

THE IMPACT OF CERTIFICATE OF NEED LAWS ON INFANT AND POST-NEONATAL
MORTALITY

by
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DEDICATION

To God, my family, and innumerable friends. Couldn't have gone this far without you.

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ABSTRACT

Certificate of Need (CON) laws require healthcare providers to obtain prior government approval before expanding facilities or services. First mandated by federal legislation in 1974 and adopted by nearly every state, these laws were designed to contain healthcare costs by restricting supply. Their effects on infant health outcomes have never been rigorously estimated. This paper provides the first causal estimates of CON laws' effects on infant and post-neonatal mortality, exploiting the staggered adoption and repeal of CON programs across states from 1968 to 2019. Using a county-year panel constructed from CDC Restricted Use Vital Statistics data, I estimate effects separately across two policy margins – adoption and repeal – and two outcome families, with race-stratified analyses motivated by the persistent Black-White infant mortality gap. Identification relies on the staggered difference-in-differences estimator of Callaway and Sant'Anna (2021), which recovers cohort-specific average treatment effects while avoiding the biases of two-way fixed effects regression under heterogeneous treatment timing. On the adoption margin, CON laws are associated with a statistically significant increase in aggregate infant mortality of 0.791 deaths per 1,000 births ($SE = 0.325$), approximately 4 percent of the adoption-era baseline rate of 19.2 deaths per 1,000 births. Pre-treatment event-study coefficients are tightly clustered near zero, supporting the parallel trends identifying assumption. Race-stratified adoption-era models are directionally consistent – point estimates are positive for White infant mortality (0.348), Black infant mortality (0.974), and Black post-neonatal mortality (0.676) – but none reach statistical significance once the panel is stratified by race. On the repeal margin, the overall infant mortality estimate is a precise null ($ATT = 0.151$, $SE = 0.378$), with confidence intervals ruling out effects larger than roughly 10 percent of the repeal-era baseline. Post-neonatal and race-stratified repeal estimates are indistinguishable from zero. These findings are consistent with a mechanism in which CON entry restrictions reduced access to obstetric, neonatal, and outpatient pediatric services during the formation period of the U.S. hospital CON regime, modestly raising infant mortality in the adoption era.

INTRODUCTION

Each year in the United States, roughly six infants die for every thousand born. That rate, while dramatically lower than it was a half-century ago, remains higher than in most peer nations and contains a stark racial disparity: Black infants die at roughly twice the rate of White infants, a gap that has proven stubbornly persistent across decades of policy attention. Understanding what drives these differences – and which policy levers, if any, move them – is one of the more consequential open questions in health economics.

This paper studies a regulatory intervention that shaped the structure of American healthcare markets for more than four decades but whose effects on infant health have never been rigorously estimated: Certificate of Need (CON) laws. CON programs require hospitals and other healthcare providers to obtain prior government approval before building new facilities, adding beds, or offering certain services. First mandated by federal law in 1974 and adopted by nearly every state in the years that followed, these laws were explicitly designed to restrain healthcare cost inflation by limiting the expansion of supply (U.S. Congress, 1974; Mitchell 2024). The policy logic rested on a premise that runs counter to standard competitive market theory. In ordinary goods markets, expanding supply puts downward pressure on prices. Healthcare, policymakers argued, was different. The intellectual foundation for CON was Roemer's Law – the empirical regularity, documented by Milton Roemer, that in an insured population a hospital bed built tends to be a bed filled. The posited mechanism is supplier-induced demand: because patients are less informed than their providers and bear little of the marginal cost of care under third-party reimbursement, a physician with available capacity can

often induce utilization that would not otherwise occur. This dynamic was especially acute in the early CON era, when Medicare and Medicaid reimbursed hospitals on a cost-plus basis – paying whatever providers spent – stripping away any price signal that might otherwise have disciplined expansion (Mitchell 2024). In this environment, policymakers reasoned, more beds would not lower unit costs; they would simply generate more utilization, more reimbursement claims, and higher total spending. Restricting supply at the planning stage was therefore framed not as a market distortion but as a second-best correction for a market already distorted by information asymmetry and insulated pricing. Both the veracity of this argument and the impact of CON laws on public health outcomes is contested (Hellinger 2009; Stratmann and Russ 2014; Mitchell 2016, 2024).

Infant mortality is a theoretically motivated outcome for studying CON laws because the supply-side restrictions these programs impose operate through channels directly relevant to birth outcomes. Obstetric and neonatal care facilities are among those most commonly covered by CON review. Restrictions on hospital entry and bed expansion may limit access to delivery facilities, reduce the availability of neonatal intensive care units, and thin the supply of outpatient pediatric services – all of which are plausibly connected to infant survival. For high-risk infants in particular, access to the right level of care at birth matters enormously: delivery outside a level III hospital is associated with a roughly 60 percent increase in the odds of neonatal death for very low birthweight infants (Lasswell et al. 2010). If CON laws constrain the supply or geographic distribution of high-level neonatal facilities, the welfare consequences may extend beyond the cost-containment motivations of the original legislation.

Among the more credible quasi-experimental evidence linking hospital quality and access to infant health outcomes is Hollingsworth et al. (2024), who exploit variation from a large-scale hospital modernization program and find that improvements in hospital capacity and physician quality were associated with reductions in infant mortality. Their estimated effects for Black infants are approximately three times as large as those for White infants. If Hollingsworth et al.'s estimates are taken at face value, they suggest that restricting hospital supply should worsen infant outcomes, and that the effect should be larger for Black infants than for White infants. That makes CON a policy worth examining for both aggregate and race-stratified infant mortality.

I estimate the effect of CON laws on infant and post-neonatal mortality using a county-year panel constructed from CDC Vital Statistics data covering the period 1968 to 2019. The panel records births and deaths by county, year, and race group, allowing me to construct infant mortality rates (IMR) and post-neonatal mortality rates (PN) for the overall population and separately for White, Black, and other race births. The analysis exploits two distinct waves of policy variation: the staggered adoption of CON laws across states beginning in the late 1960s, and the staggered repeal of those laws following the federal government's withdrawal of the NHPDA mandate in 1986. Because states adopted and repealed CON at different times, the panel contains substantial treatment timing variation across both policy margins. To credibly identify causal effects from this staggered treatment structure, I use the difference-in-differences estimator of Callaway and Sant'Anna (2021). This approach estimates cohort-specific average treatment effects – comparing counties first treated in a given year to counties that have not yet been treated at that point – and then aggregates them into event-study estimates and

average post-treatment effects. The estimator avoids the well-documented biases of two-way fixed effects regression when treatment effects are heterogeneous across cohorts, a concern that is especially relevant here given the long adoption window and the substantial variation in when states entered and exited CON regulation (Baker et al, 2025). I estimate adoption effects over the 1968–1983 window and repeal effects beginning in 1980, treating the two policy margins separately and using not-yet-treated counties as the comparison group in each case. Standard errors are clustered at the state level throughout.

For overall infant mortality, CON adoption is associated with an average post-treatment effect of 0.791 additional deaths per 1,000 births ($SE = 0.325$), statistically significant at the 5% level. This represents roughly 4% of the pre-treatment average IMR of 19.2 deaths per 1,000 births for the adoption period. The 95% confidence interval of $[0.185, 1.428]$ rules out a null effect and is consistent with a small-to-moderate increase in overall infant mortality as a result of CON law adoption. Though all adoption era models produce positive point estimates, only the overall infant mortality estimate yields significant findings. On the repeal margin, the corresponding estimate is a precise null: overall infant mortality is essentially unchanged following repeal ($ATT = 0.151$, $SE = 0.378$), and the confidence interval rules out effects larger than approximately 10.6% of the repeal-era pre-treatment weighted average baseline rate. The contrast between the two margins is consistent with the broader staggered-repeal evidence in Cantor et al. (2025), who find null effects for all-cause and cardiovascular adult mortality. The race-stratified models are directionally consistent with the overall adoption-era finding but much less precise. Adoption-era point estimates for White and Black infant mortality are both positive (0.348 and 0.974 deaths per 1,000 births respectively), but neither reaches conventional

significance, reflecting the smaller sample of county-years that satisfy the minimum-births threshold once the panel is stratified by race. Adoption-era post-neonatal mortality estimates are likewise positive across subgroups – including a point estimate of 0.676 deaths per 1,000 births ($SE = 0.579$) for Black post-neonatal mortality, about 6% of its pre-treatment baseline – but again statistically indistinguishable from zero. Repeal-era post-neonatal estimates are small and imprecise across subgroups. Overall, the results point to a modest positive effect of CON adoption on aggregate infant mortality, with race-stratified evidence that is directionally supportive but too imprecise to sustain subgroup-specific claims.

The remainder of the paper proceeds as follows. Section II provides background on the history of CON regulation and the mechanisms through which it may affect infant health. Section III details the data and sample construction. Section IV reviews the relevant literature. Section V describes the empirical strategy. Section VI reports the main results. Section VII concludes.

BACKGROUND

The Rise of Federal Health Planning

The modern American hospital sector was substantially shaped by two decades of federal investment that preceded the period this paper studies. The Hospital Survey and Construction Act of 1946, commonly known as Hill-Burton, channeled federal capital grants toward hospital construction with a particular emphasis on rural and underserved areas, expanding the supply of beds in ways that were largely uncoordinated with local demand conditions (Starr 1982). Medicare and Medicaid, enacted in 1965 as part of Lyndon Johnson's Great Society agenda, then dramatically expanded the demand side of the market by extending coverage to the elderly and low-income populations respectively. The fiscal consequences were rapid and severe. Total national health expenditures more than tripled in the decade following enactment, rising from roughly \$42 billion to \$133 billion by the mid-1970s (Centers for Medicare and Medicaid Services 2024). Policymakers attributed much of this increase to a dynamic in which federally subsidized demand met an institutionally unrestrained supply side: hospitals could invest freely in capacity and new technologies, and the costs of doing so were largely passed through to third-party payers.

This diagnosis – that insurance-induced demand subsidies combined with unconstrained provider investment produced socially wasteful levels of spending – became the intellectual foundation for a new wave of supply-side regulation. Its most consequential expression was the National Health Planning and Resources Development Act of 1974 (NHPDA). Congress framed the legislation explicitly around the view that existing federal health financing had failed to rationalize the distribution of resources, stating that the "massive infusion of Federal funds

into the existing health care system has contributed to inflationary increases in the cost of health care and failed to produce an adequate supply or distribution of health resources" (U.S. Congress 1974). The NHPRDA created a national network of state and local health planning agencies and, critically, conditioned access to certain federal healthcare funds on each state establishing a Certificate of Need (CON) program meeting federal standards. Though the funding threat was never formally enforced, it was sufficient to bring nearly every state into compliance: by the early 1980s, all but a handful of states had enacted some form of CON law (Simpson 1985).

What CON Laws Do

A Certificate of Need law is, at its core, an entry and capacity control regime. Before a hospital or other covered provider can build a new facility, add beds, purchase major equipment above a cost threshold, or offer certain new services, it must obtain prior approval from a state regulatory agency. The agency's determination is supposed to rest on a finding of "community need" – that is, that existing capacity is insufficient to meet projected demand and that the proposed investment would not result in “wasteful duplication”. In practice, the definition of "need," the scope of covered services, and the stringency of review processes varied substantially across states and over time, making CON programs a heterogeneous rather than uniform policy instrument (Simpson 1985; Mitchell 2016).

The theoretical justification for CON rested on several related claims about healthcare market failures. First, proponents argued that because healthcare demand is largely insurance-driven, effective prices to consumers are artificially low, generating excess utilization and incentivizing providers to expand capacity beyond the socially optimal level. Robinson and Luft (1985) document this dynamic empirically, finding that hospitals in less competitive markets had

higher costs, a pattern they attributed in part to the medical arms race in which hospitals competed through capacity and technology investment rather than price. Bailey (2018) formalizes the welfare implications, showing that an entry or capacity restriction can raise or lower total spending depending on the price elasticity of demand, with insurance-induced moral hazard tending to make effective demand more inelastic and therefore more conducive to price increases following entry restrictions. Second, proponents argued that fixed costs in hospital production give rise to scale economies such that duplicative entry produces inefficient underutilization across competing facilities. Third, and most relevant to this paper, they contended that regulated capacity planning could protect providers serving high proportions of uncompensated or publicly insured patients: entry restrictions would allow incumbents to earn rents on more profitable service lines that could then cross-subsidize care for the uninsured and Medicaid populations.

Critics offered a sharply different reading. If entry is restricted, incumbents face less competitive pressure on price and quality. The rent-seeking prediction is that the regulatory process would be captured by incumbent providers with the strongest incentives and organizational capacity to participate in it, effectively using CON as a shield against entry rather than a tool of public planning (Ohlhausen 2015). From this perspective, CON programs would be expected to reduce capacity, raise prices, and potentially reduce access to care — exactly the opposite of their stated purpose. The empirical record is broadly consistent with the skeptical view: CON states have systematically fewer hospital beds per capita and fewer high-technology services, without corresponding evidence that quality improves or that cross-subsidization of indigent care increases (Hellinger 2009; Stratmann and Russ 2014; Mitchell 2016, 2024).

Adoption, Repeal, and the Staggered Policy Environment

The federal mandate behind CON adoption was short-lived. Congress repealed the planning requirements of NHPDA in 1986, reflecting a broader shift in the political economy of healthcare regulation and growing skepticism about the empirical performance of health planning programs (Mitchell 2016). This created a second wave of policy variation: in the years following federal repeal, a substantial number of states chose to dismantle or significantly narrow their CON programs, while others retained and, in some cases, strengthened them. By the early 1990s, the landscape had fragmented considerably. As of 2025, 35 states retained some form of CON regulation, though the scope of covered services and stringency of review differed markedly across states (Mitchell 2024).

This two-phase policy history – a period of staggered adoption concentrated in the late 1960s and 1970s, followed by a period of staggered repeal concentrated in the late 1980s and 1990s – generates the empirical variation this paper exploits. Because adoption and repeal occurred at different times across states, and because the counties affected by each transition can be compared to counties in states that had not yet undergone the same transition, it is possible to estimate the causal effects of both policy margins under the identifying assumptions of the staggered difference-in-differences design developed by Callaway and Sant'Anna (2021).

CON Regulation and Infant Health

The potential connection between CON laws and infant and post-neonatal mortality runs through several distinct mechanisms. The most direct operates through capacity and access. If CON restricts the entry of hospitals and the expansion of obstetric and neonatal services, it may

reduce access to prenatal care, labor and delivery services, and neonatal intensive care, particularly in rural and lower-income areas where the supply of such services is already thin. Evidence from the hospital entry literature confirms that CON materially constrains the extensive margin of provider supply: Stratmann and Russ (2014) find significantly fewer hospital beds per capita in CON states, and Melo et al. (2025) show that hospital CON repeal increases the number of facilities per capita. A meta-analysis by Lasswell et al. (2010) finds that birth outside a level III hospital is associated with a roughly 60 percent increase in the odds of neonatal or predischarge mortality for very low birthweight infants. To the extent that CON laws constrain the supply or distribution of high-level neonatal facilities, this evidence suggests a plausible pathway from capacity restrictions to infant mortality.

A second mechanism operates through quality of care. Market concentration among hospitals – which CON may raise by limiting entry – has ambiguous effects on quality. The industrial organization literature on quality competition suggests that concentration can either raise or lower quality depending on whether competition disciplines provider effort and whether patients are sufficiently sensitive to quality to sustain investment incentives (Gaynor and Town 2012). For a patient population accessing care largely through public programs with administered reimbursement rates, the quality-competition channel may be relatively weak, making the quantity-access channel more important for infant health outcomes.

A third mechanism concerns the racial and socioeconomic distribution of effects. If CON restrictions bind more tightly in markets with limited incumbent capacity and relatively unresponsive demand – conditions more likely to characterize rural areas and areas with higher proportions of Black and low-income residents – then the distributional consequences of CON

may be concentrated among already-disadvantaged populations (Chandra and Skinner 2003; Baicker, Chandra, and Skinner 2005). Black infant mortality rates have historically been approximately twice those of White infants in the United States, a gap that reflects a range of socioeconomic, environmental, and structural factors including differential access to prenatal and obstetric care (MacDorman and Mathews 2011). To the extent that CON limits the supply of obstetric and neonatal services in areas with higher Black birth shares, it may contribute to the persistence of this disparity. The race-stratified analyses presented below are motivated in part by this concern.

The post-neonatal period – the interval from 28 days to one year of age – is particularly relevant for a study of access-related mechanisms. Shapiro et al. (1980) demonstrate that infant deaths divide meaningfully along these lines: neonatal mortality is primarily associated with prenatal and intrapartum factors, including congenital anomalies and complications arising at or near delivery, while post-neonatal mortality is more strongly associated with environmental conditions present after hospital discharge, including socioeconomic and sociodemographic factors that shape infant health over the first year of life. This distinction motivates separate estimation of post-neonatal effects: if CON laws affect health primarily through restricting outpatient and primary care capacity – which falls within the scope of CON regulation in many states – the consequences are more likely to appear in post-neonatal than neonatal mortality, since the post-neonatal period is when infants are most exposed to the environmental and care-access conditions that CON plausibly shapes. Specifically, CON review has historically extended beyond inpatient hospital beds to encompass specific outpatient facility types, most relevantly home health agencies and ambulatory care facilities (Mitchell 2025). By constraining the entry

and expansion of home health providers, CON may limit access to post-discharge nursing visits that monitor feeding, weight gain, jaundice, and infection in the period immediately after birth – care that is particularly consequential for infants discharged from neonatal intensive care units.

