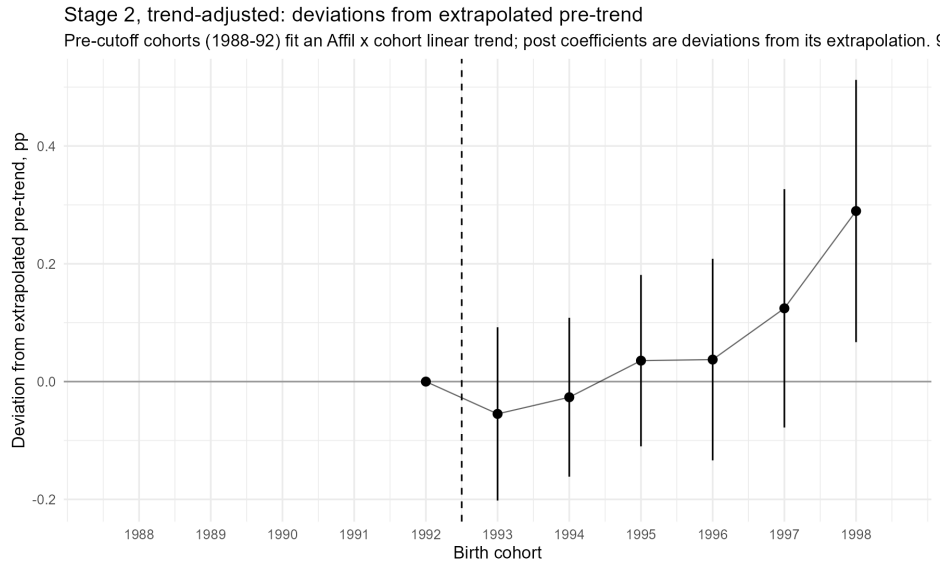


Figure 9: Stage 2, trend-adjusted: pre-cutoff cohorts (1988–92) fit an affiliated-specific linear cohort trend; post-cutoff coefficients are deviations from its extrapolation. The extrapolated trend is rejected out of sample at the 1998 cohort (+0.29, 95% CI 0.07 to 0.51).

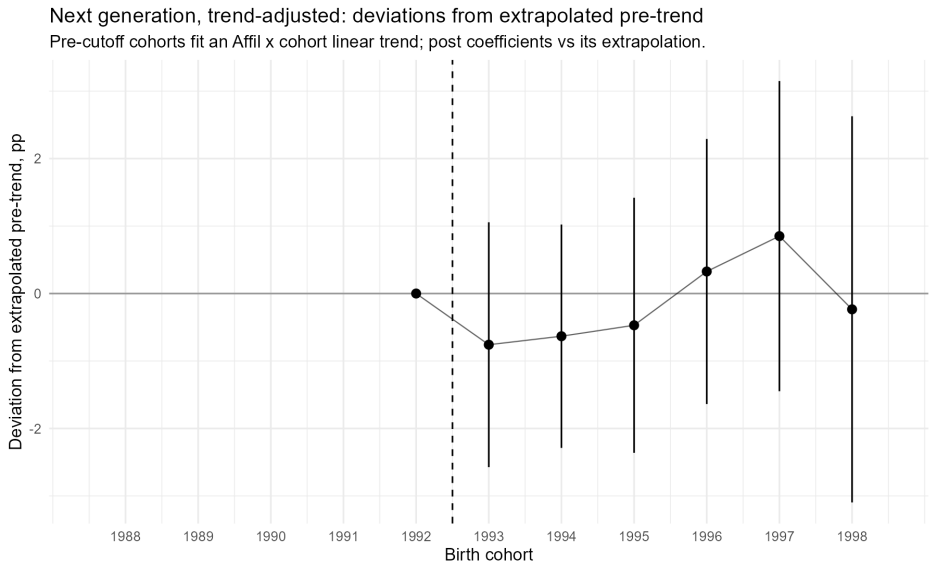


Figure 10: Delivery mode, trend-adjusted: post-cutoff deviations from the extrapolated pre-period trend in the cesarean gap. All deviations are statistically insignificant; the profile is consistent with a single continuous convergence.