The empirical record supports this concern: compared to states without CON laws, home health care expenditure growth has been significantly slower in CON states, with the most pronounced suppression of community-based care occurring in states whose programs cover both nursing home and home health industries (Rahman et al. 2016). CON laws generally do not extend to physician offices or standalone primary care clinics, but where CON constrains the supply of hospital-affiliated outpatient departments and ambulatory facilities, the overall density of accessible outpatient care settings declines and effective travel distances to routine well-infant visits increase – a pressure felt most acutely in rural and low-income markets where the provider base is already thin (Stratmann, Bjoerkheim, and Koopman 2025). It is through these outpatient and community-care channels, both home health and the broader ambulatory supply, that CON regulation most plausibly shapes the environmental and care-access conditions driving post-neonatal mortality.

DATA

Certificate of Need Laws

The CON policy indicator is constructed from two primary sources. State adoption and repeal dates are drawn from the State Policy Index (Ruger and Sorens, 2008), and from Cauchi and Noble (2016), a compilation of state CON program histories published by the National Conference of State Legislatures (NCSL) that draws on Westlaw statutory records and American Health Planning Association survey data. For states and years where these two sources disagreed, or where coverage was incomplete, I supplemented with independent verification from state legislative codebooks, official state health planning agency records, and contemporaneous news coverage. Several edge cases required particular care: Shortly after the repeal of NHPRDA, Arizona repealed its CON regulations for nearly all procedures excluding air ambulances (Mitchell and Koopman, 2016). As such it is generally counted as a non-CON state in empirical analysis. Minnesota and Wisconsin do not have official CON programs, but do maintain legal moratoriums subject to individual exemptions, in both states constituting a functional equivalent to CON for most purposes. For this reason, they are coded as CON states in the data for the years over which these laws have been in place. Indiana maintained two separate CON periods (1980–1996 and 1997–1999). Because of the data requirements of the models I estimate – namely monotonicity in the treatment variable status – it is included in the adoption era sample, but not the repeal era sample. The resulting variable is a binary indicator equal to one in each year a state had an active CON law, coded at the state level and merged to all counties within that state.

Figure 1 displays the resulting policy variation. Three features of the CON landscape are worth noting. First, adoption was rapid following the NHPRDA in 1974: by the early 1980s,

virtually all states had enacted CON programs. Second, the repeal wave following the federal withdrawal of the mandate in 1986 was substantial but incomplete, with a meaningful share of states retaining CON programs through the end of the sample period. Third, there is considerable heterogeneity in both the timing and the durability of CON regulation: some states adopted early and never repealed, others adopted and repealed within a decade, and a small number oscillated between CON and non-CON status. The analysis treats adoption and repeal as two distinct policy margins, estimated separately, and excludes counties in states that re-adopted CON after an initial repeal from the repeal analysis.

Vital Statistics Data

The mortality and birth data come from the National Center for Health Statistics (NCHS) Restricted Use Vital Statistics files, accessed through the CDC’s restricted-use data program (Centers for Disease Control and Prevention 2024). These files contain individual-level records for all registered births and deaths in the United States, with geographic identifiers at the county level. From these records I construct, for each county-year cell, the number of live births and the number of infant deaths (deaths within the first year of life) and post-neonatal deaths (deaths between 28 days and one year of age), separately for the overall population and by race group (White, Black, and Other). The panel covers 1968 through 2019.

Infant mortality rates (IMR) and post-neonatal mortality rates (PN) are expressed as deaths per 1,000 live births. Demographic covariates used in the doubly robust specifications—county-year percent White, percent Black, and percent female—are drawn from the natality files and constructed as the race and sex composition of births in each county-year cell.

Figure 2 plots the birth-weighted national average IMR by race group from 1968 through 2019. The figure illustrates two features of the data that are central to the paper’s empirical design. First, infant mortality fell sharply over this period – by roughly 75 percent for all groups – creating a steeply declining trend that the difference-in-differences design must absorb through parallel trends. Second, the Black-White mortality gap is large and persistent: Black infants died at roughly twice the rate of White infants throughout the period, a disparity that motivates the race-stratified analyses presented below. Figure 3 presents the analogous trends for the post-neonatal period. Post-neonatal mortality is substantially lower in level than overall IMR and follows a similarly steep declining trajectory; the Black–White gap is again pronounced and persistent across the full sample period.

Sample Construction

The estimation sample is constructed from the county-year panel by applying a series of filters. County-years with fewer than 10 live births in the relevant race-specific cell are dropped throughout, since mortality rates computed on very small denominators are highly volatile. For the post-neonatal models, county-years in which the PN rate exceeds the IMR rate are excluded as likely data artifacts, reflecting cases where NCHS recorded post-neonatal deaths inconsistent with total infant death counts. Table 1 reports summary statistics for the pre-treatment county-years used in each analysis.

For the adoption analysis, the sample is restricted to counties where the first observed CON status is zero – that is, counties not already under a CON law at the beginning of the panel. The adoption window covers 1968–1983; counties in states that adopted CON after 1983 are coded as never-treated and assigned to the not-yet-treated comparison group. For the repeal

analysis, the sample is restricted to counties in states that had adopted a CON law by 1983 and that did not subsequently re-adopt after an initial repeal. Re-adopters are excluded because the identification strategy treats the first repeal event as the treatment. The repeal sample covers 1980 through 2019.

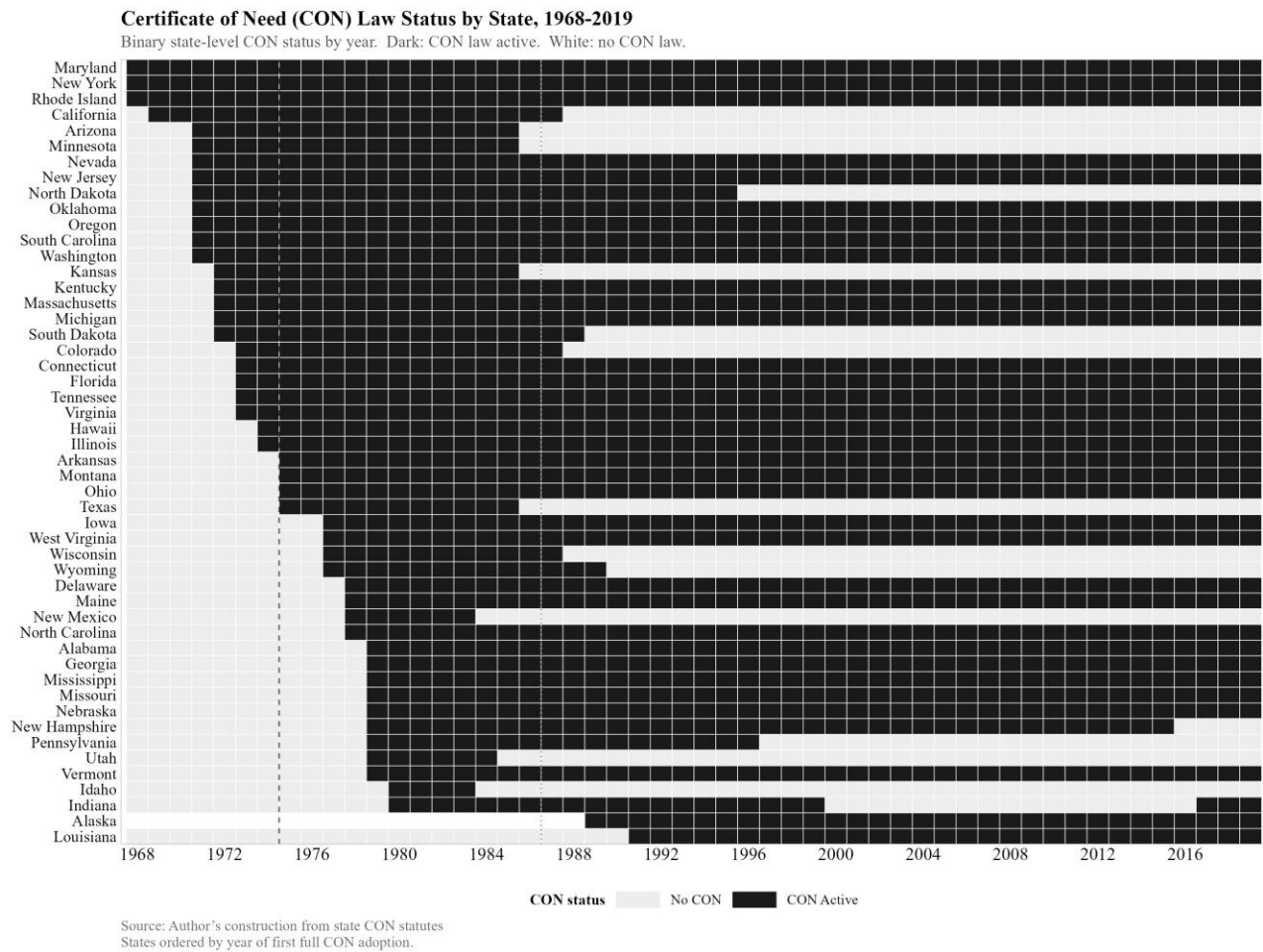
The Other race group is estimated but results are reported in the appendix, because the “Other” category encompasses a highly heterogeneous and changing population – reflecting evolving Census classification of Hispanic, Asian, and multiracial individuals – whose composition shifts over time in ways that are likely correlated with both CON policy timing and underlying infant health trends. Standard errors are clustered at the state level throughout, since treatment is assigned at the state level.

Table 1—Summary Statistics: Pre-Treatment County-Year Observations

	Adoption	Repeal
	<i>pre-treatment (1968–1983)</i>	<i>pre-treatment (1980–2019)</i>
County-year observations	21,522	85,559
Counties	2,970	2,939
<i>Mean infant mortality rate (deaths per 1,000 births)</i>		
Overall	19.19 (12.79)	8.44 (7.30)
White	16.80 (12.99)	6.99 (7.63)
Black	30.87 (101.85)	15.27 (58.72)
<i>Mean post-neonatal mortality rate (deaths per 1,000 births)</i>		
Overall	5.65 (15.38)	3.01 (10.41)
White	4.52 (10.95)	2.47 (6.10)
Black	11.25 (76.66)	5.41 (46.69)
<i>Covariates</i>		
Percent White	85.04	82.42
Percent Black	13.64	12.46
Percent Female	48.69	47.36

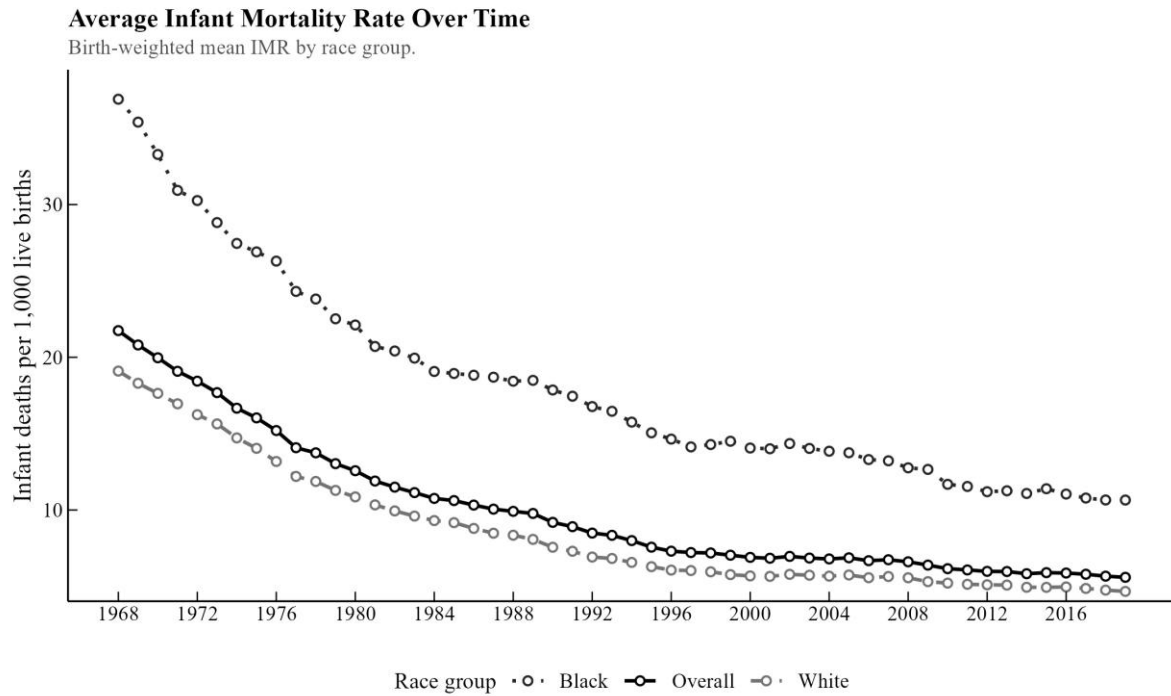
Notes: Pre-treatment county-years only. Standard deviations in parentheses; means are birth-weighted, standard deviations are unweighted. County-years with fewer than 20 births in the relevant race cell are excluded. PN statistics exclude county-years where the post-neonatal rate exceeds the infant mortality rate. IMR: infant deaths per 1,000 live births. PN: post-neonatal deaths per 1,000 live births.

Figure 1. Certificate of Need Law Status by State, 1968–2019



Notes: Binary state-level CON status by year. Dark cells: CON law active. White cells: no CON law. States ordered by year of first full CON adoption. Dashed vertical line: NHPDA enacted (1974). Dotted vertical line: NHPDA federal mandate repealed (1987). Source: Author's construction from state CON statutes, State Policy Index, Ohio State University CON compilation, and supplementary state legislative sources.

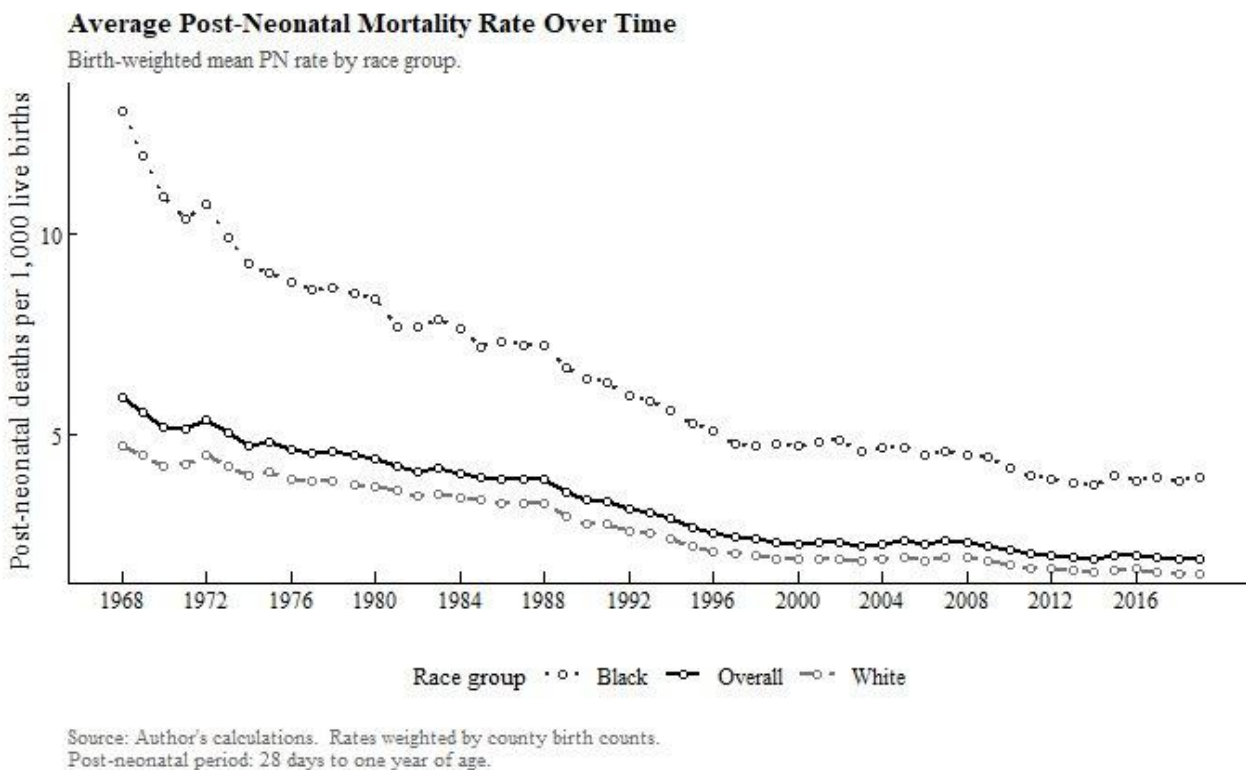
Figure 2. Average Infant Mortality Rate Over Time



Source: Author's calculations. Rates weighted by county birth counts.

Notes: Birth-weighted mean infant mortality rate by race group, 1968–2019. Rates are weighted by county-level birth counts. IMR: infant deaths per 1,000 live births. Source: Author's calculations from CDC NCHS Restricted Use Vital Statistics files.

Figure 3. Average Post-Neonatal Mortality Rate Over Time



Notes: Birth-weighted mean post-neonatal mortality rate by race group, 1968–2019. Post-neonatal period: 28 days to one year of age. Rates are weighted by county-level birth counts. County-years where the post-neonatal rate exceeds the overall infant mortality rate are excluded as data artifacts. Source: Author's calculations from CDC NCHS Restricted Use Vital Statistics files.

LITERATURE REVIEW

Institutional rationale and political economy of CON regulation

State Certificate of Need (CON) regimes are commonly justified as a planning tool intended to restrain costly capital expansion, prevent inefficient duplication, and support socially valued objectives that may be underprovided in competitive markets. A central historical account emphasizes that CON programs evolved from regional planning efforts into a policy instrument encouraged by federal legislation in the 1970s, and then persisted (often with modified scope) even after the federal policy environment shifted in the early 1980s. James B. Simpson (1985) underscores this institutional adaptability, describing how states retained and restructured CON frameworks while expanding or refocusing program goals beyond cost control, including quality assurance and technology oversight.

A parallel literature emphasizes that the welfare implications of CON are theoretically ambiguous and depend on demand conditions and institutional features that shape prices, quantities, and substitution patterns. James Bailey (2018) provides a framework in which an entry or capacity restriction can raise or lower total spending depending on the price elasticity of demand for the regulated service, with insurance-induced moral hazard tending to make effective demand more inelastic and therefore more conducive to price increases. This perspective rationalizes why CON may be observed to constrain capacity while failing to deliver systematic spending reductions, and why effects may vary sharply across services and payers.

A prominent critique stresses that CON processes can be vulnerable to rent-seeking and regulatory capture, in part because incumbent providers may have both the incentive and the institutional mechanisms to block entrants. Maureen K. Ohlhausen (2015) frames CON as a

competition-restricting policy instrument that can entrench incumbency, and the reviews by Matthew D. Mitchell (2016, 2024) synthesize a large empirical record that is, on balance, skeptical that CON consistently achieves its stated policy objectives. These high-level syntheses also highlight a recurring theme that empirical conclusions are sensitive to which CON provisions are studied (facility entry, beds, specific technologies, or service-line rules), and whether researchers can credibly separate the effects of CON from correlated policy and market conditions.

At the same time, a smaller set of studies cautions against a one-directional view of CON as necessarily reducing competition. Huan Ni et al. (2017) and Jomon A. Paul et al. (2019) report evidence – using market concentration proxies – that CON can be associated with lower measured concentration in certain settings, consistent with a model in which restricting certain forms of preemptive capital expansion could, under particular market structures, mitigate strategic deterrence by incumbents. This branch of the literature is important for interpretation because it implies that empirically observed effects of CON on “competition” or “quality” may vary by service line and by the strategic margins available to providers.

Capacity, access, and technology diffusion

Much of the empirical literature treats CON as an entry and capacity constraint and studies its implications for supply-side outcomes. Fred J. Hellinger (2009) documents that CON states have fewer short-term hospital beds per capita, with larger reductions in states with more stringent programs, and also finds that spending effects—if any—are not straightforwardly captured by a direct association between CON and per-capita expenditures. Related work emphasizes that CON can restrict both physical capacity and the availability of high-cost

technologies. Thomas Stratmann and Jacob W. Russ (2014) report evidence that CON is associated with fewer hospital beds per capita and fewer MRI/CT service providers, while also finding little support for a central policy rationale that CON-induced rents are systematically used to expand indigent care.

A distinctive contribution of the technology-focused literature is the emphasis on spatial responses and cross-border substitution. Jill R. Horwitz and Daniel Polsky (2014) show that providers may strategically locate in unregulated jurisdictions near regulated borders, implying that state-level regulation can generate geographic distortions that complicate both measurement and welfare analysis. Horwitz et al. (2024) sharpen this point by showing that CON can reduce utilization of low-value imaging while leaving high-value imaging largely unaffected, consistent with a mechanism in which regulation and enforcement scope shape the margin of adjustment and the composition of care.

The more recent “service-line repeal” literature uses staggered policy changes to estimate the effect of removing narrowly defined CON restrictions on entry and access. Stratmann et al. (2024), studying ambulatory surgical center (ASC) CON repeals, find large increases in ASC entry after repeal, especially in rural areas, and they find little evidence—though based on more limited data structure for the hospital outcomes—that such repeal systematically accelerates rural hospital closures. Melo et al. (2025) similarly study repeal of hospital-facility CON provisions and estimate that repeal increases the number of hospitals per capita but reduces beds per capita and average facility size, pointing to a provider response characterized by more, smaller facilities. Together, these findings reinforce the view that CON can materially shape the

extensive margin of provider entry and the configuration of local service supply, while leaving ambiguous the net effect on capacity for intensive services.

A parallel set of papers focuses on substance-use treatment CON provisions and highlights that CON's effects on access may be context-specific and depend on how "capacity" is operationalized. Bailey, Lu, and Vogt (2022) find limited evidence that SUD-specific CON laws reduce per-capita facilities, beds, or clients, but they do find a reduction in the share of facilities accepting private insurance, consistent with compositional changes in providers even when total counts are stable. Shakya and Bretschneider-Fries (2025) instead use a spatial access approach and find that border counties in CON states have lower geographic access to SUD treatment, in a design that directly incorporates cross-county utilization potential. This contrast illustrates a broader lesson: CON may affect where and what type of capacity is available (and to whom), not only the total number of beds or facilities.

Quality, mortality, and perinatal-relevant evidence

The literature relating CON to health outcomes is mixed and often sensitive to identification strategy and the outcome margin studied. A broad synthesis by Christopher J. Conover and Bailey (2020) compiles a large number of studies and concludes that the empirical record is heterogeneous, with narrow domains sometimes showing improvements but broader evidence frequently indicating neutral or adverse effects when considering access, spending, and welfare in aggregate. For the specific question of whether CON improves quality through centralization and planning, border-based hospital analyses provide more direct evidence linking CON to clinical outcomes: Stratmann and David Wille (2016) and Stratmann (2022) compare hospitals across borders and find that CON is associated with worse performance on several

mortality-based measures and complication outcomes, with limited evidence of offsetting gains in other quality proxies.

Evidence directly connecting CON to mortality using policy changes over time remains comparatively limited but is particularly informative for interpreting potential consequences of repeal. Cantor et al. (2025) analyze staggered CON repeal and find little association with all-cause or cardiovascular mortality, but they report a short-run decline in cancer mortality, driven largely by lung cancer, and discuss a plausible technology diffusion mechanism related to advanced imaging. Their results are especially relevant for interpreting null findings: they emphasize that measurement limitations in historical service-line coverage can attenuate estimates, and they explicitly show that conclusions can be robust to modern staggered-adoption estimators.

Within perinatal-relevant outcomes, the most directly related paper in the provided set is Scott A. Lorch et al. (2012), who link NICU CON status to NICU supply and infant outcomes in a state-level cross section using non-contemporaneous measurements of CON status and outcomes. They document substantially higher NICU supply in non-CON states, but mortality results are weak and sensitive to metro classification, and the authors note that the design cannot support a within-state causal interpretation. The limitations of this cross-sectional structure motivate designs that exploit within-state changes over time and explicitly handle staggered adoption and repeal – particularly important for infant and post-neonatal mortality where baseline risk and secular trends can differ across places and periods. Unlike Lorch et al., the present paper exploits the staggered adoption and repeal of CON laws as policy changes over time, using the Callaway and Sant’Anna (2021) estimator to recover cohort-specific causal

effects. This design resolves the causal identification concern that Lorch et al. explicitly flag and extends the scope of analysis from NICU-specific CON to the full CON regulatory regime, across both the adoption and repeal margins.

Finally, the Medicaid-focused evidence in Plemmons, Deyo, and Drain (2025) is relevant insofar as it connects CON status (for SUD treatment) to neonatal outcomes via neonatal abstinence syndrome (NAS) incidence among Medicaid births, while also highlighting the inferential limits of cross-state variation without within-state policy changes over the sample window. That paper's framing is therefore best viewed as suggestive about mechanisms linking treatment capacity and severe neonatal outcomes rather than as causal evidence about the effect of changing CON policy.

Labor markets, organizational adjustment, and mechanisms

Several papers ask whether CON operates through labor-market channels that could mediate downstream health outcomes. Jungho Bae and Bailey (2025) use CPS microdata and a triple-difference design comparing health-sector to non-health workers within state-years around CON repeal, finding largely null effects on wages and employment, with limited evidence of changes in physician employment. Their results suggest that broad staffing or wage adjustments are unlikely to be the dominant channel through which CON affects health outcomes, at least at the level of aggregated labor-market measures.

Complementary evidence from Sahil Bhai (2025), using ACS microdata, indicates that physicians in CON states have somewhat lower earnings and different practice-form patterns, and that CON status can shape how physician labor supply and organizational form adjust to external demand shocks (proxied by ACA Medicaid expansion). This perspective is useful for

interpreting how CON could change the configuration of providers and the responsiveness of the delivery system, even when average employment and wage effects appear small in aggregate. Taken together, the labor-market literature implies that the most plausible mechanisms linking CON to mortality are likely to run through service availability, facility configuration, technology adoption, or competitive pressure affecting quality, rather than through broad wage suppression or large statewide employment contractions.

Methodological implications of staggered adoption and repeal

A recurring issue across this literature is that CON policy changes are staggered and often involve both adoption and repeal, while treatment effects may be heterogeneous across cohorts, horizons, and service lines. The methodological challenges these features create for conventional two-way fixed effects (TWFE) event studies are emphasized in the practitioner guidance of Andrew Baker et al. (2025), who stress that in complex DiD settings, simple regression estimators can fail to recover meaningful causal parameters and can even yield estimates with misleading magnitudes or signs, motivating “forward-engineered” estimators that aggregate interpretable 2×2 comparisons rather than relying on a single regression coefficient.

Within the CON literature summarized here, Cantor et al. (2025) operationalize this concern explicitly by re-estimating their staggered-repeal event-study patterns with group-time ATT methods and reporting qualitatively similar conclusions. More generally, the recent service-line repeal papers (e.g., Stratmann et al. 2024; Melo et al. 2025) illustrate the value of clearly defining (i) the relevant treatment margin (hospital-facility CON, ASC CON, imaging CON, SUD-treatment CON), (ii) the implied comparison group, and (iii) how the estimand aggregates cohort-specific effects. These design choices matter both for internal validity—because “not-yet-

treated” controls can differ from “never-treated” controls—and for external validity, because the policy object is not a single scalar CON indicator but a bundle of program provisions with varying scope and enforcement. This methodological landscape helps motivate designs that separate adoption from repeal rather than pooling them into a single treatment indicator, and that tailor samples to ensure that policy reversals are interpreted as one-time changes rather than repeated-treatment sequences.

Positioning of the present study within the CON literature

The existing evidence establishes three stylized facts that frame the interpretation of infant and post-neonatal mortality effects. First, CON is consistently connected to supply-side margins – beds, facilities, and the diffusion and location of certain technologies – even when downstream spending effects are ambiguous (Hellinger 2009; Stratmann and Russ 2014; Horwitz and Polsky 2014; Horwitz et al. 2024; Stratmann et al. 2024; Melo et al. 2025). Second, the evidence on quality and mortality is mixed, with some border-based quality studies reporting worse outcomes in CON states, while staggered repeal evidence for adult mortality is closer to null overall, with more specific patterns for cancer mortality (Stratmann and Wille 2016; Stratmann 2022; Cantor et al. 2025). Third, broad health-care labor-market effects appear limited, suggesting that if CON affects mortality it may do so through service availability, facility configuration, or quality competition rather than through large statewide wage or employment shifts (Bae and Bailey 2025; Bhai 2025).

Against this backdrop, the present paper contributes to the literature in four unique ways. First, it provides the first causal estimates of CON laws’ effects on infant and post-neonatal mortality using policy variation over time. The closest precedent, Lorch et al. (2012), relies on

cross-sectional variation in NICU CON status and cannot support a causal interpretation; this paper resolves that identification problem by exploiting staggered CON adoption and repeal as natural experiments, using the Callaway and Sant’Anna (2021) estimator to recover interpretable cohort-specific treatment effects. Second, the overwhelming majority of the CON literature focuses on the impact of CON repeal after the federal mandate was rescinded. This provides a distinct advantage in terms of supporting the causal identification strategy I employ. The CON policies adopted during the existence of the NHPRDA’s enforcement all followed the same federal template and are less subject to unobserved policy heterogeneity across the adoption sample. Third, the paper extends the staggered-repeal mortality literature beyond adult outcomes. Cantor et al. (2025) study all-cause and cardiovascular mortality among adults and find largely null effects of repeal; by focusing on infants – a population whose mortality is mechanically tied to obstetric, neonatal, and outpatient pediatric access, precisely the service lines CON most directly regulates – this paper tests whether CON’s null mortality effects generalize to the population for which the access-restriction mechanism is most plausible. Fourth, motivated by Hollingsworth et al. (2024), who provide the strongest causal evidence that improvements in hospital capacity and care quality reduce infant mortality differentially for Black and White infants, the paper estimates race-stratified effects and provides the first within-state causal evidence on whether CON contributes to the Black-White infant mortality gap. The methodological emphasis in Baker et al. (2025) and the implementation choices in Cantor et al. (2025) further support the design choices here: staggered-timing estimators with transparent cohort definitions and explicit aggregation of group-time effects are essential when adoption and

repeal occur at different times with potentially heterogeneous effects across cohorts and horizons.

EMPIRICAL STRATEGY

Treatment Timing and Research Design

I estimate the effect of Certificate of Need (CON) laws on infant and post-neonatal mortality using a county-year panel and the staggered-adoption difference-in-differences estimator of Callaway and Sant’Anna (2021). Because states both adopted and later repealed CON laws at different times, I estimate adoption and repeal separately rather than pooling them into a single treatment indicator. This separation allows the relevant event time and comparison group to differ across the two policy margins.

The unit of observation is the county-year, while treatment varies at the state-year level. In the adoption analysis, a county is assigned to cohort $G_i^{adopt} = g$ if its state first has an active CON law in year g . In the repeal analysis, a county is assigned to cohort $G_i^{repeal} = g$ if its state first transitions from $CON_{t-1} = 1$ to $CON_t = 0$, with year t recorded as the repeal year. For the adoption analysis, the sample is restricted to 1968-1983. I retain only counties that are untreated at the beginning of that period, and counties in states that adopt only after 1983 are coded as untreated in the adoption specification. For the repeal analysis, the sample begins in 1980 and is limited to counties in states that had adopted CON by 1983. Counties in states that ever re-adopt CON after repeal are excluded so that repeal is treated as a one-time policy reversal rather than part of a repeated-treatment process.

Estimand and Estimation

For each treatment cohort g and calendar year t , the group-time average treatment effect is:

$$ATT(g, t) = E[Y_{it}(1) - Y_{it}(0) \mid G_i = g].$$

I estimate these effects using the `att_gt` procedure in the `did` package. In all specifications, the comparison group is composed of not-yet-treated counties. This is an important feature of the implemented estimator: newly treated counties are compared to counties that have not yet received the policy by year t , rather than to already treated counties.

To summarize treatment dynamics, I aggregate the group-time estimates into event time, where event time is defined as $\ell = t - g$. The event-study estimand is

$$\theta(\ell) = \sum_g w_g \cdot ATT(g, g + \ell), \quad \ell \in [-10, 10],$$

where w_g denotes the cohort weights implied by the aggregation procedure. I report these event-study estimates graphically and use the pre-treatment coefficients as a diagnostic for differential pre-trends. I also report the simple aggregated post-treatment average effect from `aggte`, which serves as the main summary ATT in the tables.

Outcome Measures

The analysis considers two outcome families: infant mortality and post-neonatal mortality. Both are measured as deaths per 1,000 live births in the relevant county-year-race cell. Infant mortality is defined as:

$$IMR_{it}^r = \left(\frac{Deaths_{it}^r}{Births_{it}^r} \right) \times 1000,$$

where r indexes the race group.

Post-neonatal mortality is defined as:

$$PN_{it}^r = \left(\frac{PNDeaths_{it}^r}{Births_{it}^r} \right) \times 1000.$$

For infant mortality, I estimate models for the overall population and separately for White, Black, and Other race births. For post-neonatal mortality, I estimate models for the overall, White, Black, and Other race populations in both the adoption and repeal eras, subject to the minimum-births threshold. All specifications are weighted by the number of births in the relevant county-year-race cell. Overall models are weighted by total births, while race-specific models are weighted by births for the corresponding racial group.