Table 9: Placebo eligibility cutoffs: late stillbirth

Dependent Variable:	fetal1000	
	Fake Feb-1990	Fake Feb-1996
Model:	(1)	(2)
Variables		
Affil	-0.9168*	0.1530
	(0.4606)	(0.3979)
Affil × ElgF	0.5650	-0.7752
	(0.5658)	(0.5122)
Fixed-effects		
bym	Yes	Yes
yr	Yes	Yes
mage	Yes	Yes
prov_ev	Yes	Yes
Fit statistics		
Observations	228,745	230,449
R^2	0.00085	0.00070
Within R^2	2.25×10^{-5}	1.29×10^{-5}

Clustered (bym) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 10: Placebo eligibility cutoffs: adult mortality

Dependent Variable:	died1000	
	Fake Feb-1990	Fake Feb-1996
Model:	(1)	(2)
Variables		
Affil	-0.2373***	-0.1378***
	(0.0546)	(0.0493)
Affil × ElgF	0.0476	-0.0747
	(0.0701)	(0.0680)
Fixed-effects		
bym	Yes	Yes
prov_birth	Yes	Yes
sexo	Yes	Yes
yrisk	Yes	Yes
Fit statistics		
Observations	2,732,531	3,037,632
R^2	0.00030	0.00030
Within R^2	9.11×10^{-6}	7.97×10^{-6}

Clustered (bym) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 11: Placebo eligibility cutoffs: cesarean delivery

Dependent Variable:	I(100*cesarea)	
	Fake Feb-1990 (pre only)	Fake Feb-1996 (post only)
Model:	(1)	(2)
Variables		
Affil	11.71***	6.649***
	(0.4762)	(0.3909)
Affil × Elig_f	-2.123***	-1.605***
	(0.5824)	(0.5040)
Fixed-effects		
bym	Yes	Yes
yr	Yes	Yes
mage	Yes	Yes
prov_ev	Yes	Yes
Fit statistics		
Observations	263,501	326,166
R^2	0.07770	0.07117
Within R^2	0.00868	0.00286

Clustered (bym) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 12: Additional robustness: fertility trends and the age restriction

	(1) Obst. +trend	(2) Obst. vs SSC	(3) Ill. age≥19	(4) ACS age≥19
Affil	-0.0156***	-0.0061***	0.0074***	0.0007***
	(0.0010)	(0.0015)	(0.0002)	(0.0001)
Elig	0.0028***	0.0176***	0.0008***	0.0001*
	(0.0009)	(0.0019)	(0.0003)	(0.0001)
AxE	-0.0039**	-0.0210***	-0.0025***	-0.0002***
	(0.0015)	(0.0019)	(0.0003)	(0.0001)
Affil_tr	-0.0026***			
	(0.0003)			
Constant	0.1078***	0.0967***	0.0176***	0.0021***
	(0.0006)	(0.0014)	(0.0001)	(0.0000)
Observations	4723041	1692081	8659463	8659463

A.1 Designs rejected by falsification

One further outcome family was evaluated and rejected by the falsification battery; I record it as a negative result. For the cohort members' own adult formality, measured in 40.7 million person-years of the IESS registries, placebo cutoffs reproduce the estimate at the true cutoff almost exactly (-1.68 and -1.35 against -1.70) and the dose specification is null: the intergenerational formality premium erodes smoothly across all of these cohorts, and any effect of childhood coverage on own formality cannot be measured within it. These rejections are informative about the validity of the design elsewhere: the battery detects smooth cohort trends in affiliated-family advantages wherever they exist, and the absence of rejections for the health and mortality outcomes is correspondingly meaningful.