Sample Construction

The analysis excludes Washington, D.C., as well as a small number of county identifiers dropped in the analysis code. To reduce noise from sparse cells, I exclude county-years with fewer than 10 births in the relevant race category. This threshold is applied separately to each outcome and subgroup using the birth count associated with that specification. For post-neonatal mortality, I impose an additional consistency restriction. County-years in which the post-neonatal mortality rate exceeds the infant mortality rate are excluded. Because post-neonatal mortality is mechanically a subset of infant mortality, these cases are treated as likely data artifacts or placeholder missing values. The panel is allowed to be unbalanced, so counties do not need to appear in every year to contribute to estimation.

Baseline and Adjusted Specifications

I estimate two classes of models. The first are unconditional specifications, which include no additional controls beyond treatment timing, outcome measurement, and birth-count weights. In the did implementation, these are estimated using the regression estimator. The second are adjusted specifications for overall infant mortality and overall post-neonatal mortality. These use the doubly robust estimator. In the adoption analysis, the covariates are county-year percent White, percent Black, and percent female. In the repeal analysis, I instead use baseline values of these variables, measured in the first year of the repeal sample, to avoid conditioning on covariates that may themselves respond to repeal. Standard errors are clustered at the state level throughout, because treatment varies at the state level. Inference is based on the bootstrap procedure implemented in the did package, with 1000 bootstrap iterations. For the event-study figures, I report both 95 percent pointwise confidence intervals and 95 percent simultaneous confidence bands.

Diagnostics and Identification

The causal interpretation of the estimates relies on the cohort-specific parallel-trends assumption underlying the Callaway-Sant'Anna estimator: absent treatment, outcomes for counties treated in cohort g would have evolved similarly to outcomes for counties that were not yet treated at time t . A sufficient condition for parallel trends is that treatment timing is exogenous – that is, that states' decisions to adopt or repeal CON were not driven by differential trends in infant mortality. The institutional context of the adoption era lends meaningful support to this condition. CON adoption was largely compelled by the federal NHPRDA mandate of

1974, which conditioned access to federal healthcare funds on state compliance and created a common federal template that states followed on a compliance timeline driven primarily by legislative calendars and bureaucratic capacity rather than by state-specific health conditions. This federal pressure substantially narrows the scope for adoption timing to reflect endogenous responses to infant mortality trends, making the random implementation assumption more credible on the adoption margin than it would be in a purely voluntary policy setting.

The repeal margin warrants more caution on this dimension. Following the federal government's withdrawal of the NHPDA mandate in 1986, states made discretionary choices about whether and when to dismantle their CON programs. That discretion opens the door to endogenous timing — states that faced particular fiscal, political, or health-system conditions may have been systematically different from states that retained CON, and no federal template anchors repeal timing in the way the NHPDA anchored adoption. This asymmetry between the two margins is one reason the repeal-era estimates should be interpreted with greater care, and it informs the paper's more limited causal claims on that side of the analysis.

The event-study estimates provide a direct empirical diagnostic for the parallel trends assumption. Pre-treatment coefficients should be small and statistically indistinguishable from zero if treated and not-yet-treated counties were following similar mortality trends before the policy change. I supplement this graphical evidence with two additional checks on observable balance and common support, one for each policy margin.

For the adoption sample, I compare baseline covariate means in 1968 — the first year of the panel — across early adopters, late adopters, and never-adopting counties. The three covariates examined are the racial and sex composition of births at the county level: percent White, percent

Black, and percent female. If counties that adopted CON early were systematically different from those that adopted late on these pre-treatment characteristics, that would raise concern that the two groups were on different underlying trajectories before any policy was in place, undermining the parallel trends assumption. To formalize this comparison, I compute normalized differences between early and late adopters for each covariate. Normalized differences are preferable to t-tests for this purpose because they do not mechanically grow with sample size; values above 0.25 in absolute magnitude are the conventional threshold indicating imbalance serious enough that regression adjustment may not reliably correct for it. Normalized differences within that bound provide reassurance that the adoption cohorts are sufficiently comparable on pre-treatment observables to support the identifying assumption.

For the repeal sample, the analogous exercise takes the form of a propensity score overlap check. Using baseline covariate values measured at the start of the repeal period — importantly, fixed at their pre-period values rather than allowed to vary over time, to avoid conditioning on variables that may themselves have responded to CON policy — I estimate a simple logistic regression predicting whether a county's state ever repeals CON. The resulting predicted probabilities summarize, in a single index, how distinguishable repeal and non-repeal counties are on observable pre-period characteristics. The overlap assumption requires that for every repeal-era treated county there exist comparable not-yet-treated counties in the same region of covariate space. Counties with propensity scores near zero or one — where one group has effectively no counterpart in the other — are regions of poor overlap where the identifying assumptions are most strained and where covariate-adjusted estimates become numerically unstable. Examining the tails of the propensity score distribution therefore directly informs both

the credibility of the parallel trends assumption on the repeal margin and the decision about which specifications to prefer. The instability of the doubly robust estimates in the repeal era, which produce confidence intervals spanning implausibly wide ranges, is consistent with poor overlap driven by the small number of repeal cohorts, and it reinforces the preference for the unconditional specifications as the primary reported estimates throughout the repeal analysis.

RESULTS

CON Adoption: Infant Mortality

Table 2 and Figures 4-16 present the main results. I discuss adoption effects first, then repeal effects, organized by outcome family. The preferred specifications throughout are unconditional difference-in-differences estimates; robustness checks using the doubly robust estimator, as well as results for the Other race subgroup, are reported in the Appendix. All reported average ATTs are simple weighted averages across all estimable group–time ATTs produced by the Callaway–Sant’Anna aggregation procedure; figure footers display an average over the plotted event window and should be read as descriptive of the plot rather than as the headline estimand. Figure 4 plots the unconditional event-study estimates for overall infant mortality in the adoption sample. The pre-treatment coefficients are tightly clustered near zero, providing no evidence of differential pre-trends between adopting and not-yet-treated counties.

Post-treatment, the estimates drift upward relative to the pre-treatment pattern and remain consistently above zero throughout the event window. The average post-treatment ATT is 0.791 deaths per 1,000 births ($SE = 0.325$), statistically significant at the 5% level, with a 95% confidence interval of $[0.153, 1.428]$. This effect represents approximately 4% of the adoption-era baseline infant mortality rate of 19.2 deaths per 1,000 births. The estimate is the single statistically significant result in the main specifications and constitutes the paper’s headline finding: CON adoption is associated with a modest but detectable increase in aggregate infant mortality during the adoption era. This result is robust to Benjamini-Hochberg’s method of multiple hypothesis testing correction at the 10% level. The doubly robust specification, which

controls for county-level demographic composition, is substantially less precise in this setting and is reported in Appendix Figure A1.

The race-stratified IMR models are directionally consistent with the overall finding but substantially less precise. Figure 5 shows that the average post-treatment ATT for White infant mortality is 0.348 (SE = 0.374; 95% CI: [-0.385, 1.082]), positive but statistically indistinguishable from zero. For Black infants (Figure 6), the average post-treatment ATT is 0.974 (SE = 1.075; 95% CI: [-1.133, 3.082]), again positive but imprecise. The pre-treatment estimates for Black infant mortality are noisier than the overall model, reflecting the smaller sample of county-years with sufficient Black birth counts, and this imprecision limits the confidence with which a directional conclusion can be drawn for this outcome. Results for the Other race subgroup are reported in Appendix Figure A5; because the racial composition of the "Other race" category changes substantially over the 1968–1983 window and does so in ways that likely differ across states, these estimates are treated as supplementary rather than substantively informative.

CON Adoption: Post-Neonatal Mortality

The post-neonatal mortality results for the adoption era are directionally consistent with the IMR results – every point estimate is positive – but none of the PN estimates reach statistical significance. Figure 7 shows that overall post-neonatal mortality rises modestly following CON adoption, with an average post-treatment ATT of 0.287 (SE = 0.206; 95% CI: [-0.118, 0.692]) under the unconditional specification. The pre-treatment period shows no evidence of differential trends. Although the sign and rough magnitude of the estimate (about 5% of the adoption-era PN baseline of 5.4 deaths per 1,000 births) line up with the IMR finding, the standard error is too

large to reject the null. The doubly robust specification for overall post-neonatal mortality, reported in Appendix Figure A2, is similarly uninformative ($ATT = 0.444$, $SE = 3.435$). White post-neonatal mortality (Figure 8) shows essentially no effect following adoption, with an average ATT of 0.007 ($SE = 0.184$)—statistically indistinguishable from zero.

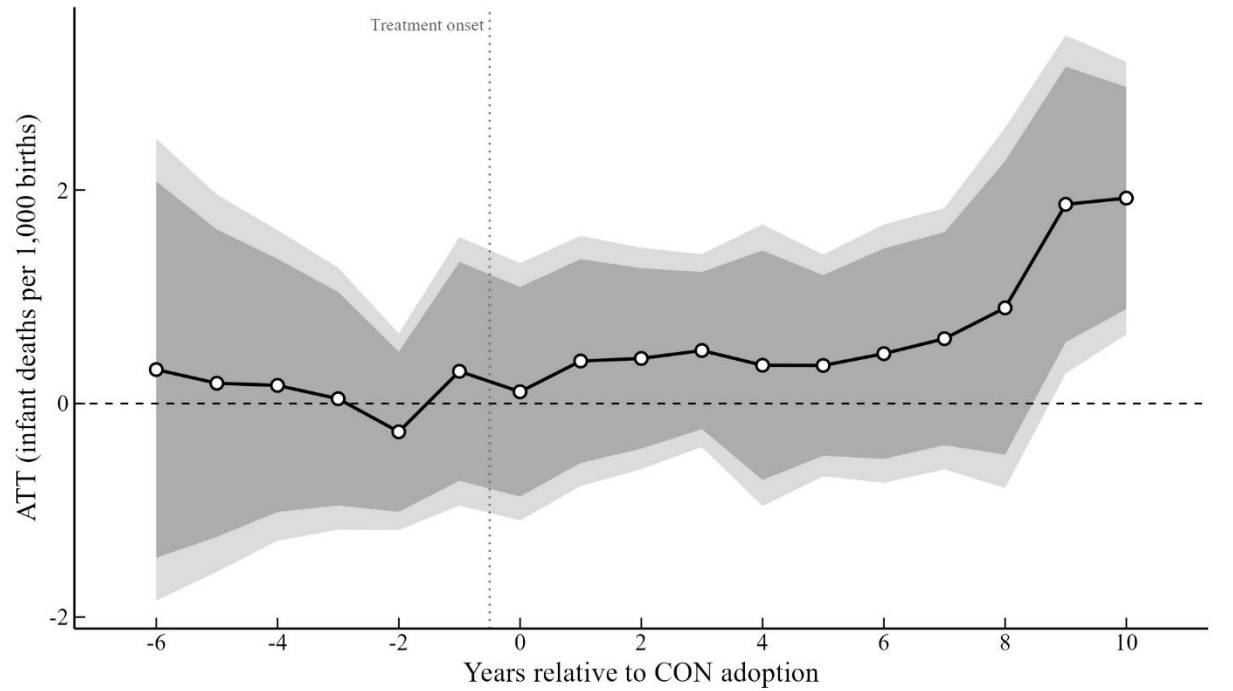
The Black post-neonatal mortality model (Figure 9) produces a positive but imprecise average post-treatment effect of 0.676 deaths per 1,000 births ($SE = 0.635$; 95% CI: $[-0.569, 1.922]$). This effect size represents approximately 6% of the adoption-era pre-treatment average Black post-neonatal mortality rate of 10.8 deaths per 1,000 births. The event-study plot shows point estimates that sit consistently above zero in the post-treatment period, directionally aligned with the overall IMR finding. However, the confidence interval comfortably includes zero, and the pre-treatment coefficients for this model are noticeably noisier than those in the overall specification, reflecting the smaller sample of county-years with sufficient Black birth counts. The pre-treatment estimates are individually statistically indistinguishable from zero and do not exhibit a systematic trend. This provides reasonable, if imperfect, support for the identifying assumption of parallel pre-trends.

The finding is consistent with a mechanism in which CON entry restrictions reduce access to outpatient pediatric services in areas with higher Black birth shares, operating through the post-neonatal period when infants are most dependent on ambulatory and primary care access. This result warrants caution – the confidence intervals are relatively wide and the pre-treatment period is noisier than for other subgroups – but the consistency and direction of the post-treatment pattern is notable.

Figure 4. CON Adoption: Overall Infant Mortality (Unconditional DiD)

Effect of CON Adoption on Overall Infant Mortality

Unconditional Callaway–Sant’Anna DiD; not-yet-treated comparison; births ≥ 20



Average post-treatment ATT = 0.719 (SE = 0.332). Comparison group: not-yet-treated counties. Standard errors clustered at state level.

Figure 5. CON Adoption: White Infant Mortality (Unconditional DiD)

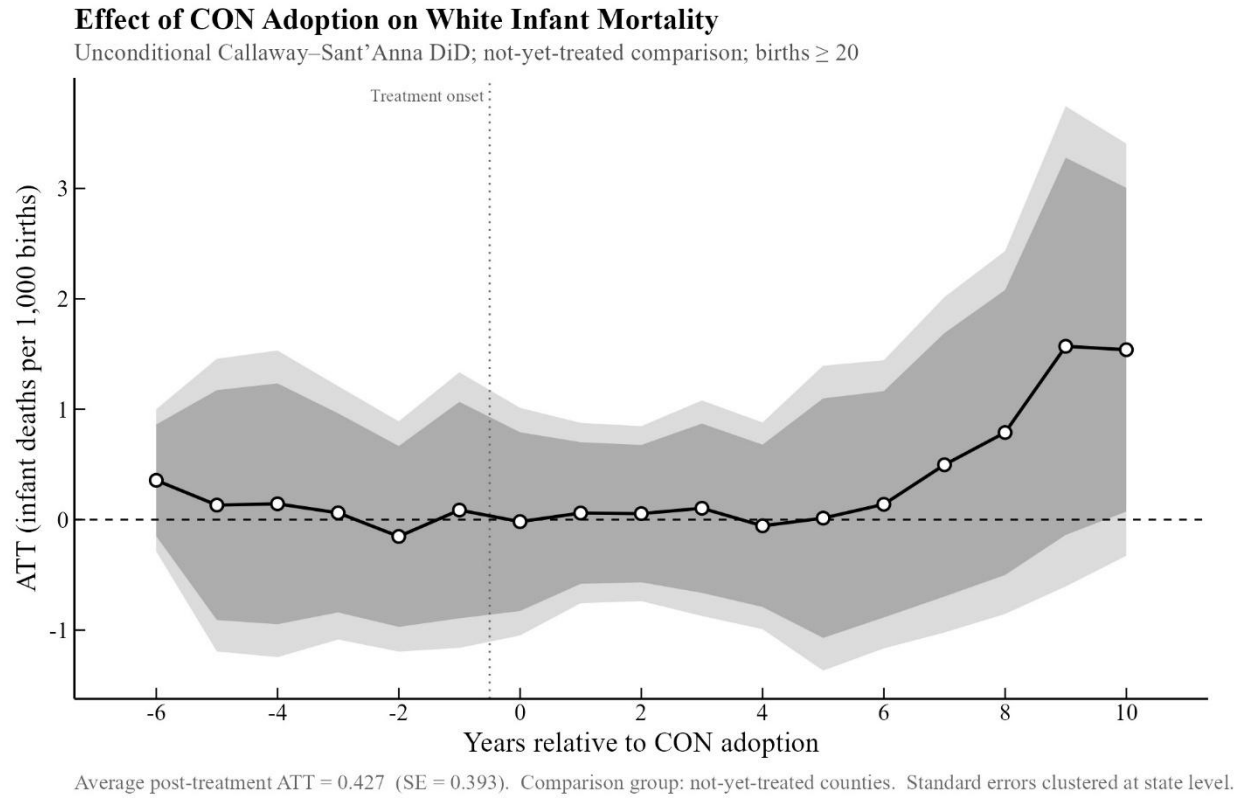


Figure 6. CON Adoption: Black Infant Mortality (Unconditional DiD)

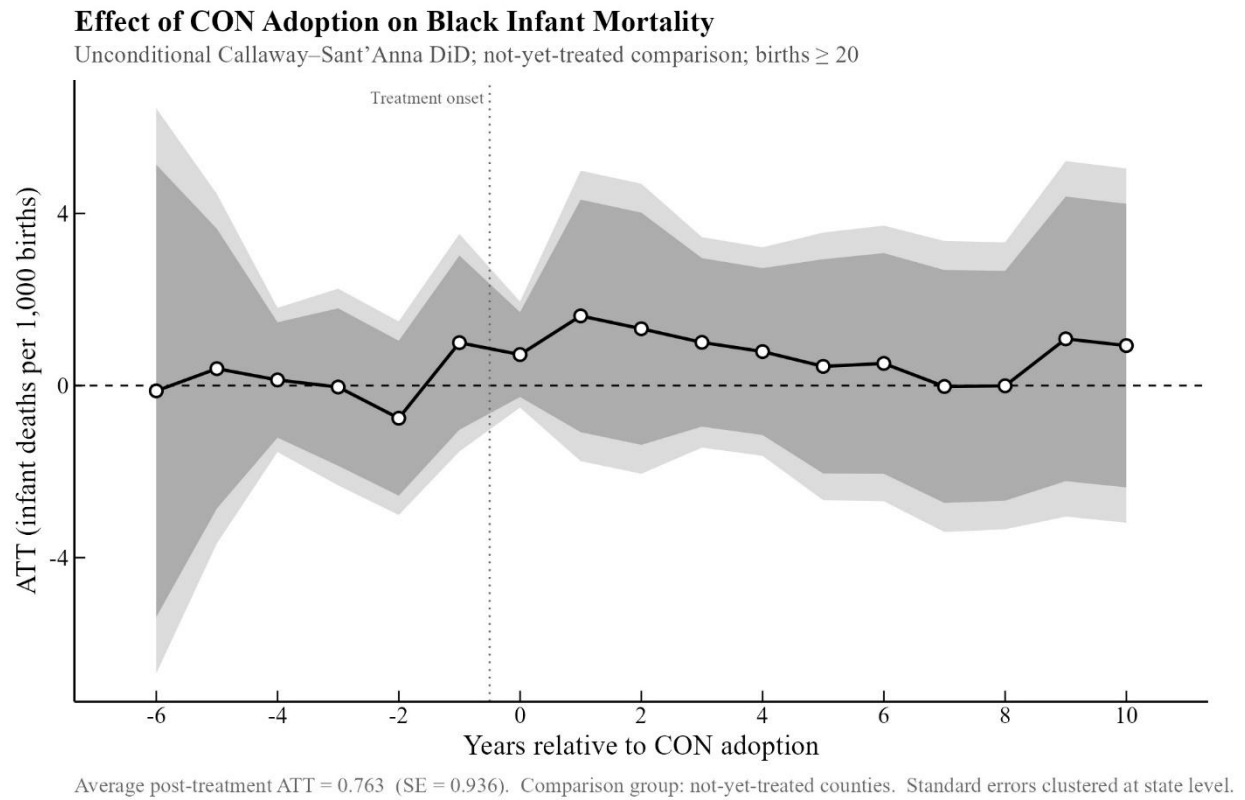


Figure 7. CON Adoption: Overall Post-Neonatal Mortality (Unconditional DiD)

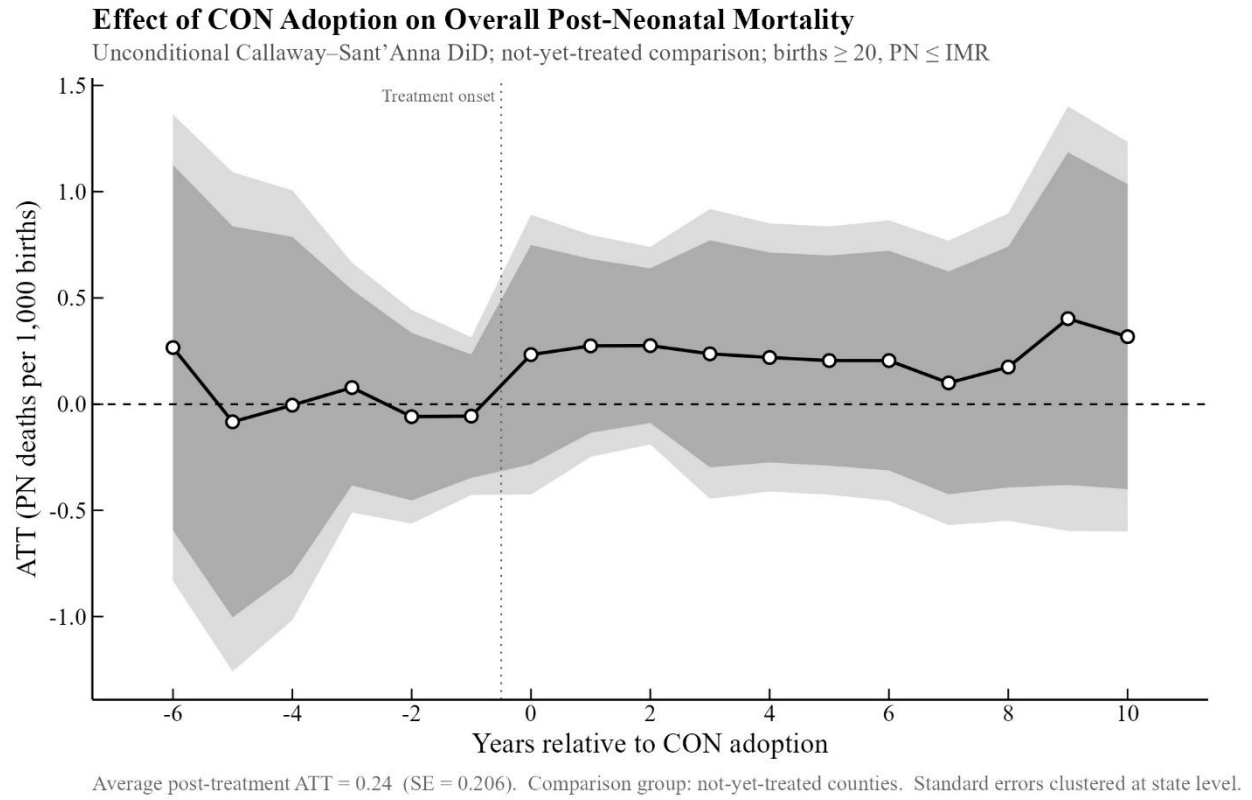


Figure 8. CON Adoption: White Post-Neonatal Mortality (Unconditional DiD)

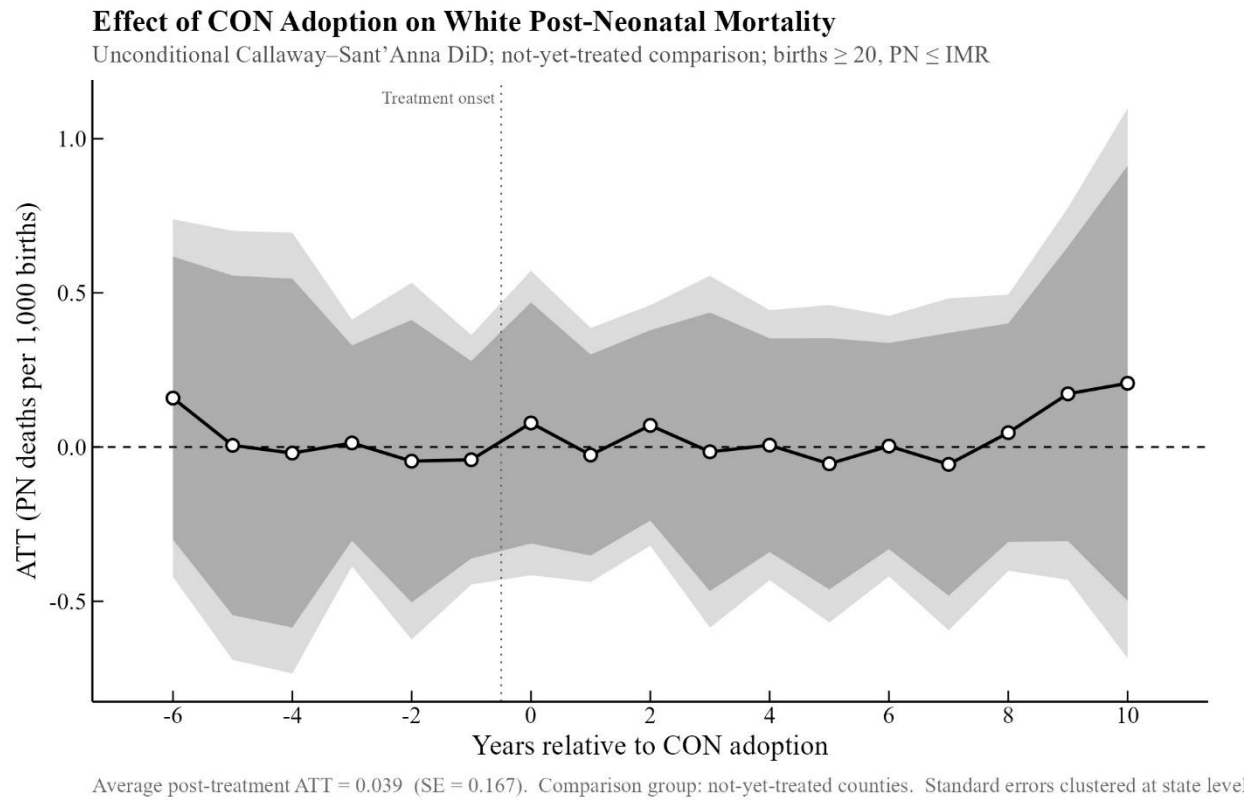
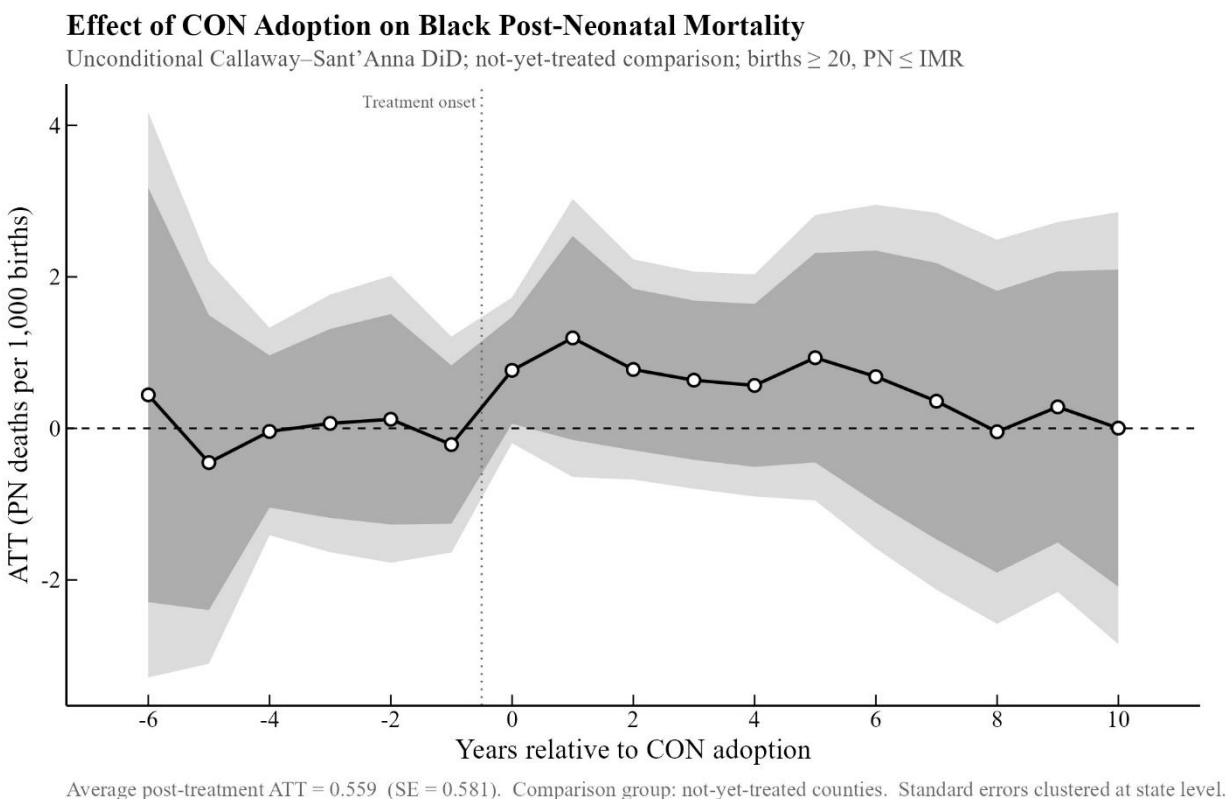


Figure 9. CON Adoption: Black Post-Neonatal Mortality (Unconditional DiD)



CON Repeal: Infant Mortality

The repeal-era IMR results are uniformly small and statistically indistinguishable from zero for the overall and race-specific specifications. The repeal sample spans 1980 onward, by which point baseline infant mortality had declined considerably from adoption-era levels; the pre-treatment average IMR in the repeal sample is approximately 8.44 deaths per 1,000 births, compared to roughly 19.2 during the adoption era. Figure 10 shows that the unconditional overall infant mortality ATT averages 0.151 (SE = 0.371) post-repeal, with a 95% confidence interval of $[-0.576, 0.879]$ that comfortably includes zero. The data are consistent with repeal having no detectable effect on aggregate infant mortality – a null result, though a less precise one than in

the adoption era because the repeal sample has fewer treated cohorts and the overall IMR estimand inherits their variance.

The doubly robust specification for overall repeal-era IMR yields a point estimate of -0.623 ($SE = 29.452$) with a confidence interval that spans roughly ± 58 deaths per 1,000 births. This effectively uninformative interval reflects the instability of covariate adjustment when the number of repeal cohorts is small: the doubly robust estimator provides no additional information beyond the unconditional model in this setting, and the unconditional estimate is the preferred specification. The doubly robust result is reported in Appendix Figure A3 for completeness.

White infant mortality following repeal (Figure 11) yields an average post-treatment ATT of -0.109 ($SE = 0.336$; 95% CI: $[-0.768, 0.550]$), indistinguishable from zero. The Black IMR repeal model (Figure 12) is noisy throughout, with an average post-treatment ATT of 0.666 ($SE = 0.922$; 95% CI: $[-1.141, 2.473]$) and wide confidence bands. The Other race repeal model is similarly uninformative ($ATT = 0.320$, $SE = 1.695$) and is reported in Appendix Figure A6.

CON Repeal: Post-Neonatal Mortality

The post-neonatal mortality results for the repeal era are uniformly small and statistically indistinguishable from zero. The repeal-era pre-treatment average post-neonatal mortality rate is approximately 2.92 deaths per 1,000 births, reflecting the substantial decline in PN mortality from the adoption era. Figure 13 shows that overall post-neonatal mortality was essentially unchanged following CON repeal, with an average post-treatment ATT of -0.170 ($SE = 0.212$; 95% CI: $[-0.586, 0.245]$). The confidence interval rules out effects larger than roughly 0.6 deaths per 1,000 births in either direction — about 20% of the repeal-era baseline — but does not rule

out zero. The event-study plot shows no downward trend in post-treatment estimates, consistent with a delayed supply-side response to the removal of capacity restrictions.

A similar pattern is observed for White post-neonatal mortality (Figure 14), with an average post-treatment ATT of -0.278 ($SE = 0.221$; 95% CI: $[-0.710, 0.155]$). Although the point estimate is negative and might be read as suggestive of a modest protective effect of repeal, the confidence interval comfortably includes zero. The pre-treatment coefficients in both PN repeal models are close to zero and individually statistically indistinguishable from zero, consistent with parallel pre-trends. The Black post-neonatal repeal model yields an average post-treatment ATT of -0.075 ($SE = 0.646$; 95% CI: $[-1.342, 1.193]$), and the Other race repeal PN model yields 0.437 ($SE = 2.214$; 95% CI: $[-3.902, 4.776]$); both are reported in the appendix and are statistically indistinguishable from zero. Taken as a whole, the repeal-era PN results do not provide evidence of a policy effect on post-neonatal mortality at the overall level or in any race subgroup. The doubly robust overall PN repeal specification, reported in Appendix Figure A4, yields a point estimate of -0.317 ($SE = 4.749$) with a confidence interval spanning roughly ± 9 deaths per 1,000 births. As with the repeal IMR doubly robust result, this instability reflects the small number of repeal cohorts available for covariate adjustment, and the unconditional specification is preferred throughout.

Figure 10. CON Repeal: Overall Infant Mortality (Unconditional DiD)

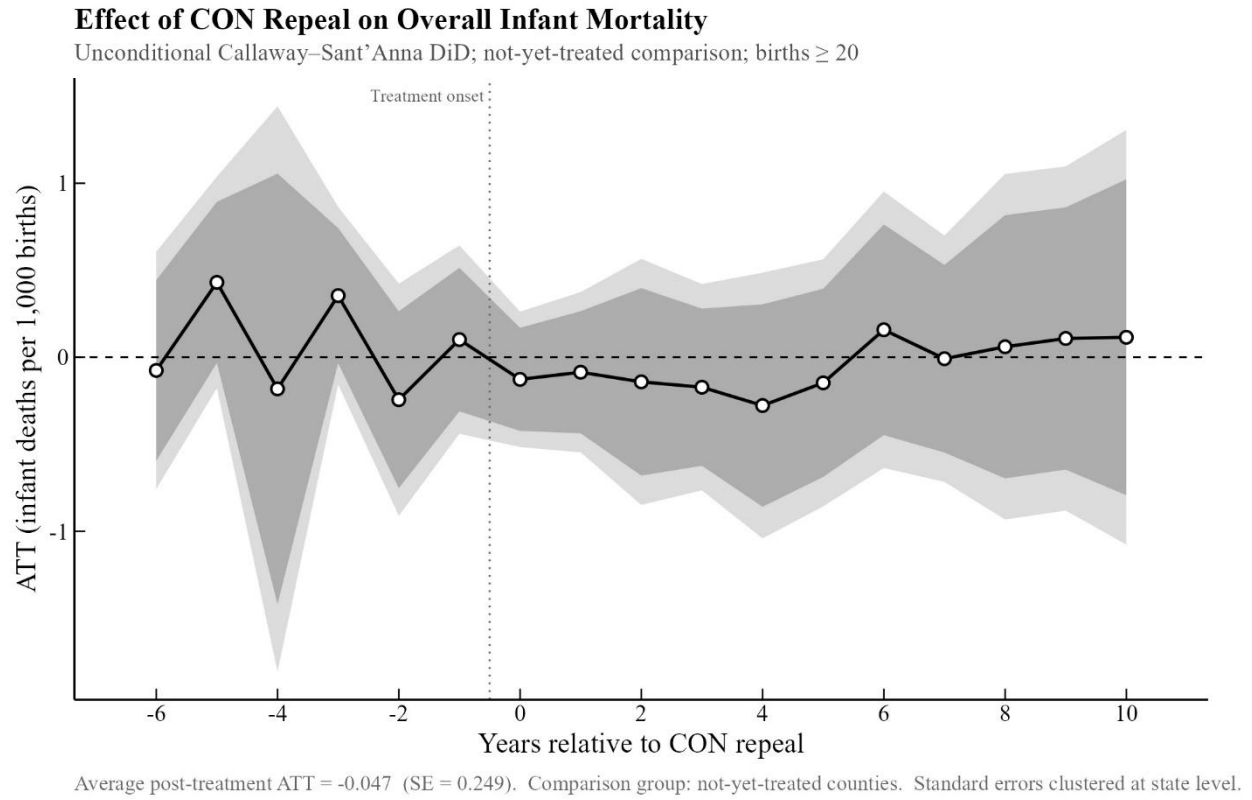


Figure 11. CON Repeal: White Infant Mortality (Unconditional DiD)

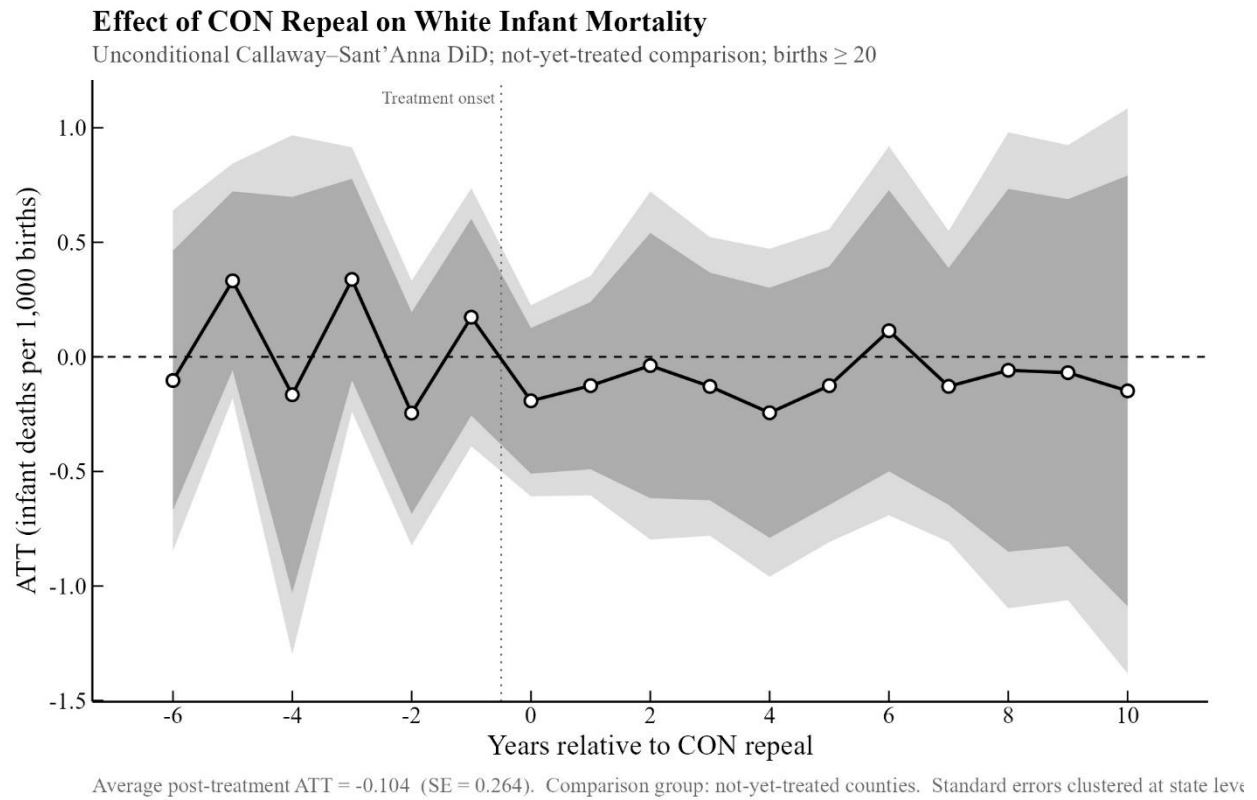


Figure 12. CON Repeal: Black Infant Mortality (Unconditional DiD)

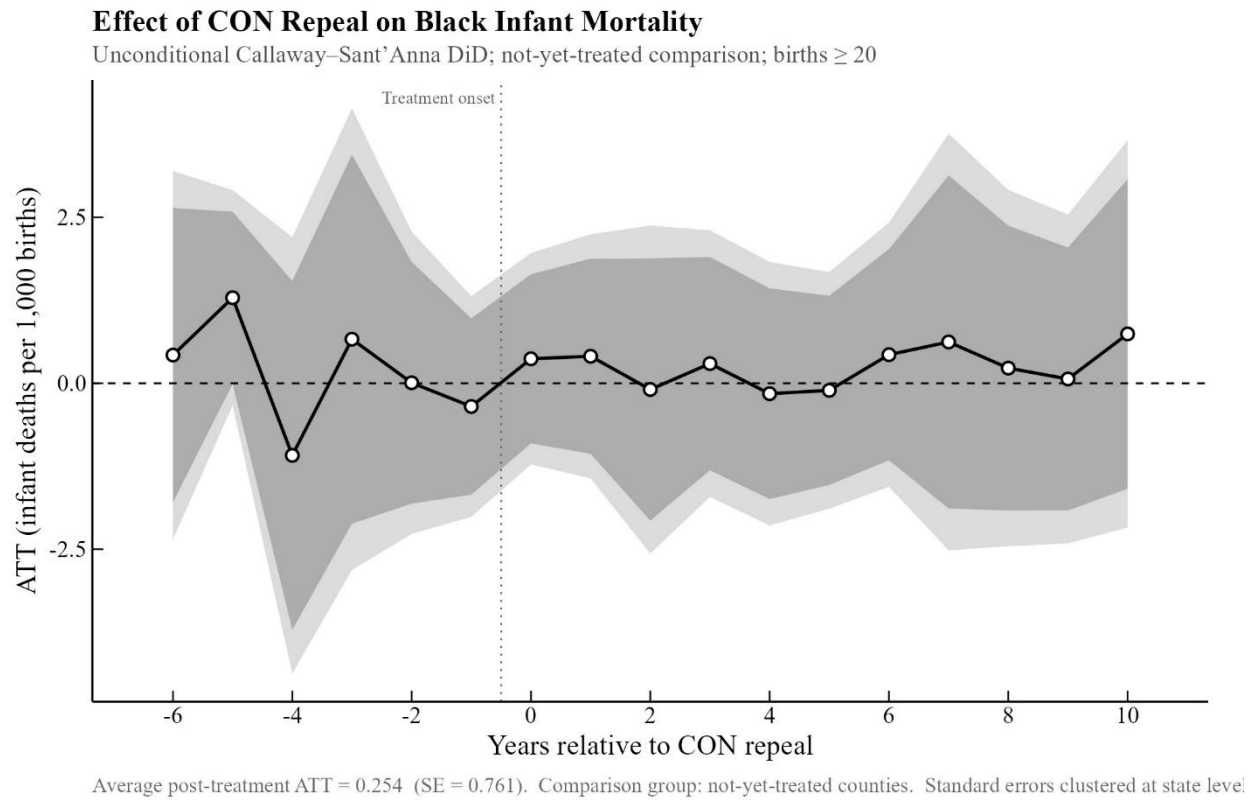


Figure 13. CON Repeal: Overall Post-Neonatal Mortality (Unconditional DiD)

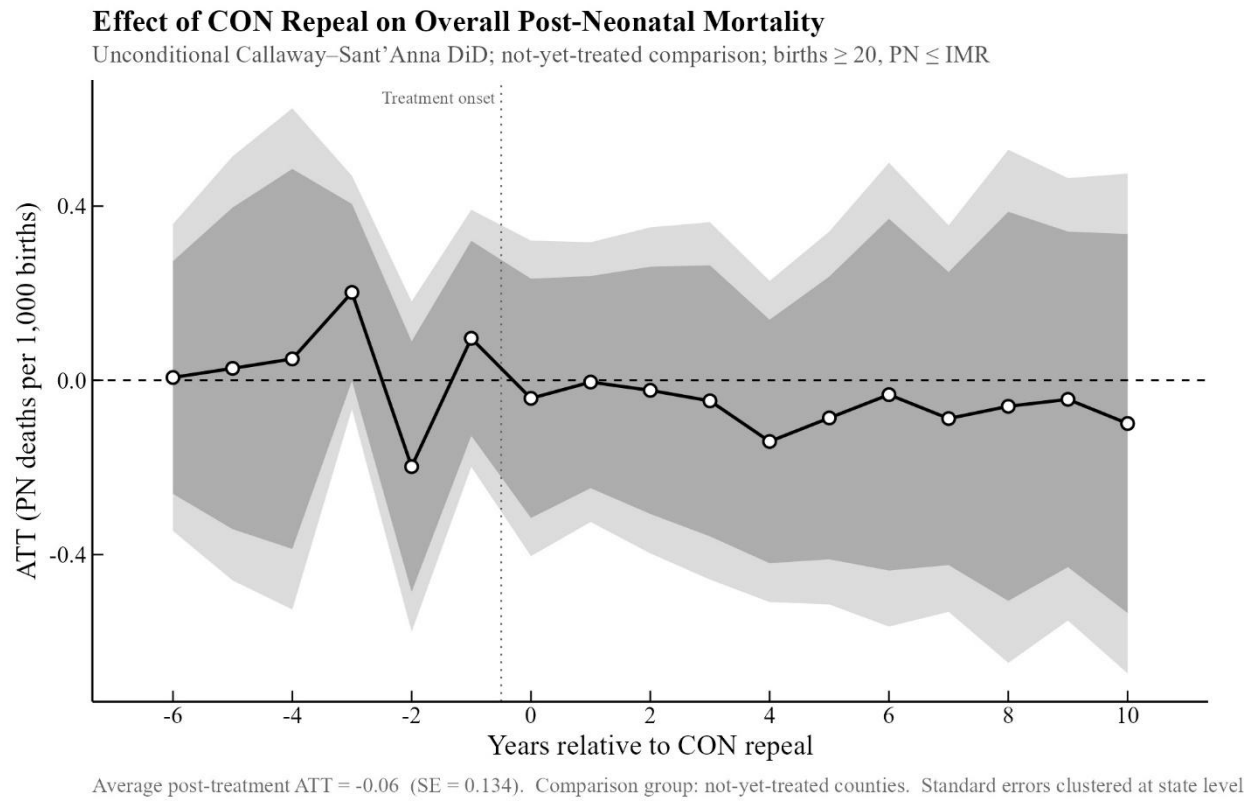


Figure 14. CON Repeal: White Post-Neonatal Mortality (Unconditional DiD)

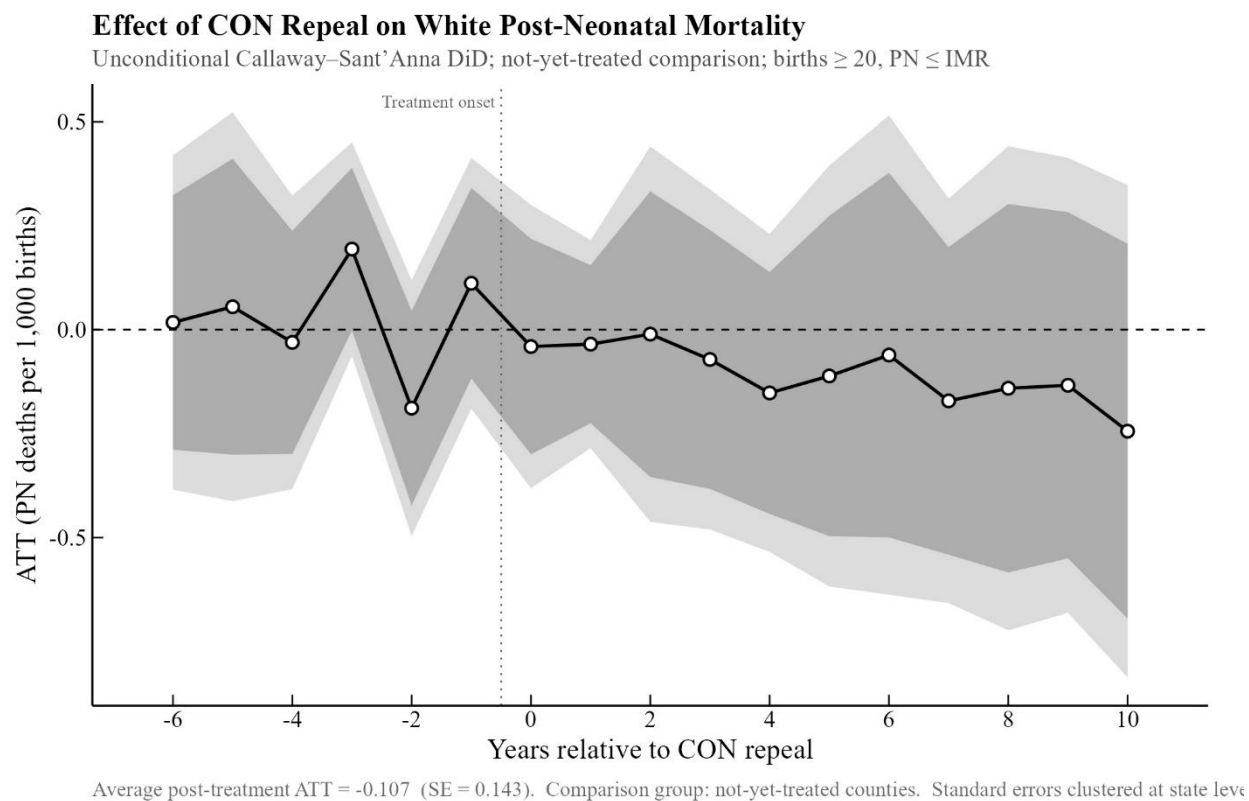
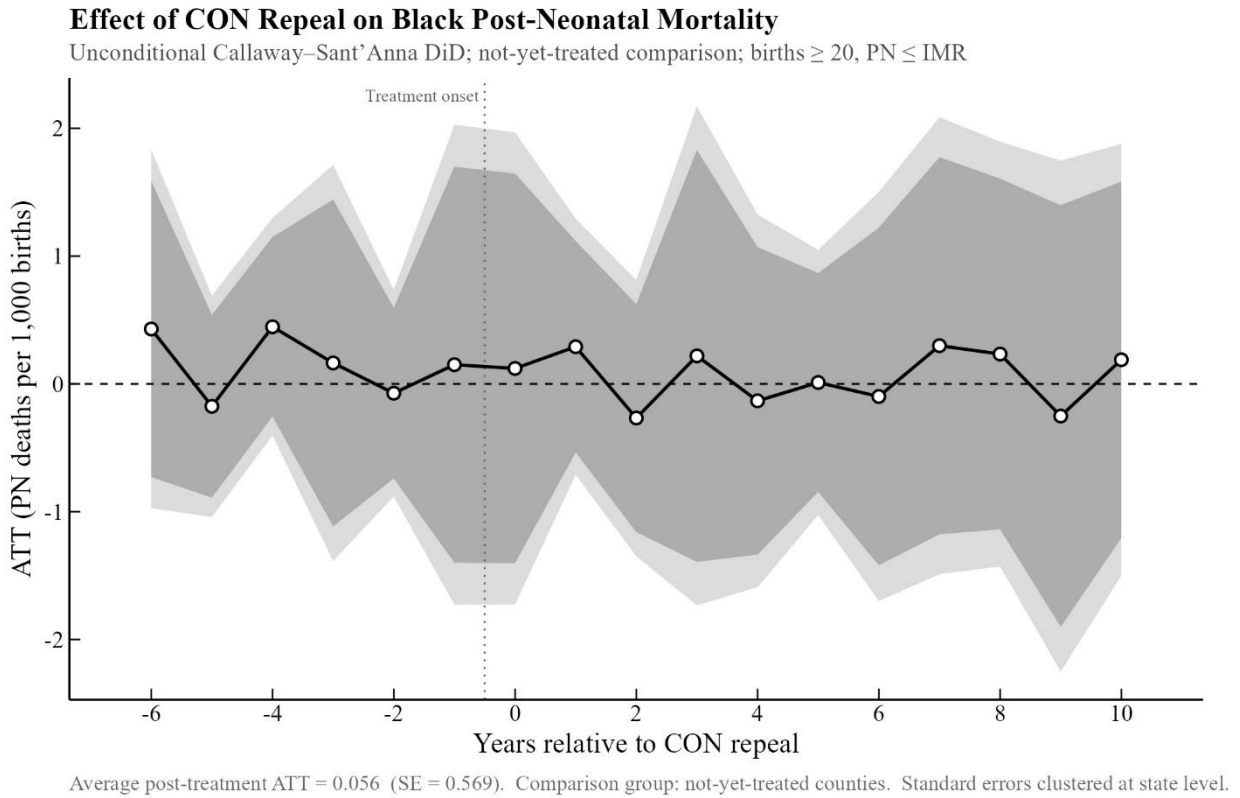


Figure 15. CON Repeal: Black Post-Neonatal Mortality (Unconditional DiD)



SUMMARY OF ATT ESTIMATES

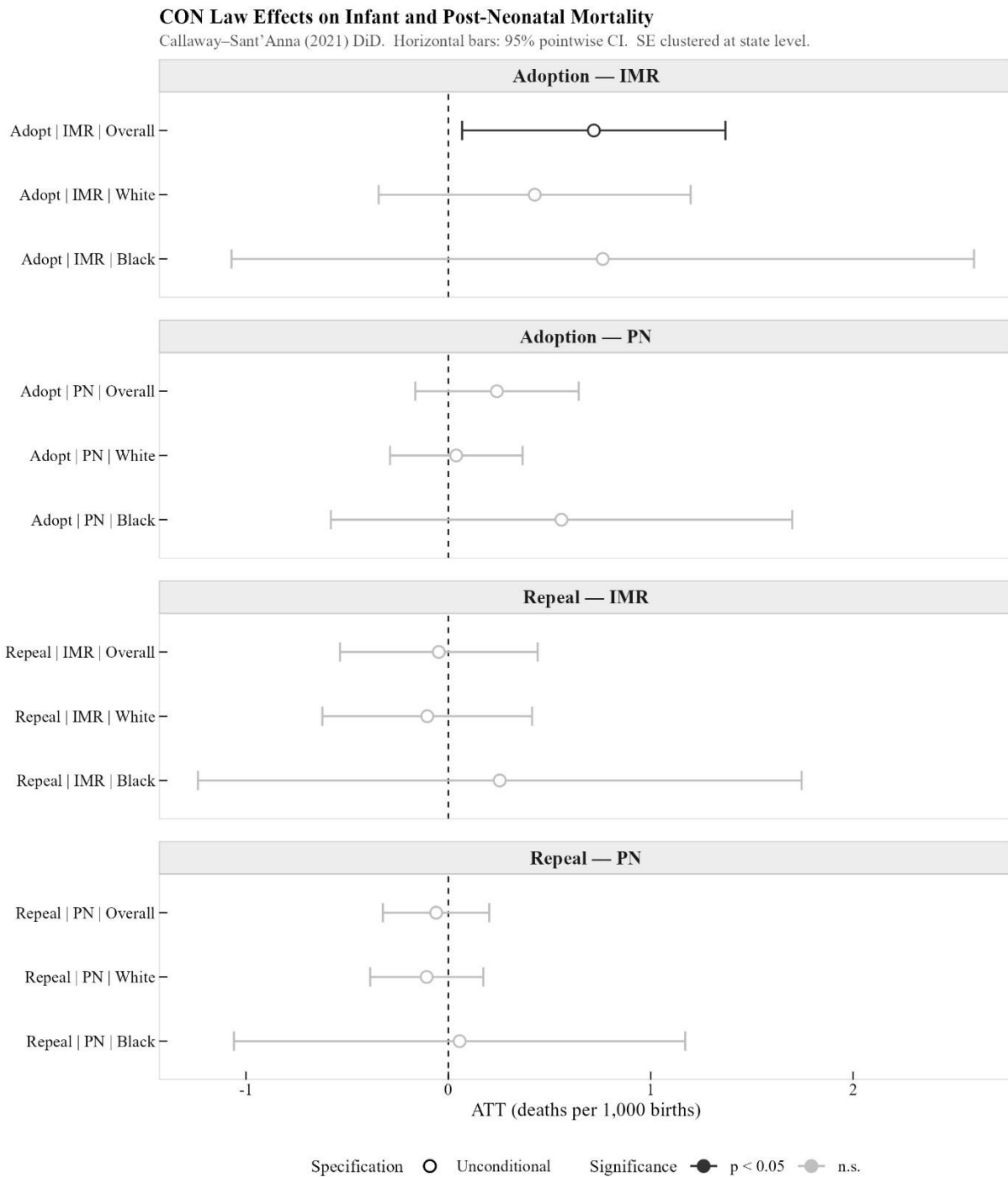
Table 2 summarizes the average post-treatment ATTs from the unconditional specifications across all main analyses. Standard errors are clustered at the state level. An asterisk (*) denotes significance at the 10% level. Doubly robust specifications and Other race models are reported in Appendix Table A1.

Table 2: Summary of ATT Estimates

Era	Family	Outcome	ATT	95% CI
Adoption	IMR	Overall	0.791**	[0.153, 1.428]
Adoption	IMR	White	0.348	[-0.385, 1.082]
Adoption	IMR	Black	0.974	[-1.133, 3.082]
Adoption	PN	Overall	0.287	[-0.145, 0.719]
Adoption	PN	White	0.013	[-0.329, 0.354]
Adoption	PN	Black	0.676	[-0.459, 1.812]
Repeal	IMR	Overall	0.151	[-0.590, 0.892]
Repeal	IMR	White	-0.109	[-0.703, 0.485]
Repeal	IMR	Black	0.666	[-1.164, 2.496]
Repeal	PN	Overall	-0.170	[-0.631, 0.290]
Repeal	PN	White	-0.278	[-0.727, 0.171]
Repeal	PN	Black	-0.075	[-1.358, 1.209]

Table 2. Average Post-Treatment ATTs — Unconditional Specifications. ** $p < 0.05$. ATTs estimated using Callaway and Sant'Anna (2021). Comparison: not-yet-treated counties. SE clustered at state level.

Figure 16: ATT Summary Boxplot



Circle = unconditional; Diamond = doubly robust. Color indicates significance level. ATTs from dynamic (event-study) aggregation. Overall, White, and Black race groups shown. Other race and doubly robust specifications in appendix.

CONCLUSION

This paper provides the first causal estimates of Certificate of Need laws' effects on infant and post-neonatal mortality, exploiting the staggered adoption and repeal of CON programs across states from 1968 to 2019. Using the Callaway and Sant'Anna (2021) difference-in-differences estimator and a county-year panel constructed from CDC Vital Statistics data, I estimate effects separately across two policy margins and two outcome families, with race-stratified analyses motivated by the well-documented and persistent Black-White infant mortality gap.

The main results differ across the two policy margins. On the adoption margin, CON is associated with a small but statistically significant increase in aggregate infant mortality, on the order of 4 percent of the adoption-era baseline. On the repeal margin, the corresponding estimate is a precise null, with a confidence interval that rules out effects larger than roughly 10 percent of the repeal-era baseline in either direction.

The headline result is a statistically significant increase in overall infant mortality of 0.791 deaths per 1,000 births ($SE = 0.325$) following CON adoption, roughly 4 percent of the adoption-era baseline rate. The corresponding repeal-era estimate is a precise null (0.151, $SE = 0.378$), consistent with the possibility that the adoption-era effect was driven by supply conditions specific to the formation period of the U.S. hospital CON regime that no longer bound by the time most states repealed. Race-stratified adoption-era models are directionally supportive – every subgroup ATT is positive, and both the White IMR (0.348) and Black post-neonatal (0.676) point estimates are of a plausible magnitude – but none reach statistical significance once

the panel is stratified. I therefore refrain from drawing subgroup-specific conclusions and treat the overall adoption-era IMR effect as the paper's only statistically supported finding.

Several limitations bear on interpretation. The binary CON indicator does not capture variation in program scope or service-line coverage, which may attenuate estimates if effects operate through specific provisions – such as NICU or obstetric CON – rather than the overall regulatory regime. This limitation is of greater concern in the repeal era models than in the adoption era models during which CON laws were required to follow the structure of the NHPRDA mandate. The race-stratified models are substantially less precise than the overall specifications because of the minimum-births threshold required for stable mortality rate construction, and several subgroup cells could not be estimated for the repeal era at all. The repeal-era doubly robust specifications are unstable, reflecting the small number of available cohorts; the unconditional estimates are preferred throughout.

Taken together, these findings are consistent with a mechanism in which CON entry restrictions during the formation period of the U.S. hospital CON regime reduced the supply of obstetric, neonatal, and outpatient pediatric services at the margin, raising aggregate infant mortality modestly during the adoption era. The absence of a symmetric effect on the repeal margin suggests that whatever bound during the late 1960s and 1970s was no longer the binding constraint on infant health outcomes by the time most states repealed their CON laws in the late 1980s and 1990s — a period over which baseline infant mortality had roughly halved and the relevant service lines had expanded substantially. In the dimension of infant and post-neonatal mortality outcomes, these results suggest that CON laws were at best inefficacious at achieving their stated policy aims in the adoption era, and plausibly inhibited them.

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APPENDIX

ALTERNATE SPECIFICATIONS AND OTHER RACE MODELS

This appendix presents two sets of supplementary estimates. First, doubly robust specifications for overall infant and post-neonatal mortality, for both the adoption and repeal eras. These specifications include county-level demographic controls (percent White, percent Black, and percent female at the county-year level for adoption; baseline values for repeal) and use the doubly robust estimator implemented in the *did* package. In the adoption era, doubly robust estimates are close to their unconditional counterparts. In the repeal era, the doubly robust estimates are substantially less stable, with sign reversals and wide confidence bands, reflecting the small number of repeal cohorts and the resulting fragility of covariate adjustment. The unconditional specifications are the preferred estimates throughout.

Second, this appendix reports results for the Other race subgroup. These estimates are excluded from the main text because the "Other race" category encompasses a heterogeneous and changing population over the 1968–2019 panel period. The racial composition of this residual group shifts substantially across states and over time—reflecting, among other factors, changes in Census classification of Hispanic, Asian, and multiracial individuals—in ways that are likely correlated with both CON policy timing and underlying infant health trends. This makes causal interpretation of Other race estimates particularly difficult, and we treat them as uninformative for the main conclusions of the paper.

Doubly Robust Specifications

Figure A1. CON Adoption: Overall Infant Mortality (Doubly Robust)

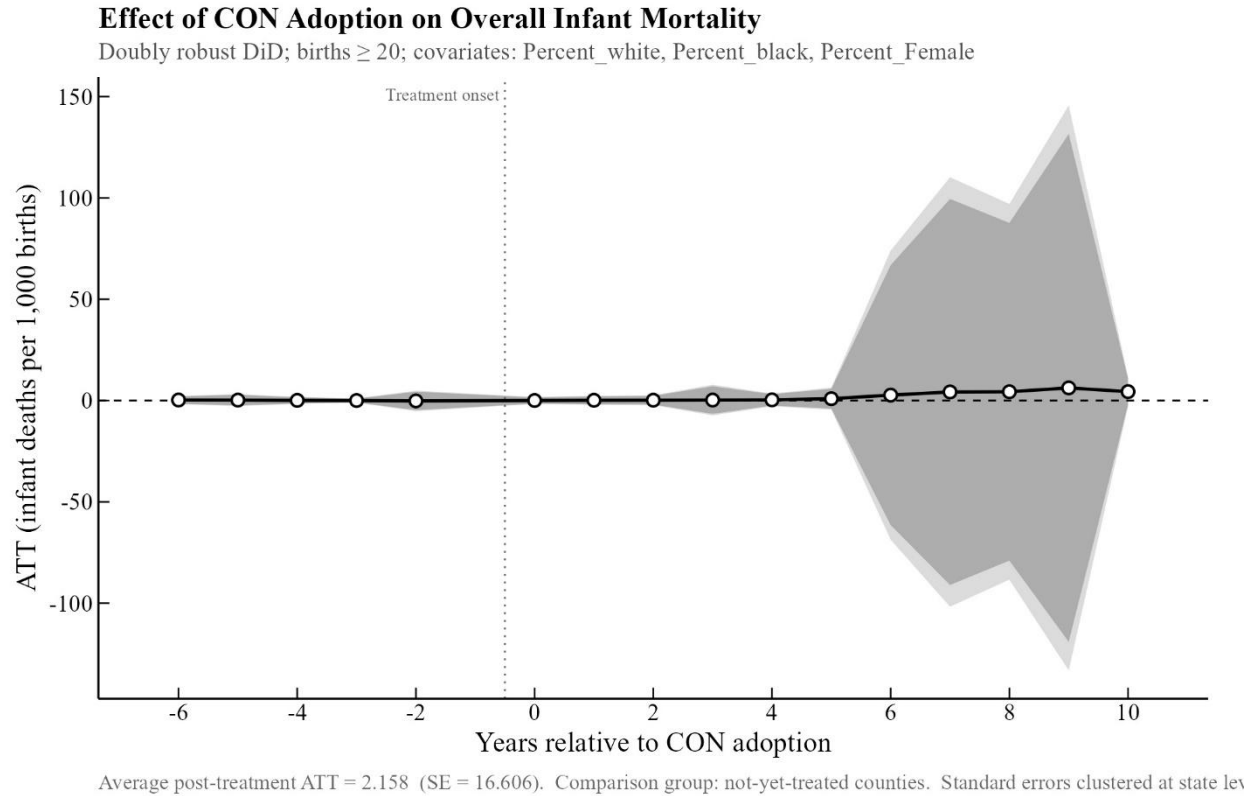


Figure A2. CON Adoption: Overall Post-Neonatal Mortality (Doubly Robust)

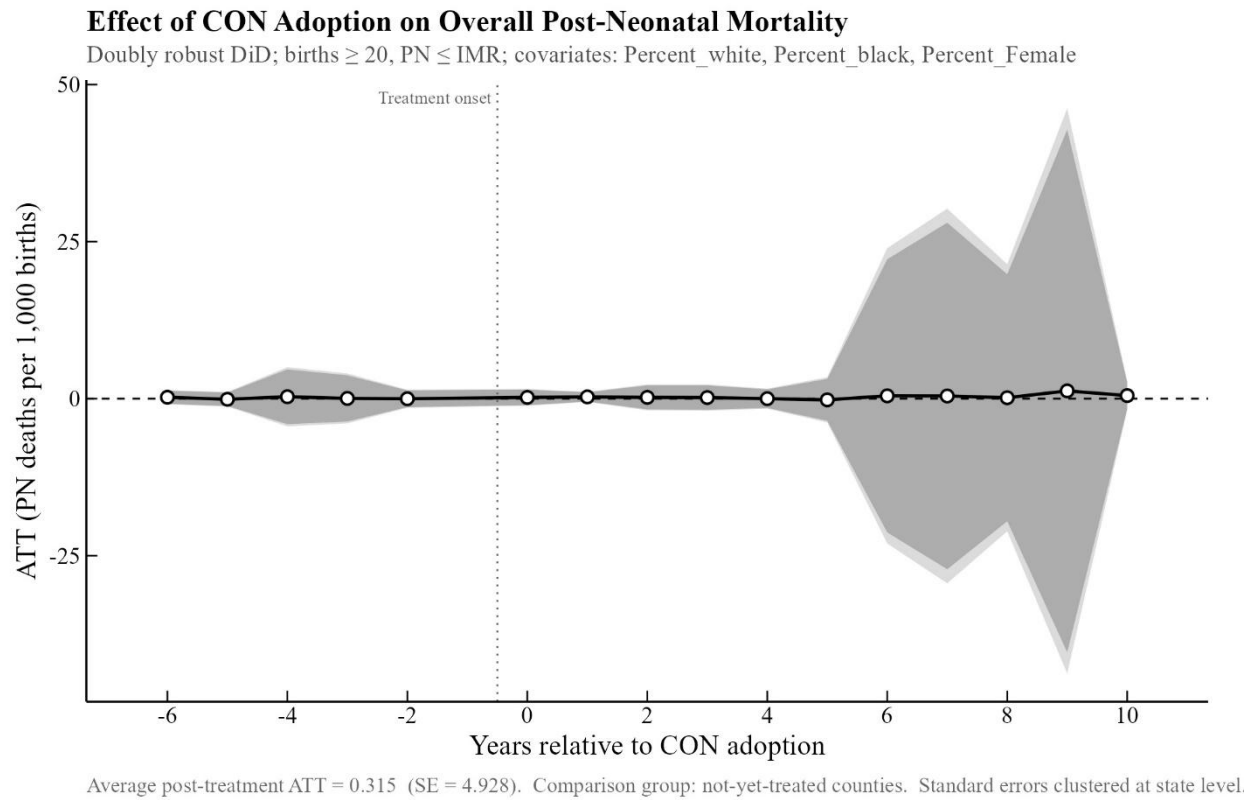


Figure A3. CON Repeal: Post-Neonatal Infant Mortality (Doubly Robust)

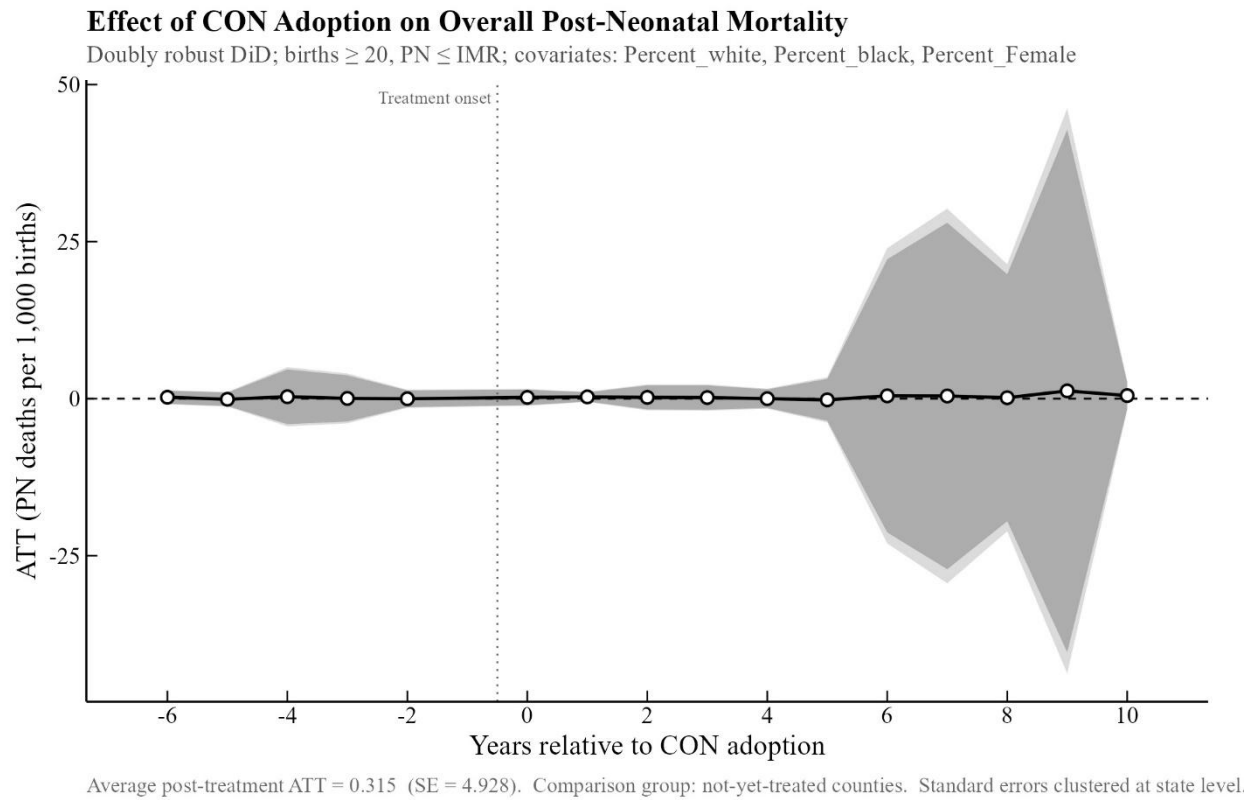
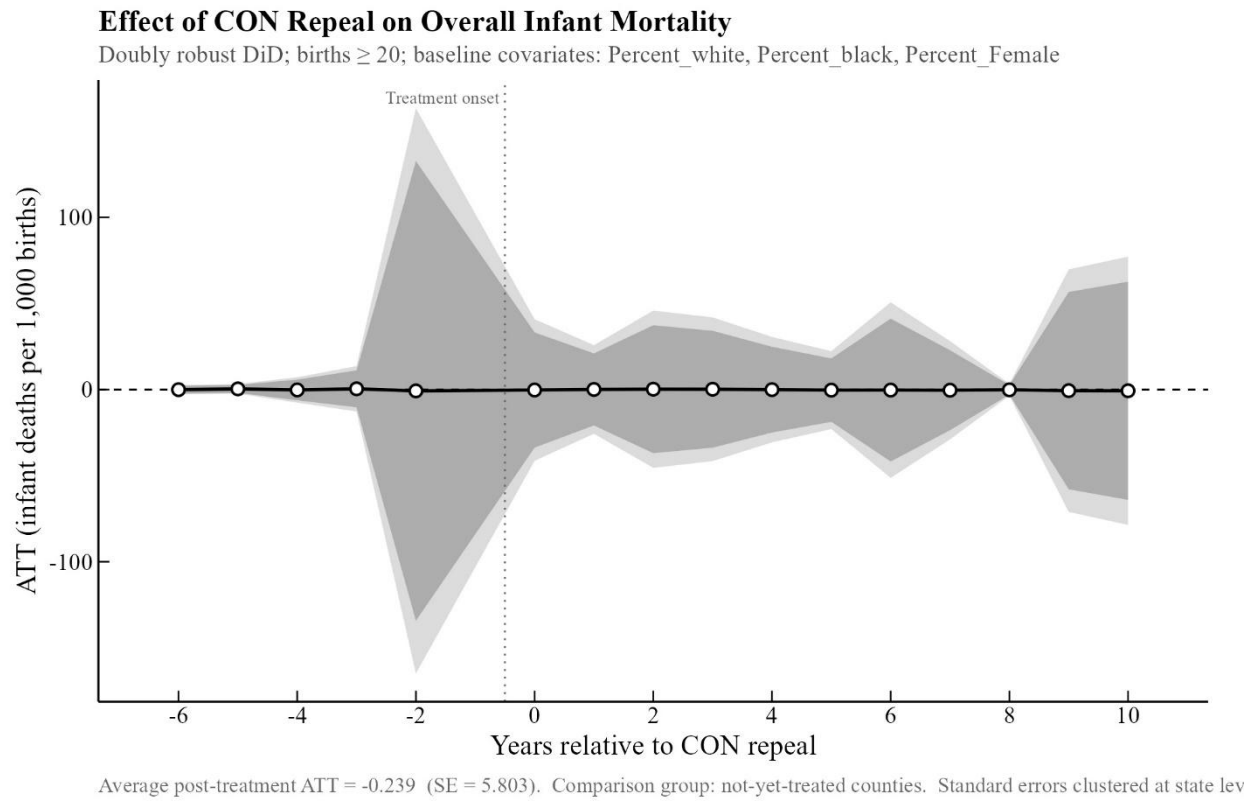


Figure A4. CON Repeal: Overall Infant Mortality (Doubly Robust)



Other Race Models

Figure A5. CON Adoption: Other Race Infant Mortality (Unconditional DiD)

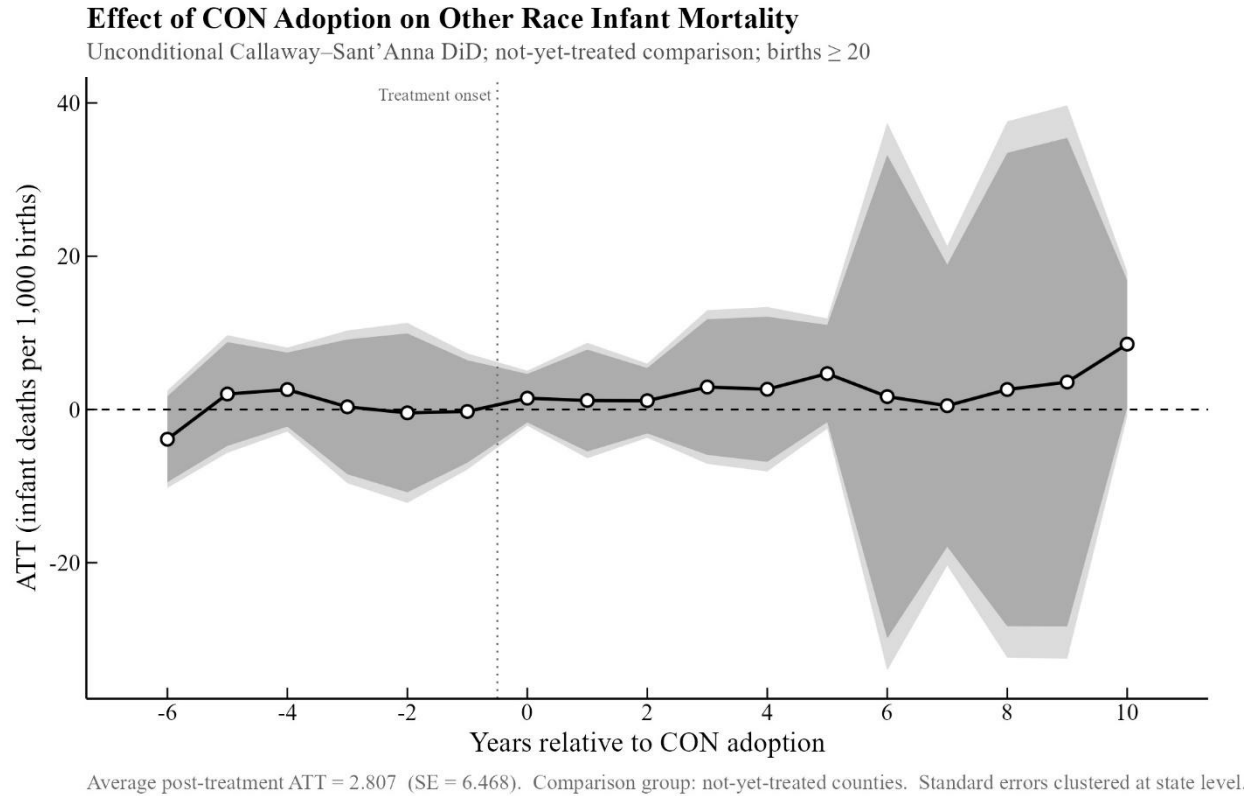


Figure A6. CON Repeal: Other Race Infant Mortality (Unconditional DiD)

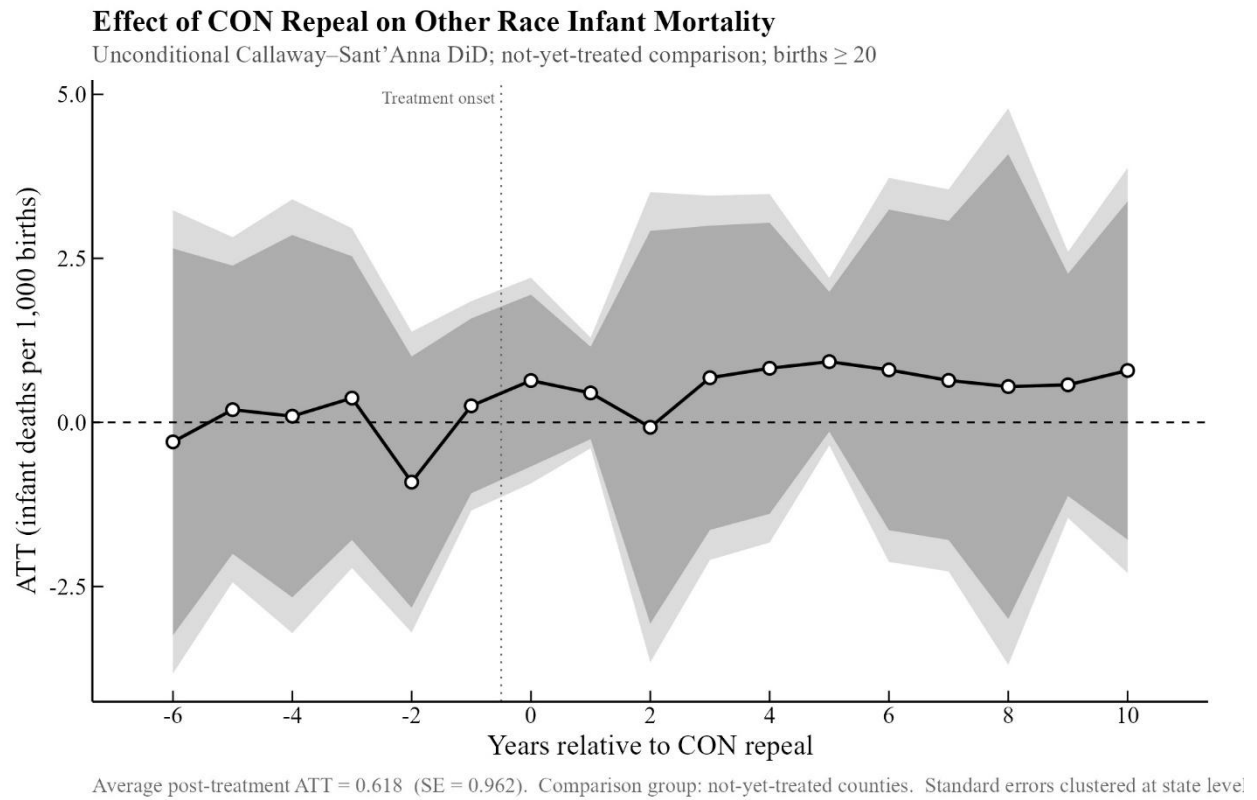
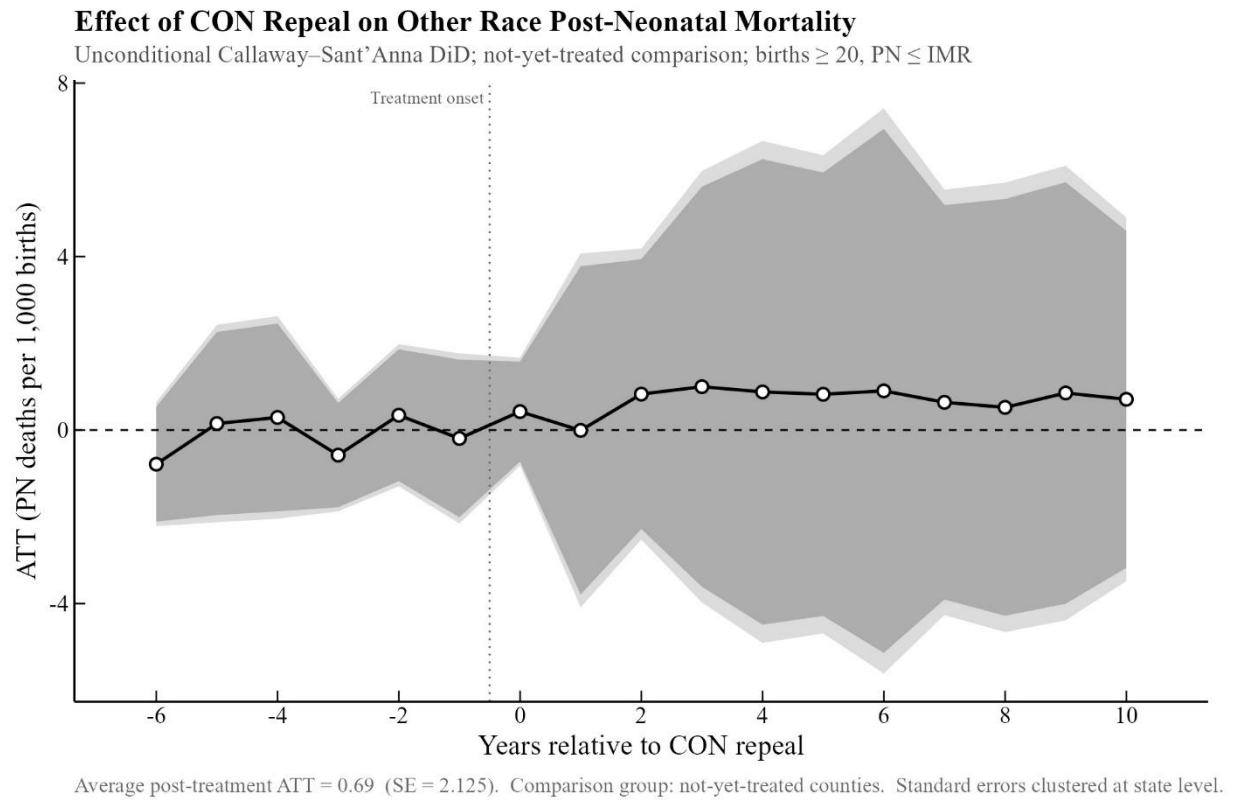


Figure A8. CON Repeal: Other Race Post-Neonatal Mortality (Unconditional DiD)



APPENDIX TABLE A1: SUPPLEMENTARY ATT ESTIMATES

Doubly robust specifications and Other race unconditional estimates. See main text for preferred unconditional specifications.

Table A1. Supplementary ATT Estimates

Era	Family	Outcome	Spec	ATT	95% CI
Adoption	IMR	Overall	Doubly Robust	2.375	[-21.065, 25.814]
Adoption	PN	Overall	Doubly Robust	0.444	[-6.374, 7.261]
Repeal	IMR	Overall	Doubly Robust	-0.623	[-58.001, 56.755]
Repeal	PN	Overall	Doubly Robust	-0.317	[-9.478, 8.844]
Adoption	IMR	Other	Unconditional	3.629	[-9.016, 16.275]
Repeal	IMR	Other	Unconditional	0.320	[-2.998, 3.639]
Repeal	PN	Other	Unconditional	0.437	[-3.871, 4.745]

Table A1. Supplementary ATT Estimates. * $p < 0.10$. ATTs estimated using Callaway and Sant'Anna (2021). Comparison: not-yet-treated counties. SE clustered at state level.